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Trade and Income in the Long Run: Are There Really Gains, and Are They Widely Shared?*

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Abstract

In the cross section of countries, there is a strong positive correlation between trade and income, and a negative relationship between trade and inequality. Does this reflect a causal relationship? We adopt the Frankel and Romer (1999) identification strategy, and exploit countries' exogenous geographic characteristics to estimate the causal effect of trade on income and inequality. Our cross-country estimates for trade's impact on real income are consistently positive and significant over time. At the same time, we do not find any statistical evidence that more trade increases aggregate measures of income inequality. Heeding previous concerns in the literature (e.g. Rodriguez and Rodrik, 2001; Rodrik, Subramanian and Trebbi, 2004), we carefully analyze the validity of our geography-based instrument, and confirm that the IV estimates for the impact of trade are not driven by other direct or indirect effects of geography through non-trade channels.

Keywords: Trade, Growth, Income, Inequality, Gravity model.

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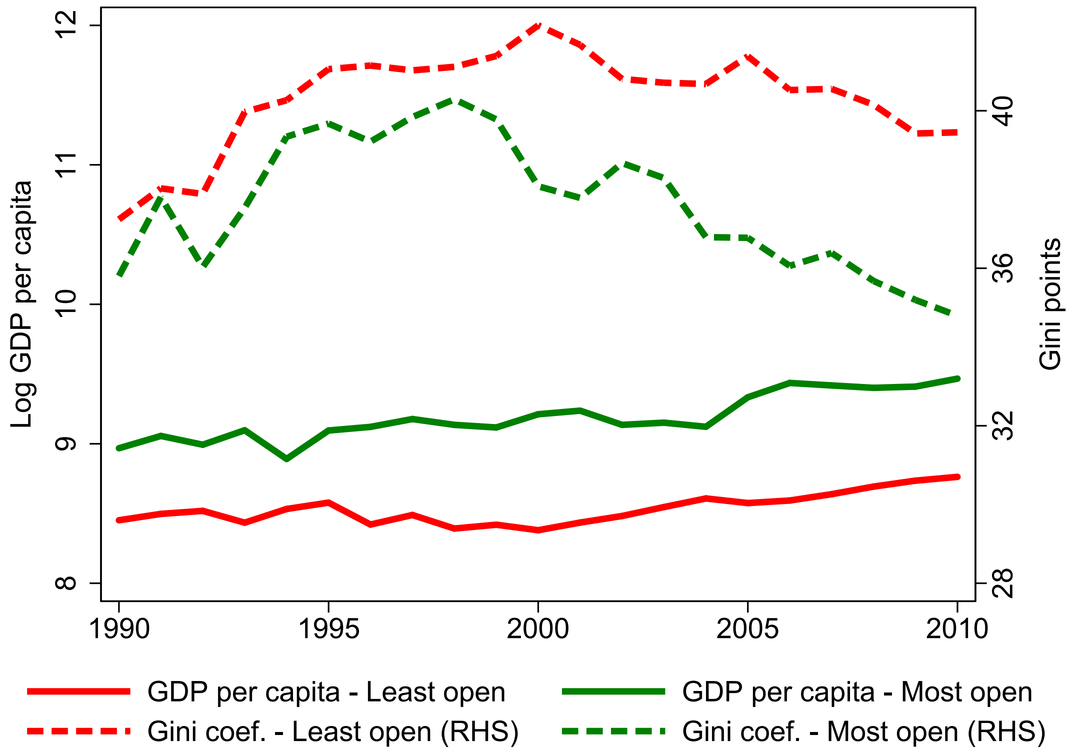
1 Introduction

The virtues of trade – or lack thereof – for the level and distribution of income have recently come under close public scrutiny. General answers are hard to come by, as the effects of opening up to trade may depend on specific characteristics of a country or sector at a given point in time. Cross-country analysis, which exploits heterogeneity among economies with very different living standards, can however shed light on the following question: is trade good for income and equity in the long run? This paper aims to provide answers to this question by building and expanding on previous work that uses geography to extract exogenous variation in countries' trading patterns.

On the face of it, we observe a strong positive correlation between trade and income, and a negative correlation between trade and inequality in the cross section of countries; countries with higher trade openness (exports plus imports as a share of GDP) tend to have higher living standards and lower income inequality. The gap between more open and less open economies in terms of their GDP per capita and income Gini coefficient is persistent, and, if anything, it has widened in the last two decades (Figure 1). However, inferring causality from this pattern is complicated. Trade openness is arguably endogenous in these simple bivariate relationships as many variables that affect income and inequality directly may also be correlated with trade itself. For example, countries that adopt open trade policies may also pursue other market-friendly domestic policies and conduct stable fiscal and monetary policies. Since these policies are likely to affect income and inequality, trade openness is likely to be correlated with important factors that are omitted from this naïve approach.

One strand of the literature attempts to exploit countries' exogenous geographic characteristics to achieve causal identification. The empirical success of the gravity model of trade demonstrates that geography is a powerful determinant of bilateral trade (e.g. Head and Mayer, 2014). The seminal paper by Frankel and Romer (1999, henceforth FR) showed that one can use this insight to construct an instrument for countries' overall trade openness. In

Figure 1: Income and inequality by level of trade openness



Notes: Countries are sorted into three tertiles each year based on their trade openness. The chart shows simple averages of log real GDP per capita (PPP-adjusted) and the net (post-tax,post-transfers) Gini coefficient (on a 0-100 scale) for the top and bottom tertiles.

particular, they estimate a gravity equation that includes only geographical variables such as bilateral distance, area, and whether the countries are landlocked, and they aggregate the fitted values to obtain the predicted trade openness of each country. FR argue that the included geographic characteristics are unlikely to have important effects on countries' income except through their impact on trade. Thus, the constructed trade openness can be used to obtain instrumental variables estimates for trade's impact on income.

In this paper, we adopt the FR identification strategy to analyze the effect of trade on income and inequality, and investigate the robustness of the results over time and to alternative specifications. We extend the work of FR in four directions. First, in addition to real income per capita, we also estimate the impact of trade openness on various measures of within-country inequality. Second, instead of focusing on one cross-section of countries at a given point in time, we utilize more complete annual data from 1990 to 2015, and check whether the estimated effects are stable qualitatively and quantitatively over time.[1] Third, as an improvement in the econometric methodology, we employ the Poisson pseudo-maximum likelihood estimator to fit our gravity model of bilateral trade, which has a number advantages over simple OLS. Finally, we pay particular attention to establishing the validity and to testing the strength of our instrument. For example, in robustness checks we address a prominent early criticism of FR's trade instrument (Rodriguez and Rodrik, 2001), and control for climate and institutions which may be correlated with geographical factors.

Our cross-country estimates for trade's impact on real income are consistently positive and significant over time. The results indicate that a one percentage point increase in trade openness raises real income per capita by between 2 and 5 percent, with the lower estimates obtained for the period after the global financial crisis – a feature we relate to cyclical factors affecting the long-run estimate. At the same time, we find that, if anything, trade tends to reduce overall income inequality. The point estimates suggest that one-percentage-point higher openness causes the income of the bottom decile to increase by about 4 percent relative to the income of the top decile of the income distribution. When measuring income inequality with the Gini coefficient, most point estimates also suggest an inequality-reducing effect of trade. Although the estimated impact of trade on inequality is almost always negative, in many cases the coefficients appear insignificant according to standard asymptotic tests. Moreover, the weak instrument diagnostics signal lower reliability of the conventional IV inference in these specifications. Therefore, we adopt a cautious interpretation of the results, and emphasize the *lack of statistical evidence* for an inequality-inducing effect of trade in the long run.

[1] The original results in Frankel and Romer (1999) were based on a limited dataset from 1985. In particular, the bilateral trade data underlying the construction of the instrument contained only 63 economies, and they used the estimated coefficients from this smaller sample to predict trade openness for the remaining countries. The same dataset seems to have been re-used in the subsequent literature (Hall and Jones, 1999; Rodriguez and Rodrik, 2001; Rodrik, Subramanian and Trebbi, 2004). Noguer and Siscart (2005) extended the country coverage to 97 economies, but still utilized only the 1985 cross section. In contrast, our bilateral trade data contain 147-173 countries depending on the year, and we only use a country in our income and inequality regressions if we have its reported bilateral trade flows.

Our results are qualitatively unaffected under various robustness checks. For example, in our baseline regressions we calculate openness using only merchandise trade, but we confirm that the instrument is also relevant for trade openness including services. We also control for the direct effect of geography through climate and the indirect effect through the historical development of institutions. We show that previous findings about the irrelevance of trade compared to institutions (the "primacy of institutions" result of Rodrik, Subramanian and Trebbi (2004)) can be attributed to important sample selection issues rather than the overall predominance of institutions. Our robustness checks support the qualitative message from our baseline regressions: trade improves living standards and there is no statistical evidence for any negative effect on aggregate income inequality. However, an important limitation is that the methodology can only provide suggestive evidence for the effects of trade policies. If geography-induced trade barriers have different impacts than policy-induced barriers, our results may not be directly informative for the makers of trade policy.

Related literature. Our work is closely related to cross-country studies that exploit geography to capture an exogenous component of trade, as in the pioneering work of Frankel and Romer (1999). In this context, Rodriguez and Rodrik (2001) have argued that geography can also affect income through its effect on public health, institutions and natural endowments (see also Rodrik, Subramanian and Trebbi, 2004). Dollar and Kraay (2003) point out that, due to the very high correlation between trade and measures of institutional quality, regressions including both variables tend to be uninformative. A critical survey of these discussions can be found in Hallak and Levinsohn (2004). More recently, Feyrer (2009*b*) circumvents the problem that physical distance can have an effect on income through non-trade channels by exploiting the effective shortening of some bilateral distances due to improvements in air transportation technology over time. Using a time-varying instrument based on geographic fundamentals, this paper also finds a positive effect of trade on income. Similar qualitative results are also found in Feyrer (2009*a*), who uses the 1967-1975 closing off of the Suez Canal as a natural experiment to investigate the trade-income causal relationship.

None of the studies mentioned above consider the effect of trade on income inequality, which is an integral part of our analysis. A growing empirical literature has used within-country micro data to look at the effect of trade shocks on income and wage inequality (for an overview, see for example Goldberg, 2015; Helpman, 2016).[2] Worker-level data for a single country has the advantage that one can control for all macro-level shocks and characteristics. However, it has the drawback of potentially low external validity. Thus, our paper complements this literature by exploiting cross-country variation in different measures of income inequality, and providing answers about the long-run effects of trade on countries' income distribution. A recent paper by Fajgelbaum and Khandelwal (2016) studies the distributional impact of relative price changes caused by international trade, considering that poor and rich households have different consumption baskets. They find that the price-effects of trade typically favor the poor, which reinforces our results that are based only on the distribution of nominal income.

The rest of the paper is organized as follows. Section 2 describes the methodology. Section 3 presents the baseline results, an analysis of the validity and strength of the instrument and the robustness checks. Section 4 concludes.

2 Methodology and Data Sources

The FR conceptual framework

Let O denote a country-level outcome variable of interest such as real income per capita or a measure of inequality. For each year in which data are available, we want to obtain an estimate of the (long-run) effect of trade on this outcome variable.[3] Consider, then, the

[2]Relatedly, Autor, Dorn and Hanson (2013) show that imports from China had large differential employment effects across U.S. commuting zones, where employment declined more in zones whose industries were more exposed to import competition from China. Note that this finding does not imply that trade integration was responsible for an increase in overall income inequality in the United States. To determine the aggregate inequality effects, one has to take into account the initial position of the affected workers in the income distribution.

[3]Since the construction of the instrument relies on some time-invariant geographical characteristics, we

reduced-form equation relating the outcome to a country's international and internal trade,

$$O_i = \alpha + \beta T_i + \gamma W_i + \varepsilon_i, \tag{1}$$

where T_i is country i's international trade (for example, exports *plus* imports over GDP), W_i is within-country trade, and ε_i captures other determinants of O_i. Equation (1) is reduced-form in the sense that it does not shed light on the specific channels through which trade's effect on O_i operates.

International and within-country trade are partly determined by geographical characteristics. Let P_i denote country i's proximity to other countries, S_i be a measure of the country's size, and δ_i and ν_i be other factors influencing i's international and within-country trade. We can then write

$$\begin{aligned} T_i &= \psi + \phi P_i + \delta_i, & (2)\\ W_i &= \eta + \phi S_i + \nu_i. & (3) \end{aligned}$$

Given that estimation will rely solely on cross-sectional variation, we will need data for as many countries as possible. Since a coherent dataset of internal trade statistics is not available, we proceed as in Frankel and Romer (1999) and focus only on estimating the effects of international trade.[4] To do so, substitute for W_i in (1) using (3), and obtain

$$O_i = (\alpha + \gamma\eta) + \beta T_i + \gamma\phi S_i + (\gamma\nu_i + \varepsilon_i). \tag{4}$$

For almost any conceivable outcome variable O_i, residual ε_i will be correlated with resid-

can only use cross-sectional variation to identify the effects of trade.

[4]To be precise, the problem of including internal trade lies in the fact that the concept is not as unambiguously defined as that of international trade. Every cross-border sale is international trade. Measuring internal trade requires some convention – it could be defined e.g. as all sales across domestic sectors as defined in input-output matrices, or all sales by firms (a superset of the sales considered in the input-output measure), etc. To the extent that domestic trade includes trade in intermediate inputs, these measures will differ and the choice of the unit of analysis (sector, firm, etc.) will thus be crucial.

uals δ_i and ν_i. If O is real income, then for example behind-the-border regulatory barriers will affect both income and the level of internal and international trade. This implies that T_i will generally be endogenous, and OLS estimates of (4) deliver inconsistent estimates of β. However, the fact that proximity P_i affects T_i through equation (2) can be used for identification. Being a physical characteristic, clearly O_i cannot influence P_i. Key, then, for P_i to be uncorrelated with $(\gamma\nu_i + \varepsilon_i)$, and thus be a valid instrument, is that proximity does not affect income through a channel different from international trade.[5]

A geography-based instrument

The aim is to extract the cross-country variability of openness that is due to geographical determinants. Let t_{ij} denote trade between country i and country j in nominal terms. A log-linear model of the geographical determinants of this trade (expressed as fraction of i's GDP, Y_i, as in the openness measure) would be

$$\begin{aligned}\ln(t_{ij}/Y_i) \;=\;& b_0 + b_1 \ln D_{ij} + b_2 \ln N_i + b_3 \ln N_j + b_4 \ln A_i + b_5 \ln A_j + b_6(L_i + L_j) + b_7 B_{ij} \\ & + B_{ij}\left[b_8 \ln D_{ij} + b_9 \ln N_i + b_{10} \ln N_j + b_{11} \ln A_i + b_{12} \ln A_j + b_{13}(L_i + L_j)\right] \\ & + e_{ij}, \qquad (5)\end{aligned}$$

where D_{ij} is the distance between i and j, N_i is population size, A_i is area, L_i is a dummy taking value 1 if the country is landlocked, and B_{ij} a common-border dummy. Note, thus, that both area and population are included as measures of a country's size.

Instead of estimating the log-linear equation (5) via OLS as in Frankel and Romer (1999), we use the Poisson pseudo-maximum likelihood (PPML) estimator popularized by Santos-Silva and Tenreyro (2006). PPML has several desirable properties for our application: (i) it admits zero bilateral trade flows which need to be dropped for OLS due to the necessary

[5] Given the negative correlation between a country's size and its proximity to other countries, the inclusion of a size measure S_i in the estimation as in (4) is essential for P_i to be a valid instrument.

logarithmic transformation; (ii) it remains consistent even if the error term in the original nonlinear gravity relationship is heteroskedastic; and (iii) the fitted values directly yield an estimate of the *level* of bilateral trade, whereas OLS requires further assumptions to move from the estimated log-linear relationship to the predicted trade levels.

Equipped with these consistent estimates of $\mathbb{E}(t_{ij}/Y_i|\cdot)$, we can construct our instrument as

$$\widehat{T}_i = \sum_j \widehat{t_{ij}/Y_i}.$$

This instrument will be used to estimate our baseline specification

$$O_i = a + bT_i + c_1 \ln N_i + c_2 \ln A_i + u_i,$$

where, again, O_i is a measure of real income or inequality, T_i is trade openness, and N_i and A_i are population and area. This specification directly corresponds to equation (4) derived within the simple conceptual framework, noting that we include two measures of size. For O_i, we will consider the following outcome variables: log real GDP per capita (PPP-adjusted), log ratio of the average income of the top and bottom income deciles, and the market and net Gini cofficients. The market Gini is based on the distribution of household market (pre-tax, pre-transfer) income, while the net Gini is based on household disposable (post-tax, post-transfer) income.

Data sources

Our dataset covers the period 1990-2015. Data on bilateral merchandise exports come from the IMF's Direction of Trade Statistics (DOTS). The advantage of this dataset is the extensive country and time coverage, with data available for more than 180 countries up to

2015. The downside is that it only reports trade in goods.[6] Data on population, land area and real income per capita were downloaded from the World Bank's World Development Indicators (WDI) database. Bilateral distance between countries, distance from the equator and the dummy variables for common border and being landlocked come from CEPII's GeoDist database. For the bilateral distance variable, we use the population-weighted distance between major cities in the two countries.

We use the World Panel Income Distribution database by Lakner and Milanovic (2015) to construct countries' ratio of top-to-bottom income per capita. Country-level income distribution data have heterogeneous periodicity and frequency across countries, and Lakner and Milanovic (2015) allocate each country's vintage to the closest five-year intervals between 1988 and 2008. Since our gravity estimates start in 1990, the income ratio data will be used to obtain four different cross-section estimates of the effect of trade on the income ratio (1993, 1998, 2003, 2008). Gini data come from the Standardized World Income Inequality Database (SWIID, Solt, 2016). The SWIID relies extensively on imputed data across and within countries (see e.g. Jenkins, 2015). Imputation is particularly prevalent for countries in less developed regions. We use this dataset given its extensive cross-country coverage, but these issues naturally call for care in the interpretation of the results.

We use the Rule of Law index from the Worldwide Governance Indicators (WGI) project as a measure of institutional quality. Countries' average temperature between 1961-1999 (a proxy for climate) is available from the World Bank Climate Change Knowledge Portal. The settler mortality rates used in Acemoglu, Johnson and Robinson (2001) were downloaded from Daron Acemoglu's website.

[6]For our results where openness in goods and services is considered (using the goods-only instrument based on DOTS data), we rely on balance of payments data from the World Economic Outlook database.

3 Trade, Income, and Inequality

3.1 Construction and Quality of the Instrument

The estimated bilateral trade equations conform to our expectations and are quite stable over time. Table 1 reports the coefficient estimates and standard errors for three separate annual cross sections chosen from the beginning, middle and end of our sample period. Distance has a large and overwhelmingly significant negative impact on bilateral trade with an elasticity of 0.9-1.0.[7] Moreover, the point estimate on distance and its precision are remarkably stable since 1995 when most post-soviet countries had joined the reporting economies in our trade database (Figure 2a). Trade between country i and country j is strongly increasing in j's market size; the elasticity with respect to j's population is about 0.8. In addition, trade (as a fraction of i's GDP) is decreasing in i's market size and in j's area. Similarly, being landlocked is a major impediment to a country's international trade. Since only a relatively small fraction of country pairs share a border, the coefficients on the common border interactions are not estimated precisely. However, point estimates suggest that sharing a border has a considerable direct effect on trade and it also alters the effects of the other variables substantially.

Because geography is a strong determinant of bilateral trade, our constructed instrument for overall trade openness is likely to be highly relevant. The R^2 of the bilateral trade regressions ranges between 0.15-0.40, with a much higher share of the variance explained in the second half of the sample period than in the earlier years.

After aggregating across trading partners, we can check if the geographic component of countries' overall trade contains sufficient information to be useful as an instrument for actual trade openness. Figure 2b shows the point estimate and t-statistic of the first stage regression of the IV procedure. Controlling for country size, the estimated geography-induced openness

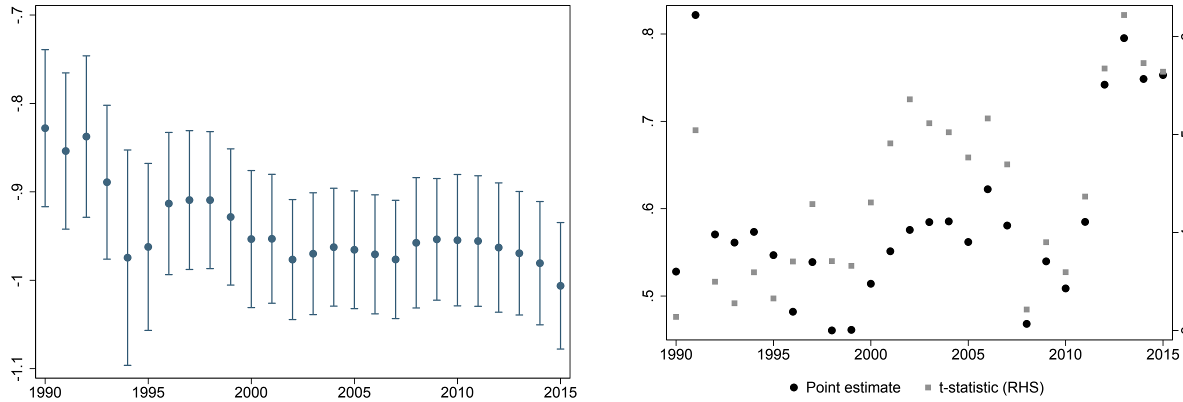
[7] This range of point estimates falls well within typical estimates found in the literature. For example, in their meta-analysis covering 1,467 estimates from 103 papers, Disdier and Head (2008) find a mean estimated elasticity of -0.9.

Table 1: Bilateral trade equation

	Year = 1995	2005	2015
Constant	-13.24	-13.02	-13.55
	(0.30)	(0.24)	(0.25)
Ln distance	-0.96	-0.97	-1.01
	(0.05)	(0.03)	(0.04)
Ln population	-0.18	-0.11	-0.12
(country i)	(0.04)	(0.03)	(0.03)
Ln area	-0.01	-0.02	-0.03
(country i)	(0.03)	(0.03)	(0.02)
Ln population	0.84	0.78	0.84
(country j)	(0.04)	(0.03)	(0.03)
Ln area	-0.23	-0.13	-0.13
(country j)	(0.05)	(0.04)	(0.03)
Landlocked	-0.70	-0.82	-0.76
	(0.08)	(0.06)	(0.06)
Common border	1.26	3.79	3.99
	(3.47)	(2.03)	(1.61)
x Ln distance	-0.09	0.39	0.40
	(0.53)	(0.31)	(0.24)
x Ln population	0.08	-0.01	-0.02
(country i)	(0.10)	(0.08)	(0.08)
x Ln area	-0.09	-0.17	-0.16
(country i)	(0.10)	(0.09)	(0.09)
x Ln population	-0.36	-0.18	-0.18
(country j)	(0.13)	(0.09)	(0.09)
x Ln area	0.56	0.16	0.16
(country j)	(0.35)	(0.20)	(0.14)
x Landlocked	0.59	0.63	0.59
	(0.21)	(0.12)	(0.13)
Sample size	26,565	28,224	28,056
R^2	0.14	0.29	0.34

Notes: Coefficient estimates from equation (5) obtained with PPML. Each column reports the results from a separate cross-sectional regression. Standard errors are in parentheses.

Figure 2: Quality of the geography-based instrument



(a) Bilateral trade regression: distance

(b) IV: first-stage regression

Notes: Panel (a): Estimated $\widehat{b_1}$ coefficient and confidence band in the bilateral trade equation (5). Panel (b): Coefficient and t-statistics of the constructed instrument ($\hat{T}$) in the first stage regression.

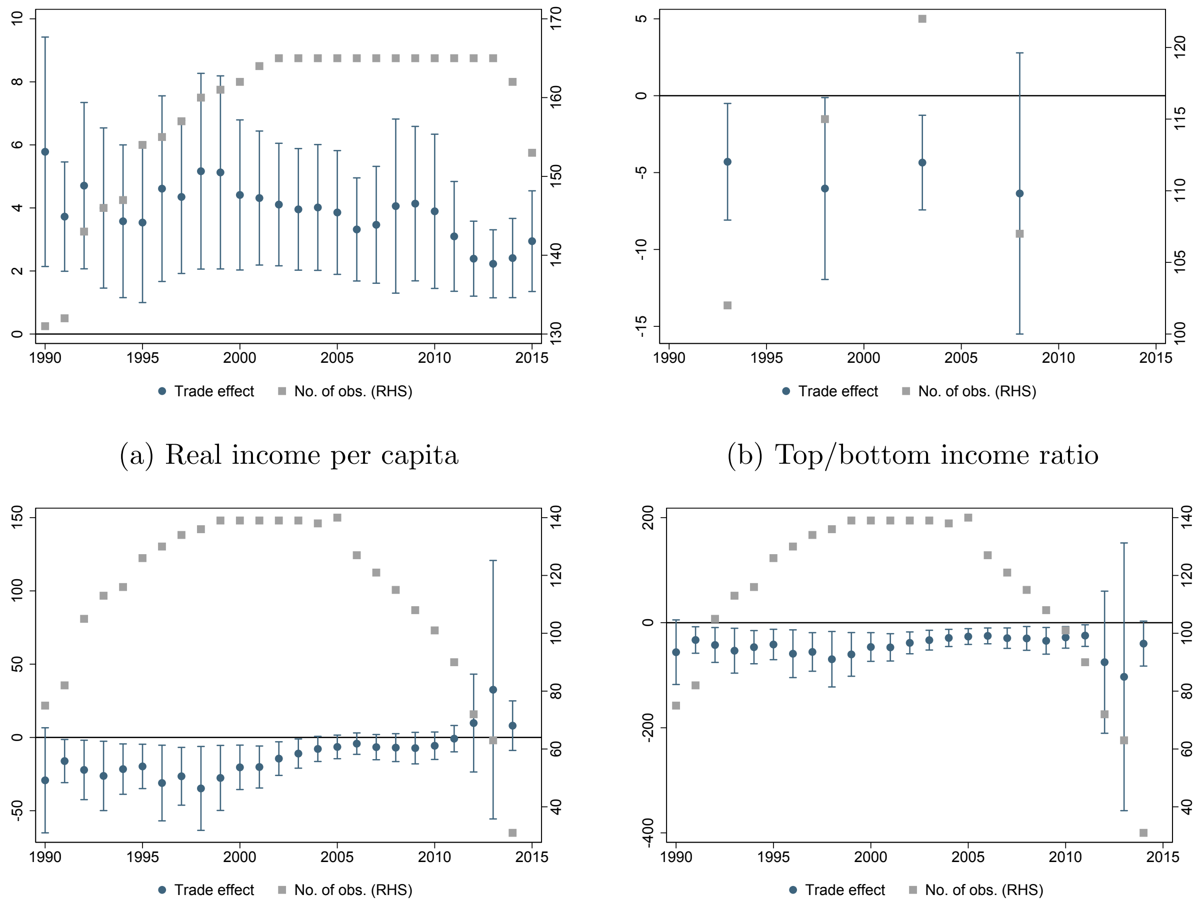
has a significant effect on actual openness. As expected, given that openness also has non-geographical determinants, the point estimate is less than 1 – it ranges between 0.5 and 0.8. Most importantly, the t-statistic shows that the relationship is sufficiently tight to provide a useful instrument. In particular, the rule of thumb that the F-statistic must be greater than 10 is satisfied for every cross section (recall that in our setup with one instrument $t^2 = F$).

3.2 Baseline Estimates

Figure 3 shows the baseline results for the effect of trade on income and inequality. Our cross-country estimates for trade's impact on real income are consistently positive and significant over time. According to the point estimates, a one percentage point increase in trade openness raises real income per capita by between 2 and 5 percent (Figure 3a). These estimates are overwhelmingly significant for all time periods. However, there is some time variation in the estimated effect: for example, after hovering between 3-5 percent since the early 1990s, the coefficients fell to about 2 percent after the global financial crisis.[8]

[8] One should be cautious in interpreting changes in the estimated coefficients as generally the differences are not statistically significant. Moreover, the figures also show that the sample of countries changes over time because of data availability constraints. It is worth noting that the fall in the estimated coefficient towards the end of the sample period can be explained by cyclical developments. Countries with lower

Figure 3: Effect of trade openness on real income and inequality: baseline estimates



(a) Real income per capita

(b) Top/bottom income ratio

(c) Market Gini

(d) Net Gini

Notes: Results from IV estimation of the regression: $O_i = a + bT_i + c_1 \ln N_i + c_2 \ln A_i + u_i$, where T_i is trade openness, and N_i and A_i are population and area. The figures show the estimated b coefficient on trade openness (dots) and the 95 percent confidence intervals (spikes). The number of countries in the sample is shown on the right axis (squares). Trade openness enters the regressions in decimal form. The dependent variables (O_i) in the four panels, respectively: log real GDP per capita (PPP-adjusted), log ratio of the average income of the top and bottom deciles in the population, and the market and net Gini coefficients on a 0-100 scale. The real incomes by decile are from the World Panel Income Distribution database (Lakner and Milanovic, 2015) and the Gini coefficients come from the Standardized World Income Inequality database (Solt, 2016).

The evidence suggests that, if anything, trade tends to reduce overall income inequality. Panel 3b shows trade's impact on the top-to-bottom income ratio for the available vintages of data. Point estimates suggest that one-percentage-point higher openness causes the income of the top decile to decrease by about 5 percent relative to the income of the bottom decile of the income distribution. This estimated effect appears statistically significant for the earlier vintages, but for the latest available cross-section of data it becomes insignificant. Panels 3c and 3d show the estimated effect of trade openness on the market and net Gini coefficients, respectively. Again, almost all of the point estimates suggest an inequality reducing effect of trade. For example, a one percentage point higher openness is associated with a 0.2-0.6 points lower net Gini coefficient.[9] Moreover, for a number of years the estimates are significant, especially for the net Gini coefficient. The insignificant results and the few positive estimates all emerge for the more recent time period simultaneously with a dramatic drop in the number of countries in the sample. Overall, the estimated impact of trade on the Gini coefficients is fairly large, considering that these inequality indicators tend to move slowly over time.

In each regression, the IV procedure yields markedly stronger effects of trade than the OLS estimates (Figure 4). As in Frankel and Romer (1999), we find that the OLS coefficient for the positive income effect of trade is much smaller than the IV coefficient. While leaving open the question of why this is the case (that is, which systematic conflating factor may raise countries' trade openness and suppress their average living standards), the fact that this result is present in all of our samples is, in our view, encouraging. Given the implausibly large measurement error variance required to attribute the lower OLS coefficient to attenuation bias, FR conclude that the most likely explanation for the difference is sampling error – that is, that by chance their 1985 sample exhibited correlation between the instrument and the regression's error term. The problem with this interpretation, however, is that it necessarily

predicted trade openness tended to be less affected by the GFC (see e.g. exhibit 2 in Bernanke, 2009), so their relative income position improved. This effect appears to start fading towards the end of our sample in 2015.

[9] Note that our openness measure is expressed in decimal form (for example, 20% openness is expressed as 0.2). Thus, an estimated coefficient $\hat{b}_1$ of -30 means that a one percentage point increase in openness reduces the Gini coefficient by $30 \times 0.01 = 0.3$.

leads to the conclusion that "OLS estimates are likely to be more accurate estimates of trade's actual impact on income" (p. 394 in Frankel and Romer, 1999). In other words, this interpretation implies that the IV estimation does not provide useful new information. However, we run our regressions for over 20 different vintages and find the same qualitative differences, which is a very strong indication that sampling error is unlikely to explain why the OLS estimates are lower, and thus that the IV estimates are meaningful.

In the inequality regressions a similar gap emerges between the OLS and IV coefficients. Panels 4b-4d show that the OLS coefficient of trade's impact on inequality is almost always insignificant and the point estimates are very close to zero. In contrast, when we use only the exogenous geography-induced component of trade, we obtain a negative effect of trade on measures of aggregate inequality, and many of the coefficients are significant. This again highlights that looking at the partial association between trade openness and inequality is not sufficient to draw causal conclusions.

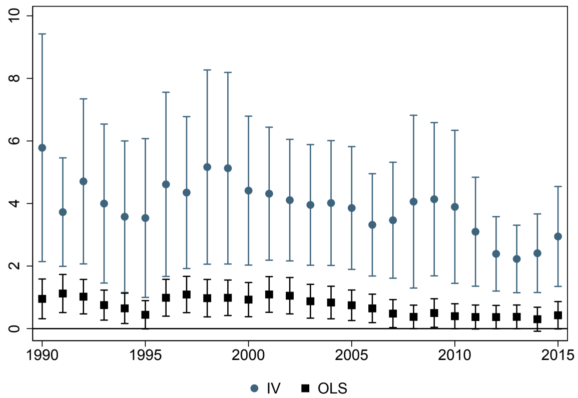
3.3 Robustness of the Baseline Results

This section considers robustness checks, including controls for other direct and indirect channels through which geography may affect income and inequality. Along the way, we also relate our results to earlier findings in the literature, and discuss some econometric and data issues that may explain the differences. The main results are summarized here, with supporting figures relegated to the Appendix.

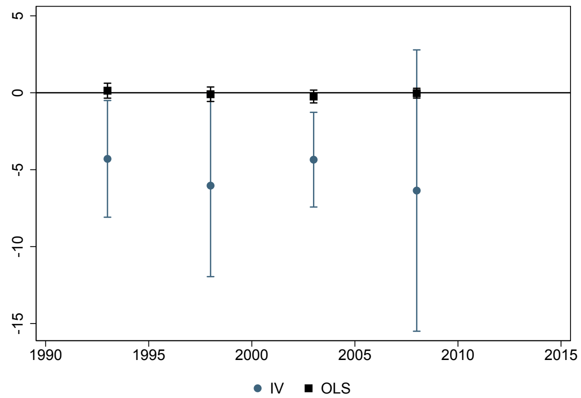
Including services trade

We obtained our baseline results using only merchandise trade to measure trade openness. However, one possible concern about comparing our estimates over different time periods is the steadily increasing importance of trade in services. According to data from the World Economic Outlook database, world services imports accounted for 20.4% of total world imports of goods and services in the year 2000. By 2015, that figure had climbed to 22.7%.

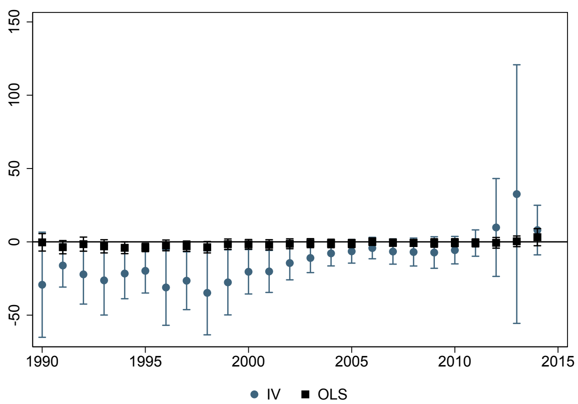
Figure 4: Comparing the IV and OLS estimates



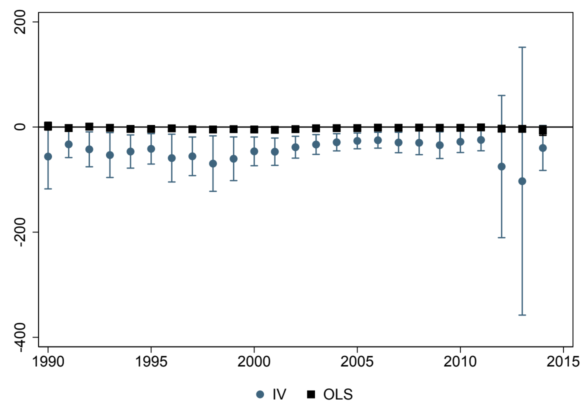
(a) Real income per capita



(b) Top/bottom income ratio



(c) Market Gini



(d) Net Gini

Notes: Results from IV and OLS estimation of the regression: $O_i = a + bT_i + c_1 \ln N_i + c_2 \ln A_i + u_i$, where T_i is trade openness, and N_i and A_i are population and area. The blue color indicates results from the IV estimation, and the black color depicts results from OLS. The figures show the estimated b coefficient on trade openness (dots and squares) and the 95 percent confidence intervals (spikes). Trade openness enters the regressions in decimal form. The dependent variables (O_i) in the four panels, respectively: log real GDP per capita (PPP-adjusted), log ratio of the average income of the top and bottom deciles in the population, and the market and net Gini coefficients on a 0-100 scale. The real incomes by decile are from the World Panel Income Distribution database (Lakner and Milanovic, 2015) and the Gini coefficients come from the Standardized World Income Inequality database (Solt, 2016).

Since services have been lately one of the most dynamic export sectors, it is possible that the estimates of the previous section are missing an important link between international trade and income and inequality.

Since we do not have bilateral services trade data, a key preliminary consideration is whether our goods-only instrument is strong enough to estimate a regression of the effect of trade including services. Figure 5 thus reproduces the first-stage diagnostics for the case when the instrumented variable is trade openness including both goods and services. The point estimate of the first-stage regression coefficient ranges between about 0.3 and 0.8, magnitudes that are not too different from the ones observed for goods-only openness. Most importantly, with the exception of the cross-sections for 1994-1995, the instrument remains remarkably strong. The reason for this is that the cross-country variability in the expanded measure of openness mainly stems from the variability in goods-only openness.

Figure 6a shows the estimated effects of goods-and-services openness on real income per capita. The estimates are well within the confidence intervals of our previous estimates, and they are very similar in magnitude – with the exception of the estimates for 1994 and 1995 when the instrument is weak. It is worth noting, in particular, that the estimates exhibit the same pattern as found in the goods-only case, with the positive effect of a one-percent increase openness on real income per capita falling from about 4% in the years before the crisis, to about 2.4% since. The comparisons for the case of trade's impact on inequality measures are similar.

Direct and indirect effects of geography through omitted variables

A natural concern about our geography-based instrument is that systematic differences among parts of the world may drive the results, even though they are unrelated to trade. Arguably, certain aspects of geography can directly or indirectly affect our outcome variables (income and inequality) through non-trade channels. For example, distance from the equator is the main determinant of average temperatures and climate in general, which in turn is a key force to shape soil fertility as well as the propagation of diseases and morbidity. Therefore, geography may exert a strong *direct* influence on incomes through its effect on agricultural productivity and the quality of human resources (Masters and McMillan, 2001). The most prominent *indirect* channel proposed in the literature emphasizes the role of institutions in economic development. Economic historians have argued that patterns of factor endowments, which are strongly correlated with geography, may have shaped the historical development of economic institutions with a profound implication for present-day income and inequality (Engerman and Sokoloff, 2002). Similarly, the seminal work of Acemoglu, Johnson and Robinson (2001) suggests that differential mortality rates during the colonization era affected the quality of institutions that the settlers established. Since there is path dependence in institutional quality, this finding also provides an indirect channel for geography to impact current economic outcomes.

Importantly, the FR trade instrument is conceptually distinct from the geography-related variables discussed above, and in practice it has a very weak association with them. Given that the second-stage regression in our baseline specification includes country size as control, the exogenous variation in trade that we use for identification originates primarily from countries' distance to their potential trading partners weighted by the trading partners' size. This weighted average of bilateral distances is a very specific geographic characteristic which has no obvious relation to a country's absolute position on the map. In fact, Figure 7 demonstrates that our trade instrument is very weakly correlated with distance from the equator and temperature which are aspects of geography that may *directly* affect moments of the income distribution through climate. We can also shed some light on the *indirect* causal channel through institutions by examining the association between trade openness and institutions on the one hand, and the exogenous geographical variables, including the settler mortality rate of Acemoglu, Johnson and Robinson (2001), on the other hand. We adopt a commonly used measure of institutional quality, the Rule of Law index published by the World Bank (for example, Rodrik, Subramanian and Trebbi, 2004). It is immediately clear from the left panel of Figure 8 that only our geography-based instrument (predicted openness) explains a meaningful share of cross-country variation in trade integration. At the same time, the right panel of Figure 8 shows that distance from the equator, temperature and settler mortality each explain roughly six times better the cross-country variation of institutional quality than our instrument for trade (R-square of 30% vs 5%). Taken together, these observations suggest that controlling for the direct and indirect effects of geography that are omitted from the baseline specification may not change our conclusions about the impact of trade on income and inequality. We proceed to test this conjecture formally.

Tables 2 through 6 report a series of extensions of our baseline specifications for income and inequality where we successively control for the direct effect of geography through climate and the indirect effect through institutions. Our preferred measure for the direct effect of geography is annual average temperature as it is the most closely related to the morbidity and agricultural productivity arguments while still being completely exogenous. To check the stability of the estimates we repeat each regression for three different annual samples five years apart. For each specification, we also test for underidentification and for weak instruments to gauge the reliability of our statistical inference. For underidentification, we use the Kleibergen and Paap (2006) rank-based rk statistic to test whether the smallest canonical correlation between the endogenous variables and the instruments is nonzero.[10] To test for weak instruments, we report the F version of the Cragg and Donald (1993) Wald statistic and corresponding 5 percent critical values tabulated by Stock and Yogo (2005) for two separate null hypotheses. The null hypotheses are that the actual size of the t-test that the point estimate(s) on the endogenous variable(s) equal zero at the 5 percent significance level is greater than 10 or 25 percent.

Columns (1)-(6) in Table 2 show that controlling for the direct effect of geography leaves our baseline results for trade's impact on income virtually unchanged. Although temperature is significant and enters the regressions with the expected negative sign, the estimated semi-

[10] Since we always reject the null of underidentification, we do not report these statistics in the tables.

elasticities of income per capita with respect to trade openness change very little and they remain significant. The weak instrument tests do not signal any major problem either. In most cases we can be confident that the actual size of the 5% t-test is below 10 percent and in all cases it is below 25 percent. Columns (7)-(12) incorporate our institutional quality variable, the rule of law, as an additional control. The results from these specifications should be treated with caution because institutions are clearly endogenous in any income regression which may also undermine the causal interpretation of the coefficient on the trade variable – regardless the validity and strength of our instrument for trade. Yet, it is reassuring that even after controlling for both temperature and institutions, the coefficients on trade openness are very similar to the baseline specification. If our baseline results about the positive impact of trade on income were driven by the indirect effect of geography through institutions, we would not expect unchanged coefficients after including institutions in the regressions. It is also noteworthy that the weak instrument test statistics are practically unchanged when we add institutions to the estimated equations.[11]

Finally, the last three columns of Table 2 report regressions where, in addition to trade, we also instrument for institutions using settler mortality rates from Acemoglu, Johnson and Robinson (2001). The coefficient estimates change considerably in this specification. While the coefficient on institutions increases and remains significant, the estimated positive effect of trade on income falls dramatically and becomes statistically indistinguishable from zero. This result is consistent with Rodrik, Subramanian and Trebbi (2004) who frame this finding as "the primacy of institutions" over geography and trade. In Table 3 we investigate further the reasons behind this marked change of estimates, and we offer an alternative explanation that does not support such negative conclusion about the role of trade in economic development.

For convenience, Table 3 reproduces the baseline regressions in columns (1)-(3) and the

[11]This is consistent with our discussion of Figure 8, where we concluded that gravity-predicted openness is very weakly correlated with the rule of law. Because of this weak association, the strength of the trade instrument does not change if we include the rule of law as an additional exogenous regressor.

results with instrumented institutions in columns (4)-(6). Note that in order to estimate the model with instrumented institutions, one is forced to drop more than half of the countries from the sample, because either they had never been colonies or simply there is no data on early settlers' mortality rates. In columns (7)-(9) we re-estimate the *baseline* specification in this reduced sample, and we find no significant effect of trade on income. This suggests that the disappearing effect of trade may have to do more with the specific sample rather than with the overall primacy of institutions. In the remaining columns of Table 3, we provide more evidence for this sample-selection hypothesis through regressions where we instrument institutions with the distance from the equator as in Hall and Jones (1999).[12] Unlike settler mortality, distance from the equator is available for all countries, so we can compare the results on the full sample (columns 10-12) and on the reduced sample (columns 13-15). Again, we find that trade openness has a significant positive coefficient in the full sample – similar in size to the baseline regressions without institutions –, whereas in the reduced sample the estimated effect of trade on income is insignificant.[13]

In Table 4 we turn to examining the robustness of our results about trade and inequality. We report the same set of regressions as for income per capita, but switching the outcome variable to the top-bottom income ratio. Columns (4)-(12) show that controlling for temperature and the rule of law causes relatively small changes in the estimated impact of trade. All point estimates are negative, and in many cases they appear significant according to the standard asymptotic t-test. However, the reported weak instrument diagnostics calls for caution on interpreting the conventional tests with their nominal size. In most cases

[12] Hall and Jones (1999) argue that distance from the equator is a good instrument for "social infrastructure" because it is correlated with the extent of Western European influence, which leads to good institutions. As we discussed earlier, distance from the equator is not a valid excludable instrument if it has a direct effect on economic performance through climate. Interestingly, Acemoglu, Johnson and Robinson (2001) find that "distance from the equator does not have an independent effect on economic performance, validating the use of this variable as an instrument in the work by Hall and Jones (1999)" (page 1373). In any case, our specification controls for the direct effect of climate through the inclusion of temperature, which greatly diminishes this concern. We note that even after controlling for size and temperature, the cross country partial correlation of distance from the equator and the rule of law is 0.33 (using 2013 values).

[13] The results of the weak instrument tests with distance to the equator are overall similar to the case when we use settler mortality as an instrument for institutions. In most cases we can reject that the actual size of the t-test is bigger than 25%.

we cannot reject the null that the actual size of the 5% t-test is above 25% and we can never reject that it is above 10%. Next, in columns (13)-(15) we instrument institutions with settler mortality as before. The coefficients on trade remain negative and similar in size, but they do not show up as significant any longer and instrument strength collapses in the smaller sample. We believe that these results reinforce the cautious interpretation that we attached to our baseline estimates in the previous section: based on the cross-country experience, there is no evidence that more trade openness causes a greater gap between the incomes of the top and bottom deciles.

We arrive at the same conclusion when we use the Gini coefficient as the measure of income inequality (Tables 5 and 6). Even after including potential non-trade effects of geography, the point estimates on trade openness are always negative, and in many cases they seem significant, especially for the net Gini coefficient. Again, the generally low values of the weak instrument test statistics remind us that we should not place undue weight on the statistical inference in these regressions. However, there is certainly no indication in any of these robustness checks that trade could have a detrimental impact on overall income inequality.

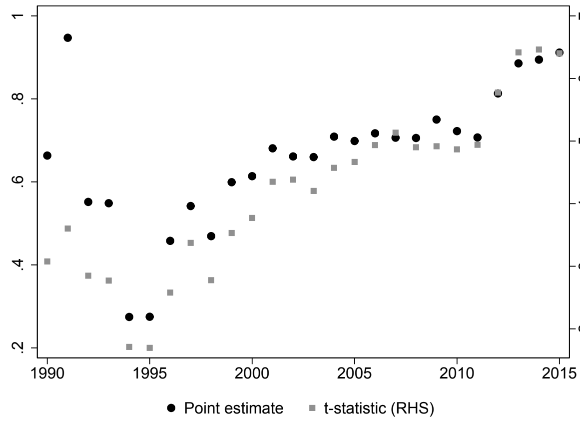
4 Concluding Remarks

This paper estimates the causal effect of international trade on countries' income level and income inequality. Building on Frankel and Romer (1999), we construct an instrument for trade openness which aggregates the exogenous component of bilateral trade flows that is driven by geographic factors. We find that although the IV estimates substantially differ from the OLS estimates in their magnitude, the main qualitative messages are in line with basic correlations in the data. In particular, more trade increases countries' long run living standards, while their is no evidence for any deterioration of the overall income distribution as a result of higher openness. Our analysis does not corroborate earlier concerns about the geography-based instrument which suggested that the positive effect of trade on income may be spurious due to other omitted channels influenced by geography.

Our results suggest that well-designed policies can leverage trade integration to support higher and inclusive growth. However, three important caveats apply. First, policy-induced trade barriers may act differently than the geographical barriers that we used for identification in this paper. Second, given that our estimates are based on cross-country data, they should be interpreted as the long-run effect of trade on growth and overall inequality. Thus, the results do not shed light on possible temporary adjustment costs from greater integration. While trade appears to have an overall beneficial effect in the long run, policies need to address the short and medium term adjustment. Finally, it is worth noting that our reduced form approach is silent about the channels through which the positive effects of trade operate. In this regard, more disaggregated studies are essential to help design policies that tap trade integration to spur inclusive growth.

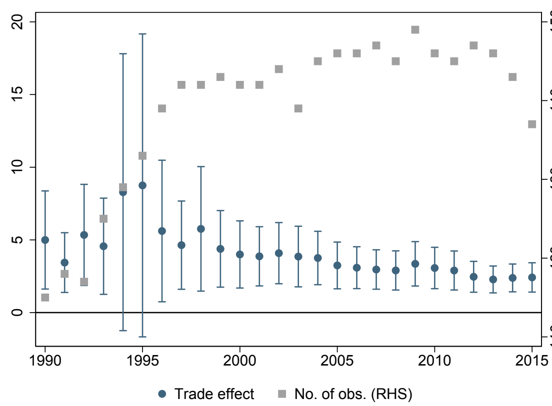
A Supplementary Figures

Figure 5: First stage regression **including services trade**

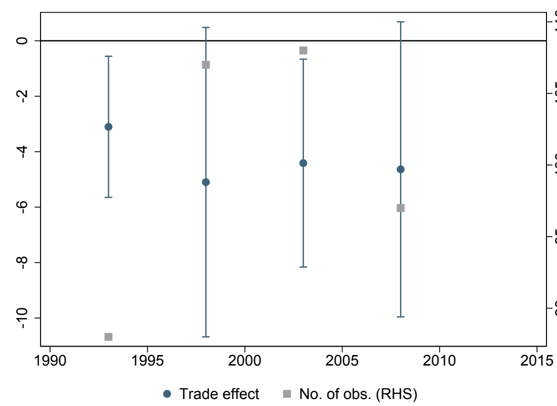


Notes: Coefficient and t-statistics of the goods-only instrument ($\hat{T}$) in the first stage regression when including services trade in the openness measure.

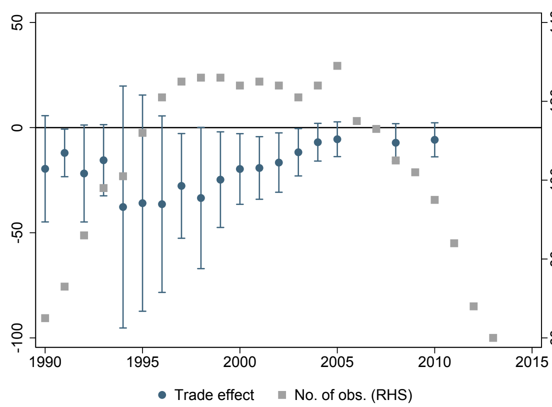
Figure 6: Effect of trade openness on real income and inequality (**incl. services**)



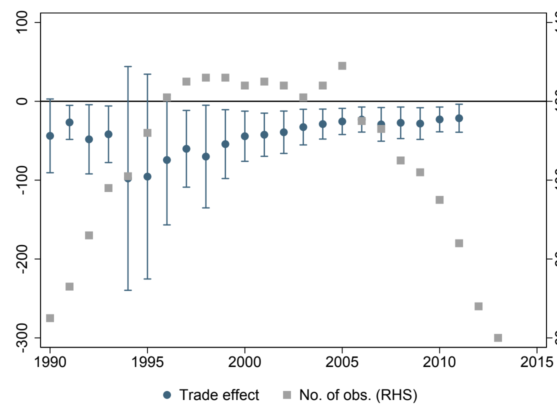
(a) Real income per capita



(b) Top/bottom income ratio



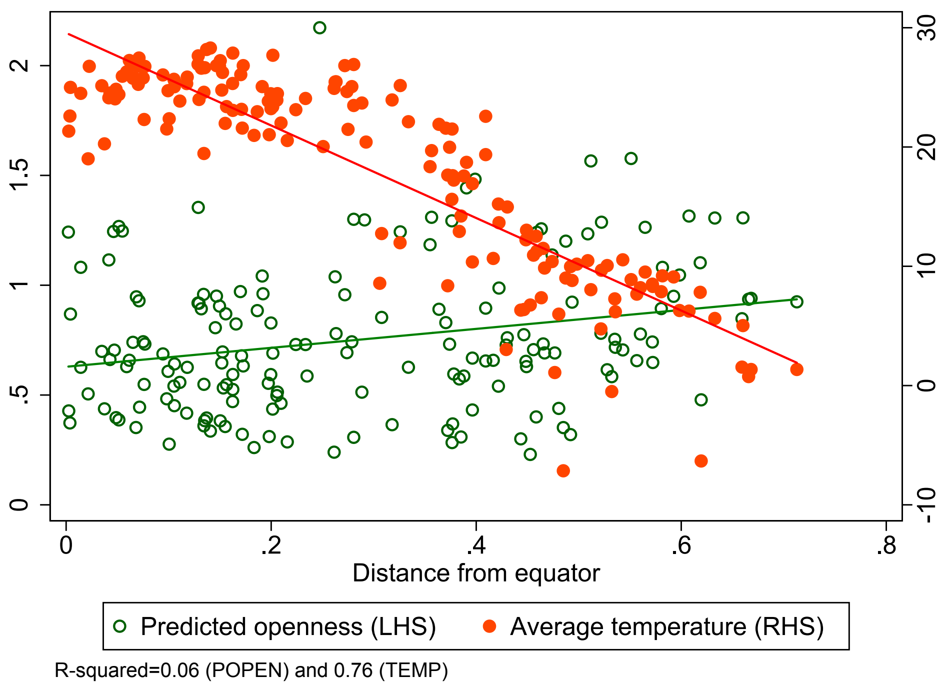
(c) Market Gini



(d) Net Gini

Notes: Results from IV estimation of the regression: $O_i = a + bT_i + c_1 \ln N_i + c_2 \ln A_i + u_i$, where T_i is trade openness *including services*, and N_i and A_i are population and area. For better readability, the figure only includes estimates that are significant at the 20% level (displayed confidence bands are still at 95% level). The figures show the estimated b coefficient on trade openness (dots) and the 95 percent confidence intervals (spikes). The number of countries in the sample is shown on the right axis (squares). Trade openness enters the regressions in decimal form. The dependent variables (O_i) in the four panels, respectively: log real GDP per capita (PPP-adjusted), log ratio of the average income of the top and bottom deciles in the population, and the market and net Gini coefficients on a 0-100 scale. The real incomes by decile are from the World Panel Income Distribution database (Lakner and Milanovic, 2015) and the Gini coefficients come from the Standardized World Income Inequality database (Solt, 2016).

Figure 7: The FR trade instrument, climate and geography

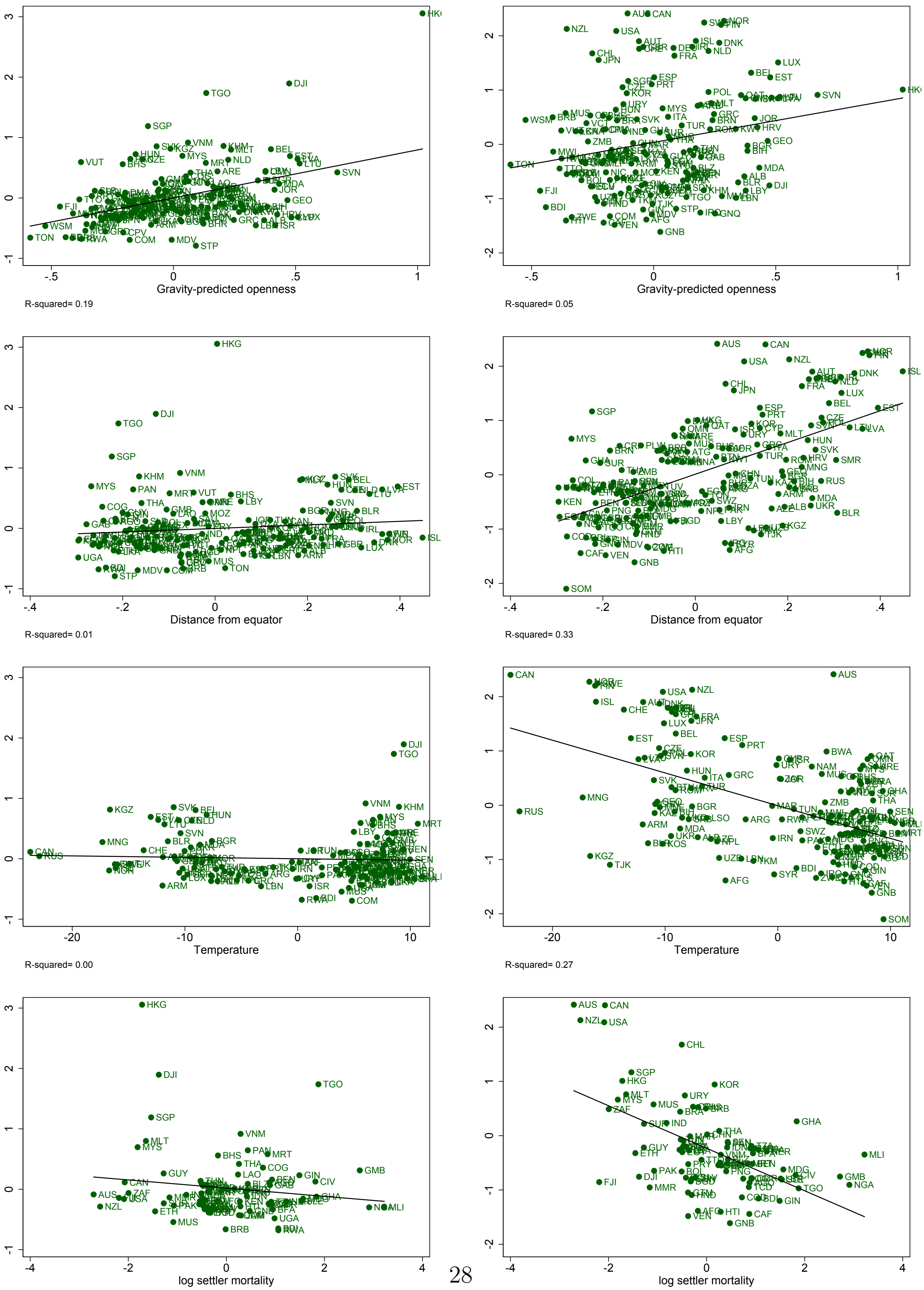


Notes: Based on 2013 data.

Figure 8: Trade, institutions and exogenous geography

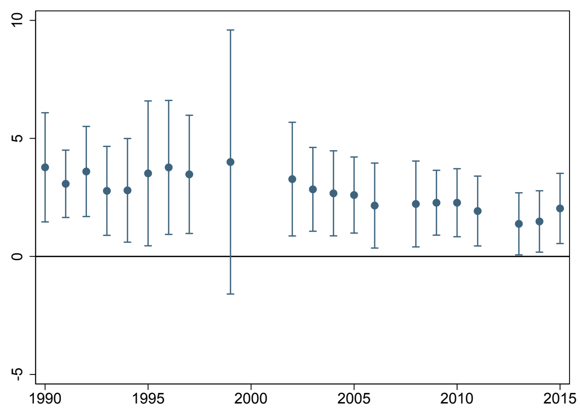
(a) Trade openness vs. geography

(b) Rule of law vs. geography

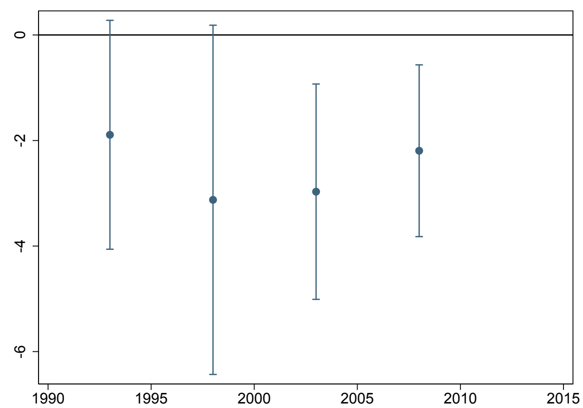


Notes: Based on 2013 data.

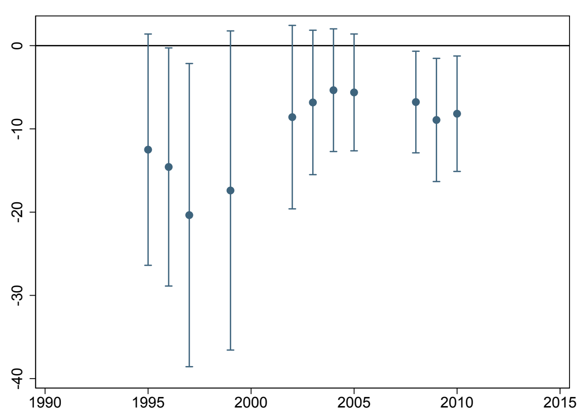
Figure 9: Effect of trade openness on real income and inequality (**non-European countries**)



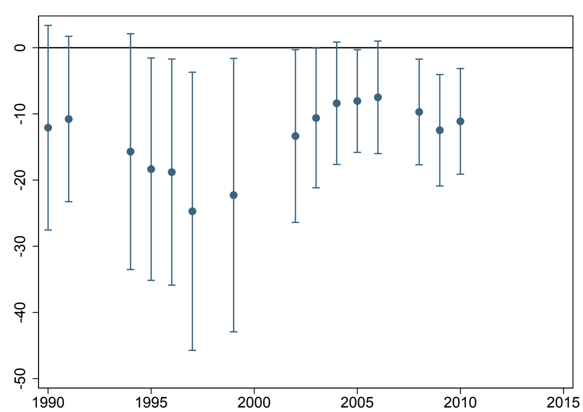
(a) Real income per capita, non-Europe



(b) Top/bottom income ratio, non-Europe



(c) Market Gini, non-Europe



(d) Net Gini, non-Europe

Notes: The figure presents IV estimates for the effect of trade after including a Europe-dummy and its interactions in the baseline regression. The plotted coefficients refer to the non-European countries. Estimates for the European countries are generally highly insignificant. For better readability, the figure only includes estimates that are significant at the 20% level (displayed confidence bands are still at 95% level). For more details, see notes below Figure 3.

B Supplementary Tables

Table 2: Trade's impact on income: Robustness to climate and institutions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
Openness	3.955***	4.059**	2.228***	4.071**	4.642*	2.502**	2.572***	2.733**	1.383***	3.282***	3.863*	2.049**	0.282	0.176	0.249
	(0.967)	(1.385)	(0.541)	(1.327)	(2.300)	(0.944)	(0.686)	(1.021)	(0.386)	(0.985)	(1.816)	(0.756)	(1.068)	(0.720)	(0.554)
Ln of area	0.169	0.260	0.110	0.0788	0.0621	0.0364	0.198**	0.270*	0.160*	0.154*	0.174	0.147	0.0842	0.165	0.146
	(0.109)	(0.170)	(0.0931)	(0.103)	(0.144)	(0.0983)	(0.0725)	(0.117)	(0.0636)	(0.0775)	(0.120)	(0.0797)	(0.105)	(0.117)	(0.107)
Ln of pop.	-0.0243	0.0865	0.0234	0.131	0.336	0.155	-0.0607	0.0155	-0.0279	0.0851	0.244	0.0846	0.0593	-0.0150	-0.00641
	(0.108)	(0.151)	(0.101)	(0.120)	(0.240)	(0.125)	(0.0720)	(0.105)	(0.0689)	(0.0893)	(0.190)	(0.100)	(0.121)	(0.133)	(0.119)
Temperature				-0.0434**	-0.0459*	-0.0632***				0.00203	0.00187	-0.0153	0.0644	0.0898	0.0867
				(0.0151)	(0.0225)	(0.0136)				(0.0125)	(0.0209)	(0.0127)	(0.0493)	(0.0584)	(0.0516)
Rule of law							0.815***	0.769***	0.817***	0.875***	0.870***	0.798***	1.924***	2.144***	2.121***
							(0.0913)	(0.134)	(0.0865)	(0.0990)	(0.157)	(0.112)	(0.556)	(0.641)	(0.566)
Observations	165	165	165	150	150	150	165	165	165	150	150	150	77	77	77
Year	2003	2008	2013	2003	2008	2013	2003	2008	2013	2003	2008	2013	2003	2008	2013
No. endog. vars.	1	1	1	1	1	1	1	1	1	1	1	1	2	2	2
Weak ID test stat.	26.20	10.52	38.72	18.18	5.467	15.76	23.20	9.146	35.23	18.15	5.612	15.49	3.990	4.235	4.953
Ho:t-test size>10pct	16.38	16.38	16.38	16.38	16.38	16.38	16.38	16.38	16.38	16.38	16.38	16.38	7.030	7.030	7.030
Ho:t-test size>25pct	5.530	5.530	5.530	5.530	5.530	5.530	5.530	5.530	5.530	5.530	5.530	5.530	3.630	3.630	3.630

Notes: The dependent variable is log real GDP per capita (PPP-adjusted). Each column presents a cross-country regression for the year indicated. Trade openness is treated as an endogenous regressor in all specifications, and it is instrumented by the geography-predicted trade share as in Frankel and Romer (1999). The Rule of Law (a measure of institutional quality) is treated as endogenous in columns (13)-(15) where the log settler mortality rate is used as an additional instrument following Acemoglu, Johnson and Robinson (2001). Area, population and average temperature always enter as exogenous control variables. The table reports the Cragg and Donald (1993) test statistic for weak instruments; the 5 percent critical values for two null hypotheses about the size distortion of the standard t-test are also shown as tabulated in Stock and Yogo (2005). Standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 3: Robustness of income regression to institutions: Effect of sample selection

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
Openness	3.955***	4.059**	2.228***	0.282	0.176	0.249	-0.734	-0.580	-0.436	3.029**	3.696*	1.817*	0.223	0.0551	0.175
	(0.967)	(1.385)	(0.541)	(1.068)	(0.720)	(0.554)	(1.105)	(0.680)	(0.519)	(0.998)	(1.870)	(0.790)	(0.744)	(0.510)	(0.447)
Ln of area	0.169	0.260	0.110	0.0842	0.165	0.146	0.0871	0.111	0.123	0.179*	0.198	0.203*	0.0672	0.116	0.125
	(0.109)	(0.170)	(0.0931)	(0.105)	(0.117)	(0.107)	(0.106)	(0.102)	(0.0993)	(0.0799)	(0.123)	(0.0908)	(0.0733)	(0.0818)	(0.0871)
Ln of pop.	-0.0243	0.0865	0.0234	0.0593	-0.0150	-0.00641	-0.125	-0.134	-0.121	0.0703	0.224	0.0484	-0.0227	-0.0648	-0.0362
	(0.108)	(0.151)	(0.101)	(0.121)	(0.133)	(0.119)	(0.119)	(0.121)	(0.110)	(0.0892)	(0.196)	(0.105)	(0.0877)	(0.0942)	(0.0979)
Temperature				0.0644	0.0898	0.0867				0.0166	0.0121	0.00920	-0.00354	0.0128	0.0417
				(0.0493)	(0.0584)	(0.0516)				(0.0184)	(0.0284)	(0.0239)	(0.0304)	(0.0394)	(0.0532)
Rule of law				1.924***	2.144***	2.121***				1.155***	1.056*	1.207***	1.049**	1.195**	1.561*
				(0.556)	(0.641)	(0.566)				(0.279)	(0.424)	(0.358)	(0.334)	(0.433)	(0.620)
Observations	165	165	165	77	77	77	77	77	77	150	150	150	77	77	77
Year	2003	2008	2013	2003	2008	2013	2003	2008	2013	2003	2008	2013	2003	2008	2013
No. endog. vars.	1	1	1	2	2	2	1	1	1	2	2	2	2	2	2
Instrument for ROL	-	-	-	LOGEM4	LOGEM4	LOGEM4	-	-	-	DISTEQ	DISTEQ	DISTEQ	DISTEQ	DISTEQ	DISTEQ
Weak ID test stat.	26.20	10.52	38.72	3.990	4.235	4.953	18.08	24.59	35.95	7.439	2.383	5.438	4.746	4.249	2.429
Ho:t-test size>10pct	16.38	16.38	16.38	7.030	7.030	7.030	16.38	16.38	16.38	7.030	7.030	7.030	7.030	7.030	7.030
Ho:t-test size>25pct	5.530	5.530	5.530	3.630	3.630	3.630	5.530	5.530	5.530	3.630	3.630	3.630	3.630	3.630	3.630

Notes: The dependent variable is log real GDP per capita (PPP-adjusted). Each column presents a cross-country regression for the year indicated. Trade openness is treated as an endogenous regressor in all specifications, and it is instrumented by the geography-predicted trade share as in Frankel and Romer (1999). The Rule of Law (a measure of institutional quality) is also treated as endogenous: in columns (4)-(6) we use the log settler mortality rate (LOGEM4) as an additional instrument following Acemoglu, Johnson and Robinson (2001), while in columns (10)-(15) we use distance from the equator (DISTEQ) as an additional instrument following Hall and Jones (1999). Area, population and average temperature always enter as exogenous control variables. The table reports the Cragg and Donald (1993) test statistic for weak instruments; the 5 percent critical values for two null hypotheses about the size distortion of the standard t-test are also shown as tabulated in Stock and Yogo (2005). Standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 4: Trade's impact on top-to-bottom income ratio: Robustness to climate and institutions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
Openness	-6.036*	-4.349**	-6.356	-3.989*	-3.317**	-4.658	-5.954	-4.322*	-5.854	-3.961*	-3.252**	-4.367	-4.243	-4.631	-2.574
	(2.949)	(1.536)	(4.552)	(1.909)	(1.175)	(3.236)	(3.295)	(1.739)	(4.509)	(1.925)	(1.159)	(2.874)	(2.802)	(2.884)	(1.747)
Ln of area	-0.268	-0.309	-0.632	0.136	-0.0857	-0.0970	-0.265	-0.308	-0.588	0.134	-0.0895	-0.140	0.106	-0.279	0.180
	(0.226)	(0.161)	(0.575)	(0.101)	(0.101)	(0.255)	(0.230)	(0.168)	(0.551)	(0.101)	(0.0999)	(0.258)	(0.184)	(0.299)	(0.251)
Ln of pop.	-0.280	-0.0493	-0.148	-0.466**	-0.181	-0.308	-0.277	-0.0488	-0.138	-0.463**	-0.173	-0.256	-0.439*	-0.208	-0.463
	(0.179)	(0.127)	(0.248)	(0.169)	(0.0978)	(0.213)	(0.185)	(0.127)	(0.232)	(0.171)	(0.0970)	(0.186)	(0.204)	(0.235)	(0.292)
Temperature				0.0267	0.0188	-0.00463				0.0257	0.0152	-0.0243	0.0788	0.135	0.189
				(0.0163)	(0.0128)	(0.0322)				(0.0172)	(0.0142)	(0.0407)	(0.0846)	(0.147)	(0.204)
Rule of law							-0.0208	-0.00836	-0.0937	-0.0197	-0.0658	-0.313	0.713	2.329	2.596
							(0.224)	(0.162)	(0.260)	(0.132)	(0.112)	(0.259)	(1.003)	(2.148)	(2.585)
Observations	115	122	107	109	119	105	115	122	107	109	119	105	56	62	52
Year	1998	2003	2008	1998	2003	2008	1998	2003	2008	1998	2003	2008	1998	2003	2008
No. endog. vars.	1	1	1	1	1	1	1	1	1	1	1	1	2	2	2
Weak ID test stat.	4.819	9.823	2.024	6.137	11.74	2.219	3.753	7.570	1.750	5.922	11.62	2.469	1.521	0.969	0.723
Ho:t-test size>10pct	16.38	16.38	16.38	16.38	16.38	16.38	16.38	16.38	16.38	16.38	16.38	16.38	7.030	7.030	7.030
Ho:t-test size>25pct	5.530	5.530	5.530	5.530	5.530	5.530	5.530	5.530	5.530	5.530	5.530	5.530	3.630	3.630	3.630

Notes: The dependent variable is the log income ratio of the top and bottom deciles in the population. Each column presents a cross-country regression for the year indicated. Trade openness is treated as an endogenous regressor in all specifications, and it is instrumented by the geography-predicted trade share as in Frankel and Romer (1999). The Rule of Law (a measure of institutional quality) is treated as endogenous in columns (13)-(15) where the log settler mortality rate is used as an additional instrument following Acemoglu, Johnson and Robinson (2001). Area, population and average temperature always enter as exogenous control variables. The table reports the Cragg and Donald (1993) test statistic for weak instruments; the 5 percent critical values for two null hypotheses about the size distortion of the standard t-test are also shown as tabulated in Stock and Yogo (2005). Standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 5: Trade's impact on market Gini: Robustness to climate and institutions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
Openness	-34.79*	-10.99*	-6.944	-33.04*	-11.12	-12.38	-36.62*	-11.43*	-8.409	-33.39*	-11.33	-12.54	-21.00	-11.86	-11.71
	(14.32)	(4.988)	(4.748)	(15.82)	(7.112)	(11.44)	(17.03)	(5.446)	(4.924)	(16.20)	(7.126)	(10.97)	(10.90)	(7.299)	(6.260)
Ln of area	-1.636	-0.866	-0.769	0.223	-0.0142	-0.380	-1.659	-0.870	-0.859	0.269	0.0302	-0.345	1.170	0.590	0.688
	(1.053)	(0.622)	(0.769)	(0.764)	(0.562)	(0.920)	(1.110)	(0.630)	(0.797)	(0.763)	(0.569)	(1.020)	(0.791)	(0.712)	(0.916)
Ln of pop.	-1.123	0.139	0.219	-2.578*	-0.644	-0.608	-1.194	0.132	0.244	-2.632*	-0.681	-0.638	-2.519*	-1.556	-1.114
	(0.947)	(0.585)	(0.563)	(1.263)	(0.648)	(0.792)	(1.015)	(0.591)	(0.588)	(1.308)	(0.652)	(0.749)	(1.022)	(0.845)	(0.911)
Temperature				0.0425	0.0900	-0.0255				0.0657	0.113	-0.0106	0.478	-0.0787	0.0888
				(0.127)	(0.0806)	(0.119)				(0.130)	(0.0908)	(0.165)	(0.419)	(0.366)	(0.348)
Rule of law							0.576	0.227	0.689	0.435	0.404	0.258	4.197	-1.236	0.704
							(1.321)	(0.703)	(0.627)	(1.057)	(0.704)	(1.126)	(4.818)	(4.210)	(4.463)
Observations	136	139	115	130	131	110	136	139	115	130	131	110	69	69	55
Year	1998	2003	2008	1998	2003	2008	1998	2003	2008	1998	2003	2008	1998	2003	2008
No. endog. vars.	1	1	1	1	1	1	1	1	1	1	1	1	2	2	2
Weak ID test stat.	7.423	19.03	9.167	6.289	13.49	2.863	5.673	16.24	9.242	6.057	13.38	3.216	2.720	3.178	2.676
Ho:t-test size>10pct	16.38	16.38	16.38	16.38	16.38	16.38	16.38	16.38	16.38	16.38	16.38	16.38	7.030	7.030	7.030
Ho:t-test size>25pct	5.530	5.530	5.530	5.530	5.530	5.530	5.530	5.530	5.530	5.530	5.530	5.530	3.630	3.630	3.630

Notes: The dependent variable is the Gini coefficient of market income. Each column presents a cross-country regression for the year indicated. Trade openness is treated as an endogenous regressor in all specifications, and it is instrumented by the geography-predicted trade share as in Frankel and Romer (1999). The Rule of Law (a measure of institutional quality) is treated as endogenous in columns (13)-(15) where the log settler mortality rate is used as an additional instrument following Acemoglu, Johnson and Robinson (2001). Area, population and average temperature always enter as exogenous control variables. The table reports the Cragg and Donald (1993) test statistic for weak instruments; the 5 percent critical values for two null hypotheses about the size distortion of the standard t-test are also shown as tabulated in Stock and Yogo (2005). Standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 6: Trade's impact on net Gini: Robustness to climate and institutions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
Openness	-69.54**	-33.24***	-29.94**	-50.78*	-27.07**	-33.26	-65.69*	-28.72**	-23.28*	-49.68*	-26.00**	-30.73	-24.66	-17.23*	-15.24*
	(26.40)	(9.421)	(11.28)	(21.91)	(9.969)	(21.23)	(28.71)	(9.126)	(9.053)	(21.82)	(9.580)	(18.26)	(12.84)	(8.641)	(7.478)
Ln of area	-3.533	-2.791*	-3.815*	0.487	0.0494	-0.528	-3.485	-2.750**	-3.407*	0.341	-0.183	-1.079	0.261	-0.221	-0.522
	(1.941)	(1.176)	(1.828)	(1.058)	(0.787)	(1.708)	(1.871)	(1.056)	(1.465)	(1.028)	(0.765)	(1.697)	(0.932)	(0.843)	(1.094)
Ln of pop.	-1.562	0.750	1.500	-3.807*	-1.514	-1.700	-1.413	0.820	1.389	-3.639*	-1.324	-1.232	-1.792	-0.990	-0.379
	(1.747)	(1.105)	(1.337)	(1.750)	(0.908)	(1.471)	(1.711)	(0.990)	(1.081)	(1.762)	(0.876)	(1.247)	(1.204)	(1.000)	(1.088)
Temperature				0.341	0.400***	0.256				0.268	0.280*	0.0200	0.691	0.186	0.248
				(0.176)	(0.113)	(0.221)				(0.175)	(0.122)	(0.275)	(0.494)	(0.433)	(0.416)
Rule of law							-1.207	-2.343*	-3.131**	-1.370	-2.111*	-4.076*	5.752	1.254	1.939
							(2.228)	(1.178)	(1.153)	(1.423)	(0.947)	(1.875)	(5.675)	(4.983)	(5.331)
Observations	136	139	115	130	131	110	136	139	115	130	131	110	69	69	55
Year	1998	2003	2008	1998	2003	2008	1998	2003	2008	1998	2003	2008	1998	2003	2008
No. endog. vars.	1	1	1	1	1	1	1	1	1	1	1	1	2	2	2
Weak ID test stat.	7.423	19.03	9.167	6.289	13.49	2.863	5.673	16.24	9.242	6.057	13.38	3.216	2.720	3.178	2.676
Ho:t-test size>10pct	16.38	16.38	16.38	16.38	16.38	16.38	16.38	16.38	16.38	16.38	16.38	16.38	7.030	7.030	7.030
Ho:t-test size>25pct	5.530	5.530	5.530	5.530	5.530	5.530	5.530	5.530	5.530	5.530	5.530	5.530	3.630	3.630	3.630

Notes: The dependent variable is the Gini coefficient of net income. Each column presents a cross-country regression for the year indicated. Trade openness is treated as an endogenous regressor in all specifications, and it is instrumented by the geography-predicted trade share as in Frankel and Romer (1999). The Rule of Law (a measure of institutional quality) is treated as endogenous in columns (13)-(15) where the log settler mortality rate is used as an additional instrument following Acemoglu, Johnson and Robinson (2001). Area, population and average temperature always enter as exogenous control variables. The table reports the Cragg and Donald (1993) test statistic for weak instruments; the 5 percent critical values for two null hypotheses about the size distortion of the standard t-test are also shown as tabulated in Stock and Yogo (2005). Standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

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