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Abstract

Does trade openness cause higher GDP per capita? Since the seminal instrumental variables (IV) estimates of Frankel and Romer [F&R](1999) important doubts have surfaced. Is the correlation spurious and driven by omitted geographical and institutional variables? In this paper, we generalize F&R's geography-based empirical strategy to a panel setting. We observe that natural disasters affect bilateral trade, and that this effect is conditioned by geographical variables such as distance to financial centers or area. This allows us to use *interactions* between geography and the incidence of disasters at the *bilateral* level to construct an instrument for *multilateral* openness that varies across countries *and* time. The instrument can be used in panel setups where it is possible to fully control for geographical and historical determinants of countries' performances as well as for the direct effect of disasters. We find that the elasticity of income with respect to openness is about 0.69, but that substantial heterogeneity exists across country samples.

JEL-Code: C230, C260, F150, F430, O400, Q540.

Keywords: per capita income, openness, natural disasters, instrumental variable estimation, panel econometrics.

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1. Introduction

Does openness to trade result in higher per capita income? Virtually all workhorse models of trade theory predict gains from trade in the form of higher per capita real GDP, in particular in the long-run. However, many observers, in the academia and outside, remain unconvinced by the empirical evidence. The central econometric problem lies in the joint endogeneity of openness and income and in the role of deep geographical and historical determinants that influence both openness and income but are only incompletely observable. Using a cross-section of countries, Frankel and Romer (1999), henceforth F&R, have used a geography-based instrument to analyze the empirical relationship between trade and per capita income. Their approach has gained enormous popularity.[1] However, it has also drawn important criticism. Rodriguez and Rodrik (2001) argue that F&R's instrument is correlated with other geographic variables that directly affect income. For example, the effect of openness is not robust to the inclusion of distance to the equator. Rodrik, Subramanian and Trebbi (2004) show that institutional quality, which has its foundation in history, matters more than geography (and geography-induced trade openness).

The issues with the F&R-approach discussed above essentially relate to omitted variable bias. Thus, authors have turned to *panel* regressions where it is possible to fully control for unobserved time-invariant idiosyncratic country characteristics–such as distance to the equator, historical factors going back to colonialism, or climatic conditions–by first-differencing or applying the within transformation. However, the F&R-instrument is not applicable in the panel setup since geography does not vary across time.[2] Very recently, Feyrer (2009) has proposed a time-varying geography-based instrument for

[1]Hall and Jones (1999), Chakrabarti (2000), Dollar and Kraay (2002), Irwin and Teviö (2002), Easterly and Levine (2003), Persson and Tabellini (2003), Alcalá and Ciccone (2004), Redding and Venables (2004), Noguer and Siscart (2005), Frankel and Rose (2005), Cavallo and Frankel (2008), to name only a few studies that draw on F&R's instrument. According to Google Scholar, the paper has been cited 2,872 times in research papers (July 6, 2011).

[2]Alternatively GMM-based approaches are used, e.g. Greenaway, Morgan and Wright (2002), or Lederman and Maloney (2003).

trade openness. The idea is that the dramatic fall in the cost of air-borne transportation should reduce average trade costs relatively more for country pairs whose geographical positions imply long detours for sea-borne traffic. So, different pairs are affected differently by the common trend of lower costs of aviation. Aggregation across the bilateral dimension yields a *multilateral, time-variant proxy* for openness that is *exogenous* to country-level GDP per capita.

In the present paper, we propose a different instrument of openness to be used in panel data environments. We observe empirically that natural disasters such as volcano eruptions, earthquakes, or storm floods affect countries' imports, exports, and the extent of bilateral openness. But their effects are crucially conditioned by geographical variables such as distance to financial centers, or area. So, we can follow F&R in using a gravity-type equation to predict exogenous variation in bilateral trade flows and aggregate this up to the country level to obtain an instrument for multilateral openness. The disaster *interactions* in the gravity model identify the effect of openness on income per capita in a two-stage regression, where we can easily account for the *direct* effect of disasters, and where we take care of geographical variables and other time-invariant country characteristics by using fixed-effects. The key identifying assumption here is that the incidence of natural disasters and countries' geographical features are orthogonal to country-level income shocks.

We can theoretically rationalize the fact that disasters affect bilateral trade flows and openness. When a country is hit by a disaster, its productive capacity is reduced. If it has access to international goods and financial markets, it can smooth consumption by importing more and exporting less. Our gravity equations tell us that this is exactly what happens. Moreover, if a country starts from a balanced current account the disaster creates intertemporal trade, making the country a net importer in the immediate aftermath of the disaster, but a net exporter later when the intertemporal budget constraint needs to be met. So, the intertemporal mechanism can also contribute towards a long-lasting increase in trade openness as a consequence of a natural catastrophe.

These are our three key results: First, using a theory-consistent gravity equation, a major natural disaster increases the affected country's bilateral imports by 2 percent on average. The effect is stronger when the country is close to a major financial center and may be negative when the country is financially remote (i.e., if it cannot borrow internationally). Exports typically fall, but they fall by less when the exporter is financially integrated. Second, when bilateral trade flows are normalized by the importer's GDP ('bilateral openness') and the estimation includes only variables that are strictly exogenous to income, these patterns remain. Aggregating over predicted bilateral openness, the constructed openness measure correlates well with observed degrees of openness: in fixed-effects regressions on 5-year averaged data (1950-2008) for different country samples, the within R^2 statistic ranges between 33 and 57 percent. Third, in our income regressions, whether estimated by a fixed-effects estimator or employing first-differencing, the instrumental variables (IV) strategy works very well: F-tests on excluded instruments and the Hansen overidentification test signal validity of our instrument. Evaluated at the mean, the elasticity of income with respect to trade is about 0.33 in the non-instrumented and about 0.69 in the IV regressions. Our preferred IV estimate is slightly larger than the one obtained by Feyrer (2009) who finds about 0.6 in comparable log-log estimations. As in F&R and Feyrer (2009), our results imply that measurement error is substantial relative to the endogeneity bias. Since our IV strategy relies on variation in the incidence of disasters interacted with geographical variables in a bilateral trade flow equation, we can include the direct effect of natural disasters and interaction terms involving them in the second stage equation that uses multilateral openness as the key right hand side variable. The effect of disasters on GDP per capita is zero on average, but strongly negative for small and/or remote countries. These findings are robust to different specifications and samples.

Related Literature. A very large number of papers[3] have applied and refined the instrument of F&R to various aspects of international trade, while others strongly criticized the approach.[4] Irwin and Terviö (2002) argue that a higher degree of trade openness correlates to higher per capita income levels. Yet, they criticize that this result is not robust to the inclusion of distance from the equator. Nagour and Siscart (2005) re-examine the relation between trade volumes and income levels using the proposed geography-based instrument. In contrast to earlier studies the authors use a richer data set that allows them to estimate the effect of openness on domestic income more precisely. Their result of income enhancing trade is remarkably robust to a wide array of geographical and institutional controls. Buch and Toubal (2009) use variation in international market access within Germany due to the fall of the Berlin Wall to study the effects of economic integration on growth at the state level. In their panel analysis, they find a positive effect of openness on income per capita, but their instrument is specific to the German case. The paper most closely related to ours is Feyrer (2009); see the discussion above.

There is only a very small literature on the consequences of natural disasters on international trade. Our main reference is the gravity analysis of Gassebner, Keck and Teh (2010) who quantify the effect of technological and natural catastrophes on real bilateral import flows. Our research draws on the same (but updated) disasters database; it confirms and complements their results. While they stress the interaction of disasters with democracy, we focus on the interaction with geographical variables and aim at creating a valid instrument for multilateral trade openness. Yang (2008) documents that hurricanes lead to increased financial flows into developing countries, helping them to increase imports to buffer income losses. Sahin (2011) uses a CGE model to illustrate how natural disasters affect an open economy. He finds that disasters affect bi-

[3]The empirical literature on the trade income nexus is very large. In the following, we provide only a very eclectic account, focusing on papers most closely related to our work.

[4]See e.g. Rodrik, Subramanian and Trebbi (2004), or Rodriguez and Rodrik (2001) for critical papers discussed above.

lateral and multilateral trade flows. They have no major contemporaneous impact on world-wide trade levels but increase intertemporal trade. The literature on the macroeconomic effects of disasters is somewhat larger. Skidmore and Toya (2002) find that nations with more climatic disasters grew faster in the long-run than less disaster-prone economies. Country case studies such as the one of Cavallo et al. (2010) for Haiti document high monetary losses due to natural disasters. Skidmore and Toya (2007) document the importance of greater financial and trade openness for countries' capacity to overcome natural disasters.

The empirical strategy in this paper is as follows. First (Section 2), we investigate the effect of large natural disasters in a conventional (theory-consistent) gravity framework of bilateral trade. We show that disasters affect bilateral trade and that interactions of geographical variables with disasters matter. While we are interested in understanding the role of disasters in a fully-fledged gravity model in Section 2, in the second step (Section 3), we use a modified equation that draws on variables strictly exogenous to variation in income with the objective to construct an instrument for openness. Time variation in this panel-data version of the F&R instrument is shown to correlate strongly with time variation in observed openness. Third (Section 4), we use the instrument to estimate the effect of openness on income per capita in different panels of countries for the period 1950-2008.

2. Disasters and the Gravity Equation

2.1. Hypotheses

How a natural disaster, such as an earthquake or a volcano eruption, can affect trade is most easily illustrated for the case of a small single-sector country that initially has balanced trade with the rest of the world. If a disaster hits, the country's productive capacity is temporarily reduced. It induces the country to smooth consumption by becoming a net importer. How large net imports become depends on the size of the shock, but also on the cost of international borrowing and the extent of transportation costs the country faces. In following periods the country has to repay its net foreign debt and switches from being a net importer to becoming a net exporter. So, the disaster *causes* intertemporal trade (which was zero initially) and makes the country permanently more open. Things are similar when the country already is a net importer to start with. When it is a net exporter initially, the disaster makes the country permanently less open to trade.

When there are multiple sectors or goods the disaster also affects *intratemporal trade* patterns. In a Heckscher-Ohlin model, where the disaster destroys part of the capital stock, an initially capital abundant country loses some of its comparative advantage. It exports less of the capital-intensive good and–if trade is balanced–imports less of the labor-intensive one. Openness falls. In a labor abundant country the opposite happens. The country appears even more capital-scarce after the disaster and therefore exports more of the labor-intensive good: openness increases. It follows that disasters can have ambiguous effects on openness, but it is clear that their effect is unlikely to be zero. How different degrees of openness to international trade affect–*ceteris paribus*–income in per capita terms is one empirical question that we ask in this paper.

2.2. Empirical strategy

To show that disasters excert an economically significant effect on trade patterns we estimate a fully fledged gravity regression on a panel of bilateral trade flows. We estimate the model in levels using the Pseudo Poisson Maximum Likelihood (PPML) approach advocated by Santos Silva and Tenreyro (2006) to account for zero trade flows, which would be left out of the analysis in a log-log model. Zeros make up more than 50% of observations in early years of our sample (1950-1960) and remain important afterwards. Noguer and Siscart (2005) have shown in the cross-section that out-of-sample predictions make the F&R instrument less precise. Thus, accounting for zeros is important.[5]

While Santos Silva and Tenreyro (2006) demonstrate their method using a pure cross-section, we work with a panel. We control for country-pair specific heterogeneity by running a conditional fixed-effects Poisson (FE PPML) model. This strategy takes all time-invariant bilateral determinants of trade (such as geographical distance, adjacency, historical ties) into account. The country-pair effects nest country dummies and therefore also control for the time-invariant component of countries' multilateral remoteness (reflecting geography and trade policy); see Anderson and van Wincoop (2003). However, over a long period of time, multilateral remoteness (MR) does change. We follow Baier and Bergstrand (2009), who have derived theory-consistent MR indices from a Taylor series expansion of the Anderson and van Wincoop (2003) gravity equation. We adapt their strategy to the PPML environment.[6,7]

In this section, we are mainly interested in showing how the number of major natural disasters D_t^i occurring in country i at time t affects M_t^{ij}, i.e., country $i's$ bilateral imports from some country j at time t. The presumption is that the direct effect of D_t^i on imports, measured by the coefficient δ_1 is positive, while the effect of a disaster on exports (measured by δ_2) is negative. We embed this into a comprehensive gravity equation of the form

$$M_t^{ij} = \exp\left[\delta_1 D_t^i + \delta_2 D_t^j + \boldsymbol{\gamma}_1'(\boldsymbol{\Gamma}_t^{ij} \times D_t^i) + \boldsymbol{\gamma}_2'(\boldsymbol{\Gamma}_t^{ij} \times D_t^j) + \boldsymbol{\xi}'\mathbf{X}_t^{ij} + \boldsymbol{\nu}^{ij} + \boldsymbol{\nu}_t\right] + \varepsilon_t^{ij}, \quad (1)$$

where $\boldsymbol{\Gamma}_t^{ij} = [\ln DIST^{ij}; \ln FINDIST^i, \ln FINDIST^j; ADJ^{ij}; \ln AREA^i, \ln AREA^j;$

[5]Besides accounting for zeros, Santos Silva and Tenreyro (2006) show that the PPML model yields consistent estimates when trade flows are measured with additive errors while OLS methods would produce inconsistent results.

[6]A popular alternative way to account for multilateral remoteness would be to include the full array of interaction terms between country and year dummies. The drawback from this strategy is that the direct effect of country-level variables such as the incidence of a disaster cannot be identified.

[7]Wooldridge (2002), p. 676, emphasizes that *"while the leading application of the Poisson estimator is to count data, the fixed-effect Poisson estimator works whenever the conditional mean assumption holds. Therefore, the dependent variable could be a nonnegative continuous variable..."*. Santos Silva and Tenreyro (2006) provide a justification of the validity of the conditional mean assumption; see also Henderson and Millimet (2008) on the advantages of the Poisson model in gravity models. See Liu (2009) for a recent example of a gravity model estimated using a conditional fixed-effects PPML strategy.

$\ln POP_t^i, \ln POP_t^j; \ln(y_t^j/y_t^j)]$. The variable $DIST^{ij}$ denotes geographical distance between countries i and $j's$ capitals (in kilometers), $FINDIST^i$ is Rose and Spiegel's (2009) measure of country $j's$ international financial remoteness (based on the country's shortest geographical distance to a major financial hub), $\ln(y_t^j/y_t^j)$ is the ratio of the importer's per capita GDP to the exporter's per capita GDP, $\ln AREA^i$ measures a country's land surface and $\ln POP^i$ its population. ADJ^{ij} denotes an adjacency dummy. Dependent on the model we use different combinations of $\mathbf{\Gamma}_t^{ij}$. The vector of controls $\mathbf{X}_t^{ij} = [\ln GDP_t^i, \ln GDP_t^j; \ln(y_t^i/y_t^j); \ln DIST^{ij}, ADJ^{ij}, COLON_t^{ij};$ $FTA_t^{ij}, WTO_t^{ij}, CU_t^{ij}; MRDIST_t^{ij}, MRADJ_t^{ij}]$ contains the logs of country $i's$ and $j's$ GDPs, their log levels of GDP per capita, the log of geographical distance, dummies describing the past or current colonial relationship of countries, dummies for joint membership in a free trade agreement (FTA_t^{ij}), in the World Trade Organization (WTO_t^{ij}) or in a currency union (CU_t^{ij}), and multilateral resistance terms based on geographical distance ($MRDIST_t^{ij}$) and bilateral adjacency ($MRADJ_t^{ij}$). We include relative GDP per capita levels as proxies for relative per capita capital stocks. The vector $\boldsymbol{\nu}^{ij}$ is a complete collection of country-pair dummies that account for all time-invariant bilateral determinants of trade. The vector $\boldsymbol{\nu}_t$ collects year dummies. Of course, in our conditional fixed-effects model, time-invariant country-pair specific variables contained in $\mathbf{X}_t^{ij}$ drop out. We estimate the variance-covariance matrix using a heteroskedasticity-robust estimator that also allows for clusters at the dyadic level. This is strongly recommended by Stock and Watson (2008) to avoid inconsistent estimates due to serial correlation. Note that coefficients obtained from the Poisson model can be interpreted in the usual way (e.g., as elasticities if the value of the covariates are in logs.)[8]

When estimating equation (1), we assume that, conditional on country-pair effects, the covariates are *orthogonal to shocks in bilateral trade volumes*. This is central for identifying the effect of disasters on trade. Note that, when constructing an instrument

[8]All estimations are carried out in STATA 11 MP. Codes and data are available upon request from the authors.

for openness in Section 3 of this paper, we are not primarily interested by consistent estimation of (1). However, we require that all variables used for the construction of the instrument are *orthogonal to shocks in country level GDP per capita.*

2.3. Data

Disasters. Data on natural disasters come from the *Emergency Events Database* (EM-DAT 2010) maintained by the Center for Research on the Epidemiology of Disasters. While the database reports natural and technological catastrophes, our analysis uses only *natural disasters*, the occurrence of technological disasters being linked in obvious ways to economic development. Moreover, we select disasters that are evidently orthogonal to the local economic situation. These are 'large' earthquakes, volcanic eruptions, tsunamis, storms, storm floods, and droughts.[9] The qualification 'large' makes sure that a disaster, such as a storm or a drought, is of a sufficiently large dimension not to be caused by the local determinants but rather by global phenomena such as, e.g., global warming. We define 'large' disasters following Gassebner, Keck and Teh (2010) as events that (i) caused 1,000 or more deaths; or (ii) injured 1,000 or more persons; or (iii) affected 100,000 or more persons. There is a total of 5,704 natural disasters between 1950 and 2008 in our dataset, 1,091 thereof are large in scale.[10] In our robustness checks we work with alternative definitions of disasters (such as a broader specification of disasters that includes *all* kinds of natural disasters that can be found in the EM-DAT data[11] or counting *all* sizes of disasters (i.e, large and small); results remain generally in line. In our benchmark regressions, the disaster variable reports the number of 'large' catastrophes that happen in a country during a year. In some regressions we use the number of disasters cumulated over a period of time.[12]

[9] Hence, we disregard extreme temperature, floods, insect infestations, (mud)slides, and wildfires. EM-DAT also classifies epidemics as natural disasters; we exclude them from our analysis.

[10] When we consider all types of natural disasters in the EM-DAT under the broad categorization, the total number amounts to 9,310 disasters, 1,740 thereof being large disasters.

[11] Still excluding epidemics, though.

[12] In principle, the data base reports the estimated loss due to a disaster in terms of money or lives. These numbers reflect the capacity of countries to deal with natural disasters, which, in turn, is likely to

Figure 1: Average Number of Natural Disasters per Country (1950-2008)

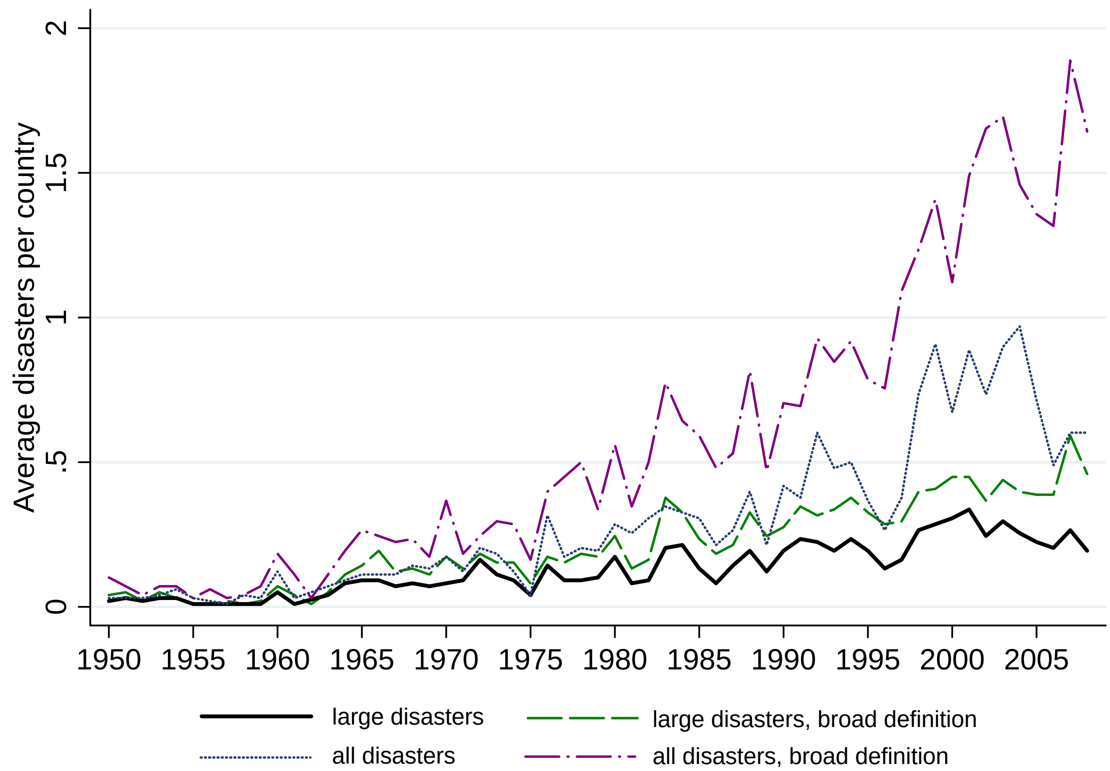


Figure 1 plots the average number of disasters per country over the period 1950-2008.[13] The bold line refers to large, narrowly defined natural disasters. In the 1960s, the average country was affected by about 0.1 disaster per year; this number has increased to 0.3 in more recent years. An EM-DAT report by Guha-Sapir et al. (2004) attributes this trend to improved monitoring and reporting and to the increased frequency of extreme weather events (possibly related to global warming). The upward trend is more pronounced when small-scale disasters are included, and when events such as extreme temperature, floods, insect infestations, (mud)slides, and wildfires are accounted for. In our benchmark regressions, we use large, narrowly defined disasters

be a function of development. Hence, we refrain from using those measures in order not to contaminate our disaster variable by income.

[13]The underlying country sample is the the Mankiw et al. (1992) sample which we also use in our cross-country regressions.

and include year effects to account for improved reporting.

Figure 2: Average Number of Large Disaster (1992-2008)

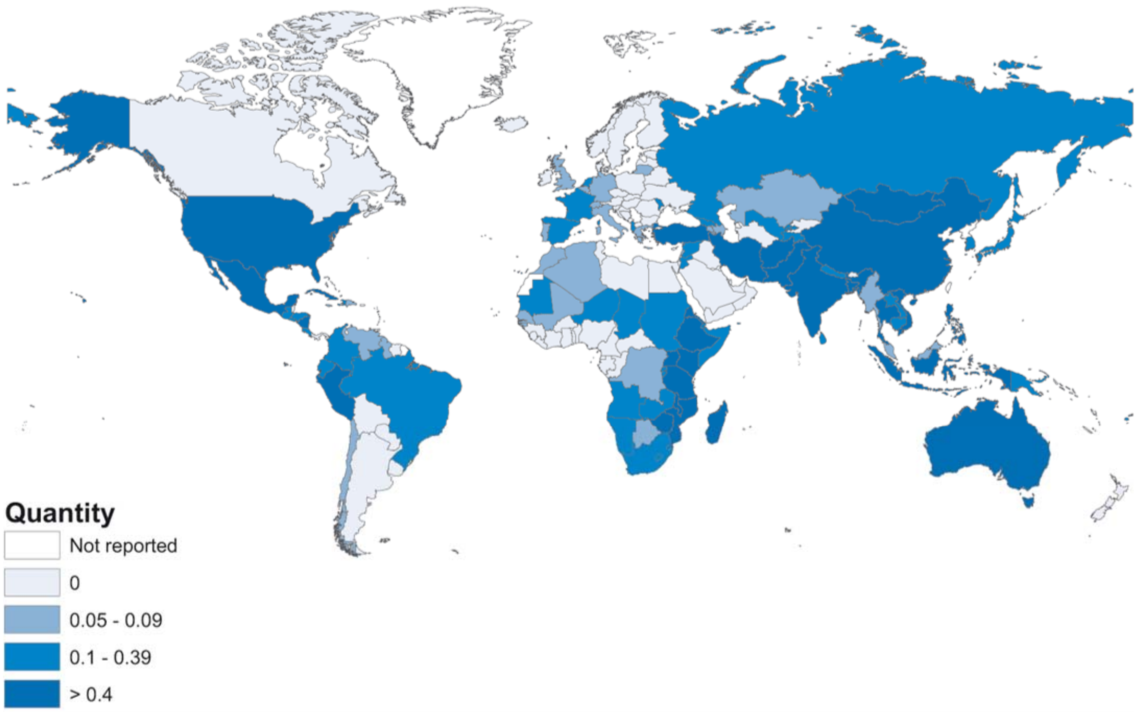
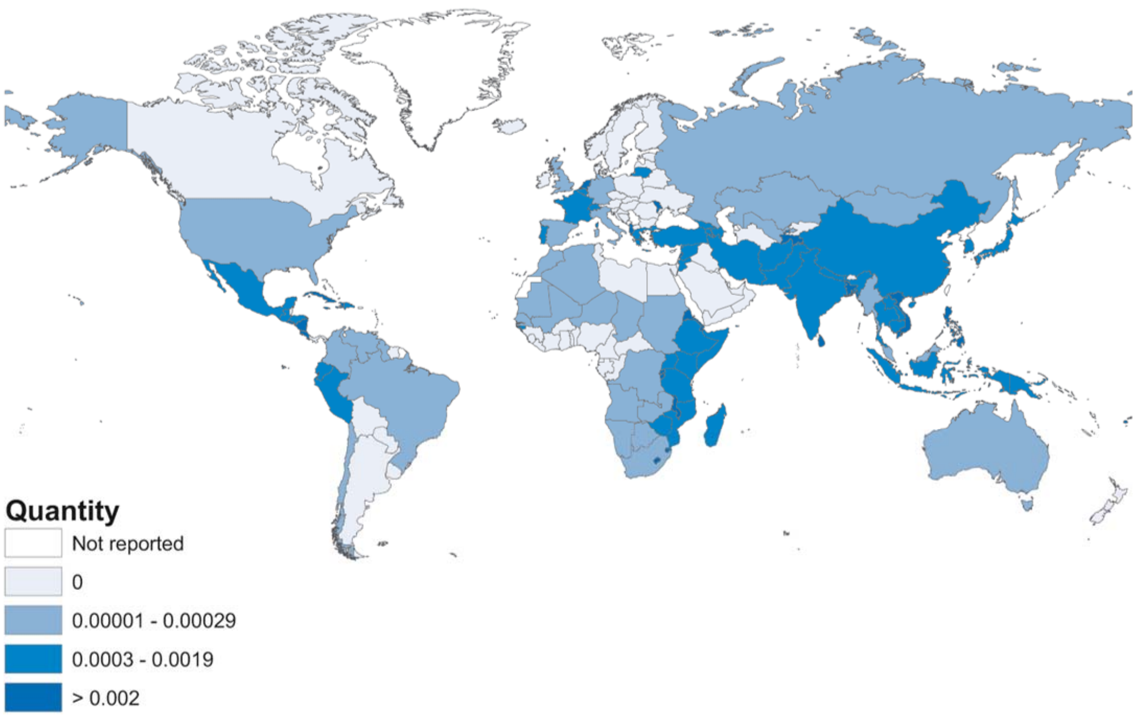


Figure 2 maps the geographical distribution of large-scale natural disasters across countries. Over the time period 1992-2008 (where the number of countries has been fairly stable), countries differ quite substantially with respect to the incidence of natural disasters. While China experienced an average of 7 large disasters per year, the Philippines had 3.6, India had 1.6, and the US 0.6, while about 80 countries, such as Canada, Libya, the Central African Republic, or Argentina were never affected. Normalizing by surface area to account for the size of countries in Figure 3, we observe that countries in Asia and at the Pacific rim are more strongly affected. This is not surprising, given the geological characteristics of those places. The popular belief that disasters lump around the equator does not seem to bear out. We find no correlation between the average number of large disasters and the distance from the equator (cal-

Figure 3: Average Number of Large Disasters by Surface Area (1992-2008)



culated as the absolute value of latitude divided by 90), while the correlation between the disaster frequency normalized by area is slightly negative (-0.14). Also, when we regress the average number of large catastrophes on distance from the equator we find no significant relation.

Other covariates. Data on nominal import and export values measured in current USD come from the IMF's *Direction of Trade Statistics* (2009). Nominal income data in current USD and total population data combine two sources: the World Bank's *World Development Indicators* database and, for 1950-1959, Barbieri (2002). Geographic and bilateral trade impediments and facilitating factors –land area, great circle distance, common border, and colonial relations– are taken from CEPII's *Geographic and Bilateral Distance Database* (2005). As a measure of international financial remoteness we use the natural logarithm of the great-circle distance to the closest offshore major financial center (London, New York, or Tokyo) which is provided by Rose and Spiegel (2009).[14] Data on levels of nominal GDP (in US dollars) come from the *World Development Indicators* database. Real GDP per capita data, aggregate openness, or population are taken from the Penn World Tables mark 7.0 database. Information on country-pairs' joint membership in FTAs, the WTO, or in a currency union are from the WTO. Tables 9 and 10 in the Appendix contain summary statistics for the gravity-type regressions and for the cross-country income regressions, respectively.

Country samples. We focus on three different samples: (i) a sample of 96 countries suggested by Mankiw et al. (1992), henceforth MRW, that excludes countries for which oil-production was the dominant industry according to Mankiw et al. (1992) and states that formerly were part of the Soviet Union, or Soviet satellite states, (ii) the slightly smaller intermediate sample of MRW, which excludes countries whose income data are likely to be subject to measurement error, and (iii) the full sample for which data is available (at most 162 countries).[15] See the Appendix for a list of countries. In the MRW sample from 1950-2008, the probability that a given country is hit by a large disaster, according to our definition, is about 0.15 each year.

2.4. Gravity results

Table 1 shows estimates of the gravity model (1). All regressions include log GDP levels, the ratio of the two countries' GDP per capita levels as proxies for relative per capita capital endowments, proxies for the stance of trade policy (joint FTA, WTO and currency union membership dummies), the multilateral resistance measures as proposed by Baier and Bergstrand (2009), and year dummies.[16]

Columns (1) to (4) report estimates for the MRW sample and yearly disasters. The

[14] We set financial remoteness to zero for the countries where those financial centers are located.

[15] The country samples suggested by MRW are well established in the growth literature. The MRW sample has also been used by F&R (1999). We obtain similar results using a sample suggested by Baier and

Table 1: The effect of natural disasters on bilateral trade flows (yearly data, 1950-2008), conditional fixed-effects Poisson model

Dependent Variable:	Bilateral import flows of i from j						
Disaster variable:			yearly			cumulated	
Sample:	MRW	MRW	MRW	MRW	FULL	MRW	FULL
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Disasters, importer (D_t^i)	0.020**	0.252**	0.288***	0.325***	0.182**	0.214***	0.126**
	(0.01)	(0.12)	(0.10)	(0.10)	(0.09)	(0.07)	(0.06)
Disasters, exporter (D_t^j)	-0.009	-0.219*	-0.267*	-0.251**	-0.256**	-0.102***	-0.080**
	(0.01)	(0.12)	(0.14)	(0.13)	(0.12)	(0.03)	(0.03)
Interaction terms							
$D_t^i \times \ln FINDIST^i$		-0.033**	-0.033*	-0.032*	-0.008	-0.014	-0.016*
		(0.02)	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)
$D_t^i \times \ln DIST^{ij}$			-0.005	-0.005	-0.009**	0.002	0.002**
			(0.01)	(0.01)	(0.00)	(0.00)	(0.00)
$D_t^i \times \ln(y_t^i/y_t^j)$				0.010***	0.012***	0.002	0.003***
				(0.00)	(0.00)	(0.00)	(0.00)
$D_t^j \times \ln FINDIST^j$		0.041**	0.040**	0.044***	0.051***	0.019***	0.031***
		(0.02)	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)
$D_t^j \times \ln DIST^{ij}$			0.007	0.009	0.006	-0.002**	-0.003***
			(0.01)	(0.01)	(0.00)	(0.00)	(0.00)
$D_t^j \times \ln(y_t^i/y_t^j)$				-0.011***	-0.006**	-0.006***	-0.005***
				(0.00)	(0.00)	(0.00)	(0.00)
Controls							
$\ln GDP_t^i$	0.492***	0.506***	0.508***	0.508***	0.757***	0.474***	0.723***
	(0.11)	(0.10)	(0.10)	(0.10)	(0.04)	(0.10)	(0.04)
$\ln GDP_t^j$	0.906***	0.909***	0.907***	0.906***	0.760***	0.926***	0.780***
	(0.12)	(0.11)	(0.11)	(0.11)	(0.04)	(0.11)	(0.04)
$\ln(y_t^i/y_t^j)$	0.199*	0.196*	0.194*	0.194*	-0.028	0.228**	-0.027
	(0.10)	(0.10)	(0.10)	(0.10)	(0.03)	(0.10)	(0.03)
FTA_t^{ij}	0.188**	0.193**	0.193**	0.193**	0.234***	0.181**	0.227***
	(0.09)	(0.09)	(0.09)	(0.09)	(0.07)	(0.09)	(0.07)
CU_t^{ij}	0.312***	0.305***	0.305***	0.305***	0.328***	0.326***	0.345***
	(0.05)	(0.05)	(0.05)	(0.05)	(0.03)	(0.05)	(0.04)
WTO_t^{ij}	0.216***	0.205***	0.204***	0.213***	0.241***	0.216***	0.256***
	(0.07)	(0.06)	(0.06)	(0.06)	(0.03)	(0.06)	(0.03)
Fixed Effects							
Pair	YES	YES	YES	YES	YES	YES	YES
Year	YES	YES	YES	YES	YES	YES	YES
Multilateral resistance	YES	YES	YES	YES	YES	YES	YES
Observations	387,529	387,529	387,529	387,529	763,240	387,529	763,240
Loglikelihood	-6.19e+06	-6.13e+06	-6.13e+06	-6.12e+06	-1.12e+07	-6.09e+06	-1.12e+07
Chi2	49141.22	53857.81	56431.64	59138.99	46855.31	53753.94	49967.53

Note: Constant and year-fixed effects are not reported. Country-pair clustered robust standard errors are reported in parentheses. Multilateral resistance terms and interactions - where applicable - are included in all models but not reported. Direct multilateral resistance terms are restricted to have identical but opposing signed coefficients as in Baier and Bergstrand (2009). Disasters are the number of large-scale disasters in country i or country j, respectively. Column (1) to (4) and (6) use the sample suggested by Mankiw et al. (1992), henceforth MRW sample, while column (5) and (7) use the full sample depending on data availability. * p<0.1, ** p<0.05, *** p<0.01.

regressions differ with respect to the included disaster×geography interaction terms. As in the cross-sectional gravity model of Santos Silva and Tenreyro (2006), the elasticities on GDPs are below unity. The joint FTA membership increases bilateral trade by about 19%. Joint WTO membership increases trade by a slightly larger amount while having a common currency boosts trade by about 31%.[17]

The most parsimonious regression reported in column (1) shows that a major disaster in the importer country increases its imports by about 2% on average. A disaster striking the exporter does not seem to adversely affect imports from that country. This picture changes in column (2), which includes the interaction between countries' financial remoteness and the disaster variable. An importer that has maximum access to international financial markets ($\ln FINDIST^i = 0$) experiences a surge of imports by about 25%. If financial remoteness takes the mean value ($\ln FINDIST^i = 7.3$), the increase in imports drops to about 1%. Disasters clearly reduce imports when financial distance is substantially larger than the sample average. Similarly, a financially central country sees a 22% fall in its exports after a disaster, but that effect vanishes when financial remoteness increases. These results are in line with intuition: a financially constrained importer cannot borrow against future output in order to increase imports when it is struck by a disaster. A financially constrained country cannot run down exports in the face of a crisis as it needs export revenue to finance imports.

Column (3) includes interactions of disasters with geographical distance to explore the possibility that the reaction of bilateral trade volumes to disasters depends on bilateral trade costs. We do not find evidence for this hypothesis when working with the MRW sample and a yearly measure of disaster incidence. Column (4) interacts the ratio of the importers per capita GDP to that of the exporter with the disaster variables.

Bergstrand (2007), results of which can be obtained on request.

[16]In Table 1, we show results obtained from using all large disasters as our key right-hand-side variable. Using the more narrow definition of the disaster variable, which is preferable for construction of the instrument, leads to very similar results but is unnecessarily restrictive in a gravity setup.

[17]See Liu (2009) for a comparable conditional fixed-effects PPML model and corresponding results on the WTO effect.

When that ratio is high, relative capital abundance is supposedly high, too. If a relatively capital abundant country–supposedly an exporter of capital-intensive goods–is struck by a disaster, its exports should go down by more than if the country is labor abundant. Its imports (labor-intensive goods according to the Heckscher-Ohlin logic) should go down. We find evidence for the first, but not for the second prediction. The reason may be that relative capital abundance makes it easier to borrow internationally as collateral is more readily available. Then, we would indeed predict that a higher value of $ln(y_i/y_j)$ should increase the effect of disasters on imports.

Column (5) uses the full rather than the MRW sample; this more than doubles the number of observations. The positive effect of disasters on imports and the negative one on exports remain intact; sign patterns of interaction terms also remain the same as in column (4), but levels of statistical significance are better for interactions with exporter characteristics than with importer characteristics. Columns (6) and (7) work with the number of disasters cumulated over the last five years instead than with the number of disasters in the current year. The idea is that disasters have a long-lasting effect on trade flows. However, when comparing (6) to (4) and (7) to (5), the point estimates of the disaster variables turn out smaller (in absolute values) than when disasters are contemporaneous to the trade variable. Sign patterns on the interaction terms remain fairly similar.

The results of Table 1 support our idea that disasters affect bilateral trade flows and that their effect is conditioned by variables such as financial remoteness. In the next section, we modify the gravity equation to the specific needs of our instrumental variables strategy.

3. The IV strategy

Our ambition is to estimate equation (4) of Frankel and Romer (1999) in a panel setup. To this end, we specify the income equation as

$$\ln \bar{y}_{\tau}^{i} = \beta OPEN_{\tau}^{i} + \pi \ln POP_{\tau}^{i} + \sum_{s \leq \tau} \chi_s D_s^i + \boldsymbol{\nu}^i + \boldsymbol{\nu}_{\tau} + \varepsilon_{\tau}^{i}, \quad (2)$$

where we use τ to denote 5-year averages to purge the data from the influence of business cycles.[18] The relationship explains log per capita income in purchasing power parity terms $\bar{y}_{\tau}^{i}$ as a function of openness to international trade $(OPEN_{\tau}^{i})$ as measured by the sum of imports plus exports over GDP. The log of population (POP_{τ}^{i}) proxies market size which, in turn, captures the extent of within country trade. The term $\sum_{s \leq \tau} \chi_s D_s^i$ accounts for the direct effect of contemporaneous and lagged natural disasters on per capita income. It is important to rule out that the channel of causality runs from disasters to GDP to trade rather than from disasters to trade to GDP.[19]

By including a full array of country fixed-effects ν^i we account for country-specific and time-invariant determinants of openness (such as geographical characteristics), and GDP per capita (such as proxies for institutional quality–distance to the equator, settler mortality). Common period-effects are controlled for by including a host of period dummies ν_{τ}.

It is well understood that $OPEN_{\tau}^{i}$ and the error term ε_{τ}^{i} in equation (2) are likely to be correlated. The first reason is *reverse causality*. If richer countries are more open (either because they are more likely to have low barriers to trade or because the elasticity of demand for traded goods is larger than unity), estimating (2) by OLS will bias the estimate of β upwards. F&R estimate a cross-section and deal with reverse causality by instrumenting $OPEN_{\tau}^{i}$ by its geographical component. Instrumentation also solves

[18] Feyrer (2009) uses observations at five year intervals without averaging. We prefer averages, but five year intervals work equally well.

[19] In the robustness checks (Table 8) we experiment extensively with different specifications of the potential direct disaster-GDP/capita link.

a second issue, namely the fact that $OPEN_\tau^i$ is very likely to be a very noisy proxy for the true role that trade plays for the determination of per capita income. OLS estimates will therefore be downward biased. The third reason is *omitted variable bias*. Rodriguez and Rodrik (2000) and others have shown that F&R's estimate is not robust to including additional geographical controls such as distance to the equator. The most compelling way to control for country-specific observed and unobserved heterogeneity is to exploit the panel dimension of the data and include country fixed-effects such as in equation (2). However, F&R's original instrument for openness is time-invariant and cannot be employed in a panel setup. The present section of this paper proposes an instrument for openness that does have time variation. The starting point, made in Table 1, is that natural disasters affect countries' trade flows.

3.1. Instrument construction

The construction of the instrument follows Frankel and Romer (1999): using a gravity-type equation, we regress bilateral trade openness $\omega_t^{ij} = (M_t^{ij} + M_t^{ji})/GDP_t^i$ on a host of variables that are strictly exogenous to real per capita income such as natural disasters, interaction of disasters with geographical variables. Then, we construct an exogenous proxy for multilateral openness Ω_t^i based on predicted bilateral openness

$$\Omega_t^i = \sum\nolimits_{j \neq i} \hat{\omega}_t^{ij}. \tag{3}$$

Averaging over 5-year intervals, we obtain Ω_τ^i which will be our instrument for $OPEN_\tau^i$.

Our bilateral openness equation is based on equation (1). However, it excludes all potentially endogenous regressors, and includes additional interaction terms between disasters and geographical variables. We continue to use Poisson Pseudo Maximum Likelihood, since 26% of our observations come with zero-trade flows and taking those into account substantially improves the correlation between $\Delta OPEN_\tau^i$ and $\Delta\Omega_\tau^i$.[20] Our

[20] Noguer and Siscart (2005) argue that the F&R strategy is much improved when avoiding out-of-sample prediction.

preferred specification includes importer, exporter and year dummies. We estimate the relationship on yearly data, but will judge the validity of the resulting instrument based on five-year averages. The preferred regression takes the form

$$\omega_t^{ij} = \exp\left[\delta_3 D_t^i + \delta_4 D_t^j + \boldsymbol{\gamma}_3'(\boldsymbol{\Phi}_t^{ij} \times D_t^i) + \boldsymbol{\gamma}_4'(\boldsymbol{\Phi}_t^{ij} \times D_t^j) + \boldsymbol{\zeta}'\mathbf{Z}_t^{ij} + \boldsymbol{\nu}^i + \boldsymbol{\nu}^j + \boldsymbol{\nu}_t\right] + \varepsilon_t^{ij}, \tag{4}$$

where the vectors $\boldsymbol{\Phi}_t^{ij}$ and $\mathbf{Z}_t^{ij}$ correspond to the vectors $\boldsymbol{\Gamma}_t^{ij}$ and $\mathbf{X}_t^{ij}$, respectively, except that they do not include GDP related variables. Instead it contains the logs of population of countries i and j. Note that in some regressions we substitute individual country effects $\boldsymbol{\nu}^i$ and $\boldsymbol{\nu}^j$ by pair effects $\boldsymbol{\nu}^{ij}$. The gravity equation used to generate the projection $\hat{\omega}_t^{ij}$ will *not* necessarily yield consistent and unbiased parameter estimates. This is not required for the construction of the instrument Ω_t^i, for which we require exogeneity of regressors in (4) and whose quality depends solely on its correlation with observed openness. Hence, we design the bilateral openness equation to maximize the correlation between $\Delta OPEN_\tau^i$ and $\Delta\Omega_\tau^i$, where Δ is the first-difference operator.

Table 2 reports estimates of the bilateral openness regressions. Columns (1) to (3) draw on the MRW sample while columns (4) to (6) use the full sample. Both samples are based on yearly data for 1950-2008. All standard errors allow for clustering at the country-pair level. Column (1) shows that bilateral trade openness increases by 10% when the importer is struck by a natural disaster. As in Table 1, that effect is reduced when the importer is financially remote. Population size of the importer, introduced as a proxy for GDP, appears to lower imports while that of the exporter increases it. This is compatible with the idea that population size is a proxy for within-country trade. Trade policy variables have the right signs but are not statistically significant at conventional levels. Column (4) reports a regression that is identical to the one shown in (1), but draws on the full sample. Here, trade policy variables have the expected signs and are statistically significant.

Columns (2) and (5) repeat the exercise of columns (1) and (4), but add more inter-

Table 2: Gravity-type models for the instrument (1950-2008), Poisson

Dependent Variable:	Bilateral trade openness of i					
Sample:		MRW			Full	
Estimation Method:	FE PPML	FE PPML	**PPML**	FE PPML	FE PPML	**PPML**
	(1)	(2)	(3)	(4)	(5)	(6)
Disasters, importer (D_t^i)	0.101*	0.821***	0.754***	0.222***	0.197	0.292**
	(0.06)	(0.23)	(0.21)	(0.05)	(0.13)	(0.13)
Disasters, exporter (D_t^j)	0.015	-0.456**	-0.872***	0.011	-0.811***	-0.836***
	(0.05)	(0.19)	(0.25)	(0.03)	(0.12)	(0.12)
Interactions						
$D_t^i \times \ln FINDIST^i$	-0.014*	-0.012	-0.011	-0.025***	-0.020***	-0.022***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
$D_t^i \times \ln AREA^i$		-0.041***	-0.048***		-0.013*	-0.023***
		(0.01)	(0.01)		(0.01)	(0.01)
$D_t^i \times \ln POP_t^i$		-0.012	-0.003		0.009	0.012
		(0.01)	(0.01)		(0.01)	(0.01)
$D_t^i \times ADJ^{ij}$		0.013	0.122		0.042	0.181***
		(0.06)	(0.08)		(0.03)	(0.05)
$D_t^i \times \ln FINDIST^i$	-0.013	-0.011	0.007	0.010*	0.006	0.018***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)
$D_t^j \times \ln AREA^j$		-0.000	-0.025		0.002	-0.018
		(0.01)	(0.02)		(0.01)	(0.01)
$D_t^j \times \ln POP_t^j$		0.025*	0.062**		0.041***	0.054***
		(0.01)	(0.03)		(0.01)	(0.01)
$D_t^j \times ADJ^{ij}$		0.089*	0.171*		0.052***	0.005
		(0.05)	(0.10)		(0.02)	(0.03)
Controls						
$\ln POP_t^i$	-0.527***	-0.525***	-0.528***	-0.152***	-0.152***	-0.173***
	(0.19)	(0.19)	(0.17)	(0.04)	(0.04)	(0.04)
$\ln POP_t^j$	1.145***	1.135***	0.169***	0.527***	0.525***	0.157***
	(0.23)	(0.23)	(0.05)	(0.16)	(0.16)	(0.04)
$\ln DIST^{ij}$			-0.785***			-0.954***
			(0.05)			(0.04)
ADJ^{ij}			0.352**			0.218**
			(0.15)			(0.10)
Colonial relation^{ij}			0.530**			0.562***
			(0.23)			(0.17)
Common colonizer^{ij}			0.470			0.710***
			(0.30)			(0.18)
Colonial relation post 1945^{ij}			1.172***			1.230***
			(0.28)			(0.21)
Same country^{ij}			0.734***			0.498***
			(0.17)			(0.13)
FTA_t^{ij}	0.030	0.028		0.091*	0.098*	
	(0.08)	(0.08)		(0.05)	(0.05)	
CU_t^{ij}	0.104	0.104		0.198***	0.201***	
	(0.13)	(0.13)		(0.07)	(0.07)	
WTO_t^{ij}	0.102	0.101		0.182***	0.169***	
	(0.08)	(0.08)		(0.05)	(0.05)	
Fixed Effects						
Pair	YES	YES	-	YES	YES	-
Importer, Exporter	-	-	YES	-	-	YES
Year	YES	YES	YES	YES	YES	YES
Multilateral resistance	YES	YES	-	YES	YES	-
Observations	395,948	395,948	418,165	787,324	787,324	833,529
Loglikelihood	-6422.992	-6421.877	-9316.681	-1.12e+04	-1.12e+04	-1.71e+04
Chi2	1450.32	1474.327	26577.73	1436.589	1793.596	35863.29

Note: Fixed-effects and multilateral resistance terms included (when applicable) but not reported. Country-pair clustered robust standard errors in parenthesis. Disasters are the number of large-scale disasters in i or j, respectively, according to the decision rule. Column (1) to (3) use the Mankiw et al. (1992) sample, while column (4) to (6) use the full sample. Column (6) is our preferred specification. * p<0.1, ** p<0.05, *** p<0.01.

action terms with geographical variables and countries' population sizes. In the MRW sample disasters increase imports, while in both the MRW and the full sample catastrophes decrease exports. These effects are importantly conditioned by the financial distance variables, geographical area, and population. Financially remote states experience an increase in exports as a consequence of a natural disaster. Countries with large areas see their imports increase (and their exports decrease) by less than countries with small areas. Similar results hold for population size. These effects are intuitive: larger countries are better able to absorb shocks domestically than smaller ones.

Column (3) and (6) are our preferred specification for the construction of the instrumental variable. Those equations do not include pair effects – we substitute these by individual country effects – , nor do they incorporate the Baier and Bergstrand (2009) multilateral resistance variables or trade policy variables (as those are very likely to be endogenous to per capita GDP). Instead, the columns add bilateral distance, adjacency, and various variables representing historical ties. In earlier equations these variables were taken care of by pair-effects. In both the MRW and the full sample, disasters affect bilateral openness; the interactions with exogenous country size variables (area, population) and with our exogenous proxy for financial remoteness, as well as time-invariant bilateral determinants are statistically significant and have the expected signs.

Column (6) is our preferred specification of the bilateral openness model. Following F&R and Feyrer (2009), we construct equation (3) on the full sample, even when we use the MRW sample in estimating (2). This makes sure that we base the openness instrument on trade with all possible trade partners.[21]

3.2. Quality of the instrument

For constructed openness to be a qualitatively good and valid instrument, Ω^i_τ and $OPEN^i_\tau$ need to be positively and sufficiently strongly correlated. The correlation between the

[21] Results are qualitatively similar and robust when we use the broader definition of large natural disasters, and can be obtained on request.

level of the predicted and the actual trade share – the latter of which we obtain from the PWT 7.0 – amounts to 79%. Since the coefficients in our income equation are identified using the time-variation of variables, the quality of the instrument is best shown by correlating $\Delta OPEN^i_\tau$ and $\Delta\Omega^i_\tau$. Figure 4 plots the change in observed openness $\Delta OPEN^i_\tau$ against the change in constructed openness $\Delta\Omega^i_\tau$ for our different samples and for a maximum of 11 differences over 5-year averages. In the benchmark sample (MRW), the relationship between constructed and observed openness is positive (coefficient of 0.225) and statistically significant at the 1% level. The correlation is slightly higher in the intermediate sample, which was described in section 2.3., and somewhat lower in the full sample. The statistical relationship, however, always remains highly statistically significant.

Figure 4: Changes in Actual versus Constructed Openness (1950-2008)

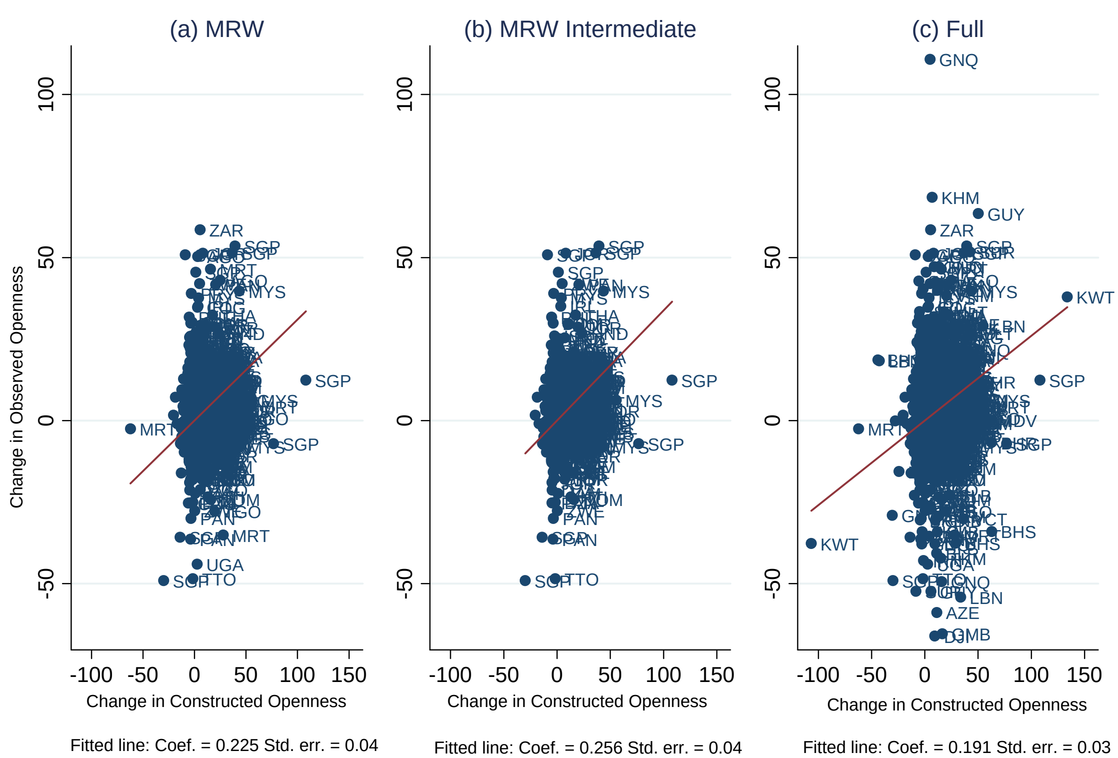


Table 3 assesses the quality of the instrument in more detail by regressing observed on constructed openness and controlling for the covariates of our second stage regression, namely population size, as well as the contemporaneous and the lagged large disaster variables. We take care of the panel dimension by using the within-estimator on five-year averages. The equations include a full set of period dummies; standard errors are corrected for by clustering at the country level. For all of our three samples, constructed openness correlates strongly with observed openness; see columns (1), (5), and (9). Holding time-invariant country characteristics fixed and controlling for period trends, a 1% increase in the constructed trade share implies an increase in observed openness ranging between 0.3 and 0.9%. These effects are stronger than what Figure 4 suggests, mostly because of the presence of period dummies in Table 3. The within-R^2 measure (based on time variation in the dependent and independent variables) ranges between 0.3 and 0.6.

As the constructed trade share Ω^i_τ is in fact highly correlated with the size of an economy and natural disasters, we need to examine whether the instrument provides information beyond that contained by these variables. Column (2), (6), and (10) show estimations controlling for the log of population, the frequency of natural disasters, and the lag thereof on observed aggregated trade patterns, additionally including the constructed instrument in column (3), (7), and (11). Regressions (4), (8), and (12) add the first time lag of the constructed openness measure. The estimates on the constructed trade share remain significant and increase slightly in magnitude due to the inclusion of the lag of disasters. In all regressions, lagged disasters tend to increase the level of openness, the effect being economically substantial and significant. The coefficient on population is insignificant in all settings. The within explanatory power of the estimation remains quite strong and stable across different setups. Hence, the information contained in the aggregated instrument reaches beyond that contained in natural disasters and total population. Regressions (4), (8), and (11) will be our preferred first-stage regressions. Since Ω^i_τ is strictly exogenous to income $\ln \bar{y}^i_\tau$, $\Omega^i_{\tau-1}$ will be similarly

Table 3: Quality of the Instrument (1950-2008) (fixed-effects estimates, 5-year averages)

Dependent variable:	Observed openness ($OPEN_\tau^i$)											
Sample:	MRW				MRW Intermediate				Full			
	(1)	(2)	(3)	**(4)**	(5)	(6)	(7)	**(8)**	(9)	(10)	(11)	**(12)**
Constructed openness (Ω_τ^i)	0.503***		0.634***	0.356***	0.495***		0.656***	0.366**	0.302***		0.361***	0.298***
	(0.07)		(0.11)	(0.11)	(0.07)		(0.11)	(0.15)	(0.09)		(0.12)	(0.09)
$\Omega_{\tau-1}^i$				0.356***				0.347***				0.101
				(0.08)				(0.10)				(0.11)
$\ln POP_\tau^i$		-0.056	-0.021	-0.024		-0.036	-0.014	-0.019		-0.066	-0.038	-0.038
		(0.07)	(0.07)	(0.07)		(0.08)	(0.07)	(0.07)		(0.07)	(0.07)	(0.07)
D_τ^i		0.021	0.009	0.019		0.022	0.012	0.020		0.017	0.009	0.010
		(0.02)	(0.02)	(0.02)		(0.02)	(0.02)	(0.02)		(0.01)	(0.01)	(0.01)
$D_{\tau-1}^i$		0.050**	0.063***	0.055***		0.043*	0.054***	0.048***		0.034**	0.039**	0.037**
		(0.02)	(0.02)	(0.02)		(0.02)	(0.02)	(0.02)		(0.02)	(0.02)	(0.02)
Fixed Effects												
Country	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Period	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Observations	1,007	919	919	919	804	736	736	736	1,448	1,311	1,311	1,311
R^2(within)	0.464	0.407	0.501	0.519	0.510	0.452	0.557	0.572	0.305	0.292	0.326	0.328
R^2(between)	0.833	0.203	0.854	0.851	0.845	0.203	0.865	0.866	0.722	0.410	0.707	0.708
R^2(overall)	0.639	0.205	0.736	0.748	0.645	0.166	0.750	0.762	0.525	0.343	0.622	0.631
F-Test	26.69	17.84	28.83	28.88	25.70	16.48	28.59	27.52	14.45	13.83	15.04	14.39

Note: Constant and period-fixed effects included but not reported. Country clustered robust standard errors in parenthesis. Disasters are the number of large-scale disasters according to the decision rule. Column (1) to (4) use the sample suggested by Mankiw et al. (1992), column (5) to (8) use a sample by Mankiw et al. (1992) that excludes countries likely to be subject to measurement error, while column (9) to (12) use the full sample. * p<0.1, ** p<0.05, *** p<0.01.

exogenous and can be used as an additional instrument so that we can test for *overidentifying restrictions*.[22] In column (4) and (8) the lag has a very similar effect on observed openness than the contemporaneous value Ω^i_τ and the within R^2 increases only slightly.[23]

Most importantly, in the 2SLS approach we need to be able to exclude $\hat{\omega}^i_\tau$ from the second-stage regression. Even though we cannot test for the exclusion restriction directly, real income per capita cannot cause predicted trade as the instrument is constructed from strictly exogenous components only. Large natural disasters, population, bilateral geographical determinants and their mutual interactions account for a major part in the variation of the overall trade share. Predicted trade openness can thus be considered as exogenous. However, as natural disasters and population growth may plausibly cause economic outcomes, it is necessary and possible to directly control for them in the IV regression.[24] By this we mitigate a potential omitted variables bias and avoid a violation of the exclusion restriction.

4. The effect of openness on income per capita

4.1. Fixed-effects regressions

We are now ready to use constructed openness Ω^i_τ as an instrument for observed openness $OPEN^i_\tau$ in equation (2). Table 4 reports the results based on the fixed-effects (within) estimator applied to 5-year averaged data. Standard errors are adjusted for clustering at the country level. Columns (1) and (2) employ the MRW sample. Without instrumentation, a one percentage point increase in observed openness increases GDP per capita by 0.55%. The cross-sectional exercise of F&R (based on 1985 data) yielded

[22] Since the full sample is highly unbalanced, we will use the contemporaneous instrument only in the IV regressions for that sample.

[23] Results are qualitatively similar and robust when we use the broader definition of large natural disasters, and can be obtained on request.

[24] Table 8 shows that one can account in a variety of ways for the direct effect of disasters without changing the key results of this paper.

an effect of 0.82 percent. So, controlling for country heterogeneity reduces the effect of openness on income per capita. This finding is robust to instrumentation and alternative samples. Unlike F&R, Feyrer (2009) uses $\ln OPEN_{\tau}^{i}$ as the dependent variable. Using a shorter sample than ours (1950-1995) of 5 year intervals (rather than averages), Feyrer finds an elasticity of GDP per capita of 0.4 in his restricted sample. To make our results comparable to his, we compute the elasticity at the mean or median levels of openness; see the two corresponding lines in the Table. Evaluating at the mean, we find a value of 0.33.[25] An increase of population by one percent *decreases* GDP per capita by about 0.69%. F&R have found a positive, but statistically only marginally positive effect of population size. Controlling for country heterogeneity turns around the sign of the population coefficient and makes it statistically significant in virtually all our regressions.[26] Contemporaneous and lagged disasters have no measurable direct effect on per capita income.

Column (2) turns to the instrumental variables (IV) regression (the corresponding first-stage regression is displayed in column (4) of Table 3). Judged by the diagnostic statistics, the IV strategy works well: the partial R^2 is 0.19 and the F-test on the excluded instruments is a comforting 31.4, well above the often-cited threshold of 10 (Staiger and Stock, 1997) and above the 10% critical value as tabulated by Stock and Yogo (2005). Since we have two instruments (contemporaneous openness and the first lag thereof), we can compute a test of overidentifying restrictions.[27] The joint null hypothesis is that the instruments are valid instruments, i.e., that they are correctly excluded from the estimated equation. The test fails to reject (p-value of 0.85), so that the IV strategy appears valid. The IV estimate implies that an increase in openness by one percentage point increases GDP per capita by 1.1%. F&R report an effect of 2.96 in the cross-section of 1985. In our exercise, instrumentation increases the effect of openness two-fold; in

[25] 0.554×0.595.

[26] Feyrer (2009) does not control for population in his regressions. In line with our finding, Rose (2006) presents panel data evidence on size effects and concludes that *"size really does not matter"*.

[27] Note that our results are robust when using a just identified model.

Table 4: Openness and real GDP per capita (1950-2008) (fixed-effects estimates, 5-year averages)

Dependent Variable: ln real GDP per Capita
Dependent Variable (First-stage): Observed openness
Instruments: Constructed openness ($\Omega^i_\tau, \Omega^i_{\tau-1}$)

Sample:	MRW (N = 919)		MRW Intermediate (N = 736)		Full (N = 1,311)	
Estimation Method:	FE	2SLS	FE	2SLS	FE	2SLS
	(1)	(2)	(3)	(4)	(5)	(6)
$OPEN^i_\tau$	0.554***	1.156***	0.635***	1.157***	0.403***	1.517***
	(0.12)	(0.18)	(0.12)	(0.16)	(0.09)	(0.38)
$\ln POP^i_\tau$	-0.689***	-0.656***	-0.608***	-0.589***	-0.590***	-0.517***
	(0.10)	(0.11)	(0.10)	(0.11)	(0.10)	(0.12)
D^i_τ	0.003	-0.010	-0.017	-0.028	0.097**	0.078
	(0.03)	(0.03)	(0.03)	(0.02)	(0.04)	(0.05)
$D^i_{\tau-1}$	-0.043	-0.073*	-0.054	-0.076**	0.040	0.002
	(0.03)	(0.04)	(0.03)	(0.04)	(0.03)	(0.05)
Fixed Effects						
Country	YES	YES	YES	YES	YES	YES
Period	YES	YES	YES	YES	YES	YES
Elasticity of income with respect to trade evaluated						
at mean	0.33	0.69	0.38	0.69	0.28	1.06
at median	0.29	0.59	0.33	0.60	0.24	0.91
Countries	94	94	72	72	162	162
R^2	0.944	0.936	0.956	0.951	0.923	0.881
F-Test	216.87	206.49	216.16	194.84	252.26	181.78
Partial R^2		0.19		0.22		0.05
F-Test on excl.Instrument		31.43		36.40		8.55
Stock-Yogo weak ID test		19.93		19.93		6.66
Hansen p-value		0.85		0.59		

Note: Constant and period-fixed effects included but not reported. Country clustered robust standard errors in parenthesis. Disasters are the number of large-scale disasters according to the decision rule. Column (1) to (2) use the sample suggested by Mankiw et al. (1992), column (3) to (4) use a sample by Mankiw et al. (1992) that excludes countries likely to be subject to measurement error, while column (5) to (6) use the full sample. Stock-Yoko (2005) critical values of 10% reported in column (2) and (4), and 20% for the weak instrument test based on IV size reported in column (6). The weak instruments hypothesis is rejected with the most stringent criterion for the preferred samples and the third stringent criterion for the full sample. * p<0.1, ** p<0.05, *** p<0.01.

F&R it increases it by the factor 3.6. Also Feyrer's (2009) panel estimates yield higher IV estimates than OLS. Evaluated at the mean openness value, our estimate implies an elasticity of 0.69, which compares well to the finding of Feyrer (2009).[28] Our specification implies that this elasticity is not constant: Countries with an initial openness level one standard deviation below the mean have an elasticity of 0.20, while countries with openness one standard deviation above the mean have an elasticity of 1.17.[29] The effect of population remains negative and highly statistically significant; its magnitude hardly changes relative to column (1).

Columns (3) and (4) repeat the exercise for the somewhat smaller MRW intermediate sample. Both the non-instrumented and the instrumented equations yield very similar results to the MRW sample and the IV strategy remains valid. Columns (5) and (6) turn to the full sample. Here, the instrumentation strategy still works as the F-test on the excluded instrument is very close to the Stock-Yogo (2005) 15% reference value and well above the 20% critical value cited. Since this large panel is strongly unbalanced, we restrict the set of instruments to the contemporaneous realization. Hence, no test of overidentifying restrictions can be performed. Nonetheless, we still find a positive effect of openness on income per capita;[30] the elasticity of income with respect to openness is about unity.

The marginal contribution of our instrument on the actual trade share is between 5 to 22%. Hence, our instrument explains 19% for the MRW, 22 percent for the MRW intermediate, and 5% for the full sample residual movement in observed trade once we account for country- and time-dummies. This compares very well to the respective shares in Feyrer (2009).[31]

Contemporaneous large-scale natural disasters have no direct effect on per capita

[28] $1.156 \times 0.595 = 0.69$. Feyrer (2009) finds elasticities ranging from 0.42 to 0.59; see his Table 5.

[29] $1.156 * (0.595 - 0.421) = 0.20$;$1.156 * (0.595 + 0.421) = 1.17$.

[30] Adding the lag of constructed openness the sample size shrinks, but point estimates and standard errors are comparable.

[31] We obtain results similar to those reported in Table 4 when using only contemporaneous constructed trade share as an instrument for observed trade or by deploying the country sample suggested by Baier and Bergstrand (2007). Results can be obtained on request.

income in the MRW and the MRW intermediate samples. This is probably not surprising since we consider averages over five years and disasters supposedly have transitory effects. The lag has a negative effect on per capita income for both the MRW and the intermediate MRW sample. In the full sample, we find that disasters increase GDP per capita quite substantially under OLS (10 percent at the 5% significance level).[32] The latter finding is in line with empirical observations made by Skidmore and Toya (2002, 2007), who found that geological disasters may boost economic activity in affected nation states in contrast to less disaster-prone countries. Further, Noy (2009) explains that external aid money and materials tend to flow into disaster struck developing economics, also spurring growth. Also, the negative impact of a disaster on output can partly be mitigated by either substituting capital by labor, or by increasing capacity utilization of plants or by relocating machinery from regions not affected by the natural disaster (Horwich, 2000).

4.2. First-differenced regressions

Table 5 provides results from first-differenced regressions. These models have the advantage that–because of clustering of standard errors at the country level–standard errors are robust to serial correlation. Our IV strategy still works fine; the instruments pass the weak identification test of Stock and Yogo (2005) as well as the Hansen test of overidentification restrictions. The results from first-differenced estimation do not lead to different conclusions than those obtained from fixed-effects estimation: openness increases GDP per capita, population size decreases it. However, two observations stand out: first, the difference in point estimates between the OLS and the IV estimates is larger; second, there is absolutely no evidence for a direct effect of disasters on GDP per capita in the MRW and the full sample, yet, if at all, a negative effect of large disas-

[32] Note that the positive impact of disasters on growth is not in line with the prediction of the standard intertemporal trade models where one would model a disaster exactly as a temporary reduction of output. More elaborate models may, however, be consistent with a positive effect on real GDP per capita, e.g., through an increased renewal of the capital stock (Caballero and Hammour, 1994; Crespo Cuaresma et al., 2008).

Table 5: Openness and real GDP per capita (1950-2008) (first-differenced estimates, 5-year averages)

Dependent Variable: Δ ln real GDP per capita

Dependent Variable (First-stage): Δ observed openness

Instruments: $\Delta\Omega^i_\tau, \Delta\Omega^i_{\tau-1}$

Sample:	MRW (N = 825)			MRW Intermediate (N = 644)			Full (N = 1,148)		
Method:	OLS	2SLS		OLS	2SLS		OLS	2SLS	
		First-Stage	Second-Stage		First-Stage	Second-Stage		First-Stage	Second-Stage
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$\Delta OPEN^i_\tau$	0.131***		0.856***	0.133**		0.830***	0.064		0.858**
	(0.05)		(0.23)	(0.05)		(0.23)	(0.04)		(0.43)
$\Delta \ln POP^i_\tau$	-0.463***	-0.057	-0.510***	-0.602***	-0.062	-0.548***	-0.451***	-0.100	-0.391***
	(0.15)	(0.07)	(0.12)	(0.11)	(0.08)	(0.11)	(0.12)	(0.06)	(0.12)
ΔD^i_τ	-0.023	0.009	-0.027	-0.026	0.006	-0.029*	0.006	0.016*	0.002
	(0.02)	(0.01)	(0.02)	(0.02)	(0.01)	(0.02)	(0.03)	(0.01)	(0.03)
$\Delta D^i_{\tau-1}$	-0.024	0.015	-0.034	-0.023	0.007	-0.029	0.016	0.015	0.016
	(0.02)	(0.02)	(0.03)	(0.02)	(0.01)	(0.02)	(0.03)	(0.01)	(0.03)
$\Delta\Omega^i_\tau$		0.193***			0.182***			0.193***	
		(0.03)			(0.04)			(0.07)	
$\Delta\Omega^i_{\tau-1}$		0.212***			0.239***				
		(0.08)			(0.09)				
Fixed Effects									
Period	YES	YES	YES	YES	YES	YES	YES	YES	YES
Countries	94	94	94	72	72	72	162	162	162
R^2		0.119	0.260		0.146	0.341		0.060	0.056
F-Test		12.78	37.56		10.50	34.32		5.79	31.96
Partial R^2			0.04			0.05			0.02
F-Test on excl.Inst.			23.83			17.03			8.34
Stock-Yogo weak ID test			19.93			11.59			6.66
Hansen p-value			0.60			0.39			

Note: Constant and period-fixed effects included but not reported. Country clustered robust standard errors in parenthesis. Disasters are the number of large-scale disasters according to the decision rule. Column (1) to (3) use a well established sample suggested by Mankiw et al. (1992), column (4) to (6) use a sample by Mankiw et al. (1992) that excludes where countries likely to be subject to measurement error, while column (7) to (9) use the full sample. Stock-Yogo (2005) critical values of 10% reported in column (3), critical values of 15% reported in column (6), and critical values of 20% reported in column (9). The weak instruments hypothesis is rejected with the most stringent criterion for the preferred sample, with the second stringent criterion for the MRW intermediate sample, and the third stringent criterion for the full sample. * p<0.1, ** p<0.05, *** p<0.01.

ters on income per capita in the restricted MRW sample in column (6).[33]

4.3. Robustness checks

In our robustness checks, we address three concerns. First, one may conjecture that the effect of openness on income depends on the time period under investigation or on the country sample used. Second, results could depend on the types of natural disasters and how exactly we account for them. Third, we address the concern that causality might run from disasters to GDP to trade. Hence, we will control for interaction terms of disasters with geographical variables to strengthen the conception that the instrument is identified through the bilateral interactions of disasters with geographical variables and their effect on trade.

Sample sensitivity. Table 6 reports IV estimates and associated first-stage diagnostics for different samples. The fixed-effects model without instrumentation is not shown; only the effect of openness is reported in a memo line at the bottom of the table. Sample modifications are always relative to the full sample. Column (1) follows Feyrer (2009) and restricts the sample to the years 1950-1995 (thereby discarding the years 1996-2008). Results remain similar to the findings in Table 4, only the F-Test on the excluded instrument looks better. Column (2) uses a balanced sample over the period 1960 to 2008 that supports our findings and also largly improves first-stage diagnostics. This also applies if Sub-Saharan Africa is excluded from the balanced sample in column (3).

In a next step, we split the sample into rich and poor countries, and in OECD and non-OECD economies. While we obtain a smaller positive and significant effect for the 50% richest economies from 1950-2008 in column (4), the effect for the 50% poorest countries increases as compared to the benchmark results. Moreover, the comparison between rich and poor reverses in the IV regression compared to results under fixed-effects OLS (see the memo line). Splitting the sample in OECD and non-OECD member

[33] Results are qualitatively similar and robust when we use the broader definition of large natural disasters, and can be obtained on request.

Table 6: Robustness checks: Alternative time coverage and country samples (fixed-effects estimates, 5-year averages)

Dependent Variable: ln real GDP per capita
Dependent Variable (First-stage): Observed Openness
Instrument: Constructed Openness (Ω_τ^i)

Sample:	Feyrer 1950-1995	Balanced 1960-2008	Balanced w/o Africa	50% rich	50% poor	OECD	NonOECD
Method:	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$OPEN_\tau^i$	1.165***	1.246***	1.101***	0.926***	2.038**	-0.178	1.767***
	(0.37)	(0.22)	(0.23)	(0.29)	(0.82)	(0.29)	(0.47)
$\ln POP_\tau^i$	-0.468***	-0.677***	-0.560***	-0.112	-0.932***	0.201	-0.638***
	(0.13)	(0.12)	(0.14)	(0.12)	(0.31)	(0.38)	(0.17)
D_τ^i	0.049	0.109***	0.112***	-0.028	0.083*	-0.152	0.077
	(0.06)	(0.04)	(0.03)	(0.05)	(0.05)	(0.10)	(0.05)
$D_{\tau-1}^i$	-0.052	0.008	0.018	-0.071	-0.018	-0.178	-0.009
	(0.06)	(0.04)	(0.04)	(0.07)	(0.07)	(0.12)	(0.05)
Fixed Effects							
Country	YES	YES	YES	YES	YES	YES	YES
Period	YES	YES	YES	YES	YES	YES	YES
Elasticity of income with respect to trade evaluated							
at mean	0.71	0.79	0.74	0.70	1.31	-0.10	1.28
at median	0.61	0.69	0.63	0.61	1.13	-0.09	0.81
Observations	831	894	677	684	627	284	1,027
Countries	138	93	69	80	82	29	133
R^2	0.925	0.923	0.947	0.956	0.734	0.979	0.816
F-Test	234.19	187.35	198.92	278.52	48.22	897.76	101.57
Partial R^2	0.05	0.13	0.14	0.08	0.03	0.20	0.05
F-Test on excl. Instrument	13.55	26.94	25.91	6.76	3.71	13.99	7.44
Memo: FE OLS							
$OPEN_\tau^i$	0.435***	0.486***	0.446***	0.437***	0.392***	0.217	0.424***

Note: Constant, country-, and period-fixed effects included but not reported. Country clustered robust standard errors in parenthesis. The number of the disaster is the number of large-scale natural disasters according to the decision rule. Fixed-effects OLS coefficients for observed openness are stated in the bottommost row for comparison reasons. * p<0.1, ** p<0.05, *** p<0.01.

Table 7: Robustness checks: Alternative definitions of disasters (fixed-effects estimates, 5-year averages, MRW sample)

Dependent Variable: ln real GDP per capita
Dependent Variable (First-stage): Observed Openness
Instrument: Constructed Openness (Ω^i_τ, $\Omega^i_{\tau-1}$)

Sample:	MRW (N = 919)							
Disaster variable:	yearly				cumulated			
Disaster definition:			broad	broad			broad	broad
	large	all	large	all	large	all	large	all
Method:	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$OPEN^i_\tau$	1.156***	1.192***	1.182***	1.211***	1.039***	1.171***	1.061***	1.180***
	(0.18)	(0.18)	(0.18)	(0.18)	(0.22)	(0.19)	(0.22)	(0.19)
$\ln POP^i_\tau$	-0.656***	-0.662***	-0.672***	-0.661***	-0.662***	-0.662***	-0.678***	-0.660***
	(0.11)	(0.11)	(0.11)	(0.11)	(0.11)	(0.11)	(0.11)	(0.11)
D^i_τ	-0.010	0.004	0.031	0.004	-0.003	0.000	0.004	0.000
	(0.03)	(0.01)	(0.03)	(0.01)	(0.01)	(0.00)	(0.01)	(0.00)
$D^i_{\tau-1}$	-0.073*	0.013	-0.028	0.014*	-0.012	0.003	-0.002	0.003**
	(0.04)	(0.01)	(0.04)	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)
Fixed Effects								
Country	YES	YES	YES	YES	YES	YES	YES	YES
Period	YES	YES	YES	YES	YES	YES	YES	YES
Elasticity of income with respect to trade evaluated								
at mean	0.68	0.70	0.70	0.72	0.61	0.69	0.63	0.70
at median	0.59	0.61	0.60	0.62	0.53	0.60	0.54	0.60
Countries	94	94	94	94	94	94	94	94
R^2	0.936	0.934	0.935	0.934	0.938	0.935	0.938	0.935
F-Test	206.49	192.31	190.86	189.14	207.31	189.00	192.99	186.85
Partial R^2	0.19	0.19	0.19	0.19	0.20	0.20	0.19	0.20
F-Test on excl.Inst.	31.43	31.85	29.70	31.59	36.58	34.84	32.47	33.84
Hansenp-value	0.85	0.86	0.93	0.97	0.72	0.94	0.63	0.97
Memo: FE OLS								
$OPEN^i_\tau$	0.554***	0.544***	0.545***	0.546***	0.554***	0.545***	0.544***	0.546***

Note: Constant, country-, and period-fixed effects included but not reported. Country clustered robust standard errors in parenthesis. The number of the corresponding disaster according to the disaster definition in the heading for column (1) to (4). Disasters are the corresponding number of 5-year cumulated disasters according to the disaster definition in the heading for column (5) to (8). The weak instruments hypothesis is rejected with the most stringent criterion according to Stock-Yogo (2005) critical values of 10%. Fixed-effects OLS coefficients for observed openness are stated in the bottommost row for comparison reasons. * p<0.1, ** p<0.05, *** p<0.01.

states in column (6) and (7), we find that openness does not significantly affect real GDP per capita in OECD countries. In contrast, in the sample of non-OECD economies a fairly strong positive growth effect from trade openness remains. The instrument remains technically valid for the OECD and the non-OECD sample.

Alternative definition of disasters. Next we modify the definition of natural disasters. Table 7 reports second-stage IV results and first-stage diagnostics, again relegating results from non-instrumented regressions to a memo line. Column (1) repeats our benchmark second-stage IV results, while column (2) to (4) report results on the income-trade regression for various other yearly disaster frequency rules.[34] In column (2) we use the *total* number of natural disasters (rather than focusing on 'large' ones) that occurred between 1950 and 2008. Regression coefficients are essentially similar as to when applying the large-scale disaster decision rule for the MRW sample in column (1).[35] In column (3) and (4) we use a broader definition of natural disasters including all possible types of natural disasters as listed in EM-DAT. Still, IV results remain very robust.

As a further robustness check we perform regressions using the above mentioned definitions of the disasters variable but now we consider 5-year cumulated catastrophes to see whether the frequency of disasters, as well as past and present disasters yield different results. Columns (5) to (8) in Table 7 depict the regression coefficients. Again, we find a positive effect on per capita GDP comparable to our baseline findings and a comfortingly high F-Test on the excluded instrument as well as a high partial R^2.

Richer accounting for direct effects of disasters. Table 8 provides results for including interaction terms between natural disasters (contemporaneous, lagged; linear or squared) and geographical variables into our second stage regression, both estimated

[34]We construct the instrument as before, now using *all* natural catastrophes and a broader definition including all types of natural disasters as stated in the EM-DAT. Results on the gravity-type estimation from which the instrument is predicted can be found in Table (11), Appendix.

[35]Results for the full sample can be found in Table 12 in the Appendix.

Table 8: Robustness: Extensive accounting for direct effects of disasters (FE and FD estimates, 5-year averages)

Dependent Variable: ln real GDP per Capita

Dependent Variable (First-stage): Observed openness

Instruments: Constructed openness ($\Omega^i_\tau, \Omega^i_{\tau-1}$)

Sample:	MRW			MRW Intermediate			Full		
Estimation Method:	FE 2SLS		FD 2SLS	FE 2SLS		FD 2SLS	FE 2SLS		FD 2SLS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$OPEN^i_\tau$	1.209***	1.215***	0.816***	1.246***	1.248***	0.795***	1.504***	1.232***	0.848**
	(0.16)	(0.16)	(0.24)	(0.13)	(0.13)	(0.23)	(0.38)	(0.21)	(0.37)
$\ln POP^i_\tau$	-0.635***	-0.657***	-0.538***	-0.561***	-0.575***	-0.568***	-0.471***	-0.474***	-0.411***
	(0.11)	(0.11)	(0.12)	(0.10)	(0.11)	(0.10)	(0.12)	(0.11)	(0.12)
D^i_τ	-1.153***	-1.112***	-0.404**	-1.322***	-1.227***	-0.453**	-0.808***	-0.670**	-0.420**
	(0.30)	(0.42)	(0.20)	(0.35)	(0.46)	(0.20)	(0.17)	(0.27)	(0.17)
$D^i_{\tau-1}$	-1.271***	-1.692***	-0.654***	-1.383***	-1.729***	-0.551***	-0.693***	-1.086***	-0.322
	(0.28)	(0.38)	(0.24)	(0.29)	(0.37)	(0.21)	(0.19)	(0.36)	(0.27)
$D^i_\tau \times \ln POP^i_\tau$	0.052**	0.068**	0.027*	0.065**	0.063*	0.016	0.048**	0.074***	0.043***
	(0.02)	(0.03)	(0.02)	(0.03)	(0.03)	(0.01)	(0.02)	(0.03)	(0.02)
$D^i_{\tau-1} \times \ln POP^i_{\tau-1}$	0.069***	0.104***	0.046***	0.063***	0.087***	0.029**	0.031	0.066***	0.018
	(0.02)	(0.03)	(0.02)	(0.02)	(0.03)	(0.01)	(0.02)	(0.02)	(0.02)
$D^i_\tau \times \ln AREA^i$	0.043***	0.041**	-0.003	0.043***	0.051***	0.014	0.018	-0.001	-0.017
	(0.02)	(0.02)	(0.01)	(0.01)	(0.02)	(0.01)	(0.02)	(0.02)	(0.02)
$D^i_{\tau-1} \times \ln AREA^i$	0.034	0.011	-0.005	0.045*	0.031	0.004	0.018	0.008	-0.002
	(0.03)	(0.03)	(0.01)	(0.02)	(0.04)	(0.01)	(0.02)	(0.03)	(0.01)
$D^i_\tau \times \ln FINDIST^i$		-0.012	0.018**		-0.018	0.011*		-0.011	0.025***
		(0.02)	(0.01)		(0.02)	(0.01)		(0.02)	(0.01)
$D^i_{\tau-1} \times \ln FINDIST^i$		0.060**	0.033		0.050*	0.026		0.038	0.020
		(0.03)	(0.02)		(0.03)	(0.02)		(0.04)	(0.03)
$D^i_\tau \times POLITY^i_\tau$		-0.003	-0.002		-0.002	-0.003		-0.000	-0.002
		(0.01)	(0.00)		(0.01)	(0.00)		(0.00)	(0.00)
$D^i_{\tau-1} \times POLITY^i_{\tau-1}$		-0.006	-0.004**		-0.007*	-0.005**		-0.006*	-0.003
		(0.00)	(0.00)		(0.00)	(0.00)		(0.00)	(0.00)
${D^i_\tau}^2$		-0.019	0.014**		-0.006	0.020***		-0.034***	-0.000
		(0.02)	(0.01)		(0.02)	(0.01)		(0.01)	(0.01)
${D^i_{\tau-1}}^2$		-0.002	0.018**		0.009	0.022***		-0.005	0.006
		(0.02)	(0.01)		(0.02)	(0.01)		(0.01)	(0.00)
Observations	919	914	820	736	734	662	1311	1194	1047
Countries	94	94	94	72	72	72	162	146	146
R^2	0.936	0.937	0.290	0.952	0.952	0.370	0.887	0.909	0.105
Partial R^2	0.19	0.19	0.04	0.22	0.22	0.05	0.05	0.11	0.03
F-Test on excl. Inst.	30.99	29.59	19.46	35.48	35.88	10.67	8.30	37.37	35.89

Note: Constant, country-, and period-fixed effects included but not reported. Country clustered robust standard errors in parenthesis. Disasters are the number of large-scale disasters according to the decision rule.Column (1) to (3) use the sample suggested by Mankiw et al. (1992), column (4) to (6) use the intermediate sample by Mankiw et al. (1992), while column (7) to (9) use the full sample. Column (1) and (2), (4) and (5), as well as (7) and (8) use a fixed-effects estimation approach. Column (3), (6), and (9) deploy a first-difference approach with $\Delta OPEN^i_\tau$ and ΔD^i_τ, and so on, where Δ is the first difference operator. The weak instruments hypothesis is rejected with the most stringent criterion according to Stock-Yogo (2005) critical values of 10% for all columns, except column (6) is rejected on the 15%, and column (7) on the 20% critical value. * p<0.1, ** p<0.05, *** p<0.01.

using fixed-effects or first-differenced regressions, and using all of our three samples. We make this effort to make sure that our estimated openness effect is not contaminated by the potential correlation between disasters and GDP. In all models, our IV strategy continues to perform well. The instrument passes the weak identification test of Stock and Yogo (2005), and the overidentifying test is good, too.[36]

The results compare well, both qualitatively as well as quantitatively, to our findings in Table 4 and 5: openness increases GDP per capita, while population size decreases it. But now, in all specifications, we find a significant negative effect of disasters on real per capita GDP: Columns (1), (4), and (7) show that the direct effect depends crucially on the size of an economy –the logs of population and area size. This is intuitive. A given disaster destroys a smaller share of the total capital stock in a larger and bigger country; moreover, the larger internal market allows for swifter recovery. A similar logic would apply to financial remoteness: a disaster is less disruptive if a country has better access to international credit markets. Yet, the significance of the disaster-financial distance interaction effect is mixed for the different samples and specifications. This is also true for the interaction of disasters with the polity index, obtained from the Polity IV Project (2010), rescaled from 0 to 20, with 0 being the most autocratic state and 20 being the most democratic state. While the contemporaneous interaction term has no significant effect, the lag signals a negative effect in column (3), (5), (6), and (8). Hence, the GDP per capita of a country with a higher polity index is slightly stronger negatively affected due to a disaster. Finally, we include squared disasters in the regression. For the MRW and the MRW Intermediate sample income per capita is still decreased by a disaster, but proportionally less if a country is hit by more than 1 disaster under the first-differenced approach in column (3) and (6). In the full sample under fixed-effects estimation in column (8), the per capita GDP of a state is decreased by a disaster, and proportionally even more if more than one catastrophe occurs.

[36]All models contain period effects. Adding interaction terms with squared disaster variables does not change the picture.

5. Conclusions

This paper proposes a novel instrument for trade openness that is applicable in a panel framework. Time variation of the instrument stems from the effect of natural disasters on bilateral trade patterns. Our findings are in line with earlier cross-sectional findings by Frankel and Romer (1999), Irwin and Terviö (2002), Noguer and Siscart (2005), and with the panel exercise proposed by Feyrer (2009). We find substantial evidence of a beneficial effect of trade on GDP per capita in the 94 (72 and 162) country sample for 1950-2008. This finding is robust to the categorization of natural disasters.

Our analysis extends the approach by Frankel and Romer (1999). We show that natural disasters affect bilateral trade flows in a fashion that is broadly compatible with economic intuition. Using a gravity model that contains the incidence of natural disasters and interactions thereof with geographical variables, but that excludes variables that are orthogonal to income per capita, we predict the component of bilateral trade flows that is unrelated to GDP per capita. From this we construct an instrument for trade openness. Our procedure allows us to account for zero trade flows, thereby avoiding out of sample prediction bias. The instrument performs well: its time changes correlate highly with changes in observed openness. It is also uncorrelated to income shocks, as it is based on exogenous regressors, such as natural disasters and geographical variables.

To assess the effect of openness on GDP per capita we use data that is averaged over 5 years. We deal with unobserved deep geographical or historical determinants of income per capita by exploiting the panel dimension of the data. Using our instrument in a fixed-effect or a first-differenced model, we find that openness robustly increases GDP per capita. Our regressions include time effects, cluster standard errors at the country level, and account for the direct effects of disasters and population on domestic income. Our IV strategy and our second stage regression findings are robust to a large number of sensitivity checks: the first-stage partial R^2 remains satisfactory throughout, and the F-Test on the excluded instrument remains above the Stock-Yogo critical value. We conjecture that our stragegy could be fruitfully applied to many other cross-country studies on the role of trade openness for macroeconomic outcomes. Those outcomes could include subcomponents of GDP (investment in human or physical capital), output volatility, R&D investment or technology adoptions, social, political, or economic institutions, economic inequality, environmental outcomes, and many more.

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A Appendix

Table 9: Summary Statistics and Data Sources (Gravity Section)

	MRW Sample (N = 418,165)		**Full Sample** (N = 833,529)		
Variable	Mean	St. Dev.	Mean	St. Dev.	Data Source
$\omega^{ij} = (M_t^{ij} + M_t^{ji})/GDP_t^i$	0.006	0.070	0.005	0.053	DoTS (2009)
M^{ij}	271.491	3092.2	177.407	2433.9	DoTS (2009)
$\ln POP^i$	15.745	2.388	15.515	2.318	WDI (2009) & Barbieri (2002)
$\ln POP^j$	15.645	2.529	15.427	2.457	WDI (2009) & Barbieri (2002)
$\ln GDP^i$	23.256	2.247	23.077	2.307	WDI (2009) & Barbieri (2002)
$\ln GDP^j$	23.217	2.273	23.039	2.347	WDI (2009) & Barbieri (2002)
$\ln y^i/y^j$	0.028	2.148	0.029	2.131	WDI (2009) & Barbieri (2002)
D^i	0.150	0.500	0.161	0.642	EM-DAT (2010)
D^j	0.147	0.496	0.159	0.636	EM-DAT (2010)
$\ln FINDIST^i$	7.307	1.618	7.361	1.420	Rose & Spiegel (2009)
$\ln FINDIST^j$	7.356	1.591	7.398	1.391	Rose & Spiegel (2009)
$\ln DIST^{ij}$	8.737	0.795	8.677	0.802	CEPII (2005)
ADJ^{ij}	0.031	0.174	0.025	0.155	CEPII (2005)
Colonial relation^{ij}	0.023	0.150	0.017	0.129	CEPII (2005)
Common colonizer^{ij}	0.084	0.278	0.096	0.295	CEPII (2005)
Colonial relation post 1945^{ij}	0.012	0.109	0.010	0.099	CEPII (2005)
Same country^{ij}	0.016	0.124	0.011	0.105	CEPII (2005)
FTA^{ij}	0.054	0.226	0.050	0.218	WTO
CU^{ij}	0.022	0.147	0.021	0.145	WTO
WTO^{ij}	0.467	0.499	0.339	0.473	WTO
$MRDIST^{ij}$	9.887	0.781	9.780	0.715	á la Baier & Bergstrand (2009)
$MRADJ^{ij}$	-0.001	0.084	-0.007	0.073	á la Baier & Bergstrand (2009)

Table 10: Summary Statistics and Data Sources (Trade-Income Section)

	MRW Sample (N = 1,007)			**MRW Intermediate Sample** (N = 804)			**Full Sample** (N = 1,448)			
Variable	Median	Mean	St. Dev.	Median	Mean	St. Dev.	Median	Mean	St. Dev.	Data Source
$OPEN^i$	0.516	0.595	0.421	0.522	0.599	0.444	0.604	0.702	0.450	PWT 7.0
$\ln POP^i$	9.144	9.347	1.323	9.248	9.491	1.347	8.864	8.828	1.764	PWT 7.0
$\ln \bar{y}^i$	7.226	7.396	1.505	7.648	7.681	1.497	7.545	7.586	1.544	PWT 7.0
D^i, large-scale	0	0.145	0.366	0	0.152	0.396	0	0.143	0.489	EM-DAT (2010)
D^i, all	0.4	0.847	1.711	0.4	0.962	1.886	0.25	0.763	1.694	EM-DAT (2010)
D^i, broad large-scale	0	0.242	0.578	0	0.259	0.631	0	0.234	0.769	EM-DAT (2010)
D^i, broad all	0.6	1.435	2.575	0.8	1.620	2.825	0.5	1.282	2.603	EM-DAT (2010)
Ω^i	0.396	0.456	0.290	0.407	0.464	0.298	0.462	0.543	0.349	own calculations

Table 11: Robustness: PPML specification to construct instrument (1950-2008), full sample

Dependent Variable:	Bilateral trade openness of i						
Disaster variable:		yearly			cumulated		
Disaster definition:	broad large	all	broad all	large	broad large	all	broad all
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Disasters, importer (D_t^i)	0.328***	0.031	0.084*	0.485***	0.340***	0.011	0.017
	(0.10)	(0.07)	(0.05)	(0.10)	(0.06)	(0.03)	(0.02)
Disasters, exporter (D_t^j)	-0.911***	-0.160***	-0.172***	0.027	-0.127*	-0.010	-0.009
	(0.19)	(0.05)	(0.03)	(0.10)	(0.07)	(0.02)	(0.01)
Interactions							
$D_t^i \times \ln FINDIST^i$	-0.032***	0.001	-0.002	-0.012*	-0.018***	0.000	-0.001
	(0.01)	(0.00)	(0.00)	(0.01)	(0.00)	(0.00)	(0.00)
$D_t^i \times \ln AREA^i$	-0.021***	-0.013***	-0.014***	-0.008*	-0.007	-0.006***	-0.006***
	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
$D_t^i \times \ln POP_t^i$	0.011*	0.008	0.007*	-0.011*	-0.005	0.004*	0.004***
	(0.01)	(0.01)	(0.00)	(0.01)	(0.00)	(0.00)	(0.00)
$D_t^i \times \ln ADJ^{ij}$	0.116***	0.043*	0.028*	0.049***	0.035***	0.013**	0.009**
	(0.04)	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.00)
$D_t^j \times \ln FINDIST^j$	0.020***	0.010***	0.007***	0.006***	0.006***	0.002***	0.002***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
$D_t^j \times \ln AREA^j$	-0.003	0.000	-0.002	-0.003	0.000	0.002	0.001*
	(0.01)	(0.01)	(0.00)	(0.01)	(0.01)	(0.00)	(0.00)
$D_t^j \times \ln POP_t^j$	0.045***	0.007	0.010***	-0.002	0.005*	-0.001**	-0.001**
	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
$D_t^j \times \ln ADJ^{ij}$	-0.001	0.011	0.009	0.000	-0.002	0.003	0.002
	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)	(0.00)
Controls							
$\ln POP_t^i$	-0.174***	-0.169***	-0.170***	-0.174***	-0.173***	-0.163***	-0.160***
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
$\ln POP_t^j$	0.150***	0.143***	0.121***	0.167***	0.158***	0.174***	0.173***
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
$\ln DIST^{ij}$	-0.954***	-0.953***	-0.952***	-0.955***	-0.954***	-0.953***	-0.954***
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
ADJ^{ij}	0.219**	0.188*	0.176*	0.217**	0.217**	0.182*	0.166
	(0.10)	(0.11)	(0.11)	(0.10)	(0.10)	(0.11)	(0.11)
Colonial relation^{ij}	0.564***	0.563***	0.563***	0.564***	0.566***	0.567***	0.569***
	(0.16)	(0.16)	(0.17)	(0.17)	(0.16)	(0.17)	(0.17)
Common colonizer^{ij}	0.712***	0.711***	0.713***	0.718***	0.711***	0.708***	0.707***
	(0.18)	(0.18)	(0.18)	(0.18)	(0.18)	(0.18)	(0.18)
Colonial relation post 1945^{ij}	1.228***	1.225***	1.224***	1.230***	1.228***	1.219***	1.216***
	(0.21)	(0.21)	(0.21)	(0.21)	(0.21)	(0.21)	(0.21)
Same country^{ij}	0.499***	0.508***	0.511***	0.490***	0.496***	0.506***	0.507***
	(0.13)	(0.13)	(0.13)	(0.13)	(0.13)	(0.13)	(0.13)
Fixed Effects							
Importer, Exporter	YES	YES	YES	YES	YES	YES	YES
Year	YES	YES	YES	YES	YES	YES	YES
Observations	833,529	833,529	833,529	833,529	833,529	833,529	833,529
Log likelihood	-1.70e+04	-1.70e+04	-1.70e+04	-1.71e+04	-1.71e+04	-1.71e+04	-1.71e+04
Chi2	36095.95	37780.33	37539.83	35626.02	35128.06	36178.7	35856.88

Note: Constant, importer-, exporter-, and time-fixed effects are not reported. Trading pair clustered robust standard errors in parenthesis. Disasters are the number of disasters corresponding to the disaster definition in the heading. * p<0.1, ** p<0.05, *** p<0.01.

Table 12: Robustness: Trade Openness and Real GDP per Capita (fixed-effects estimates, 5-year averages, full sample)

Dependent Variable: ln real GDP per capita
Dependent Variable (First-stage): Observed Openness
Instrument: Constructed Openness (Ω_{τ}^{i})

Sample:	Full (N = 1,311)							
Disaster variable:	yearly				cumulated			
Disaster definition:			broad	broad			broad	broad
	large	all	large	all	large	all	large	all
Method:	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$OPEN_{\tau}^{i}$	1.517***	1.530***	1.543***	1.511***	1.077***	1.579***	1.220***	1.560***
	(0.38)	(0.34)	(0.37)	(0.32)	(0.34)	(0.38)	(0.34)	(0.36)
$\ln POP_{\tau}^{i}$	-0.517***	-0.492***	-0.518***	-0.497***	-0.546***	-0.488***	-0.541***	-0.493***
	(0.12)	(0.12)	(0.12)	(0.12)	(0.10)	(0.12)	(0.11)	(0.12)
D_{τ}^{i}	0.078	0.011	0.069***	0.003	0.017**	0.002	0.015***	0.001
	(0.05)	(0.02)	(0.03)	(0.01)	(0.01)	(0.00)	(0.01)	(0.00)
$D_{\tau-1}^{i}$	0.002	0.023*	-0.007	0.020**	0.005	0.005*	0.002	0.004**
	(0.05)	(0.01)	(0.03)	(0.01)	(0.01)	(0.00)	(0.01)	(0.00)
Fixed Effects								
Country	YES	YES	YES	YES	YES	YES	YES	YES
Period	YES	YES	YES	YES	YES	YES	YES	YES
Elasticity of income with respect to trade evaluated								
at mean	1.06	1.07	1.06	1.08	0.76	1.10	0.85	1.09
at median	0.91	0.92	0.93	0.91	0.65	0.95	0.73	0.94
Countries	162	162	162	162	162	162	162	162
R^2	0.881	0.881	0.880	0.882	0.908	0.877	0.901	0.879
F-Test	181.78	192.86	184.17	190.87	214.40	186.06	211.11	185.38
F-Test on excl. Inst.	8.55	11.53	9.22	13.14	9.75	9.53	10.14	10.38
Partial R^2	0.05	0.06	0.05	0.06	0.05	0.05	0.05	0.06
FE OLS								
$OPEN_{\tau}^{i}$	0.403***	0.410***	0.399***	0.408***	0.401***	0.409***	0.397***	0.407***

Note: Constant, country-, and period-fixed effects are not reported. Country clustered robust standard errors in parenthesis. The number of the corresponding disaster according to the disaster definition in the heading for column (1) to (4). Disasters are the corresponding number of 5-year cumulated disasters according to the disaster definition in the heading for column (5) to (8). The weak instruments hypothesis is rejected with the second stringent criterion according to Stock-Yogo (2005) critical values of 15%. Fixed-effects OLS coefficients for observed openness are stated in the bottommost row for comparison reasons. * p<0.1, ** p<0.05, *** p<0.01.

Table 13: Country Samples

Country	**MRW**	**MRW-I**	**Full**	**Country**	**MRW**	**MRW-I**	**Full**
Albania			x	Laos			x
Algeria	x	x	x	Latvia			x
Angola	x		x	Lebanon			x
Argentina	x	x	x	Libya			x
Armenia			x	Lithuania			x
Australia	x	x	x	Luxembourg			x
Austria	x	x	x	Macedonia			x
Azerbaijan			x	Madagascar	x	x	x
Bahamas, The			x	Malawi	x	x	x
Bahrain			x	Malaysia	x	x	x
Bangladesh	x	x	x	Maldives			x
Barbados			x	Mali	x	x	x
Belarus			x	Malta			x
Belgium	x	x	x	Mauritania	x		x
Belize			x	Mauritius	x		x
Benin	x		x	Mexico	x	x	x
Bolivia	x	x	x	Moldova			x
Bosnia and Herzegovina			x	Mongolia			x
Brazil	x	x	x	Morocco	x	x	x
Brunei			x	Mozambique	x		x
Bulgaria			x	Nepal	x		x
Burkina Faso	x		x	Netherlands	x	x	x
Burundi	x		x	New Zealand	x	x	x
Cambodia			x	Nicaragua	x	x	x
Cameroon	x	x	x	Niger	x		x
Canada	x	x	x	Nigeria	x	x	x
Cape Verde			x	Norway	x	x	x
Central African Republic	x		x	Oman			x
Chad	x		x	Pakistan	x	x	x
Chile	x	x	x	Panama	x	x	x
China			x	Papua New Guinea	x		x
Colombia	x	x	x	Paraguay	x	x	x
Comoros			x	Peru	x	x	x
Congo, Democratic Republic of	x		x	Philippines	x	x	x
Congo, Republic of	x		x	Poland			x
Costa Rica	x	x	x	Portugal	x	x	x
Cote d'Ivoire	x	x	x	Qatar			x
Croatia			x	Romania			x
Cuba			x	Russia			x
Cyprus			x	Rwanda	x		x
Czech Republic			x	Saint Lucia			x
Denmark	x	x	x	Saint Vincent and the Grenadines			x
Djibouti			x	Samoa			x
Dominican Republic	x	x	x	Sao Tome and Principe			x
Ecuador	x	x	x	Saudi Arabia			x

Table 13 – continued

Country	MRW	MRW-I	Full	Country	MRW	MRW-I	Full
Egypt	x		x	Senegal	x	x	x
El Salvador	x	x	x	Sierra Leone	x		x
Equatorial Guinea			x	Singapore	x	x	x
Estonia			x	Slovak Republic			x
Ethiopia	x	x	x	Slovenia			x
Fiji			x	Solomon Islands			x
Finland	x	x	x	Somalia	x		x
France	x	x	x	South Africa	x	x	x
Gabon			x	Spain	x	x	x
Gambia, The			x	Sri Lanka	x	x	x
Georgia			x	Sudan	x		x
Germany	x	x	x	Suriname			x
Ghana	x		x	Sweden	x	x	x
Greece	x	x	x	Switzerland	x	x	x
Guatemala	x	x	x	Syrian Arab Republic	x	x	x
Guinea			x	Tajikistan			x
Guinea-Bissau			x	Tanzania	x	x	x
Guyana			x	Thailand	x	x	x
Haiti	x	x	x	Togo	x		x
Honduras	x	x	x	Trinidad and Tobago	x	x	x
Hungary			x	Tunisia	x	x	x
Iceland			x	Turkey	x	x	x
India	x	x	x	Turkmenistan			x
Indonesia	x	x	x	Uganda	x		x
Iran			x	Ukraine			x
Ireland	x	x	x	United Arab Emirates			x
Israel	x	x	x	United Kingdom	x	x	x
Italy	x	x	x	United States	x	x	x
Jamaica	x	x	x	Uruguay	x	x	x
Japan	x	x	x	Uzbekistan			x
Jordan	x	x	x	Vanuatu			x
Kazakhstan			x	Venezuela	x	x	x
Kenya	x	x	x	Viet Nam			x
Korea, South	x	x	x	Yemen			x
Kuwait			x	Zambia	x	x	x
Kyrgyz Republic			x	Zimbabwe	x	x	x