

Gröschl, Jasmin Katrin

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Gravity Model Applications and Macroeconomic Perspectives

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ifo Beiträge zur Wirtschaftsforschung

Gravity Model Applications and Macroeconomic Perspectives

Jasmin Katrin Gröschl

ifo Institut

Leibniz-Institut für Wirtschaftsforschung
an der Universität München e.V.

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zur Wirtschaftsforschung**

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Preface

This volume was prepared by Jasmin Gröschl while she has been working at the Ifo Institute. It was completed in December 2012 and accepted as a doctoral thesis by the Department of Economics at the University of Munich. It includes five self-contained empirical studies. They aim at contributing to the understanding of non-standard determinants of trade and migration: historical and cultural characteristics (Chapter 1), policy-induced regulations (Chapter 2), and natural disasters (Chapter 3); and how international integration of countries shapes economic growth and helps to contain the costs of natural disasters: trade and growth (Chapter 4), and disasters, international integration, and growth (Chapter 5).

Chapter 1 investigates to what extent the historical border between the Union and the Confederacy still acts as a trade barrier in the US today. The former border reduces trade between former Confederacy and Union states by 7 to 22 percent today. The findings point toward the long-run economic costs of military conflict and a potential role for cultural factors and trust. Chapter 2 estimates the effect of non-tariff measures on trade in agricultural and food products. Using novel data of the WTO, findings indicate that concerns over sanitary and phytosanitary (SPS) measures constitute an effective market entry barrier to trade, while, conditional on market entry, trade flows increase due to SPS measures. Chapter 3 analyzes whether international migration serves as an adaption mechanism in the presence of natural disasters. Results indicate that climate-related disasters force people out of affected areas, while people less often move toward countries hit by climate-related events. The pattern is mainly driven by migration from developing to industrialized countries. Chapter 4 sheds new light on the question whether trade openness increases income per capita. We use a trade flow equation to construct an instrument for trade openness that depends on natural disasters in trade partner countries. Using an instrumental variable strategy, results indicate that trade openness causally increases GDP per capita. Chapter 5 contains a detailed analysis of the direct effect of natural disasters on economic activity. It proposes a comprehensive database of disaster intensity measures. Results show that the probability of an event to be recorded in EM-DAT is a function of income, that disasters reduce growth on impact, and that more open economies are better able to adapt to disasters.

Keywords: American Secession, Intranational Trade, US State Level, Gravity, International Trade, Sanitary and Phytosanitary Measure, Natural Disaster, International Migration, Income Per Capita, Openness, Instrumental Variable, Institutions, Geography.

JEL-No.: C23, C26, F14, F15, F22, F43, N72, N92, O15, O4, Q17, Q54, Z10.

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The chapter on "The Impact of Sanitary and Phytosanitary Measures on Market Entry and Trade Flows" was stimulated during my stay at the Economic Research and Statistics Division of the WTO in Geneva. I would like to express my sincere appreciation of the hospitality of the institution and thank Roberta Piermartini, Gianluca Orefice, and Michele Ruta for their advice. I am also grateful to my coauthor Pramila Crivelli, who has been a great source of mutual support and motivation.

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Gravity Model Applications and Macroeconomic Perspectives

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Introduction

This dissertation is a collection of five self-contained empirical essays that address distinct topics in international economics and economic growth. Each chapter includes its own introduction and appendix so that they represent independent pieces of research. Although each essay covers a different question the five chapters can be classified into two broad categories.

The first three chapters comprise various gravity model applications of trade and migration. In international economics, gravity models are one of the most utilized empirical methods. They qualify extraordinarily well when spatial variation in trade or factor movements are observed, owing to the good fit and robustness of estimates. Recent surveys or applications are Anderson and Van Wincoop (2003); Feenstra (2004); Santos Silva and Tenreyro (2006); Baier and Bergstrand (2009); Liu (2009); Anderson (2011) or Egger and Larch (2011) to name only a few. As Anderson (2011) puts it, gravity models feature through their tractability and parsimonious description of economic transactions in a world with many countries. For this reason, gravity approaches are well-suited to empirically investigate economic interactions across countries or economic entities. In chapter 1, Gabriel Felbermayr and I utilize various gravity specifications to examine the role of history and economic geography, in particular the role of the American Secession, on economic transactions and trade patterns within the United States. In chapter 2, Pramila Crivelli and I evaluate the effect of non-tariff measures (NTMs) on market entry and trade flows in agricultural and food products using a gravity model specification. NTMs have come to the fore as the impact of traditional trade policy instruments is fading in the wake of multilateral and bilateral trade agreements. Chapter 3 focuses on international migration as an adaption mechanism to climate change. In a global environment, in which climate-related natural disasters are thought to be on the rise due to global warming, this is an important question. Using an

innovative gravity approach, I elaborate whether migration pressure increases due to climate shifts and associated extreme weather events.

The second part of the dissertation (chapters 4 and 5) analyzes the consequences of international openness and natural disasters on macroeconomic outcomes. As the world experiences climate change and climate-related disasters are believed to turn more frequent and extreme, the debate over natural disasters and their effect picks up speed. But, our knowledge concerning the relevance of natural disasters for economic activity is still in the early stages of development.¹ The consequences of disasters to the economy depend on a number of factors, such as location, disaster type, the time of occurrence, and physical disaster intensity. In particular, in chapter 4, Gabriel Felbermayr and I use foreign natural disasters in a gravity specification to build a time-varying instrument for trade openness. We then evaluate the impact of openness to trade and natural disasters on real GDP per capita using the newly constructed instrument. Chapter 5 shifts the focus to the direct effect of natural disasters on economic activity and whether trade openness, financial openness or better quality institutions determine a nation's ability to better cope with disaster shocks. To examine these relations, we construct a novel and comprehensive database on measures of pure physical disaster intensity from primary sources.

The first chapter of this dissertation is motivated by the fact that 150 years after Confederate troops opened fire on Fort Sumter in South Carolina, the Pew Research Center conducted a US-wide survey and reports that more than half of US citizens believe that the American Secession is still relevant to politics and public life today. In this chapter, we shed light on the role of history, particularly, on whether the long defunct border between US states that belonged to different alliances still has a trade impeding effect on their economic relations today.

Chapter 1 contributes to a growing literature on the long-shadow of history for economic interactions (Falck et al., 2012; Head et al., 2010; Nitsch and Wolf, 2012). We show that the historical border, caused primarily by endowment dissimilarities between Union and Confederate states, has survived over time and still constitutes a discontinuity in the economic geography of the United States. To identify the impact of the border on contemporaneous North-South trade, we use a gravity model approach following Anderson

¹ A thorough reading of the existing literature (Skidmore and Toya, 2002; Raddatz, 2007; Noy, 2009; Loayza et al., 2012; Cavallo et al., 2012; Fomby et al., 2012) suggests a number of issues that require further scrutiny.

and Van Wincoop (2003), Feenstra (2004), and Santos Silva and Tenreyro (2006). Using data from the US commodity flow surveys, we show that the former border between the Confederacy and the Union still disrupts contemporaneous trade between US states that belonged to different alliances by about 13 percent today. Trade with other US states is, on average, not affected. Even after including contemporaneous controls, such as network, institutional, and demographic variables, or historical controls, such as the incidence of slavery, we still find a strong and significant effect of the historical border. Adding US states unaffected by the Civil War to the specification, we argue that the friction is not merely reflecting unmeasured North-South differences. We stress the potential role for cultural factors and trust by showing that the border effect is larger for differentiated than for homogeneous goods.

Chapter 2 examines the impact of concerns over sanitary and phytosanitary (SPS) measures on trade patterns in agricultural and food products, following the World Trade Report 2012 on non-tariff measures (NTMs) of the World Trade Organization (WTO, 2012). With an increasing number of multilateral and bilateral agreements in place, classical trade policy instruments lose ground. But, governments come up with alternatives, such as NTMs (Roberts et al., 1999). In fact, SPS measures are meant to protect the health of animals, humans and plants, although, they also provide means to protect what was previously accomplished by tariffs, quotas and prohibitions. In recent years, disputes are regularly brought to the WTO expressing concerns that SPS measures are utilized as protectionist devices.

Despite the general interest, the trade effects of SPS are not fully understood. For this reason, we contribute to the literature by estimating the impact of SPS measures on market entry and trade flows in agricultural and food products. We use data on concerns over SPS measures obtained from the SPS Information Management System database on specific trade concerns of the WTO. In an attempt to evaluate the effect, we estimate a Heckman selection model at the HS4 disaggregated level of trade, whereby we have the possibility to control for both selection and zero trade flows. We find that concerns over SPS measures constitute an effective market entry barrier to trade in agricultural and food products, while, conditional on market entry, trade flows increase due to SPS measures. This suggests that once a certain standard is met the increase in market share outweighs adaption costs. In the second part of chapter 2, we split SPS measures into requirements related to conformity

assessment (i.e., certificate requirements, testing, inspection, and approval procedures) and product characteristics (i.e., requirements on quarantine treatment, pesticide residue levels, labeling, or packaging). Governments implement both types to reduce health safety risks, but, these measures may entail diverse trade costs. We find that conformity assessment-related measures hamper market entry and trade flows, as adaption costs are extremely costly, while measures related to product characteristics, which contain product quality information, have no impact on the probability to trade but increase trade flows.

Chapter 3 analyzes the impact of natural disasters on international migration. The amount of people affected by natural disasters stands at a staggering number of 243 million people per year. With progressing global warming, hundreds of millions of people are at risk of sea-level rise, extreme droughts, bigger storms, or changing rainfall patterns (INCCCD, 1994; Myers, 2002). As a result, the numbers of those needing to leave disaster-struck places will continue to rise (Stern, 2006; IPCC, 2012; Economist, 2012). While not all of the affected move across borders, international migration provides one adaption mechanism to the growing pressure of climate change. On these grounds, the impact of increasingly extreme natural disasters on the worldwide relocation of people is one of the major potential problematic issues that mankind faces in the future.

In this chapter, I analyze whether international migration serves as an adaption mechanism in the presence of natural disasters. To guide the empirics, I construct a stylized theoretical gravity model of migration that introduces natural disasters as random shocks to labor productivity. In the empirical part, I examine whether climate change proxied by climate-related disaster events leads to international migration. I deploy a newly available dataset of international migration available for increments of 10 years from 1960 to 2010. Accounting for zero migration and multilateral resistance in the gravity specification, I find robust evidence that large climate-related disasters force people out of affected areas, while people less often move toward countries hit by large climate-related events. International migration increases by about 5 percent, on average, due to an increase of large climate-related disasters in the home country by one standard deviation, all else equal. I find evidence that substantial heterogeneity exists across country samples. The pattern is mainly driven by migrants from developing to industrialized countries. The reason may lie in the fact that people in low and middle income countries are, on average, less often insured against damage and loss, but are at the same time more vulnerable to the irreversible and persistent effects

caused by climate change and concomitant disasters (i.e., land degradation) that remove their subsistence possibility.

In chapter 4, we shed new light on the question whether openness to trade increases income per capita by proposing a novel instrument for trade openness applicable in panel environments. Our analysis extends the geography-based empirical strategy of Frankel and Romer (1999) to a panel setup using natural disasters in foreign countries, such as earthquakes, tsunamis, volcanic eruptions, storms, or storm floods, to construct an instrument for trade openness. Variation over time in the instrument comes mainly from the impact of natural disasters occurring in trade partner countries and population on bilateral economic transactions.

For the period 1950 to 2008, we observe that natural disasters affect bilateral trade and that this effect is conditioned by geographical variables such as distance to financial centers or geographic country size. Following this, we employ interactions between geography and disaster occurrence in trade partner countries at the bilateral level to construct an instrument for openness to trade that varies across countries and time. We account for zero trade flows in the gravity framework, thereby avoiding an out of sample prediction bias. We examine the effect of trade openness on GDP per capita in the panel, whereby we are able to fully control for geographical characteristics and institutional quality as well as for the direct effect of disasters in the target country. Using an instrumental variable (IV) strategy in a fixed-effects specification, we find that openness increases per capita income. For a sample of 94 countries, the elasticity of GDP per capita with respect to trade openness is about 0.7. But, we find that substantial heterogeneity exists across country samples.

The final chapter of this dissertation contains a detailed analysis of the direct effect of natural disasters on economic activity. In addition, we examine how a country's integration into global financial and goods markets, or the quality of its institutions, shape this effect. While global warming has led to renewed interest in these questions, so far, empirical analysis has been hampered by inadequate data on natural disasters. Up to now, most studies based their investigation on disaster outcome data (killed, affected, monetary damage). This may lead to biased results in regression analyses as selection into the database may systematically depend on country characteristics, and, regressing disaster outcome variables on economic aggregates (i.e., GDP) may induce further correlation between the disaster variable and the error term.

Chapter 5 constructs an alternative set of data based on pure physical measurement. We compile physical disaster intensity information, such as Richer scale, wind speed, Volcanic Explosivity Index, and precipitation, from primary sources for 1979 to 2010, essentially covering all countries in the world. Using an event based dataset, we show that the probability of a disaster with given physical magnitude to be reported in the Emergency Events Database (EM-DAT) depends strongly on the affected country's GDP per capita. To highlight the impact of natural disasters on GDP per capita, we present results using our novel and comprehensive dataset on a country-year basis. Our analyses provide pervasive evidence that natural disasters do lower GDP per capita temporarily. A disaster whose physical strength belongs to the top decile of the country-level disaster distribution reduces growth by about 3 percentage points on impact. This effect is halved when looking at disasters belonging to the top 15% and is again halved when looking at the top 20%. Finally, we examine whether high quality institutions, adequate financing conditions, or access to international markets help spur the reconstruction process so that, even upon impact, the adverse effect of a natural disaster on growth is mitigated. Indeed, we find that countries with high quality institutions, countries more open to trade, financially more open economies, and countries that receive lower official development assistance can, on average, better cope with earthquakes, floods and droughts.

Chapter 1

Within US Trade and the Long Shadow of the American Secession*

1.1 Introduction

150 years after Confederate troops attacked Fort Sumter in South Carolina, a recent US-wide survey by the Pew Research Center summarizes the findings as: “*The Civil War at 150: Still Relevant, Still Divisive*”.¹ The poll reports that 56% of Americans believe that the Civil War is still relevant to politics and public life today. And that 4 in 10 Southerners sympathize with the Confederacy. But does the long defunct border between the Confederation and the Union still affect *economic* relations between US states that belonged to different alliances today? Is the former border still relevant, still divisive, for economic transactions? This paper sheds light on this question using bilateral trade flows between states.

The Civil War has cost 620,000 American lives, more than any other military conflict. Goldin and Lewis (1975) document that it has retarded the economic development of the whole nation and of the South in particular. And, as the Pew poll shows, the nation is still divided along the lines of the former alliances over whether the war was fought over moral issues – slavery – or over economic policy. Yet, long before the war, the Southern and the Northern economies differed: The South was dominated by large-scale plantations

* This chapter is based on joint work with Gabriel Felbermayr. It is based on the article "Within US Trade and the Long Shadow of the American Secession", *Economic Inquiry*, forthcoming, accepted 2012. This is a revised version of our working paper that circulated under Ifo Working Paper 117, 2011.

¹ Pew Research Centre for the People and the Press, “Civil War at 150: Still Relevant, Still Divisive”, April 8, 2011; available at <http://pewresearch.org/pubs/1958/>.

of cotton, tobacco, rice, and sugar, whose profitability relied on forced labor. It exported crops to Europe and imported manufacturing goods from there. The North, dominated by smaller land-holdings, was rapidly urbanizing; slavery was practically abolished north of the Mason-Dixon Line by 1820.² Its infant manufacturing industries were protected by import tariffs against European competition.

The North-South divide is very visible in contemporaneous state-level data. On average, the South is still poorer, more rural, more agricultural, less educated, more religious, and has different political views. The economic gap may have narrowed (Mitchener and McLean, 1999), in particular after the end of segregation in the Sixties of the last century. But, political disagreement, in particular on the role of federal government, continues to beset the country. A special sense of Southern identity continues to mark a cultural divide within the US.

This paper contributes to a growing literature on the long-shadow of history for economic transactions (Falck et al., 2012; Head et al., 2010; Nitsch and Wolf, 2012). It shows that the former border still constitutes a discontinuity in the economic geography of the United States. The modern literature has identified cultural differences across countries as impediments of international trade, but typically not within the same country. Estimates of various border effects abound in the literature and there are well-tested empirical methods to measure their trade-inhibiting force. The more challenging question in this paper is: Can the estimated border effect be interpreted as a *genuine* Union-vs-Confederation effect?

We proceed in three steps. First, employing an OLS approach with state fixed effects for bilateral trade between states, we find a robust, statistically significant, and economically meaningful trade-inhibiting effect of the former border. In the preferred 1993 data, on average, the historical border reduces trade between states of the former Confederation or Union by about 13 to 14%. In comparison, the Canada-US border restricts trade by 155 to 165% (Anderson and Van Wincoop, 2003). Nitsch and Wolf (2012) find that the former border between East and West Germany restricts trade by about 26 to 30% in 2004. Running a million placebos, we show that no other border between random groups of (old) US states yields a stronger trade-reducing effect.

The result is robust to employing alternative methodologies (in particular a Poisson model), using different waves of the Commodity Flow Survey (1997, 2002, 2007), drawing on

² The Mason-Dixon Line settled a conflict between British Colonies and set the common borders of Pennsylvania, Maryland, Delaware, and West Virginia.

sectoral rather than aggregate bilateral trade data, measuring transportation costs differently (travel time instead of sheer geographical distance), or allowing for more flexibility by using distance intervals as in Eaton and Kortum (2002) instead of a log linear distance measure. Including the rest of the world, or different treatment of states, whose allegiance to either the Union or the Confederation is historically not obvious, does not change the results. The estimated border effect represents an *ad valorem* tariff equivalent of about 2 to 7%. Interestingly, the effect is stronger (and more robust) in the food, manufacturing, and chemicals sectors than in mining, which is characterized by a completely standardized good, or machinery, where the pattern of specialization across North and South is very strong.

In a second step, we add a large array of contemporaneous variables to the original model to account for observable differences between the South and the North. The controls are meant to capture migrant, ethnic, or religious networks. While these variables matter empirically, they do not reduce the estimated border effect. We account for cultural differences expressed by different colonial relations across states, and for different patterns of urbanization. We include variables that relate to the institutional setup of states, or that measure differences in the judicial system. We control for differences in endowment proportions, or for differences in the structure of the states' economies. Finally, we add demographic factors and test the Linder hypothesis. Most of these controls have some explanatory power, but they do not undo the border effect. The estimate falls from 13 to 11%. This finding survives the same battery of robustness checks applied to the parsimonious model.

Third, we acknowledge that the North-South border, marked by the Secession, is likely not to be exogenous. Engerman and Sokoloff (2000, 2005) suggest that it is related to endowment differences between Northern and Southern states in cropland, or in the size and structure of agricultural production. The emergence of the border may have to do with historical ethnic patterns, historical educational achievements of the population, or institutional differences as captured by the historical incidence of malaria as in Acemoglu et al. (2002). Finally, and most importantly, it may result from the incidence of slavery. Not all of these variables matter empirically for contemporaneous trade patterns, but they cannot easily be excluded from the explanation of contemporaneous bilateral trade on conceptual grounds. Including them into the gravity equation does not undo the 'Secession effect'. Quite to the opposite, the estimated effect actually increases. Finally, we extend the analysis to Western states, but keep the same coding of the border. Thus, we add pairs of states which have been completely

unaffected by the Secession. Then, the border dummy essentially captures whether two states have been on opposing sides of the Civil War rather than belonging to the North or the South. We continue to find a border effect (7 to 19%), which can now be attributed more plausibly to the Secession.

The literature offers explanations of border effects in terms of ‘political barriers’, ‘artefact’, and ‘fundamentals’. The first should be largely absent in an integrated economy such as the US. The second relates to difficulties in separating the impact of border-related trade barriers from the impact of geographical distance (Head and Mayer, 2002) or to problems of statistical aggregation (Hillberry and Hummels, 2008). We deal with these issues by using alternative measures of trade costs and by a large number of placebo exercises. We view our results as consistent with the ‘fundamentals’ approach: historical events have shaped cultural determinants of trade which still matter today.

Our results show that the US is not a single market, even 150 years after the Civil War. The historical conflict still is divisive today. This is an important lesson for the European integration process, which is more complex due to the lack of a common language, a common legal/judicial system, common regulatory framework, and – most important in our context – the fact that the last huge conflict is not 150 but only 67 years away. Hence, one should not be too optimistic in assessing the economic effects of political union. From a welfare perspective, our results allow two interpretations. First, it could be that the Secession has had lasting effects on trade costs. By shaping the distribution of (railway) infrastructure or business networks (production clusters), and more generally, by affecting bilateral trust, South-North trade frictions are still higher than intra-group frictions. To the extent that our estimates measure this, it signals a long-lasting welfare loss due to the Secession. Second, it could be that the Secession had lasting effects on preferences. The trade embargo during the war could have led to persistent preferences for local goods due to habit formation. In that case, a welfare interpretation of our findings is more problematic, in particular quantitatively. However, if the divergence of preferences was indeed caused by the war, depending on the precise characterization of preferences, the estimate can still be interpreted as an indicative of welfare losses.

The literature on border effects was pioneered by McCallum (1995), who finds that trade volumes between Canadian provinces were about 22 times larger than those between Canada and the US in 1988. Subsequent research shows that states usually trade 5 to 20 times more

domestically than internationally.³ Few studies have moved from simply exploring border barriers to investigating and explaining potential causes. Wei (1996) and Hillberry (1999) do not find that tariffs, quotas, exchange rate variability, transaction costs, and regulatory differences can explain the border effect. Recent studies illustrate that the impact of borders also extends to the sub-national level, implying that additional reasons for high local trade levels must exist. Examples are Wolf (1997, 2000), Hillberry and Hummels (2003), Combes et al. (2005), Buch and Toubal (2009), and Nitsch and Wolf (2012).

The remainder of the paper is structured as follows. Section 1.2 provides details of the empirical strategy. Section 1.3 describes the benchmark results, placebo estimations and a sensitivity analysis. Section 1.4 uses a large array of contemporaneous controls to address a potential omitted variables problem. While Section 1.5 attempts to explain the ‘Secession effect’ by historical variables and by adding Western states to the analysis. The last section concludes.

1.2 Empirical Strategy and Data

1.2.1 Empirical Strategy

Our empirical strategy follows Anderson and Van Wincoop (2003), henceforth AvW, and the subsequent research. Based on a multi-country framework of the Krugman (1980) constant elasticity of substitution (CES) model with iceberg trade costs, the literature stresses that the consistent estimation of bilateral barriers requires to take *multilateral trade resistance* into account.

Anderson and Van Wincoop (2003) show that the CES demand system with symmetric trade costs can be written as

$$\ln z_{ij} = \beta_0 + \beta_1 \text{Border}_{ij} + \beta_2 \ln \text{Dist}_{ij} + \gamma \mathbf{X}_{ij} - \ln P_i^{1-\sigma} - \ln P_j^{1-\sigma} + \epsilon_{ij}, \quad (1.1)$$

and $z_{ij} \equiv x_{ij} / (Y_i Y_j)$ is the value of bilateral exports x_{ij} between state i and state j relative to the product of the states’ GDPs, Y_i and Y_j . β_0 is a constant across state pairs, $\beta_1 =$

³ Helliwell (1997, 1998, 2002); Wei (1996); Hillberry (1999, 2002); Wolf (1997, 2000); Nitsch (2000); Parsley and Wei (2001); Hillberry and Hummels (2003); Anderson and Van Wincoop (2003); Chen (2004); Feenstra (2004); Combes et al. (2005); Millimet and Osang (2007); Baier and Bergstrand (2009); Buch and Toubal (2009); Nitsch and Wolf (2012) to name only a few.

$-\alpha(\sigma - 1)$ and $\beta_2 = -\rho(\sigma - 1)$, where $\sigma > 1$ is the elasticity of substitution. $\text{Border}_{ij} = (1 - \delta_{ij})$ represents the historical border line between Union and Confederate states, which takes a value of unity if states in the pair historically belonged to opposing alliances and zero otherwise. $\ln \text{Dist}_{ij}$ is the log of geographical distance between states. \mathbf{X}_{ij} denotes a vector of additional controls. And the multilateral resistance terms are defined as $P_j^{1-\sigma} = \sum_k P_k^{\sigma-1} \theta_k e^{\beta_1 \text{Border}_{kj} + \beta_2 \ln \text{Dist}_{kj}}$, where θ_k is the share of income of state k in world income. In our exercise, we substitute multilateral resistance terms with state fixed effects and switch γ on and off and work with various vectors \mathbf{X}_{ij} . ϵ_{ij} is the standard error term.

The complication with estimating that model is that the multilateral resistance terms $\ln P_i^{1-\sigma}$ and $\ln P_j^{1-\sigma}$ depend on estimates of $\hat{\beta}_1$ and $\hat{\beta}_2$ in a non-linear fashion. We follow a large strand of literature (Hummels, 1999; Anderson and Van Wincoop, 2003; Feenstra, 2004; Redding and Venables, 2004) and apply origin and destination fixed effects in an OLS gravity regression. The fixed effects capture *all* time-invariant origin and destination specific determinants, such as multilateral resistance terms, but also geographical characteristics and historical or cultural facts. The model deploying state fixed effects accounts for any state-level unobserved heterogeneity. We proxy trade costs by geographical distance, adjacency and the historical border between the former alliances of states in the Union and the Confederacy.

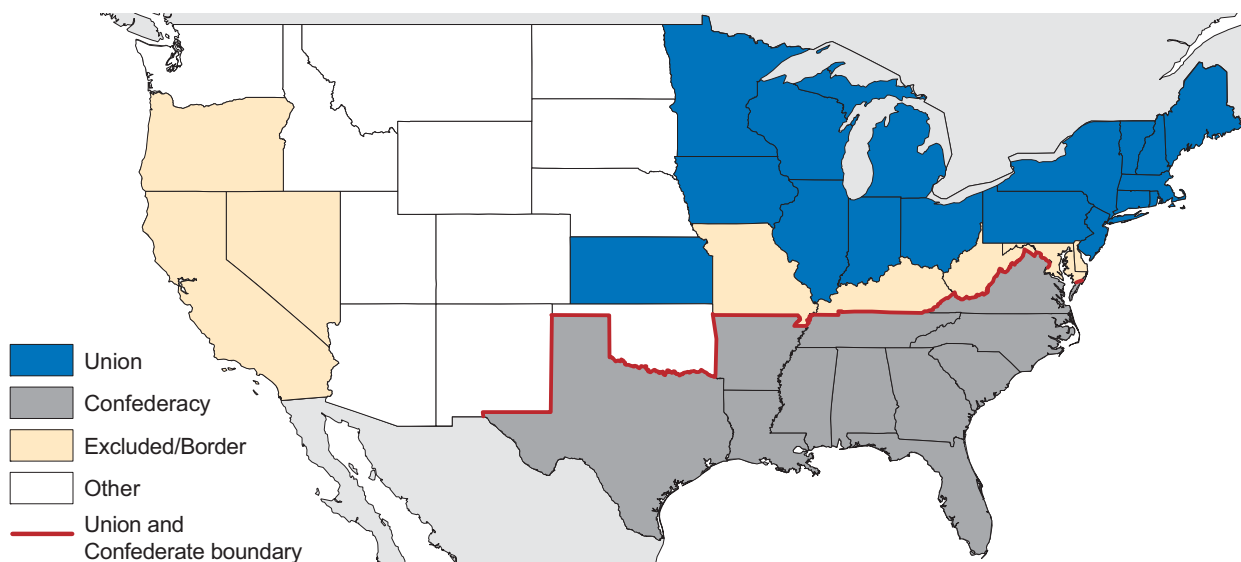
In the paper, we also use the Poisson Pseudo Maximum Likelihood (PPML) method with state fixed effects suggested by Santos Silva and Tenreyro (2006). The PPML approach has important advantages when trade flows are measured with error. Then, heteroskedastic residuals do not only lead to inefficiency of the log-linear estimator, but also cause inconsistency. This is because of Jensen's inequality which says that the expected value of the logarithm of a random variable is different from the logarithm of its expected value. This suggests that $E(\ln z_{ij})$ not only depends on the mean of z_{ij} but also on higher moments of the distribution. Heteroskedasticity in the residuals, which at first glance only affects efficiency of the estimator, feeds back into the conditional mean of the dependent variable, which, in general, violates the zero conditional mean assumption on the error term needed to guarantee consistency.

For robustness reasons, we also estimate the nonlinear least squares (NLS) model suggested by Anderson and Van Wincoop (2003) to identify the border effect.⁴ Finally, we implement the idea of Baier and Bergstrand (2009) to linearize the model by help of a first order expansion of the multilateral resistance terms and estimate by OLS.

1.2.2 Data Sources

For within- and cross-state trade flows, we focus on bilateral export data from the 1993, 1997, 2002, and 2007 *Commodity Flow Surveys* (CFS) collected by the Bureau of Transportation Statistics. The Commodity Flow Survey tracks shipments in net selling values in millions of dollars. The Commodity Flow Survey covers 200,000 (100,000; 50,000; 100,000) representative US firms for 1993 (1997; 2002; 2007). The literature is concerned about the low number of firms surveyed in the waves after 1993, see Erlbaum et al. (2006). For this reason, existing studies have usually focused on the 1993 wave which represents about 25% of registered US firms; we follow in this tradition. GDP by state stems from the *Regional Economic Accounts*, provided by the Bureau of Economic Analysis. Bilateral distance is calculated as the great circle distance between state capitals. Our primary sample consists

Figure 1.1: Union versus Confederate States



of 28 US states divided into two groups that originate from the split caused by the Secession (as shown in Figure 1.1). The **South** comprises 11 states, while the **North** consists of 17

⁴ Anderson and Van Wincoop (2003) propose to estimate their gravity model by means of an iterative procedure that minimizes the sum of squared residuals, while simultaneously obtaining values for the multilateral resistance terms.

states, as listed in Table 1.1. Five states (Delaware, Kentucky, Maryland, Missouri, and West Virginia) are excluded from the benchmark sample since soldiers from these states fought on both sides of the Civil War and the allegiance to either group of states is unclear. Still today, these five states do not belong to the (fuzzily defined) “deep South”.⁵ Somewhat abusing terminology, we call these five states *border states*. We conduct sensitivity analysis with respect to the choice of excluding those states.⁶

Table 1.1: Sample

North = Union	South = Confederacy	Excluded/Border States
Connecticut	Alabama	Delaware
Illinois	Arkansas	Kentucky
Indiana	Florida	Maryland
Iowa	Georgia	Missouri
Kansas	Louisiana	West Virginia
Maine	Mississippi	
Massachusetts	North Carolina	California
Michigan	South Carolina	Nevada
Minnesota	Tennessee	Oregon
New Hampshire	Texas	
New Jersey	Virginia	
New York		
Ohio		
Pennsylvania		
Rhode Island		
Vermont		
Wisconsin		

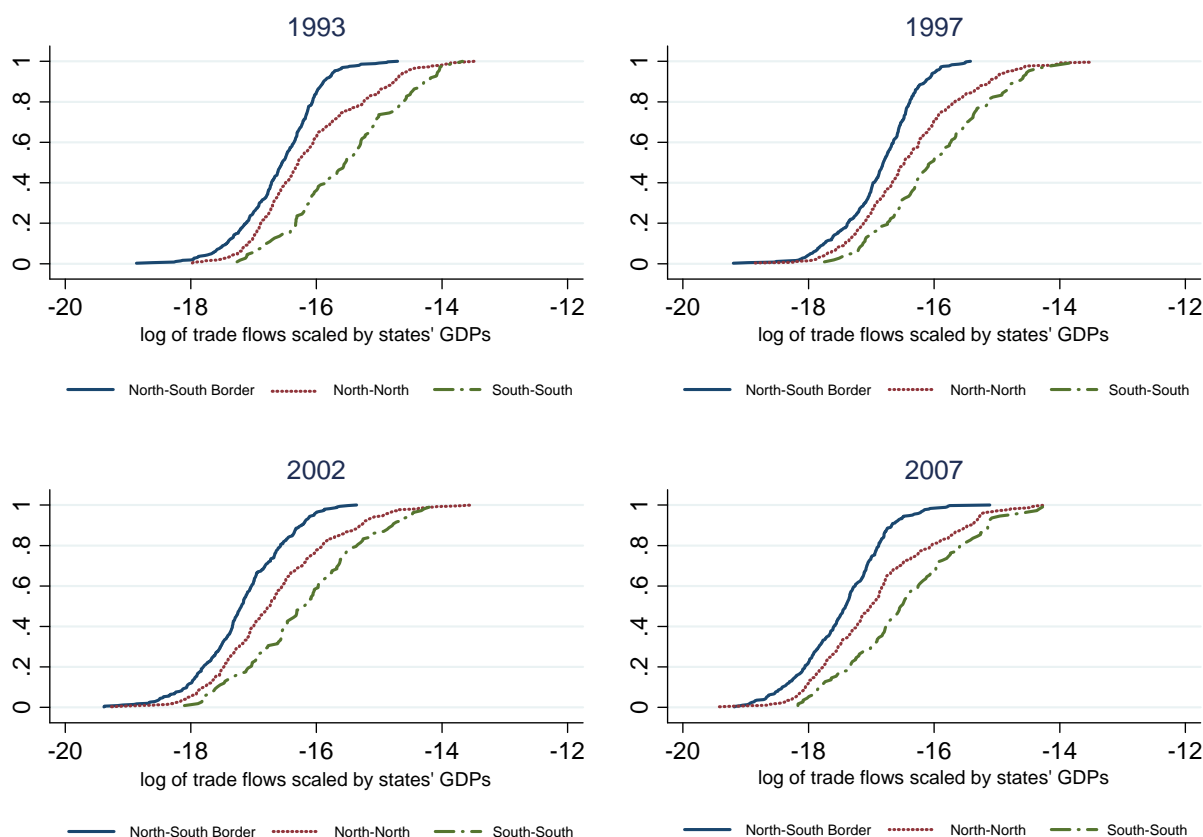
Table 1.9 in Appendix 1.A shows averages and standard deviations (for the year of 1993) of the variables used in this study. Southern states have on average substantially larger shares of Afro-Americans (22.9 versus 7.4%); the share of Christians is higher while the share of Jewish citizens is smaller (0.8 versus 2.1%). The %age share of urban population is lower in South than in North (65.7 versus 72.9). Historically (as of 1860), average farm sizes were substantially larger in the South than in the North; this gap has closed since then. The same is true for educational outcomes (illiteracy and average schooling). The GDP per capita average across the South is about 12% lower than the average across the North.

⁵ Reed and Reed (1997) define the “deep South” as an area roughly coextensive with the old cotton belt from eastern North Carolina through South Carolina west into East Texas, with extensions north and south along the Mississippi.

⁶ Note that California, Oregon and Nevada were officially part of the Union but played no particular role in the Civil War. So, we exclude them from our benchmark sample, but include them in our robustness check in Table 1.19 in Appendix 1.A.

The most dramatic differences in 1993 data pertain to institutional variables: The North is much more unionized than the South. All Northern states had a minimum wage while only 45.5% of the Southern states had one. In the 1992 presidential election, 64% of Southern states voted Republican while only 12 of Northern states did.⁷ Figure 1.2 plots cumulative

Figure 1.2: Cumulative Distribution Functions of Scaled Trade Flows



Notes: Based on Epanechnikov Kernel density estimates with optimal bandwidths.

distribution functions (CDFs) of bilateral trade flows scaled by both states' GDPs.⁸ For all years, the cumulative distribution function for North-South flows lies to the left of flows within the North or the South. Interestingly, South-South flows stochastically dominate North-North flows. In 1993, where data quality is best, the median flow is about 30% larger within the South than across South and North. This is of course a rough exercise as it does not control for other variables, such as distance, but it gives a first visual sense of how big the border effect is.

⁷ North-South differences are also clearly visible when looking at pairs of states. Table 1.10 in Appendix 1.A differentiates between the sample of *all* pairs ($N = 756$) and the sample of cross-border pairs (states from different sides of the historical border; $N = 374$).

⁸ We have estimated Epanechnikov Kernel density functions, with the width of the density window around each point set to the "optimal" level; see Silverman (1992). Optimal bandwidths are approximately 0.17, 0.25 and 0.32 for North-South, North-North and South-South flows, respectively.

1.3 Effect of the Former Union-Confederation Border

1.3.1 Benchmark Results

Estimating equation (1.1) allows to assess the *average* impact of the border on cross-border North-South trade flows relative to within region flows. Table 1.2 provides our benchmark results for the year of 1993. In line with the gravity literature, the estimated elasticity of distance is very close to -1 and highly significant at the 1% level. In our sample, and in accordance with the literature, adjacency increases bilateral trade. Due to the omission of border states from our baseline estimations, adjacency correlates negatively with the border. If adjacency increases trade, its omission would bias the border effect away from zero. In column (1), we estimate the model using origin and destination fixed effects, which account for all unobserved importer and exporter characteristics. Our model explains 84% of the variation in trade patterns. Under fixed effects, cross-border trade is on average 12.8% ($e^{-0.137} - 1$) smaller than within region trade. Hence, the border equals a tariff of 2 to 7%, depending on the choice of elasticity of substitution.⁹ Compared to international border effects, this is a substantial amount for a subnational barrier caused by an event more than a century ago. Anderson and Van Wincoop (2003) find that cross-border trade for the Canada-US case is about 80.8% lower than within trade.¹⁰ This amounts to a tariff equivalent of 20 to 128%. Results by Nitsch and Wolf (2012) suggest that the former East-West border within Germany reduces cross-border trade by about 20.5% relative to within-region trade.¹¹

In column (2), we use two indicator variables to measure within-group trade relative to cross-border trade separately for the North and the South. We find that trade within the South is 1.66 times larger than cross-border trade with the North in 1993. Counterintuitively, the North trades 1.26 times less within the region than across the border. This is puzzling, but fits the evidence displayed in Figure 1.2. Next, we estimate a Poisson Pseudo Maximum Likelihood (PPML) approach with state fixed effects suggested by Santos Silva and Tenreyro (2006). Column (3) shows that the border estimate remains very close compared to the OLS

⁹ Broda et al. (2006) estimate elasticities of substitution with a median of 3.8 and a mean of 12.1. The elasticity of substitution they estimate for the US is 2.4. We follow the recent literature and calculate tariff equivalents according to a range of the elasticity of substitution between 3 and 10.

¹⁰ Table 2 in AvW, two-country model: $e^{-1.65} - 1$.

¹¹ Table 2a in Nitsch and Wolf (2012), pooled OLS in 2004: $e^{-0.229} - 1$.

fixed effects estimation. The border impeding trade effect between the North and the South persists with a magnitude of 14%.

Table 1.2: Basic Border Effect Results

Dependent Variable: ln bilateral exports between i and j relative to states' GDPs						
Year of Data:	1993					
Data:	Aggregated				Commodity	
Specification:	OLS FE		PPML FE		PPML Multi	Chen (2004) FE
	(1)	(2)	(3)	(4)	(5)	(6)
Border Dummy $_{ij}$	-0.137*** (0.03)		-0.152*** (0.03)		-0.144*** (0.04)	-0.080*** (0.02)
North-North Dummy $_{ij}$		-0.230** (0.09)		0.063 (0.08)		
South-South Dummy $_{ij}$		0.504*** (0.10)		0.241*** (0.09)		
ln Distance $_{ij}$	-0.919*** (0.03)	-0.919*** (0.03)	-0.953*** (0.03)	-0.953*** (0.03)	-0.828*** (0.03)	-0.670*** (0.02)
Adjacency $_{ij}$	0.434*** (0.06)	0.434*** (0.06)	0.426*** (0.05)	0.426*** (0.05)	0.629*** (0.05)	0.492*** (0.04)
Fixed Effects						
Importer	YES	YES	YES	YES	YES	-
Exporter	YES	YES	YES	YES	YES	-
Importer×Commodity	-	-	-	-	-	YES
Exporter×Commodity	-	-	-	-	-	YES
Observations	740	740	756	756	1,764	12,271
Adjusted/Pseudo R^2	0.841	0.841	0.030	0.030	0.060	0.601

Notes: Constant and fixed effects not reported. Robust standard errors reported in parenthesis. States in sample as in Table 1.1. District of Columbia is excluded. In column (5), we adapt a multi-country PPML fixed effects approach, respectively, and add exports of individual US states to 20 OECD countries and between OECD trade. *** Significant at the 1 percent level, ** Significant at the 5 percent level, * Significant at the 10 percent level.

Importantly, the puzzle on North-North trade is not robust. The negative effect turns positive but insignificant when estimating the model using PPML, while results on other variables remain very much the same; see column (4). PPML can account for zeros in the trade data (16 observations in our data set). However, the main difference to OLS lies in the fact that it obtains consistent estimates even in the presence of measurement error causing heteroskedasticity. Therefore, we interpret the puzzling finding in column (2) as an artifact.¹²

¹² The puzzle also vanishes when counting the border states into the South (Table 1.18 of Appendix 1.A) or when including California, Oregon and Nevada into the Union (Table 1.19 of Appendix 1.A).

In column (5), we estimate a “multicountry” model. We consider trade between US states, between 20 OECD countries¹³ and exports from individual US states to OECD countries¹⁴ into the PPML fixed effects model of column (3). We use OECD trade, distance and GDP data provided by AvW and US state exports to OECD countries from Robert Feenstra’s webpage.¹⁵ Column (5) reports that the distance parameter remains relatively close to -1, while the border reduces North-South trade within the US by 13.4%. Sample size increases to 1,764 observations, while the explanation power of our model increases only slightly.

In the final step we explore the Commodity Flow Survey data in more detail, as disaggregated trade flows at the two-digit commodity level are available. This is in the spirit of Hillberry (1999), who estimated commodity specific border effects for products traded between Canada and the US in 1993. We pool over all commodities available in the specific year. As commodities are subject to varying transportation costs, we include origin×commodity and destination×commodity fixed effects following Chen (2004). For 1993, results for the pooled commodity fixed effects estimation are depicted in Table 1.2 column (6). We find that the border reduces North-South trade by 7.7%.

Estimates of the Anderson and Van Wincoop (2003) non-linear least squares model indicate that the border reduces trade flows between the North and the South by about 19.6% in 1993. When we estimate the model by including MR terms into the gravity estimation as suggested by Baier and Bergstrand (2009), we find that the adjusted explanation power of the estimation slightly falls to 67%, while the border estimate remains very close compared to the fixed effects estimation. The impeding trade effect of the border between the North and the South remains at 12%.¹⁶

1.3.2 Placebo Estimations

Is there something special about trade across the former Union-Confederation border as opposed to trade across other hypothetical borders? To deal with this question, we randomly assign 11 out of the 28 ‘old’ US states to a hypothetical “South” and the remainder to

¹³ These include Canada, Australia, Japan, New Zealand, Austria, Belgium-Luxembourg, Denmark, Finland, France, Germany, Greece, Ireland, Italy, Netherlands, Norway, Portugal, Spain, Sweden, Switzerland, and the United Kingdom.

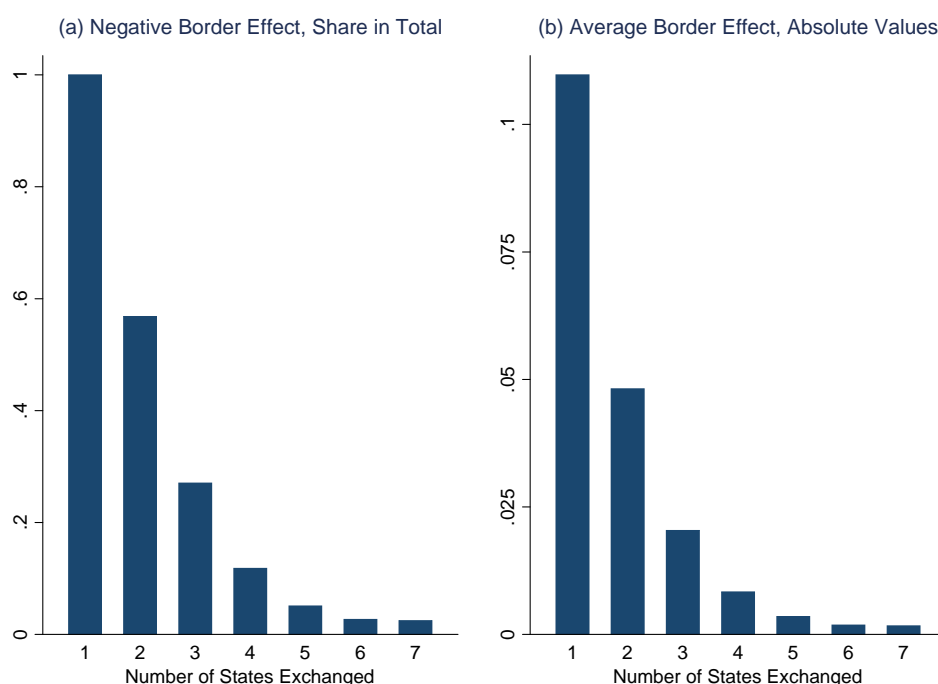
¹⁴ We focus on exports from US states to the OECD as import data of individual US states from OECD states (and vice versa) are not available.

¹⁵ <http://cid.econ.ucdavis.edu/>

¹⁶ Detailed results are found in Table 1.13 in Appendix 1.A.

a hypothetical “North”.¹⁷ Based on regression (1) of Table 1.2, we run a million placebo regressions. We find a negative and significant (at the 10% level) border effect in 7% of the cases. In 12 cases the border effect is slightly larger than the 12.8% found in our benchmark case. The largest effect we find is 1.2 percentage-points larger than our original effect, but the standard error is so large that one cannot reject the hypothesis that the effect is identical to the 12.8 benchmark result. In all 12 cases, the “South” consists predominantly of New England and the Great Lakes States. Figure 1.3 compares the hypothetical South to the

Figure 1.3: Placebo Estimations. Frequency and Average Size of Significant Border Effects in Different State Groupings



“true” sample by counting the number of misallocated states (put into the “wrong” group). Diagram (a) depicts that all samples, where one state was misallocated, yield a negative and statistically significant border effect. If two states are misallocated that share drops to about 58%; if more than five states are put into the “wrong” group the share falls below 10%. Diagram (b) displays the absolute value of the average border effect found in different subsamples. If one state is allocated to the “wrong” group, the average border effect is about 0.11 (as compared to 0.14 in the “correct” grouping). The average effect falls quickly as more states are misallocated and is below 0.01 if five or more states are exchanged.

¹⁷ The number of potential “South” subsamples and hence of state groups is huge: 21,474,180. Estimating all possible border effects between these groups of states is computationally extremely costly. A single regression takes about one second. Computation time then amounts to 249 days.

In further placebo exercises, we investigate border effects between coastal and interior states as well as between Eastern and Western states *in the whole US*. We do not find a border effect between coastal and interior states. There is no border effect neither at a hypothetical East-West border (approximately drawn at the 90° longitude line). Differences between these states can be explained by our contemporaneous controls.¹⁸ To provide further falsification tests, we consider regions where states are clustered together and split the 28 state sample into Eastern and Western states. We find no significant border effect.¹⁹ Further, we arbitrarily break North and South into two regions (Northeast-Midwest; Southeast-Southwest) each. We find no evidence of a border effect within the subregions.²⁰

1.3.3 Sensitivity Analysis

Table 1.3 summarizes border effect estimates obtained from using the 1997, 2002 or 2007 waves of the Commodity Flow Survey rather than the more reliable 1993 data. Across the OLS fixed effects model, the PPML fixed effects approach, and the commodity-level regression, we find negative border effects that are all highly statistically significant. Interestingly, there is no evidence that the border effect shrinks over time. Comparison across time is hindered by different sampling across waves. The former border reduces trade by between 7 and 16%, with the average effect clustering around at about 12%.

The use of geographical distance as a measure of transportation costs has been criticized by Head and Mayer (2002). Since 71 to 75% of shipments in the US are transported by truck (Department of Transportation), we use actual travel time from Google maps as an alternative measure of transportation costs. Ozimek and Miles (2011) provide a tool to retrieve these data. We find that the use of travel time reduces the estimated border effect in the preferred 1993 sample from 10 to 7%, thereby confirming the hypothesis that geographical distance slightly inflates the estimated border effects. However, across waves, the effect remains negative and statistically significant.²¹

As it is important to measure distance correctly, we allow for further flexibility and use distance intervals as in Eaton and Kortum (2002) instead of a log linear distance measure. We therefore create 5 distance intervals (in kilometers) including distances as:

¹⁸ Detailed results are found in Table 1.14 in Appendix 1.A.

¹⁹ Detailed results are found in Table 1.15 in Appendix 1.A.

²⁰ Detailed results are found in Table 1.16 in Appendix 1.A.

²¹ The 1997 wave is an exception. Detailed results are found in Table 1.17 Panel A of Appendix 1.A.

Table 1.3: Sensitivity Across Different Survey Waves

Dependent Variable: \ln bilateral exports between i and j relative to states' GDPs

Data:	Aggregated		Commodity
Specification:	OLS FE	PPML FE	FE Chen (2004)
PANEL A: 1997			
	(A1)	(A2)	(A3)
Border Dummy _{ij}	-0.070** (0.03)	-0.096*** (0.03)	-0.132*** (0.02)
Observations	738	756	10,342
Adjusted/Pseudo R^2	0.821	0.030	0.795
PANEL B: 2002			
	(B1)	(B2)	(B3)
Border Dummy _{ij}	-0.120*** (0.03)	-0.141*** (0.04)	-0.177*** (0.02)
Observations	711	756	6,979
Adjusted/Pseudo R^2	0.816	0.030	0.767
PANEL C: 2007			
	(C1)	(C2)	(C3)
Border Dummy _{ij}	-0.110*** (0.03)	-0.143*** (0.04)	-0.172*** (0.02)
Observations	740	756	11,834
Adjusted/Pseudo R^2	0.847	0.030	0.763

Notes: Constant, fixed effects, effects on log distance and adjacency are not reported. Robust standard errors reported in parenthesis. Column (3) includes Importer \times Commodity and Exporter \times Commodity fixed effects following Chen (2004). States in sample as in Table 1.1. District of Columbia is excluded.*** Significant at the 1 percent level, ** Significant at the 5 percent level, * Significant at the 10 percent level.

[0,250),[250,500),[500,1000),[1000,2000), and [2000,max] and include dummies thereof into the regression. We find border effects to be slightly more trade impeding compared to when using the log linear distance measure and still highly significant for all years.²² Interestingly, we find a similar distance ranking as in Eaton and Kortum (2002) for US states. Distance intervals that capture relatively close state pairs have a smaller negative effect on trade than pairs that are further apart relative to the closest distance interval [0,250). From this we conclude that our border results are not qualitatively affected by the distance measure.

²² Detailed results are found in Table 1.17 Panel B of Appendix 1.A.

To make sure that our treatment of border states (i.e., states whose allegiance was unclear and that are therefore excluded from our benchmark sample), does not bias our results, we assign them alternatively to the South or to the North. The border states were slave states, but officially never seceded, so it is counterfactual to include them into the South. We find that the assignment of those border states does not matter qualitatively for our findings. Estimated effects are slightly lower than when border states are excluded altogether.²³ California, Oregon and Nevada fought on the side of the Union and may thus be included in the sample on the side of the North, even though they were separated from the other states by a large distance and the territories that did not yet belong to the United States. Results do not change qualitatively if we include the three states in the North. The inclusion of the three states rather increases the border effect, which turns out to reduce North-South trade by 17% under OLS fixed effects and 18% under PPML fixed effects.²⁴ In addition, the Northern states trade more with another under the OLS fixed effects approach if we include the three states in the North.²⁵

1.3.4 Estimates by Sector

Finally, we also run regressions sector-by-sector. Table 1.4 provides summary results, suppressing other coefficients except the one on the border dummy. The estimated border effect is $\hat{\beta}_1 = -\alpha \widehat{(\sigma - 1)}$, confounding the elasticity of substitution and the trade-cost increasing effect of the border. It is therefore not surprising, that the low- σ agricultural sector features a high but only moderately robust estimate, while the low- σ mining sector does not display a border effect (except in 1997). No border effect exists in the machinery sector, neither. This is presumably due to North-South differences in comparative advantage that the simple model does not capture. The border effect is most pronounced in the chemical and manufacturing sectors, where the degree of product differentiation is high (hence, σ low).

One may conjecture that the Secession has continuing negative effects on the level of trust between market participants. It may also have affected the strength of preferences for local products. Both mechanisms should have no bearing on standardized (homogeneous) goods whose quality can easily be verified and where idiosyncratic features of demand should

²³ Detailed results are found in Table 1.18 of Appendix 1.A.

²⁴ The increase in the border effect when the three "disconnected" states are included supports the view that the border effect is really about a "genuine" Union-vs-Confederation effect.

²⁵ Detailed results are found in Table 1.19 in Appendix 1.A.

Table 1.4: Sectoral Results (fixed-effects estimation)

Dependent Variable: ln bilateral exports between i and j relative to states' GDPs

Sector	Agriculture	Mining	Chemical	Machinery	Manufacturing
PANEL A: 1993					
	(A1)	(A2)	(A3)	(A4)	(A5)
Border Dummy $_{ij}$	-0.254*** (0.08)	-0.052 (0.26)	-0.236*** (0.07)	-0.036 (0.07)	-0.051 (0.05)
Observations	4,585	1,156	2,940	4,140	11,484
Adjusted R^2	0.659	0.611	0.545	0.565	0.684
PANEL B: 1997					
	(B1)	(B2)	(B3)	(B4)	(B5)
Border Dummy $_{ij}$	-0.133 (0.08)	-0.453** (0.18)	-0.065 (0.06)	-0.078 (0.05)	-0.181*** (0.04)
Observations	5,210	2,403	3,075	3,315	7,340
Adjusted R^2	0.720	0.658	0.688	0.681	0.752
PANEL C: 2002					
	(C1)	(C)	(C3)	(C4)	(C5)
Border Dummy $_{ij}$	-0.158 (0.10)	-0.123 (0.36)	-0.150* (0.08)	-0.037 (0.07)	-0.246*** (0.06)
Observations	4,190	1,377	2,680	3,065	6,800
Adjusted R^2	0.679	0.623	0.659	0.618	0.722
PANEL D: 2007					
	(D1)	(D2)	(D3)	(D4)	(D5)
Border Dummy $_{ij}$	-0.242*** (0.07)	0.007 (0.17)	-0.287*** (0.06)	-0.016 (0.07)	-0.238*** (0.04)
Observations	3,910	1,679	2,976	3,332	7,156
Adjusted R^2	0.752	0.674	0.715	0.614	0.766

Notes: Importer and exporter fixed effects included in all regressions. Constant, fixed effects and effects on log distance and adjacency not reported. Robust standard errors reported in parenthesis. Commodities pooled into sectors as listed in Table 1.11 and 1.12 in Appendix 1.A. States in sample as in Table 1.1. District of Columbia excluded.*** Significant at the 1 percent level, ** Significant at the 5 percent level, * Significant at the 10 percent level.

not matter (for example, steel). It is therefore comforting that the border effect is largest in sectors with typically strongly differentiated output. The finding therefore supports the view that the former border reflects a cultural divide.

1.4 Accounting for Observed Contemporaneous Heterogeneity

1.4.1 Benchmark Results

In this section, we investigate whether observable characteristics of state pairs bias the estimated coefficient. We include a large number of contemporaneous determinants of trade that are discussed in the empirical literature stepwise into the regression. If the variables are not bilateral in nature, we bilateralize them by either taking the absolute difference of variables in state i and state j , denoted by the operator Δ , or by using the product of variables in state i and state j , denoted by the operator \times . The product of variables relates to *network* effects between pairs, while the Δ operator focuses on the *difference* between state pairs.²⁶ Table 1.5 reports results for our benchmark year 1993. All estimations include origin and destination fixed effects.

Column (1) of Table 1.5 depicts the benchmark result including geographical variables with a border effect of 13%. In column (2), we account for the impact of ethnic, religious, or cultural networks (Rauch, 1999; Rauch and Trindade, 2002; Combes et al., 2005) and migration within the US (Helliwell, 1997; Head and Ries, 1998; Millimet and Osang, 2007). The literature reasons that common culture and tastes increase trade flows as they facilitate contracts and instill trust; they also make it more likely that states produce and consume similar goods. Migration and networks might bias the border effect estimate upwards as they increase trade but are negatively associated with the border. To test the impact of networks we include (i) cross-state migration stocks of people residing in one state but were born in another taken from the *American Community Survey Decennial Census*; (ii) the product of the share of Afro-Americans in total state population from the *Population Estimates Program*; (iii) the product of the Jewish population in total state population from the *American Jewish Yearbook*; and (iv) self-reported affinity to Christianity, other religious groups, or no religion from the *American Religious Identification Survey 2008 Report*, into the estimation. We find that migration networks, high shares of Afro-Americans, of population shares affiliated to Buddhism, Hinduism or Islam, and of people not self-identifying with any religious group spur trade flows. A 1% increase in the bilateral migration stock indicates

²⁶ We tried a range of other variables and combinations, as well as network and difference variables separately and combinations thereof. The results are robust to these modifications.

Table 1.5: Contemporaneous Controls, 1993 (fixed-effects estimation)

Dependent Variable: ln bilateral exports between i and j relative to states' GDPs						
	(1)	(2)	(3)	(4)	(5)	(6)
Border Dummy $_{ij}$	-0.137*** (0.03)	-0.120*** (0.03)	-0.117*** (0.04)	-0.109*** (0.04)	-0.120*** (0.04)	-0.119*** (0.04)
Geographical Controls						
ln Distance $_{ij}$	-0.919*** (0.03)	-0.631*** (0.04)	-0.633*** (0.04)	-0.627*** (0.05)	-0.611*** (0.05)	-0.612*** (0.05)
Adjacency $_{ij}$	0.434*** (0.06)	0.356*** (0.05)	0.352*** (0.05)	0.380*** (0.05)	0.397*** (0.05)	0.399*** (0.05)
Network Controls						
ln Migration Stock $_{ij}$		0.129*** (0.03)	0.125*** (0.03)	0.089** (0.03)	0.088** (0.03)	0.086** (0.03)
× Black Share $_{ij}$		0.001*** (0.00)	0.001*** (0.00)	0.001*** (0.00)	0.001*** (0.00)	0.001*** (0.00)
× Jewish Share $_{ij}$		-0.005 (0.00)	-0.004 (0.00)	-0.003 (0.00)	-0.002 (0.00)	-0.002 (0.00)
× Christian Share $_{ij}$		0.002 (0.00)	0.002 (0.00)	0.002 (0.00)	0.002* (0.00)	0.002 (0.00)
× Other Religion Share $_{ij}$		0.062** (0.03)	0.064** (0.03)	0.067** (0.03)	0.055* (0.03)	0.056* (0.03)
× No Religion Share $_{ij}$		0.007 (0.00)	0.006 (0.00)	0.007 (0.00)	0.005 (0.00)	0.005 (0.00)
× Urban Share $_{ij}$		3.494*** (0.77)	3.425*** (0.81)	3.675*** (0.92)	3.648*** (1.13)	3.651*** (1.13)
Common Colonizer $_{ij}$		0.198*** (0.04)	0.202*** (0.04)	0.173*** (0.04)	0.168*** (0.04)	0.169*** (0.04)
Labor Market/Political Institutions						
Δ Union Membership $_{ij}$			-0.003 (0.02)	-0.013 (0.02)	-0.016 (0.02)	-0.017 (0.02)
Δ Union Density $_{ij}$			0.003 (0.02)	0.013 (0.02)	0.016 (0.02)	0.017 (0.02)
Δ Minimum Wage $_{ij}$			-0.177 (0.15)	-0.207 (0.15)	-0.167 (0.15)	-0.168 (0.15)
Δ Republican $_{ij}$			-0.001 (0.03)	-0.003 (0.03)	-0.002 (0.03)	-0.002 (0.03)
Judiciary Election $_{ij}$			-0.074** (0.03)	-0.073** (0.03)	-0.072** (0.03)	-0.072** (0.03)
Heckscher-Ohlin Controls						
Δ ln Capital-Labor Ratio $_{ij}$				0.024 (0.16)	0.022 (0.16)	0.000 (0.20)
Δ ln High-Low Skilled Ratio $_{ij}$				0.076 (0.09)	0.079 (0.09)	0.080 (0.09)
Δ ln Average Schooling $_{ij}$				-1.404 (1.13)	-1.473 (1.15)	-1.584 (1.27)
Δ ln Cropland $_{ij}$				-0.053*** (0.02)	-0.052*** (0.02)	-0.052*** (0.02)
Δ ln Farm Size $_{ij}$				0.021 (0.05)	0.010 (0.07)	0.007 (0.07)
Δ ln Agricultural to Total Output $_{ij}$				0.066 (0.04)	0.038 (0.04)	0.038 (0.04)
Δ ln Manufacturing to Total Output $_{ij}$				-0.102 (0.11)	-0.075 (0.11)	-0.064 (0.12)
Demography						
Δ ln Population $_{ij}$					-0.018 (0.03)	-0.019 (0.03)
Δ ln Population Density $_{ij}$					0.029 (0.04)	0.030 (0.04)
Δ ln Fertility $_{ij}$					-0.675 (0.41)	-0.658 (0.41)
Linder Hypothesis						
Δ ln Income per Capita $_{ij}$						0.069 (0.29)
Observations	740	740	740	740	740	740
Adjusted R^2	0.841	0.865	0.866	0.868	0.869	0.868

Notes: Importer and exporter fixed effects included in all regressions. Constant and fixed effects not reported. Robust standard errors reported in parenthesis. The operator Δ denotes the absolute difference of variables in state i and state j . The operator × denotes the product of variables in state i and state j . *** Significant at the 1 percent level, ** Significant at the 5 percent level, * Significant at the 10 percent level.

an increase in trade by 13% in column (2).²⁷ If we include network controls, the border still turns out to reduce bilateral trade by 11.3%. In addition, common colonial heritage, also included in column (2), may have lasting effects on bilateral trade.²⁸ We construct an indicator variable that takes value one if a pair of states had a common colonizer (Britain, France or Spain) and zero otherwise. We find that a common colonial past increases bilateral trade by about 22%. Yet, while most of those network variables matter statistically, they reduce the estimated border effect only slightly.

Column (3) examines the impact of labor market and political institutions. We control for labor market institutions by including dissimilarities in union membership and density from Hirsch et al. (2001), as well as a dummy for the existence of minimum wage legislation provided by the US Department of Labor. In theory, differences in labor market institutions could increase bilateral trade, because differential legislation acts as a source of comparative advantage as in Cunat and Melitz (2012). In our analysis, we find that institutional differences tend to reduce trade (albeit statistical precision of estimates is nonexistent). This may signal that institutional differences are caused by some deeper differences in cultural norms and that the latter discourage trade by more. Column (3) also controls for differences in the political alignment in the 1992 presidential election (Clinton against Bush sen.) and whether states elect or appoint the judiciary. Voting behavior has no statistically measurable effect on trade, while the difference in judiciary appointment procedure turns out to depress bilateral trade flows. The estimated border effect, however, remains virtually unchanged.

In column (4), we include controls for the difference in relative factor endowments of states, thereby accounting for the Heckscher-Ohlin trade theory. Omitting differences in factor proportions might lead to an upward bias of the border coefficient, as differences in factor proportions should increase trade flows and appear to be more pronounced when the border is present. To measure contemporaneous differences in relative factor proportions and human capital accumulation, we include the absolute difference in (i) capital-labor shares from Turner et al. (2008); (ii) shares of high and low skilled in the population²⁹; (iii) average years of schooling for the population over 25 from Turner et al. (2007); (iv) cropland from the *National Resource Inventory Summary Report*; (v) average farm size from the *Census of Agriculture*; (vi) agricultural relative to total output; and (vii) manufacturing

²⁷ A similar effect has been identified by Combes et al. (2005) for trade within France.

²⁸ See, for instance, Head et al. (2010).

²⁹ We measure high skilled by a Bachelor's degree or above and low skilled by a High School degree or below. Data stem from the *Census of Population* and the *American Community Survey*.

relative to total output from the Bureau of Economic Analysis. As in other gravity exercises, classical Heckscher-Ohlin variables do not show up statistically significant, though both the variables on the difference in the capital-labor ratio and the difference in relative skill endowment bear the right sign. Differences in the availability of cropland reduce bilateral trade. Contemporaneous differences in factor endowments do not capture the border, which still reduces North-South trade by 10.3%.

Column (5) includes demographic variables such as the difference in contemporaneous population and population density from the *Population Estimates Program*, as well as fertility rates from the *Vital Statistics of the United States*. Common demographic features across states may suggest common preferences, so that bilateral trade is larger for such states. The estimated parameters, however, are insignificant throughout. The border effect remains negative and significant.

Finally, following the literature on the Linder effect, we include the difference in the log of per capita income as in Thursby and Thursby (1987); Bergstrand (1989) and Hallak (2010). The hypothesis is that states with dissimilar GDP per capita should have differing preference structures and, hence, trade less. Since the border correlates negatively with GDP per capita in the data, omitting the Linder term may bias the border effect away from zero. This is, however, not what we find. In column (6), we find no support for the Linder hypothesis; the estimated border effect does not move. We have also experimented with direct measures of inequality (Gini coefficients), but without success.

Column (6) represents our most comprehensive and preferred model. The border effect is about 11.2%. It explains 87% of the variation in bilateral trade flows, 67% of which are attributable to included variables and controls.³⁰

1.4.2 Sensitivity Analysis

Table 1.6 summarizes sensitivity results pertaining to the comprehensive model in column (6) of Table 1.5. Panel A deploys the OLS fixed effects approach. Our baseline border effect of -0.119 is reported in column (A1). We find a negative and significant border effect for 1993 and 2002, while the effect for 1997 and 2007 are insignificant. Results based on the commodity flow survey from 1997 onwards suffer from the fact that the number of

³⁰ A model that explains bilateral trade solely using importer and exporter fixed effects can only explain 20% of the variation in the dependent variable.

firms surveyed is only around 25% of those surveyed in 1993. In Panel B, we turn to the PPML model that includes fixed effects. The border barrier turns out to be strong only in 1993. If we use the pooled commodity fixed effects setup with importer \times commodity and destination \times commodity fixed effects following Chen (2004) in Panel C, we find strong trade impeding effect for all years (except for 2002). Overall, we can conclude that the findings on the border effect compare well to our earlier results. The border reduces cross-border trade by 7 to 21%, depending on the year and the specification.³¹

Table 1.6: Controls, Alternative Samples and Models: Summary Results

Dependent Variable: ln bilateral exports between i and j relative to states' GDPs				
Year of Data:	1993	1997	2002	2007
PANEL A: OLS FE				
	(A1)	(A2)	(A3)	(A4)
Border Dummy $_{ij}$	-0.119*** (0.04)	-0.039 (0.05)	-0.119* (0.06)	0.016 (0.06)
Observations	740	738	711	740
Adjusted R^2	0.868	0.854	0.844	0.874
PANEL B: PPML FE				
	(B1)	(B2)	(B3)	(B4)
Border Dummy $_{ij}$	-0.133*** (0.05)	-0.027 (0.05)	-0.019 (0.07)	0.115 (0.07)
Observations	756	756	756	756
Pseudo R^2	0.028	0.028	0.033	0.031
PANEL C: POOLED COMMODITY FE (Chen 2004)				
	(C1)	(C2)	(C3)	(C4)
Border Dummy $_{ij}$	-0.234*** (0.04)	-0.101*** (0.03)	-0.050 (0.04)	-0.076** (0.04)
Observations	12,271	10,342	6,979	11,834
Adjusted R^2	0.611	0.805	0.775	0.773

Notes: Constant, fixed effects, and controls not reported. Robust standard errors reported in parenthesis. All models include variables of column (6) in Table 1.5 as additional controls. *** Significant at the 1 percent level, ** Significant at the 5 percent level, * Significant at the 10 percent level.

³¹ When we work with sectoral data and include the additional controls, results suggests that the trade impeding effect is mainly caused by barriers to manufacturing products in all years. Compared to our earlier results, the border effect is negative but less robust for agriculture and chemicals – except for 2002 and 2007. Mining and machinery products again depict in most cases an indistinguishable coefficient from zero. Table 1.20 in Appendix 1.A reports detailed results.

1.5 Accounting for Historical Determinants

1.5.1 Benchmark Results

The economic literature on the emergence of armed conflicts depicts that strong bilateral trade links decrease the probability that two countries go to war, while multilateral openness increases the odds of conflict (Martin et al., 2008). If determinants of bilateral trade are persistent over time, the border could not be considered exogenous in the statistical sense. Historical bilateral trade data are, however, not available. But, one can include historical variables that may, through their impact on historical trade patterns, affect the probability of conflict (and thus the incidence of the border). Moreover, Eichengreen and Irwin (1998) suggest that history might affect contemporaneous trade flows through persistent effects on institutions.

According to Engerman and Sokoloff (2000, 2005), dissimilarities in agricultural land use, driven by soil endowments and climate, led to the South adopting slavery and, more broadly, to the emergence of conflicting economic interests between the North and the South, and ultimately, to the Secession. The different economic models may have long-lasting effects on inequality within states, which may, in turn, be relevant for today's level of economic transactions (Linder effect). It may also have persistent effects on institutions, which affect contemporaneous bilateral trade. The historical settlement structure may have induced networks along cultural lines that survived over time.³² Absolute differences in historical variables are positively correlated to the border, so that their omission may bias the estimated border effect away from zero.

To account for these possibilities, Table 1.7 includes historical differences in (i) cropland; (ii) average farms size; (iii) population density; and (iv) illiteracy rates of the non-slave population.³³ In column (1) to (3), we find that none of these variables matter statistically, except for historical farm size differences which are significant at the 5% level. Including farm size increases rather than decreases the border coefficient to -0.234 . This is surprising as historical farm size differences correlate positively with the border.

³² The analysis relates to the literature on the long-term impact of factor endowments and institutions (Acemoglu et al., 2002; Nunn, 2009; Galor et al., 2009).

³³ Additionally, all models include our additional contemporaneous controls from Table 1.5 column (6) and importer as well as exporter fixed effects.

Table 1.7: Contemporaneous and Historical Controls, 1993 (fixed-effects estimation)

Dependent Variable: ln bilateral exports between i and j relative to states' GDPs							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Border Dummy $_{ij}$	-0.234*** (0.07)	-0.121*** (0.04)	-0.129** (0.06)	-0.177** (0.08)	-0.118*** (0.04)	-0.141*** (0.04)	-0.251** (0.10)
Controls as of Table 1.5 column (6) included	YES	YES	YES	YES	YES	YES	YES
Historical Controls							
$\Delta \ln 1860$ Cropland $_{ij}$	-0.027 (0.02)						-0.035* (0.02)
$\Delta \ln 1860$ Farm Size $_{ij}$	0.160** (0.08)						0.100 (0.09)
$\Delta \ln 1860$ Population Density $_{ij}$		0.032 (0.02)					0.028 (0.02)
$\Delta \ln 1860$ Illiteracy Rates $_{ij}$			0.001 (0.00)				0.006 (0.01)
$\Delta 1860$ Slave Share $_{ij}$				0.002 (0.00)			-0.000 (0.00)
$\Delta 1860$ Free Black Share $_{ij}$				0.030 (0.02)			0.035* (0.02)
$\times 1860$ French Share $_{ij}$					0.492*** (0.16)		0.474*** (0.17)
$\times 1860$ Spanish Share $_{ij}$					2.462 (16.94)		0.085 (17.74)
$\times 1860$ Irish Share $_{ij}$					-0.002** (0.00)		-0.002** (0.00)
$\times 1860$ German Share $_{ij}$					0.001 (0.00)		0.001 (0.00)
$\times 1860$ British Share $_{ij}$					0.002 (0.00)		0.003 (0.00)
$\Delta 1860$ Malaria Risk $_{ij}$						0.345 (0.25)	0.255 (0.29)
Observations	740	740	740	740	740	740	740
Adjusted R^2	0.869	0.869	0.868	0.869	0.873	0.869	0.873

Notes: Importer and exporter fixed effects included in all regressions. All models include variables as of column (6), Table 1.5 as additional controls. Constant, fixed effects, and contemporaneous controls not reported. Robust standard errors reported in parenthesis. The operator Δ denotes the absolute difference of variables in state i and state j . The operator \times denotes the product of variables in state i and state j . *** Significant at the 1 percent level, ** Significant at the 5 percent level, * Significant at the 10 percent level.

One would expect the legacy of slavery to partly capture the border barrier in column (4). However, we find that differences in slave shares in 1860 exert no impact on bilateral trade patterns and do not explain away the border barrier.³⁴ Interestingly, the inclusion of

³⁴ If we use the difference in the share of slaves in 1840, when there were still slaves also living in the North, we still find robust results on the border effect but an insignificant coefficient close to zero for the slave share. In column (7), the effect of differences in 1840 slaves is still zero, while the effects of all other historical controls prevail. The border effect remains negative and significant on the 1% level.

the absolute difference in shares of free blacks in 1860 exerts a positive and significant effect on contemporaneous trade in column (7).

In addition, similarities in culture due to similar settlement structures in US states before the war could have induced social and business networks that have survived over time and still affect trade. We therefore include the product in the shares of French, Spanish, Irish, British and German settlers in 1860. While Spanish, German, or British heritage has no particular impact on trade, Irish heritage decreases bilateral trade significantly in column (5). States with a large share of French settlers trade more amongst each other.

According to Acemoglu et al. (2002), historical climatic differences measured by the incidence of malaria, may have affected the characteristics and quality of institutions. In the present case, it is conceivable that the high risk of malaria in the South has led to the acceptance of slavery by the local elite and may therefore constitute a deep reason for the conflict. It may also affect contemporaneous trade flows through its lasting effect on institutions. So, we include the malaria risk index in 1860 from Hong (2007). We find neither a significant effect on trade nor does historical climate explain away the border. In the last column, we include all historical controls simultaneously in our model. All in all, we find that the border reduces trade by 22%, even when we include variables capturing the historical determinants of the Secession.³⁵

1.5.2 Including the West

From the previous analysis, one cannot conclude that the Secession has *caused* the observed border effect in contemporaneous trade data. Including historical variables that relate to the deep reasons for the Civil War goes some way in dealing with reverse causation. However, it fails to account for unobserved shocks that both make the odds for Secession and today's bilateral trade flows larger. Unfortunately, no instrument is ready-to-use in an IV approach.

One way to nudge the analysis closer to identifying a causal effect consists in separating the *whole* of the US – including the West – into states that underwent a treatment by the Secession and states that were not affected by these historical events. We separate the states into three groups – the North, the South, and the West –, still excluding border states, the

³⁵ We have also experimented with direct measures for the historical transportation system (differences or networks of railroad miles per 100 square miles of land area after the Civil War in 1870). The result is robust to the inclusion of the historical transportation system.

District of Columbia, Alaska and Hawaii.³⁶ The border dummy is unity for states that found themselves on opposite sides of the Civil War and zero for all other pairs of states. Adding the West adds a control set of state pairs that are characterized by their absence of a past shaped by the Civil War.

Table 1.8: Additionally Including the West, 1993

Dependent Variable: ln bilateral exports between i and j relative to states' GDPs					
Data:	Aggregated				Commodity
Specification:	OLS FE		PPML FE		Chen (2004) FE
	(1)	(2)	(3)	(4)	(5)
Border Dummy $_{ij}$	-0.068*		-0.090*		-0.213***
	(0.04)		(0.05)		(0.03)
South-South Dummy $_{ij}$		0.235***		0.267***	
		(0.07)		(0.09)	
North-North Dummy $_{ij}$		-2.665**		-0.307	
		(1.24)		(0.30)	
West-West Dummy $_{ij}$		-0.039		-0.084	
		(0.09)		(0.11)	
ln Distance $_{ij}$	-0.421***	-0.422***	-0.275***	-0.282***	-0.238***
	(0.05)	(0.05)	(0.06)	(0.05)	(0.03)
Adjacency $_{ij}$	0.463***	0.458***	0.338***	0.326***	0.475***
	(0.07)	(0.07)	(0.06)	(0.06)	(0.04)
Additional Controls	YES	YES	YES	YES	YES
Observations	1,696	1,696	1,806	1,806	23,400
Adjusted/Pseudo R^2	0.808	0.808	0.039	0.039	0.567

Notes: Constant, fixed effects, and controls not reported. Robust standard errors reported in parenthesis. All models include variables as of column (6), Table 1.5 available for all US states as additional controls. *** Significant at the 1 percent level, ** Significant at the 5 percent level, * Significant at the 10 percent level.

Table 1.8 reports the results. All models include additional contemporaneous controls.³⁷ In columns (1), (3) and (5), we find for the OLS fixed effects, the PPML fixed effects, and the pooled commodity fixed effects regression a significant trade impeding effect of the Secession treatment. The effect ranges between 7 and 19%. In addition, we again find in column (2) that the South trades more amongst each other while the effect on the North is negative, but turns insignificant when we control for heteroskedasticity in the PPML fixed effects approach in column (4). There seems not to be any particular trade effect within Western states.³⁸

³⁶ West includes all US states that were not assigned to the North, the South or the border states in Table 1.1, excluding the District of Columbia, Alaska and Hawaii.

³⁷ Historical controls are not available for most of the Western states before the war, as these were only Territories in 1860.

³⁸ Results are similar for the other years and can be found in Table 1.21 in Appendix 1.A.

When we estimate border effects for a sample of South and West states³⁹ and a separate sample of North and West states⁴⁰, we see no border effect. In some cases, we even find a positive and significant coefficient such that Southern and Western states trade more rather than less with another.

1.6 Civil War at 150: Still Relevant, Still Divisive

The former border between the Union and the Confederation is still relevant today: The defunct border represents a trade barrier that lowers trade between US states by on average 7 to 22%. In a million placebo estimations, we find supportive evidence that the magnitude of this border effect is unique. The result is robust to using alternative waves of the Commodity Flow Survey, to different econometric methods, or to the inclusion of Western states or the rest of the world. It cannot be substantially attenuated, let alone eliminated, by adding a vast array of contemporaneous and historical variables that correlate both with the border dummy and, potentially, also with bilateral trade.

The great Mississippi novelist and poet William Faulkner famously writes “The past is never dead. It’s not even past.” (Requiem for a Nun, 1951). This holds true for the Secession that tore the US apart 150 years ago, even when the judgment is based on bilateral trade data and econometric analysis: Trade between the former Confederation and the former Union is about 13% smaller on average than within the alliance. Several additional results stand out: First, the effect of the long defunct border on today’s trade is not attributable to the legacy of slavery alone. It becomes weaker if not the Secession but the status of slave states is the criterion for belonging to one of the two groups. Second, the border effect is not merely a North-South effect. When the border is redefined to reflect whether two states have been on opposing sides in the Civil War, it remains significantly negative. Third, the trade inhibiting force of the former border has to do with the degree of differentiation of products: the higher, the stronger. This suggests that the channel through which the border still matters may be through cultural affinity or trust.

Our results imply that one cannot view the US as a single market. The effect of the former Union-Confederation border persists after 150 years. The finding suggests that one should not be overly optimistic as to other regional integration projects. This applies most notably

³⁹ Detailed results are found in Table 1.22 in Appendix 1.A.

⁴⁰ Detailed results are found in Table 1.23 in Appendix 1.A.

to Europe, where the last major war ended only 67 years ago and the history of conflict is much longer and bloodier. Moreover, in contrast to the US there is no pre-war history of integration, and other frictions related to languages, legal systems etc. are plentiful.

In terms of welfare, our results imply that trade disruptions in the past can still constitute barriers today. By distorting the flow of trade away from the structure that would have obtained without the Secession, they present continuing welfare losses. So, by its long-run effects on economic integration armed conflicts may cast a very long shadow on the welfare of future generations.

1.A Appendix

Table 1.9: Summary Statistics by State, 1993

Unit of Observation: State Level					
Sample Variable	North (N = 17)		South (N = 11)		Description
	Mean	Std. Dev.	Mean	Std. Dev.	
Black Share	7.412	5.519	22.855	7.871	Share (%) of blacks in population.
Jewish Share	2.105	2.339	0.809	1.285	Share (%) of Jewish in population.
Christian Share	86.882	3.059	91.636	3.139	Share (%) of Christian in population.
Other Religion Share	3.235	2.278	1.727	1.272	Share (%) of people with other religion.
No Religion Share	7.647	1.998	5.000	1.673	Share (%) of people with no religion.
Urban Share	72.853	16.095	65.655	12.098	Share (%) of urban population.
ln 1860 Cropland	15.038	1.045	15.228	0.806	1860 cropland in 1,000 acres.
ln 1860 Farm Size	4.785	0.184	5.940	0.291	1860 average farm size in acres.
ln 1860 Population Density	3.338	1.384	2.454	0.929	1860 population by square km.
ln 1860 Illiteracy Rates	1.604	0.415	2.683	0.303	1860 share of non-slave illiterate.
1860 Slave Share	0.000	0.001	39.700	11.369	1860 slaves in population.
1860 Free Black Share	1.018	0.999	1.170	1.326	1860 free blacks in population.
1860 French Share	0.302	0.202	0.254	0.619	1860 French in population.
1860 Spanish Share	0.004	0.005	0.032	0.076	1860 Spanish in population.
1860 Irish Share	6.890	4.303	0.918	1.057	1860 Irish in population.
1860 German Share	4.772	4.244	0.886	1.271	1860 German in population.
1860 British Share	4.250	2.216	0.306	0.204	1860 (American) British in population.
1860 Malaria Risk	0.126	0.073	0.351	0.057	1860 Malaria Risk Index.
ln Capital-Labor Ratio	11.610	0.261	11.520	0.227	Capital relative to Labor.
ln High-Low Skilled Ratio	0.264	0.316	-0.256	0.256	Bachelor to high school, age ≥ 25 .
ln Average Schooling	2.579	0.023	2.538	0.023	Years of Schooling.
ln Cropland	7.821	2.223	8.574	0.656	Cropland in 1,000 acres.
ln Farm Size	5.309	0.570	5.574	0.424	Average farm size in acres.
ln Agri. / Tot. Output	-4.515	0.687	-4.159	0.427	Agri. over total output, mio US \$.
ln Manuf. / Tot. Output	-1.615	0.250	-1.661	0.364	Manuf. over total output, mio US \$.
ln Population	15.237	1.009	15.534	0.624	Total Population in thousands.
ln Population Density	5.175	1.145	4.602	0.485	Population by square km.
ln Fertility	4.127	0.071	4.184	0.065	Live births per 1,000 women, age 15-44.
ln Income Per Capita	10.129	0.129	10.011	0.115	Total GDP per capita.
Union Membership	18.106	5.470	8.436	2.826	Percentage of union membership.
Union Density	19.812	5.218	10.382	3.009	Percentage of union density.
Minimum Wage	1.000	0.000	0.455	0.522	1 if state has minimum wage, 0 else.
Republican	0.118	0.332	0.636	0.505	1 if republ., 1992 pres. election, 0 else.
Judiciary Election	1.824	0.883	1.182	0.405	1 if judiciary is elected, 0 else.

Notes: Data sources as in Table 1.10.

Table 1.10: Summary Statistics and Data Sources, 1993

Unit of Observation: Pairs of States					
Sample	Full (N = 756)		North-South (N = 374)		Data Source
Variable	Mean	St. Dev.	Mean	St. Dev.	
$\ln z_{ij}$	-16.257	0.863	-16.590	0.637	Commodity Flow Survey; Bureau of Economic Analysis.
z_{ij}	1.31e-07	1.64e-07	7.36e-08	5.15e-08	Commodity Flow Survey.
Border _{ij}	0.495	0.500	1.000	0.000	own calculations.
$\ln \text{Dist}_{ij}$	6.854	0.663	7.139	0.411	Anderson and van Wincoop (2003).
Adjacency _{ij}	0.112	0.316	0.000	0.000	own calculations.
$\ln \text{Migration Stock}_{ij}$	9.722	1.528	9.482	1.525	American Community Survey.
$\times \text{Black Share}_{ij}$	178.111	203.585	169.404	140.489	Population Estimates Program.
$\times \text{Jewish Share}_{ij}$	2.393	5.376	1.702	4.218	The American Jewish Yearbook.
$\times \text{Christian Share}_{ij}$	7876.034	465.705	7961.583	376.909	ARIS 2008 Report.
$\times \text{Other Religion Share}_{ij}$	1.081	0.993	1.039	0.886	ARIS 2008 Report.
$\times \text{No Religion Share}_{ij}$	43.471	20.768	38.235	15.907	ARIS 2008 Report.
$\times \text{Urban Share}_{ij}$	0.490	0.144	0.478	0.134	Census of Population and Housing.
Colonizer _{ij}	0.540	0.499	0.524	0.500	own calculations.
$\Delta \ln 1860 \text{ Cropland}_{ij}$	1.062	0.816	1.025	0.778	Census of Agriculture 1860.
$\Delta \ln 1860 \text{ Farm Size}_{ij}$	0.698	0.525	1.155	0.331	Census of Agriculture 1860.
$\Delta \ln 1860 \text{ Population Density}_{ij}$	1.429	1.120	1.492	1.069	Census of Population and Housing 1860.
$\Delta \ln 1860 \text{ Illiteracy Rates}_{ij}$	6.462	5.127	9.901	4.663	Census of Population and Housing 1860.
$\Delta 1860 \text{ Slave Share}_{ij}$	21.596	20.239	39.700	10.854	Census of Population and Housing 1860.
$\Delta 1860 \text{ Free Black Share}_{ij}$	1.200	1.029	1.215	1.043	Census of Population and Housing 1860.
$\times 1860 \text{ French Share}_{ij}$	0.074	0.167	0.077	0.218	Census of Population and Housing 1860.
$\times 1860 \text{ Spanish Share}_{ij}$	0.000	0.001	0.000	0.000	Census of Population and Housing 1860.
$\times 1860 \text{ Irish Share}_{ij}$	19.925	32.596	6.326	8.991	Census of Population and Housing 1860.
$\times 1860 \text{ German Share}_{ij}$	9.995	20.730	4.226	8.473	Census of Population and Housing 1860.
$\times 1860 \text{ British Share}_{ij}$	7.052	11.245	1.302	1.140	Census of Population and Housing 1860.
$\Delta 1860 \text{ Malaria Risk}_{ij}$	0.151	0.105	0.225	0.089	Hong (2007).
$\Delta \ln \text{Capital-Labor Ratio}_{ij}$	0.281	0.211	0.276	0.208	Turner et al. (2008).
$\Delta \ln \text{High-Low Skilled Ratio}_{ij}$	0.454	0.308	0.565	0.324	Census of Population; American Community Survey.
$\Delta \ln \text{Average Schooling}_{ij}$	0.036	0.025	0.045	0.027	Turner et al. (2007).
$\Delta \ln \text{Cropland}_{ij}$	1.959	1.619	1.924	1.383	National Resource Inventory Summary Report.
$\Delta \ln \text{Farm Size}_{ij}$	0.580	0.465	0.567	0.467	Census of Agriculture.
$\Delta \ln \text{Agricultural To Total Output}_{ij}$	0.711	0.503	0.709	0.486	Bureau of Economic Analysis.
$\Delta \ln \text{Manufacturing To Total Output}_{ij}$	0.329	0.254	0.338	0.259	Bureau of Economic Analysis.
$\Delta \ln \text{Population}_{ij}$	1.000	0.735	0.953	0.703	Population Estimates Program.
$\Delta \ln \text{Population Density}_{ij}$	1.109	0.814	1.090	0.769	Population Estimates Program.
$\Delta \ln \text{Fertility}_{ij}$	0.081	0.065	0.084	0.070	Vital Statistics of the United States.
$\Delta \ln \text{Income Per Capita}_{ij}$	0.150	0.118	0.159	0.128	Bureau of Economic Analysis; Population Estimates Program.
$\Delta \text{Union Membership}_{ij}$	7.627	5.431	9.939	5.497	Hirsch et al. (2001).
$\Delta \text{Union Density}_{ij}$	7.434	5.267	9.668	5.420	Hirsch et al. (2001).
$\Delta \text{Minimum Wage}_{ij}$	0.087	0.124	0.081	0.105	US Department of Labor.
$\Delta \text{Republican}_{ij}$	0.452	0.498	0.604	0.490	The American Presidency Project.
Judiciary Election _{ij}	0.426	0.495	0.428	0.495	own calculations.

Notes: Data from the Bureau of Economic Analysis stem from the Regional Economic Accounts. Contemporaneous variables if not stated otherwise. The operator Δ denotes the absolute difference of variables between state i and state j . The operator \times denotes the product of variables in state i and state j . $\ln z_{ij}$ has 740 observations for the full sample and 364 for the North-South sample.

Table 1.11: 1993 Standard Transportation Commodity Codes (STCC)

Commodity	Meaning	Agriculture	Mining	Chemical	Machinery	Manufacturing
1	Farm Products	x				
8	Forest Products	x				
9	Fresh Fish or Other Marine Products	x				
10	Metallic Ores		x			
11	Coal		x			
13	Crude Petroleum, Natural Gas, Gasoline		x			
14	Non-metallic Minerals		x			
19	Ordinance or Accessories					
20	Food or Kindred Products	x				
21	Tobacco Products, excluding Insecticides	x				
22	Textile Mill Products					x
23	Apparel or Other Finished Textile Products					x
24	Lumber or Wood Products, excluding Furniture					x
25	Furniture or Fixtures					x
26	Pulp, Paper, Allied Products					x
27	Printed Matter					x
28	Chemicals or Allied Products			x		
29	Petroleum or Coal Products			x		
30	Rubber or Miscellaneous Plastics Products			x		
31	Leather or Leather Products					x
32	Clay, Concrete, Glass, Stone Products					x
33	Primary Metal Products					x
34	Fabricated Metal Products					x
35	Machinery, excluding Electrical				x	
36	Electrical Machinery, Equipment, Supplies				x	
37	Transportation Equipment				x	
38	Instruments, Photographic and Optical Goods				x	
39	Miscellaneous Products of Manufacturing					x
40	Waste or Scrap Materials					
41	Miscellaneous Freight Shipments					
99	LTL-General Cargo					

Table 1.12: 1997, 2002, 2007 Standard Classification of Transported Goods (SCTG)

Commodity	Meaning	Agriculture	Mining	Chemical	Machinery	Manufacturing
1	Live animals and live fish	x				
2	Cereal grains	x				
3	Other agricultural products	x				
4	Animal feed and products of animal origin, n.e.c.	x				
5	Meat, fish, seafood, and preparations	x				
6	Milled grain products, bakery products	x				
7	Other prepared foodstuffs, fats, oils	x				
8	Alcoholic beverages	x				
9	Tobacco products	x				
10	Monumental or building stone		x			
11	Natural sands		x			
12	Gravel and crushed stone		x			
13	Nonmetallic minerals n.e.c.		x			
14	Metallic ores and concentrates		x			
15	Coal		x			
17	Gasoline and aviation turbine fuel		x			
18	Fuel oils		x			
19	Coal and petroleum products, n.e.c.		x			
20	Basic chemicals			x		
21	Pharmaceutical products			x		
22	Fertilizers			x		
23	Chemical products and preparations, n.e.c.			x		
24	Plastics and rubber			x		
25	Logs and other wood in the rough	x				
26	Wood products					x
27	Pulp, newsprint, paper, and paperboard					x
28	Paper or paperboard articles					x
29	Printed products					x
30	Textiles, leather, articles of textiles or leather					x
31	Nonmetallic mineral products					x
32	Base metal in primary or semifinished forms					x
33	Articles of base metal					x
34	Machinery				x	
35	Electronic and office equipment and components				x	
36	Motorized and other vehicles (including parts)				x	
37	Transportation equipment, n.e.c.				x	
38	Precision instruments and apparatus				x	
39	Furniture, mattresses and supports, lamps					x
40	Miscellaneous manufactured products					x
41	Waste and scrap					
43	Mixed freight					

Table 1.13: Alternative Methods: AvW and OLS with MR Terms

Dependent Variable: ln bilateral exports between i and j relative to states' GDPs				
Year of Data	1993 (N = 740)	1997 (N = 738)	2002 (N = 711)	2007 (N = 740)
Specification	PANEL A: AVW NLS			
	(A1)	(A2)	(A3)	(A4)
Border Dummy $_{ij}$	-0.218*** (0.04)	-0.128*** (0.04)	-0.175*** (0.04)	-0.175*** (0.04)
ln Distance $_{ij}$	-0.979*** (0.03)	-0.978*** (0.03)	-1.071*** (0.03)	-1.087*** (0.03)
Specification	PANEL B: OLS WITH MR TERMS			
	(B1)	(B2)	(B3)	(B4)
Border Dummy $_{ij}$	-0.128*** (0.04)	-0.097** (0.04)	-0.113** (0.05)	-0.117*** (0.04)
ln Distance $_{ij}$	-0.813*** (0.04)	-0.780*** (0.04)	-0.835*** (0.05)	-0.885*** (0.04)
Adjacency $_{ij}$	0.580*** (0.08)	0.553*** (0.08)	0.580*** (0.09)	0.565*** (0.09)
Multilateral Resistance	YES	YES	YES	YES
Adjusted R^2	0.673	0.645	0.627	0.664

Notes: Constant and multilateral resistance (MR) terms not reported. Robust standard errors reported in parenthesis. AvW NLS denotes the Anderson and van Wincoop (2003) Nonlinear Least Squares Method. MR terms are derived from Baier and Bergstrand (2009) as $MRDist_{ij} = \left[\left(\sum_{k=1}^N \theta_k \ln Dist_{ik} \right) + \left(\sum_{m=1}^N \theta_m \ln Dist_{mj} \right) - \left(\sum_{k=1}^N \sum_{m=1}^N \theta_k \theta_m \ln Dist_{km} \right) \right]$, $MRAdj_{ij} = \left[\left(\sum_{k=1}^N \theta_k \ln Adj_{ik} \right) + \left(\sum_{m=1}^N \theta_m \ln Adj_{mj} \right) - \left(\sum_{k=1}^N \sum_{m=1}^N \theta_k \theta_m \ln Adj_{km} \right) \right]$, and $MRBorder_{ij} = \left[\left(\sum_{k=1}^N \theta_k \ln Border_{ik} \right) + \left(\sum_{m=1}^N \theta_m \ln Border_{mj} \right) - \left(\sum_{k=1}^N \sum_{m=1}^N \theta_k \theta_m \ln Border_{km} \right) \right]$. θ denotes a states' share of GDP over "total" GDP, Y_k/Y^T and Y_m/Y^T . States in sample as in Table 1.1. District of Columbia is excluded. Significance levels as in Table 1.2.

Table 1.14: Placebo Coast-Interior and East-West, 1993

Dependent Variable: ln bilateral exports between i and j relative to states' GDPs				
Specification	Coast-Interior		East-West	
	OLS FE (1)	PPML FE (2)	OLS FE (3)	PPML FE (4)
Border Dummy $_{ij}$	-0.005 (0.03)	0.040 (0.03)	0.069 (0.04)	0.091 (0.06)
ln Distance $_{ij}$	-0.450*** (0.05)	-0.296*** (0.06)	-0.464*** (0.05)	-0.307*** (0.05)
Adjacency $_{ij}$	0.407*** (0.06)	0.310*** (0.05)	0.400*** (0.06)	0.311*** (0.05)
Fixed Effects				
Importer	YES	YES	YES	YES
Exporter	YES	YES	YES	YES
Additional Controls	YES	YES	YES	YES
Observations	2,089	2,256	2,089	2,256
Adjusted/Pseudo R^2	0.804	0.037	0.804	0.037

Notes: Constant, fixed effects, and controls not reported. Robust standard errors reported in parenthesis. All models include variables as of column (6), Table 1.5 available for all US states as additional controls. Coast: Connecticut, California, Delaware, Florida, Georgia, Maine, Massachusetts, Maryland, New Hampshire, New Jersey, New York, North Carolina, Oregon, Pennsylvania, Rhode Island, South Carolina, Virginia, Washington. Interior: Alabama, Arizona, Arkansas, Colorado, Idaho, Illinois, Indiana, Iowa, Kansas, Kentucky, Louisiana, Michigan, Minnesota, Mississippi, Missouri, Montana, Nebraska, Nevada, New Mexico, North Dakota, Ohio, Oklahoma, South Dakota, Tennessee, Texas, Utah, Vermont, West Virginia, Wisconsin, Wyoming. West: Arizona, Arkansas, California, Colorado, Idaho, Iowa, Kansas, Louisiana, Minnesota, Missouri, Montana, Nebraska, Nevada, New Mexico, North Dakota, Oklahoma, Oregon, South Dakota, Texas, Utah, Washington, Wyoming. East: Alabama, Connecticut, Delaware, Florida, Georgia, Illinois, Indiana, Iowa, Kentucky, Maine, Maryland, Massachusetts, Michigan, Minnesota, Mississippi, Missouri, New Hampshire, New Jersey, New York, North Carolina, Ohio, Pennsylvania, Rhode Island, South Carolina, Tennessee, Vermont, Virginia, West Virginia, Wisconsin. District of Columbia, Hawaii and Alaska excluded. Significance levels as in Table 1.2.

Table 1.15: Robustness: In-Sample Eastern-Western States

Dependent Variable: ln bilateral exports between i and j relative to states' GDPs

Specification:	OLS FE (1)	PPML FE (2)	OLS FE (3)	PPML FE (4)	OLS FE (5)	PPML FE (6)
Eastern-Western Border Dummy $_{ij}$	-0.006 (0.03)	-0.003 (0.03)	-0.012 (0.04)	0.001 (0.04)	-0.001 (0.03)	0.006 (0.03)
ln Distance $_{ij}$	-0.950*** (0.03)	-0.985*** (0.03)	-0.661*** (0.05)	-0.580*** (0.05)	-0.699*** (0.06)	-0.633*** (0.06)
Adjacency $_{ij}$	0.475*** (0.06)	0.467*** (0.05)	0.416*** (0.05)	0.357*** (0.05)	0.360*** (0.06)	0.313*** (0.05)
Additional Controls	-	-	YES	YES	YES	YES
Additional Historical Controls	-	-	-	-	YES	YES
Observations	740	756	740	756	740	756
Adjusted/Pseudo R^2	0.836	0.027	0.867	0.028	0.872	0.028

Notes: Constant, fixed effects and controls not reported. Robust standard errors reported in parenthesis. Columns (3) – (6) include variables as of column (6), Table 1.5 available for all US states as additional controls. Columns (5) and (6) additionally include all historical controls as of column (7), Table 1.7. Eastern: Connecticut, Florida, Georgia, Indiana, Maine, Massachusetts, Michigan, New Hampshire, New Jersey, New York, North Carolina, Ohio, Pennsylvania, Rhode Island, South Carolina, Vermont, Virginia. Western: Alabama, Arkansas, Illinois, Iowa, Kansas, Louisiana, Minnesota, Mississippi, Tennessee, Texas, Wisconsin. Significant levels as in Table 1.2.

Table 1.16: Robustness: Subsamples

Dependent Variable: ln bilateral exports between i and j relative to states' GDPs

Specification:	OLS FE	PPML FE	OLS FE	PPML FE	OLS FE	PPML FE
PANEL A: SUBSAMPLE NORTH						
	(A1)	(A2)	(A3)	(A4)	(A5)	(A6)
Northeast-Midwest Border Dummy $_{ij}$	0.013 (0.07)	-0.051 (0.07)	-0.023 (0.11)	-0.101 (0.10)	-0.058 (0.12)	-0.175 (0.11)
ln Distance $_{ij}$	-0.978*** (0.06)	-0.987*** (0.06)	-0.509*** (0.14)	-0.473*** (0.11)	-0.591*** (0.17)	-0.462*** (0.12)
Adjacency $_{ij}$	0.347*** (0.08)	0.375*** (0.07)	0.250*** (0.08)	0.230*** (0.06)	0.166* (0.09)	0.083 (0.07)
Additional Controls	-	-	YES	YES	YES	YES
Additional Historical Controls	-	-	-	-	YES	YES
Observations	266	272	266	272	266	272
Adjusted/Pseudo R^2	0.870	0.027	0.901	0.028	0.900	0.028
PANEL B: SUBSAMPLE SOUTH						
	(B1)	(B2)	(B3)	(B4)	(B5)	(B6)
Southeast-Southwest Border Dummy $_{ij}$	-0.041 (0.09)	0.033 (0.06)	-0.010 (0.19)	-0.084 (0.14)	0.286 (0.58)	0.507 (0.43)
ln Distance $_{ij}$	-0.946*** (0.11)	-0.833*** (0.08)	-0.478*** (0.15)	-0.441*** (0.10)	-0.225 (0.65)	-0.512 (0.43)
Adjacency $_{ij}$	0.363*** (0.11)	0.493*** (0.07)	-0.171 (0.18)	-0.180 (0.13)	0.200 (0.27)	0.006 (0.16)
Additional Controls	-	-	YES	YES	YES	YES
Additional Historical Controls	-	-	-	-	YES	YES
Observations	110	110	110	110	110	110
Adjusted/Pseudo R^2	0.895	0.021	0.917	0.022	0.931	0.022

Notes: Constant, fixed effects and controls not reported. Robust standard errors reported in parenthesis. Columns (A3) – (A6) and (B3) – (B6) include variables as of column (6), Table 1.5 available for all US states as additional controls. Columns (A5), (A6), (B5) and (B6) additionally include all historical controls as of column (7), Table 1.7. Northeast: Connecticut, Maine, Massachusetts, New Hampshire, New Jersey, New York, Pennsylvania, Rhode Island, Vermont. Midwest: Illinois, Indiana, Iowa, Kansas, Michigan, Minnesota, Ohio, Wisconsin. Southeast: Alabama, Florida, Georgia, North Carolina, South Carolina, Tennessee, Virginia. Southwest: Arkansas, Louisiana, Mississippi, Texas. Significant levels as in Table 1.2.

Table 1.17: Alternative Distance Measure (fixed-effects estimation)

Dependent Variable: ln bilateral exports between i and j relative to states' GDPs

PANEL A: GOOGLE TRAVEL TIME								
Year of Data	1993		1997		2002		2007	
	(A1)	(A2)	(A3)	(A4)	(A5)	(A6)	(A7)	(A8)
Border Dummy $_{ij}$	-0.105*** (0.03)		-0.039 (0.03)		-0.084** (0.03)		-0.073** (0.03)	
North-North Dummy $_{ij}$		-0.201** (0.09)		-0.061 (0.10)		-0.304 (0.20)		0.098 (0.10)
South-South Dummy $_{ij}$		0.411*** (0.10)		0.138 (0.10)		0.473** (0.21)		0.049 (0.11)
ln Travel Distance $_{ij}$	-0.974*** (0.03)	-0.974*** (0.03)	-0.964*** (0.03)	-0.964*** (0.03)	-1.049*** (0.04)	-1.049*** (0.04)	-1.081*** (0.04)	-1.081*** (0.04)
Adjacency $_{ij}$	0.413*** (0.05)	0.413*** (0.05)	0.395*** (0.06)	0.395*** (0.06)	0.355*** (0.06)	0.355*** (0.06)	0.374*** (0.06)	0.374*** (0.06)
Observations	740	740	738	738	711	711	740	740
Adjusted R^2	0.842	0.842	0.822	0.822	0.819	0.819	0.850	0.850

PANEL B: DISTANCE INTERVALS AS IN EATON AND KORTUM (2002)								
Year of Data	1993		1997		2002		2007	
	(B1)	(B2)	(B3)	(B4)	(B5)	(B6)	(B7)	(B8)
Border Dummy $_{ij}$	-0.169*** (0.03)		-0.102*** (0.03)		-0.146*** (0.04)		-0.154*** (0.03)	
North-North Dummy $_{ij}$		-0.206*** (0.11)		-0.077 (0.11)		-0.343 (0.22)		0.106 (0.12)
South-South Dummy $_{ij}$		0.545*** (0.12)		0.280** (0.12)		0.636*** (0.23)		0.202 (0.13)
Distance $_{ij}$ [250,500)	-0.591*** (0.10)	-0.591*** (0.10)	-0.549*** (0.11)	-0.549*** (0.11)	-0.686*** (0.12)	-0.686*** (0.12)	-0.766*** (0.12)	-0.766*** (0.12)
Distance $_{ij}$ [500,1000)	-1.145*** (0.10)	-1.145*** (0.10)	-1.159*** (0.11)	-1.159*** (0.11)	-1.294*** (0.12)	-1.294*** (0.12)	-1.429*** (0.12)	-1.429*** (0.12)
Distance $_{ij}$ [1000,2000)	-1.683*** (0.10)	-1.683*** (0.10)	-1.680*** (0.11)	-1.680*** (0.11)	-1.857*** (0.11)	-1.857*** (0.11)	-1.976*** (0.11)	-1.976*** (0.11)
Distance $_{ij}$ [2000,max)	-2.253*** (0.12)	-2.253*** (0.12)	-2.277*** (0.13)	-2.277*** (0.13)	-2.585*** (0.13)	-2.585*** (0.13)	-2.562*** (0.13)	-2.562*** (0.13)
Adjacency $_{ij}$	0.610*** (0.07)	0.610*** (0.07)	0.562*** (0.07)	0.562*** (0.07)	0.549*** (0.07)	0.549*** (0.07)	0.552*** (0.08)	0.552*** (0.08)
Observations	740	740	738	738	711	711	740	740
Adjusted R^2	0.801	0.801	0.787	0.787	0.791	0.791	0.806	0.806

Notes: Importer and exporter fixed effects included in all regressions. Constant and fixed effects not reported. Robust standard errors reported in parenthesis. Panel A uses true travel distance between states – obtained from Google – used as distance measure. Panel B uses distance intervals as in Eaton and Kortum (2002) as non-linearized distance measures. Distance $_{ij}$ [0,250) is the reference category. States in sample as in Table 1.1. District of Columbia is excluded. Significance levels as in Table 1.2.

Table 1.18: Sensitivity Analysis: Allocation of Border States, 1993Dependent Variable: ln bilateral exports between i and j relative to states' GDPs

Specification	Border States in South				Border States in North			
	OLS FE		PPML FE		OLS FE		PPML FE	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Border Dummy $_{ij}$	-0.093*** (0.02)		-0.083*** (0.03)		-0.106*** (0.03)		-0.104*** (0.03)	
South-South Dummy $_{ij}$		0.474*** (0.09)		0.033 (0.11)		0.213*** (0.05)		0.207*** (0.06)
North-North Dummy $_{ij}$		0.186*** (0.05)		0.167*** (0.06)		-0.306*** (0.09)		0.142 (0.11)
ln Distance $_{ij}$	-0.910*** (0.03)	-0.910*** (0.03)	-0.931*** (0.03)	-0.931*** (0.03)	-0.911*** (0.03)	-0.911*** (0.03)	-0.932*** (0.03)	-0.932*** (0.03)
Adjacency $_{ij}$	0.464*** (0.05)	0.464*** (0.05)	0.486*** (0.05)	0.486*** (0.05)	0.453*** (0.05)	0.453*** (0.05)	0.468*** (0.05)	0.468*** (0.05)
Fixed Effects								
Importer	YES	YES	YES	YES	YES	YES	YES	YES
Exporter	YES	YES	YES	YES	YES	YES	YES	YES
Observations	1,024	1,024	1,056	1,056	1,024	1,024	1,056	1,056
Adjusted R^2	0.808	0.808	0.03	0.03	0.808	0.808	0.03	0.03

Notes: Constant and fixed effects not reported. Robust standard errors reported in parenthesis. Column (1) to (3) allocate border states (Delaware, Kentucky, Maryland, Missouri, West Virginia) to South as listed in Table 1.1. North as in Table 1.1. Column (5) to (8) allocate border states (Delaware, Kentucky, Maryland, Missouri, West Virginia) to North as listed in Table 1.1. South as in Table 1.1. District of Columbia excluded. Significance levels as in Table 1.2.

Table 1.19: Additionally Including California, Oregon and Nevada, 1993

Dependent Variable: ln bilateral exports between i and j relative to states' GDPs

PANEL A: BASIC SPECIFICATION

Specification:	OLS FE		PPML FE	
	(A1)	(A2)	(A3)	(A4)
Border Dummy $_{ij}$	-0.187*** (0.03)		-0.200*** (0.03)	
North-North Dummy $_{ij}$		0.226* (0.12)		0.111 (0.09)
South-South Dummy $_{ij}$		0.148 (0.13)		0.290*** (0.09)
ln Distance $_{ij}$	-0.838*** (0.04)	-0.838*** (0.04)	-0.872*** (0.03)	-0.872*** (0.03)
Adjacency $_{ij}$	0.451*** (0.06)	0.451*** (0.06)	0.452*** (0.05)	0.452*** (0.05)
Observations	893	893	930	930
Adjusted/Pseudo R^2	0.824	0.824	0.029	0.029

PANEL B: CONTEMPORANEOUS AND HISTORICAL CONTROLS

Specification:	OLS FE	PPML FE	OLS FE	PPML FE
	(B1)	(B2)	(B3)	(B4)
Border Dummy $_{ij}$	-0.127*** (0.04)	-0.142*** (0.04)	-0.258** (0.10)	-0.318*** (0.08)
ln Distance $_{ij}$	-0.555*** (0.05)	-0.488*** (0.04)	-0.650*** (0.06)	-0.614*** (0.05)
Adjacency $_{ij}$	0.373*** (0.06)	0.317*** (0.05)	0.378*** (0.05)	0.332*** (0.05)
Additional Controls	YES	YES	YES	YES
Additional Historical Controls	-	-	YES	YES
Observations	893	930	842	870
Adjusted/Pseudo R^2	0.851	0.030	0.861	0.029

Notes: Constant, fixed effects, and controls not reported. Robust standard errors reported in parenthesis. All models in Panel B include variables as of column (6), Table 1.5 available for all US states as additional controls. Column (B3) and (B4) additionally include all historical controls as of column (7), Table 1.7. California, Oregon and Nevada are included in the North as they officially were part of the Union. Otherwise, North and South include states as in Table 1.1. Significant levels as in Table 1.2.

Table 1.20: Sectoral Regressions Including Controls (fixed-effects estimation)

Dependent Variable: ln bilateral exports between i and j relative to states' GDPs					
Sector	Agriculture	Mining	Chemical	Machinery	Manufacturing
Year of Data	1993				
	(A1)	(A2)	(A3)	(A4)	(A5)
Border Dummy $_{ij}$	-0.252** (0.12)	-0.107 (0.40)	-0.209* (0.12)	-0.155 (0.11)	-0.175** (0.09)
ln Distance $_{ij}$	-0.724*** (0.13)	-0.727** (0.33)	-0.358*** (0.13)	-0.341*** (0.13)	-0.270*** (0.09)
Adjacency $_{ij}$	0.567*** (0.13)	1.170*** (0.33)	0.351** (0.15)	-0.019 (0.14)	0.229** (0.10)
Additional Controls	YES	YES	YES	YES	YES
Observations	4,585	1,156	2,940	4,140	11,484
Adjusted R^2	0.688	0.676	0.591	0.618	0.730
Year of Data	1997				
	(B1)	(B2)	(B3)	(B4)	(B5)
Border Dummy $_{ij}$	-0.166 (0.13)	-0.374 (0.35)	-0.010 (0.12)	-0.207** (0.11)	-0.143** (0.07)
ln Distance $_{ij}$	-0.846*** (0.14)	-0.750** (0.33)	-0.646*** (0.14)	-0.539*** (0.15)	-0.567*** (0.07)
Adjacency $_{ij}$	0.486*** (0.13)	0.972*** (0.27)	0.217* (0.13)	0.071 (0.13)	0.278*** (0.08)
Additional Controls	YES	YES	YES	YES	YES
Observations	5,210	2,403	3,075	3,315	7,340
Adjusted R^2	0.761	0.705	0.738	0.721	0.788
Year of Data	2002				
	(C1)	(C2)	(C3)	(C4)	(C5)
Border Dummy $_{ij}$	-0.109 (0.18)	-0.128 (0.91)	0.092 (0.16)	0.089 (0.13)	-0.068 (0.11)
ln Distance $_{ij}$	-0.700*** (0.16)	0.270 (0.51)	-0.784*** (0.16)	-0.586*** (0.14)	-0.713*** (0.10)
Adjacency $_{ij}$	0.466*** (0.17)	1.227** (0.58)	0.613*** (0.14)	0.140 (0.14)	0.173* (0.10)
Additional Controls	YES	YES	YES	YES	YES
Observations	4,190	1,377	2,680	3,065	6,800
Adjusted R^2	0.719	0.706	0.693	0.640	0.761
Year of Data	2007				
	(D1)	(D2)	(D3)	(D4)	(D5)
Border Dummy $_{ij}$	-0.026 (0.12)	0.226 (0.33)	0.018 (0.13)	-0.055 (0.13)	-0.265*** (0.09)
ln Distance $_{ij}$	-0.928*** (0.13)	-1.523*** (0.31)	-0.831*** (0.13)	-0.544*** (0.13)	-0.623*** (0.09)
Adjacency $_{ij}$	0.324*** (0.12)	0.832** (0.33)	0.146 (0.13)	0.311** (0.13)	0.186** (0.09)
Additional Controls	YES	YES	YES	YES	YES
Observations	3,910	1,679	2,976	3,332	7,156
Adjusted R^2	0.783	0.700	0.737	0.656	0.808

Notes: Importer and exporter fixed effects included in all regressions. Constant, controls and fixed effects not reported. Robust standard errors reported in parenthesis. All models include variables as of column (6), Table 1.5 as additional controls. Commodities pooled into sectors as listed in Table 1.11 and 1.12 in Appendix 1.A. States in sample as in Table 1.1. District of Columbia is excluded. Significance levels as in Table 1.2.

Table 1.21: Additionally Including the West: Sensitivity

Dependent Variable: ln bilateral exports between i and j relative to states' GDPs

Data	Aggregated				Commodity
	OLS FE		PPML FE		Chen (2004) FE
Year of Data	1997				
	(A1)	(A2)	(A3)	(A4)	(A5)
Border Dummy $_{ij}$	-0.077*		-0.083		-0.145***
	(0.04)		(0.05)		(0.02)
South-South Dummy $_{ij}$		0.097		0.148	
		(0.08)		(0.10)	
North-North Dummy $_{ij}$		-0.839		0.133	
		(1.18)		(0.31)	
West-West Dummy $_{ij}$		0.001		-0.148	
		(0.08)		(0.11)	
ln Distance $_{ij}$	-0.372***	-0.379***	-0.324***	-0.343***	-0.521***
	(0.05)	(0.05)	(0.05)	(0.05)	(0.02)
Adjacency $_{ij}$	0.371***	0.379***	0.339***	0.333***	0.397***
	(0.06)	(0.06)	(0.06)	(0.06)	(0.03)
Additional Controls	YES	YES	YES	YES	YES
Observations	1,656	1,656	1,806	1,806	16,806
Adjusted/Pseudo R^2	0.789	0.789	0.039	0.039	0.783
Year of Data	2002				
	(B1)	(B2)	(B3)	(B4)	(B5)
Border Dummy $_{ij}$	-0.113**		-0.130**		-0.180***
	(0.05)		(0.07)		(0.03)
South-South Dummy $_{ij}$		0.095		0.059	
		(0.09)		(0.12)	
North-North Dummy $_{ij}$		0.110*		0.153	
		(0.06)		(0.09)	
West-West Dummy $_{ij}$		-0.141		-0.196	
		(0.09)		(0.14)	
ln Distance $_{ij}$	-0.458***	-0.464***	-0.340***	-0.346***	-0.478***
	(0.05)	(0.05)	(0.06)	(0.06)	(0.03)
Adjacency $_{ij}$	0.424***	0.424***	0.338***	0.338***	0.377***
	(0.07)	(0.07)	(0.07)	(0.07)	(0.04)
Additional Controls	YES	YES	YES	YES	YES
Observations	1,606	1,606	1,806	1,806	10,890
Adjusted/Pseudo R^2	0.801	0.801	0.042	0.042	0.768
Year of Data	2007				
	(C1)	(C2)	(C3)	(C4)	(C5)
Border Dummy $_{ij}$	-0.008		-0.041		-0.131***
	(0.04)		(0.07)		(0.03)
South-South Dummy $_{ij}$		0.058		0.057	
		(0.08)		(0.12)	
North-North Dummy $_{ij}$		-0.005		0.015	
		(0.06)		(0.09)	
West-West Dummy $_{ij}$		0.027		-0.068	
		(0.09)		(0.15)	
ln Distance $_{ij}$	-0.515***	-0.508***	-0.336***	-0.343***	-0.592***
	(0.05)	(0.05)	(0.07)	(0.07)	(0.02)
Adjacency $_{ij}$	0.399***	0.400***	0.325***	0.324***	0.437***
	(0.06)	(0.06)	(0.07)	(0.07)	(0.03)
Additional Controls	YES	YES	YES	YES	YES
Observations	1,682	1,682	1,806	1,806	20,700
Adjusted R^2	0.812	0.812	0.042	0.042	0.747

Notes: Constant, fixed effects, and controls not reported. Robust standard errors reported in parenthesis. All models include variables as of column (6), Table 1.5 available for all states as additional controls. Significance levels as in Table 1.2.

Table 1.22: Robustness: Alternative Samples Including the South–West

Dependent Variable: \ln bilateral exports between i and j relative to states' GDPs				
Years:	1993	1997	2002	2007
PANEL A: OLS FE				
	(A1)	(A2)	(A3)	(A4)
South–West Border Dummy $_{ij}$	0.111 (0.07)	0.239*** (0.07)	0.297*** (0.08)	0.206*** (0.08)
\ln Distance $_{ij}$	-0.419*** (0.10)	-0.381*** (0.09)	-0.438*** (0.11)	-0.539*** (0.09)
Adjacency $_{ij}$	0.327*** (0.11)	0.262*** (0.09)	0.361*** (0.11)	0.340*** (0.09)
Additional Controls	YES	YES	YES	YES
Observations	609	589	583	599
Adjusted R^2	0.830	0.806	0.814	0.825
PANEL B: PPML FE				
	(B1)	(B2)	(B3)	(B4)
South–West Border Dummy $_{ij}$	0.004 (0.09)	0.219** (0.10)	0.277*** (0.09)	0.240** (0.11)
\ln Distance $_{ij}$	-0.310*** (0.08)	-0.368*** (0.07)	-0.327*** (0.10)	-0.288*** (0.10)
Adjacency $_{ij}$	0.207** (0.10)	0.248*** (0.09)	0.366*** (0.12)	0.283*** (0.11)
Additional Controls	YES	YES	YES	YES
Observations	650	650	650	650
Pseudo R^2	0.046	0.045	0.047	0.049
PANEL C: POOLED COMMODITY FE (Chen, 2004)				
	(C1)	(C2)	(C3)	(C4)
South–West Border Dummy $_{ij}$	0.115** (0.05)	0.176*** (0.05)	0.118* (0.07)	0.129*** (0.05)
\ln Distance $_{ij}$	-0.251*** (0.07)	-0.437*** (0.05)	-0.428*** (0.07)	-0.641*** (0.05)
Adjacency $_{ij}$	0.341*** (0.06)	0.230*** (0.05)	0.270*** (0.08)	0.218*** (0.05)
Additional Controls	YES	YES	YES	YES
Observations	7,638	5,556	3,566	7,074
Adjusted R^2	0.601	0.791	0.788	0.765

Notes: Constant, fixed effects, and controls not reported. Robust standard errors reported in parenthesis. All models include variables as of column (6), Table 1.5 available for all US states as additional controls. West: Arizona, California, Colorado, Idaho, Montana, Nebraska, Nevada, New Mexico, North Dakota, Oklahoma, Oregon, South Dakota, Utah, Washington, Wyoming. South as in Table 1.1. Alaska, Hawaii, and District of Columbia excluded. Significance levels as in Table 1.2.

Table 1.23: Robustness: Alternative Samples Including the North–West

Dependent Variable: ln bilateral exports between i and j relative to states' GDPs				
Years:	1993	1997	2002	2007
PANEL A: OLS FE				
	(A1)	(A2)	(A3)	(A4)
North–West Border Dummy $_{ij}$	0.034 (0.05)	-0.007 (0.05)	-0.010 (0.05)	-0.055 (0.05)
ln Distance $_{ij}$	-0.160** (0.07)	-0.178*** (0.06)	-0.254*** (0.07)	-0.278*** (0.06)
Adjacency $_{ij}$	0.278*** (0.09)	0.217*** (0.08)	0.256*** (0.09)	0.175** (0.08)
Additional Controls	YES	YES	YES	YES
Observations	920	883	875	912
Adjusted R^2	0.817	0.815	0.814	0.824
PANEL B: PPML FE				
	(B1)	(B2)	(B3)	(B4)
North–West Border Dummy $_{ij}$	0.066 (0.06)	0.107 (0.07)	0.024 (0.08)	0.020 (0.07)
ln Distance $_{ij}$	-0.128** (0.06)	-0.250*** (0.06)	-0.195*** (0.07)	-0.218*** (0.08)
Adjacency $_{ij}$	0.206** (0.08)	0.179** (0.07)	0.263*** (0.09)	0.150** (0.08)
Additional Controls	YES	YES	YES	YES
Observations	992	992	992	992
Pseudo R^2	0.045	0.046	0.047	0.046
PANEL C: POOLED COMMODITY FE (Chen, 2004)				
	(C1)	(C)	(C3)	(C4)
North–West Border Dummy $_{ij}$	0.095*** (0.04)	-0.036 (0.03)	-0.069 (0.05)	-0.011 (0.03)
ln Distance $_{ij}$	-0.208*** (0.05)	-0.393*** (0.04)	-0.306*** (0.05)	-0.435*** (0.04)
Adjacency $_{ij}$	0.318*** (0.05)	0.298*** (0.04)	0.258*** (0.06)	0.350*** (0.04)
Additional Controls	YES	YES	YES	YES
Observations	12,617	8,274	5,419	10,580
Adjusted R^2	0.532	0.795	0.780	0.749

Notes: Constant, fixed effects, and controls not reported. Robust standard errors reported in parenthesis. All models include variables as of column (6), Table 1.5 available for all US states as additional controls. West: Arizona, California, Colorado, Idaho, Montana, Nebraska, Nevada, New Mexico, North Dakota, Oklahoma, Oregon, South Dakota, Utah, Washington, Wyoming. North as in Table 1.1. Alaska, Hawaii, and District of Columbia excluded. Significance levels as in Table 1.2.

Chapter 2

The Impact of Sanitary and Phytosanitary Measures on Market Entry and Trade Flows*

2.1 Introduction

In the light of decreasing tariffs, quotas and prohibitions due to multilateral and bilateral agreements over the last decades, non-tariff measures (NTMs), such as sanitary and phytosanitary (SPS) measures¹, are on the rise. Countries seek alternatives to protect what was previously carried out by classical trade policy instruments (Roberts et al., 1999). SPS measures pose methods partly regulated under the SPS Agreement of the World Trade Organization (WTO), but their design and use are less restricted and rather flexible. In principle, SPS measures are meant to provide countries with a possibility to protect the health of animals, humans and plants, but major concerns are regularly expressed that SPS regulations are used as protectionist devices. Due to their design, SPS measures may also be used as instruments to achieve certain policy objectives, such as protecting domestic

* This chapter is based on joint work with Pramila Crivelli. It is based on the article "The Impact of Sanitary and Phytosanitary Measures on Market Entry and Trade Flows", Ifo Working Paper 136, 2012. The chapter is based on research that was conducted for the World Trade Report 2012. The authors worked as consultants for the WTO during the writing of the paper.

¹ This paper focuses on SPS measures, most prevalent in agricultural and food trade.

producers, even though WTO members² are required to restrain from applying measures for any protectionist purposes.

Limited knowledge on the particular trade effects of SPS measures exists. Economic theory does not provide a clear cut prediction on the impact of standards on trade. Instead, theory suggests that the impact of SPS measures on agriculture and food trade may be diverse and need not always be negative. While increased production costs that may arise in order to meet higher SPS standards reduce trade, information on food safety and product quality may lead to increased consumer confidence and trust in foreign products, reduced transaction costs and thus foster trade. Further, trade may also rise due to increased producer efficiency, as quality signals help to promote the competitiveness of foreign producers who meet stringent standards. This suggests that the implied trade effect of standards depends on the relative costs of domestic to foreign production and the willingness of consumers to pay a higher price for safer products (WTO, 2012). To achieve a certain health safety objective, policy makers can choose from a range of different SPS measures. These measures entail diverse effects on trade as some affect fixed costs and thus market entry, while others affect post-entry activities, hence, variable trade costs. Assessing the effects of SPS measures on the intensive and extensive margins of trade is thus an empirical issue.

Recent empirical research on the nexus between NTMs and trade has mostly been focusing on the forgone trade via the gravity equation. They provide evidence that NTMs hamper trade (Gebrehiwet et al., 2007; Disdier et al., 2008; Anders and Caswell, 2009), while harmonization of regulation fosters trade (De Frahan and Vancauteran, 2006). But, when looking at various sectors, Fontagné et al. (2005) and Disdier et al. (2008) find positive and negative NTM effects. These approaches focus on aggregate NTMs rather than on the trade effect of diverse regulations that equivalently reduce risk with respect to health safety. Evidence on product-specific regulations, such as maximum residue levels, suggests that such measures hamper trade (Otsuki et al., 2001a,b; Wilson and Otsuki, 2004; Disdier and Marette, 2010; Jayasinghe et al., 2010; Drogue and DeMaria, 2012; Xiong and Beghin, 2012).

Three main issues arise within the literature. First, most of the previous studies assess the impact of either a global or a specific SPS measure on the volume of trade at the aggregate or sectoral level. But, they rarely provide evidence regarding potential market entry barriers

² All of which are also members of the SPS Agreement.

caused by regulations. To our knowledge, only three studies identifying the impact of SPS measures on the intensive and extensive margins. Using a Heckman selection model, Disdier and Marette (2010) find an insignificant effect of maximum residue levels (MRLs)³ on market entry but a negative significant impact on the import volume of crustaceans. Jayasinghe et al. (2010) show that the probability to trade and the trade volume of US corn seeds are both negatively affected by MRLs. Xiong and Beghin (2012) analyze the effect of EU aflatoxin standards on trade in groundnuts between the EU15 and nine African countries from 1989 to 2006. They find no significant impact of the MRL set by the EU on trade in groundnuts. Contrasting results may arise from sector or country specific factors or from different definitions of SPS measures. While Disdier and Marette (2010) define SPS measures using country specific MRLs, Jayasinghe et al. (2010) use SPS regulations based on EXCERPT (Export Certification Project Demonstration), and Xiong and Beghin (2012) use data from the Food and Agricultural Organization (FAO) on global regulations and from the European Communities on aflatoxin contaminants. Thus, further research is needed in order to provide solid evidence on the impact of SPS measures on both market entry and trade volumes in agricultural and food products.

Second, most studies focus on a specific measure, such as MRLs, and can thus not compare the impact of various SPS instruments on trade, even though policy makers may choose from a range of possible measures to achieve equivalent health safety objectives. Heterogeneity across countries in implementing diverse SPS requirements may cause ambiguous trade outcomes. To our knowledge, the only two studies that deal with the impact of different regulatory measures on trade are Schlueter et al. (2009) and Fassarella et al. (2011). Both studies look specifically at the meat sector. Schlueter et al. (2009) estimate the impact of various types of SPS measures on trade in meat products. The authors estimate a Poisson pseudo maximum likelihood (PPML) gravity model on trade flows of meat on the HS4 digit level. Aggregated over all regulatory instruments, they find a positive effect of SPS on trade flows in meat products. Disaggregated results show diverse effects. In particular, conformity assessment promotes trade in the meat sector. In a similar manner, Fassarella et al. (2011) analyze the effect of SPS and TBT measures on Brazilian exports of poultry meat between 1996 and 2009. Deploying a PPML model, they find an insignificant impact of aggregated measures on Brazilian exports of poultry meat. On the disaggregated level, the

³ MRLs are standards imposed by countries on maximum pesticide levels or toxic compounds in food or agricultural products. Disdier and Marette (2010) use limits on chloramphenicol in crustacean imports.

authors find that conformity assessment-related measures decrease the volume of poultry meat exports from Brazil to its major trade partners, while requirements on quarantine treatment and labeling increase the amount of poultry trade. As results on SPS measures on the aggregated and on the disaggregated level are only available for the meat sector and are ambiguous across studies, even contradict each other, the topic needs more insight and investigation.

Third, previous studies often use notification-based data. Contrasting this, our paper deploys the more sophisticated specific trade concerns database of the WTO. The trade concerns database overcomes limitations of notification-based data⁴ because government incentives to report a concern increase if a SPS measure potentially affects their trade. In addition, the database allows us to consistently differentiate SPS measures and to perform bilateral estimations.

This paper builds on the previous literature but contributes by assessing the impact of SPS measures on the extensive and the intensive margin of trade, not only in a specific industry, but aggregated over all agricultural and food sectors. More specifically, we assess the impact of SPS measures on the probability to enter a destination market and on the trade volume. To control for zero trade flows and a potential sample selection bias, we use a Heckman selection model. The key findings of the study are that concerns over SPS measures pose a negative impact on the probability to export to a concerned market. Although, conditional on market entry, the amount of exports to markets with SPS measures in place tends to be higher. A possible explanation of the positive effect relates to the fact that information provision to the consumer may be relatively stronger than the costs of the producer. By enhancing consumer trust in foreign products, SPS measures increase trade for foreign exporters that manage to overcome the fixed cost of entering a market. We further differentiate the impact of bilateral from multilateral SPS measures by assessing the impact of a SPS concern on the market entry and trade volumes of all potential trade partners of a protected market. Our results suggest that SPS measures deter market entry uniformly across all trading partners, whereas SPS measures positively affect bilateral exports, namely of the country raising the concern. Besides, SPS measures have a negative impact on the trade volumes of other exporters.

In an attempt to identify the channels that lead to our results, we systematically assess the relevance of different SPS measures applied for various safety purposes on trade in

⁴ WTO members have usually no incentives to notify their own SPS measures.

agriculture and food. The analysis distinguishes concerns related to conformity assessment (i.e., certificate requirements, testing, inspection and approval procedures) and concerns related to the characteristics of a product (i.e., requirements on quarantine treatment, pesticide residue levels, labeling or packaging). In particular, we show that conformity assessment-related SPS measures constitute a market entry barrier, as such measures might be particularly burdensome and costly, while SPS measures related to product characteristics explain most of the increase in the amount of trade. The latter suggests that SPS product characteristic measures sufficiently enhance consumer trust such as to foster trade. This contribution is particularly interesting for policy makers as they often have to choose from a range of measures that are assumed to equivalently reduce health risks but entail diverse trade costs. Depending on a policy maker's choice of SPS measures, the implied impact on trade varies strongly. In addition, we show that conformity assessment-related SPS measures constitute a market entry barrier to all potential trade partners, whereas product characteristic measures positively affect the trade volume of the country raising a concern at the SPS committee of WTO.

The remainder of the paper is structured as follows. Section 2.2 provides detailed information on the empirical strategy and describes the data. In section 2.3, we provide benchmark results on the Heckman selection model using the aggregate SPS measure and a sensitivity analysis of results. Section 2.4 distinguishes by type of concern (conformity assessment versus product-related concerns). The last section concludes.

2.2 Empirical Strategy and Data

2.2.1 Empirical Strategy

In an attempt to disentangle the impact of SPS measures on trade in agricultural and food products, we estimate a Heckman selection model (Heckman, 1979) to control for a possible bias in our results from non-random selection or zero trade flows in the data. Controlling for zero trade flows is important as SPS measures implemented in the wake of a disease outbreak might provoke a complete ban in the trade of some products. An alternative way to control for zeros would be to estimate a Poisson model. In contrast to the Heckman model, the Poisson method assumes that there is nothing special about zero trade and would not

allow us to tackle the sample selection issue with respect to reporting. While the Poisson model is able to better control for heterogeneity, it disregards the existence of another data generating process that produces excessive zeros in the trade matrix caused by self-selection into no trade.⁵ Hence, we prefer the Heckman method. Besides, the Heckman model enables us to distinguish the effect of SPS measures on the extensive margin (the probability to trade) and the intensive margin (the amount of trade conditional on market entry). The latter considers zero trade values by potential censoring. We estimate both, the selection and the outcome equations, simultaneously using the maximum likelihood technique.⁶ Both equations include the same independent variables, except for the selection variable, in our case common religion as in Helpman et al. (2008). The selection variable helps to identify the model as it is assumed to have an impact on the fixed costs of trade, but to have a negligible effect on variable trade costs. We estimate a probit binary choice model of the form

$$\begin{aligned} \Pr(M_{ijts} > 0) = & \Phi[\hat{\alpha}_1 SPS_{ij(t-1)s} + \hat{\alpha}_2 \ln(GDP_{it} \times GDP_{jt}) \\ & + \hat{\alpha}_3 \ln(POP_{it} \times POP_{jt}) + \hat{\alpha}_4 \mathbf{X}_{ij} + \hat{\alpha}_5 \mathbf{MR}_{ijts} \\ & + \nu_i + \nu_j + \nu_s + \nu_t + \varepsilon_{ijts}] \end{aligned} \quad (2.1)$$

where $\Phi(\cdot)$ is a standard normal distribution function. And an outcome equation of the form

$$\begin{aligned} \ln(M_{ijts} | M_{ijts} > 0) = & \beta_1 SPS_{ij(t-1)s} + \beta_2 \ln(GDP_{it} \times GDP_{jt}) \\ & + \beta_3 \ln(POP_{it} \times POP_{jt}) + \beta_4 \mathbf{X}_{ij} + \beta_5 \mathbf{MR}_{ijts} \\ & + \beta_\lambda \lambda(\hat{\alpha}) + \nu_i + \nu_j + \nu_s + \nu_t + \epsilon_{ijts} \end{aligned} \quad (2.2)$$

where M_{ijts} denotes the import values of a specific HS4 product s of country j from country i at time t . $SPS_{ij(t-1)s}$ reports a concern over a SPS measure between the reporting country i and the maintaining country j at time $t - 1$ for a specific HS4 product line. $\ln(GDP_{it} \times GDP_{jt})$ depicts the log of the product of GDPs of country i and country j at time t and $\ln(POP_{it} \times POP_{jt})$ denotes the log of the product of country i 's and country j 's total population at time t . These variables proxy for the supply capacities and market capacities

⁵ Alternatively estimating zero inflated Poisson or a negative binomial model is not easy either. Due to strong non-linearity, they are difficult to implement (Greene, 2003).

⁶ Wooldridge (2002, p.566) states that the maximum likelihood method produces more efficient estimates, preferable standard errors, and likelihood ratio statistics compared to the two-step estimation technique.

of the exporting and the importing countries. The vector \mathbf{X}_{ij} contains the usual gravity controls, such as the log of distance, measured as the geographical distance between capitals, adjacency, common language and variables of colonial heritage. The vector \mathbf{MR}_{ijts} contains multilateral resistance terms based on adjacency, distance, common language and variables of colonial heritage, as well as on the SPS concern. We follow Baier and Bergstrand (2009), who derive theory-consistent MR indexes from a Taylor series expansion of the Anderson and Van Wincoop (2003) gravity equation. We adapt their strategy to the panel environment. Hence, all regressions include multilateral resistance terms.⁷ To control for any country-specific characteristics, product specifics and time trends, we include full arrays of importer ν_i , exporter ν_j , HS4 product ν_s , and year dummies ν_t separately in the equation. Hence, we control for a wide array of observable and unobservable factors, i.e., geographical variables or global business cycles.⁸ Error terms ε_{ijts} are heteroskedasticity-robust and clustered at the country-pair level. $\lambda(\hat{\boldsymbol{\alpha}})$ denotes the inverse mills ratio which is predicted from equation (2.1).⁹

The focus of this paper is on SPS concerns reported by exporters to the WTO. For SPS measures, we consider two different variables: (i) a dummy variable equal to one if at least one concern is notified at the 4-digit level of the HS classification, and (ii) a normalized frequency measure $\text{SPSFreq}_{ij(t-1)s}$. The normalized SPS measure is defined as the number of concerns on HS4 products within a HS2 product category and divided by the total number of HS4 product items within the HS2 sector. In a second approach, we dissociate the impact of the measure on the country raising the concern from the impact on all potential exporters. We thus additionally include a *multilateral* variable equal to one if at least one concern regarding a measure maintained by a given importer exists ($\text{SPS}_{j(t-1)s}$), and its associated normalized frequency SPS measure ($\text{SPSFreq}_{j(t-1)s}$). To circumvent potential reverse causality between imports and SPS measures, we use the first lag of the variable on SPS concerns.¹⁰

⁷ A popular alternative way to account for multilateral remoteness would be to include the full array of interaction terms between country and year dummies and combined fixed effects. However, due to the large number of observations this is computationally not possible in our sample. Within transformation is unfortunately not possible with the Heckman specification due to the nonlinearity of the first stage.

⁸ The large number of observations does not allow for the use of combined fixed effects and within transformation is not possible using the Heckman model due to the nonlinearity of the first stage.

⁹ The inverse mills ratio is the ratio of the probability density function over the cumulative distribution function of \hat{M}_{ijts} from equation (2.1).

¹⁰ Using further instrumentation methods is not straightforward in the Heckman model. For robustness reasons, we estimate a probit and a two stage least squares (2SLS) model separately. The two instruments used in the 2SLS model are (i) the sum of SPS concerns of all other partner countries $k \neq i$ against the importer j in sector s and (ii) the sum of SPS concerns raised by country i against the importer j in

2.2.2 Data Sources and Sample

The SPS Information Management System (SPS IMS) of the WTO contains information on specific SPS concerns reported to the WTO by a raising country towards a maintaining country for 1995 to 2010, respectively.¹¹ For each single concern, we have information on the raising and maintaining country, the HS4 product code concerned, the year in which the concern was reported to the WTO, and whether it has been resolved. To measure SPS restrictions, we generate a simple dummy variable on SPS concerns that is equal to one when a concern is reported to the WTO and shifts to zero whenever the concern is resolved. Alternatively, we also calculate a normalized frequency measure, which counts the number of SPS measures in place on HS4 product lines within an HS2 sector and divides them by the number of products within an HS2 sector. Similar 'normalized' frequency measures on various levels of disaggregation have also been used by Fontagné et al. (2005); Disdier et al. (2008); Fontagné et al. (2012). If HS4 product codes are not available, but instead the HS2 sector is listed in the concern, we assume that all HS4 product lines under the HS2 sector are affected. The database reports the HS2002 classification, which are converted to the HS1992 classification to be able to merge them to the trade data.

Further, to consider the possible heterogeneity of various SPS measures, we divide concerns into two categories in accordance to the specific description of concerns contained in the SPS database, referenced documents, or occasionally national documents, if the database and referenced documents were too vague about a certain concern. We create two dummy variables indicating whether a specific concern relates to conformity assessment or product characteristics. Conformity assessment-related measures refer to Annex C of the SPS Agreement and include concerns about certification requirements, testing, inspection and approval procedures. Annex C was understood broadly. Hence, conformity assessment-related measures also include concerns on delays, unrevoked suspensions, or administrative procedure problems. Measures related to product characteristics refer to concerns regarding the requirements on process and production methods, transport, packaging, and labeling that are directly related to food safety, concerns on the requirements of pesticide residue levels and quarantine or cold treatments, as well as concerns over strict bans, regional division, or protected zones. Concerns depicted in the WTO database may relate to one, or both issues

sectors l different from s but within the same HS2 category. Results in Table 2.5 Panel A and Panel B confirm our findings. Hence, forward looking actors seem not to be a problem in our framework.

¹¹ The SPS Information Management System is available under <http://spsims.wto.org>.

at the same time. Out of the 312 trade concerns raised by one or several countries against a specific importing country, 57 percent are associated with conformity assessment-related measures, while 78 percent relate to concerns over product characteristics.

Data on bilateral trade come from the United Nations Commodity Trade Statistics Database (Comtrade) and are obtained in the HS1992 classification. The European Union is considered as a single country, hence, trade data is summed up over all EU member states. Total population and nominal GDP in US dollars provide a proxy for market size. Data stem from the World Bank's World Development Indicator (WDI) database and enter equations through the log of the product of the GDPs of the importer and the exporter and the log of the product of the total population of the importer and the exporter. Bilateral distance is the geographic distance between capitals.¹² Data is extracted from the CEPII database on distance and geographical variables, as are all other gravity variables contained in the equations, such as adjacency, common language, and variables on colonial heritage. Data on common religion across countries is obtained from Elhanan Helpman's homepage. Helpman et al. (2008) define the index of common religion across countries as $(\% \text{ Protestants in country } i \times \% \text{ Protestants in country } j) + (\% \text{ Catholics in country } i \times \% \text{ Catholics in country } j) + (\% \text{ Muslims in country } i \times \% \text{ Muslims in country } j)$.

For robustness checks, we include applied tariff data that are combined from the WTO's Integrated Data Base (IDB) and UNCTAD's Trade Analysis and Information System (TRAINS). As tariff data have little time variation and are missing to a large part, we only include them in a robustness check.¹³ IDB tariff data are preferred over TRAINS if both are available, as IDB contains comprehensive information on applied preferential tariffs *and* provides data on general tariff regimes whenever available. To handle missing observations and to keep as many observations as possible, we adapt an 'interpolation' rule. If a tariff is available for a certain HS4 product in a certain year, we assume that the same tariff was also valid for the HS4 product up to 4 years previous to the tariff reported in the database if these are missing. After the 'interpolation' rule has been adapted, we further assume that all remaining missing observations are zero, to keep the exact similar sample as to when not including tariff data. Following the literature, we use applied tariff data that is weighted by imports.

¹² The distance to and from the EU is measured as the distance to and from Brussels.

¹³ Results on the impact of SPS measures on trade do not change qualitatively nor quantitatively by the inclusion of tariffs.

Our sample consists of 164 importer and 150 exporting countries, and 224 HS4 product categories in 34 HS2 sectors (compare Table 2.11 in Appendix 2.A) observed over a time period of fifteen years, from 1996 to 2010, due to the lag considered in the SPS measure implemented to circumvent reverse causality.

2.3 SPS Measures and Trade

2.3.1 Benchmark Results

The first two columns of Table 2.1 present results using the SPS dummy variable, while columns (3) and (4) use the normalized SPS frequency measure. All regressions include importer, exporter, and HS4 product fixed effects, a full array of year dummies and multi-lateral resistance terms. In addition, all columns include gravity controls. These are the log of the product of GDPs, the log of the product of populations, the log of distance, adjacency, common language and colonial heritage. Common religion is the selection variable and thus excluded in column (2) and (4), respectively. All specifications apply the Heckman selection procedure using the maximum likelihood approach and thus account for potential sample selection and zero trade flows.

Overall, gravity variables are in line with the literature. Countries similar in income trade more with another, while countries similar with respect to population size show a higher probability to trade, but no significant effect on the amount of trade conditional on market entry. As expected, distance has a negative impact on trade, and adjacency, common language and common colonial heritage increase trade, while country pairs in a colonial relation after 1945 experience a negative impact on the probability and the amount of trade. Common religion reduces the fixed costs of trade, hence, positively affects the probability of market entry. This result is in line with the findings of Helpman et al. (2008). As in Helpman et al. (2008), we assume that common religion does not affect trade flows once the exporting decision has been made.

In column (1), we find a significantly lower probability to trade bilaterally in the presence of SPS concerns. Our results suggest that the probability to enter an export market is about 4.3 percent lower in the presence of a SPS measure (compare Table 2.2 column (1) for marginal effects). This indicates that SPS measures increase fix costs of trading and thus

Table 2.1: The Impact of SPS on Agricultural and Food Trade (1996 - 2010)

Equation: Dependent Variable:	Heckman Selection Model (maximum likelihood)			
	Selection	Outcome	Selection	Outcome
	Pr(import _{ijts} > 0)	ln(import _{ijts})	Pr(import _{ijts} > 0)	ln(import _{ijts})
	(1)	(2)	(3)	(4)
SPS _{ij(t-1)s}	-0.139** (0.06)	0.642*** (0.14)		
SPSFreq _{ij(t-1)s}			-0.155** (0.06)	0.625*** (0.15)
Controls				
ln GDP _{it} × GDP _{jt}	0.220*** (0.02)	0.468*** (0.03)	0.219*** (0.02)	0.471*** (0.03)
ln POP _{it} × POP _{jt}	0.248*** (0.05)	0.076 (0.09)	0.250*** (0.05)	0.058 (0.09)
ln Distance _{ij}	-0.329*** (0.01)	-0.946*** (0.03)	-0.329*** (0.01)	-0.946*** (0.03)
Adjacency _{ij}	0.123*** (0.03)	0.392*** (0.10)	0.122*** (0.03)	0.392*** (0.10)
Common Language _{ij}	0.123*** (0.02)	0.265*** (0.05)	0.123*** (0.02)	0.265*** (0.05)
Ever Colony _{ij}	-0.021 (0.05)	0.055 (0.15)	-0.021 (0.05)	0.056 (0.15)
Common Colonizer _{ij}	0.081*** (0.02)	0.268*** (0.07)	0.081*** (0.02)	0.268*** (0.07)
Colonizer post 1945 _{ij}	-0.112*** (0.04)	-0.441*** (0.11)	-0.112*** (0.04)	-0.443*** (0.11)
Common Religion _{ij}	0.150*** (0.02)		0.150*** (0.02)	
Estimated correlation (rho)		0.460*** (0.01)		0.461*** (0.01)
Estimated selection (lambda)		1.369*** (0.04)		1.371*** (0.04)
Observations		5,452,147		5,452,147

Note: ***, **, * denote significance at the 1%, 5%, and 10% level, respectively. Constant, importer, exporter, HS4 product and time fixed effects and MR terms are included but not reported. Common religion is the selection variable and thus excluded in columns (2) and (4). Country clustered robust standard errors reported in parenthesis.

constitute an effective market entry barrier in agricultural and food sectors. Interestingly, the outcome equation in column (2) indicates that SPS measures significantly increase the amount of trade once a market has been entered. This positive effect can be explained by the fact that SPS measures provide information on product safety to consumers. If SPS measures enhance consumer trust in the quality of imported goods proportionally more than they increase variable trade costs due to product adaption, producers gain market share. This leads to an increase in trade volumes for exporters that manage to overcome the fixed cost of entering a market. The dummy variable indicates that SPS measures increase the amount of trade in agriculture and food products by 77 percent on average. The marginal

effect for the outcome equation¹⁴ is depicted in Table 2.2 column (2). Results are confirmed when using the SPS frequency measure in Table 2.1 columns (3) and (4). For both, the frequency and the dummy SPS variable, the estimated correlation coefficient (ρ) and the estimated selection coefficient (λ) are statistically significant and different from zero, confirming that not controlling for selection effects and zero trade flows would generate biased coefficients.

Table 2.2: Marginal Effects from Heckman Selection Model (maximum likelihood)

Equation:	Marginal Effects			
	Selection (1)	Outcome (2)	Selection (3)	Outcome (4)
SPS _{ij(t-1)s}	-0.043** (0.02)	0.775*** (0.00)		
SPSFreq _{ij(t-1)s}			-0.048** (0.02)	0.774*** (0.00)

Note: ***, **, * denote significance at the 1%, 5%, and 10% level, respectively. Marginal Effects of the outcome equations are calculated according to Greene (2003). Country clustered robust standard errors reported in parenthesis.

2.3.2 Bilateral versus Multilateral Effects

The specific trade concerns data help to overcome limitations of notification-based data. First, government incentives to report a concern over a SPS measure increase if an implemented measure potentially affects their trade. Second, specific trade concerns allow to account for the bilateral character of SPS measures. This is particularly important as some SPS measures are really bilateral, i.e. due to a disease outbreak in the exporter country, but, even if measures are multilateral in the sense that they apply to all trade partners, they may eventually affect exporters in different ways. In an attempt to differentiate bilateral from multilateral effects, we estimate a gravity model additionally including a variable equal to one if at least one concern has been raised against the importing country j in sector s . This

¹⁴ The estimated coefficient in the Heckman outcome equation does not indicate the marginal effect of SPS measures on the trade flows as the independent variables appear in the selection and the outcome equation and $\rho \neq 0$. Hence, we calculate the marginal effect of the outcome equation according to Greene (2003, p.784). The marginal effect on the volume of trade is composed of the effect on the selection and the outcome equation. If the outcome coefficient is β and the selection coefficient is α , then

$$dE[y|z^* > 0]/dx = \beta - (\alpha^* \rho^* \sigma^* \delta(\alpha)),$$

where $\delta(\alpha) = \text{inverse Mills' ratio}^*(\text{inverse Mill's ratio}^* \text{selection prediction})$.

variable aims at capturing the impact of *multilateral* SPS measures affecting simultaneously all exporter of a given product. For consistency reasons, we also calculate the associated normalized *multilateral* SPS frequency measure.

Results are reported in Table 2.3. Columns (1) and (3) provide evidence that SPS measures exert a negative impact on the extensive margin of trade for all potential trading partners, including the country raising the concern. Hence, SPS measures constitute a market entry barrier to all exporters and are thus not discriminatory. On the contrary, bilateral SPS measures in columns (2) and (4) indicate that, once exporters meet the stringent standard, the trade flows of countries concerned over a specific SPS measure increase to the detriment of other trade partners (the bilateral coefficient is positive, while the multilateral variable depicts a negative effect). Most importantly, our results suggest that SPS measures diversely affect exporters already active in a market.

Table 2.3: The Impact of Bilateral and Multilateral SPS on Agricultural and Food Trade (1996 - 2010)

Equation: Dependent Variable:	Heckman Selection Model (maximum likelihood)			
	Selection	Outcome	Selection	Outcome
	Pr(import _{ijts} > 0)	ln(import _{ijts})	Pr(import _{ijts} > 0)	ln(import _{ijts})
	(1)	(2)	(3)	(4)
SPS _{ij(t-1)s}	-0.027 (0.05)	0.709*** (0.14)		
SPS _{j(t-1)s} multilateral	-0.173*** (0.01)	-0.103** (0.05)		
SPSFreq _{ij(t-1)s}			-0.024 (0.06)	0.697*** (0.16)
SPSFreq _{j(t-1)s} multilateral			-0.205*** (0.02)	-0.111* (0.06)
Estimated correlation (rho)	0.497*** (0.01)		0.497*** (0.01)	
Estimated selection (lambda)	1.091*** (0.01)		1.091*** (0.01)	
Observations	5,452,147		5,452,147	

Note: ***, **, * denote significance at the 1%, 5%, and 10% level, respectively. Controls, constant, importer, exporter, HS4 product and time fixed effects and MR terms are included but not reported. Common religion is the selection variable and thus excluded in columns (2) and (4). Country clustered robust standard errors reported in parenthesis.

2.3.3 Sensitivity

In the sensitivity analysis, we address two concerns. First, to avoid a potential misspecification and to be able to distinguish the impact of SPS interventions on trade in agricultural and food products from that of bilateral tariffs, we conduct a robustness check that includes

bilateral applied tariff protection as a further control variable. Second, reverse causality might be an issue in our framework. As further instrumentation methods are not straightforward in the Heckman model, we estimate a simple two stage least squares (2SLS) model to give an indication that forward looking actors are not a problem.

Bilateral Tariffs. Table 2.4 includes bilateral applied tariff protection as a further control variable, to avoid a potential misspecification of the model and to be able to distinguish the impact of SPS interventions on trade in agricultural and food products from that of bilateral tariffs.

Table 2.4: Robustness: SPS, Tariffs and Trade (1996 - 2010)

Equation: Dependent Variable:	Heckman Selection Model (maximum likelihood)			
	Selection	Outcome	Selection	Outcome
	Pr(import _{ijts} > 0)	ln(import _{ijts})	Pr(import _{ijts} > 0)	ln(import _{ijts})
PANEL A: BILATERAL SPS (N = 5,452,147)				
	(A1)	(A2)	(A3)	(A4)
SPS _{ij(t-1)s}	-0.134** (0.06)	0.641*** (0.14)		
SPSFreq _{ij(t-1)s}			-0.149** (0.06)	0.623*** (0.15)
Tariff _{ijts} , weighted average	0.001*** (0.00)	-0.001** (0.00)	0.001*** (0.00)	-0.001** (0.00)
Estimated correlation (rho)		0.459*** (0.01)		0.460*** (0.01)
Estimated selection (lambda)		1.366*** (0.04)		1.368*** (0.04)
PANEL B: BILATERAL & MULTILATERAL SPS (N = 5,452,147)				
	(B1)	(B2)	(B3)	(B4)
SPS _{ij(t-1)s}	-0.029 (0.05)	0.710*** (0.14)		
SPS _{ij(t-1)s} multilateral	-0.163*** (0.01)	-0.108** (0.05)		
SPSFreq _{ij(t-1)s}			-0.025 (0.06)	0.698*** (0.16)
SPSFreq _{ij(t-1)s} multilateral			-0.193*** (0.02)	-0.117* (0.06)
Tariff _{ijts} , weighted average	0.001*** (0.00)	-0.001*** (0.00)	0.001*** (0.00)	-0.001** (0.00)
Estimated correlation (rho)		0.459*** (0.01)		0.459*** (0.01)
Estimated selection (lambda)		1.364*** (0.04)		1.367*** (0.04)

Note: ***, **, * denote significance at the 1%, 5%, and 10% level, respectively. Controls, constant, importer, exporter, HS4 product and time fixed effects and MR terms are included but not reported. Common religion is the selection variable and thus excluded in the outcome equations. Country clustered robust standard errors reported in parenthesis.

We include a specific control for bilateral tariffs only in the robustness section for several reasons. First, even though data on bilateral tariffs are provided by IDB and TRAINS, the data pose several limitations with respect to missing values over time. Second, data do not include all specific duties, tariff quotas and anti-dumping duties applied by importers. Third, we cannot distinguish preferential from general tariffs, as data are not always available. In the following, we include import weighted bilateral applied tariffs, with missing values interpolated as discussed in section 2.2.2. We provide evidence that our previous results do not suffer from a bias due to the omission of tariff data in the framework. Table 2.4 provides the results. Coefficients on gravity controls remain qualitatively and quantitatively similar compared to Table 2.1. So do our results on the effect of SPS measures on the extensive and the intensive margin of trade. While SPS measures pose a barrier to market entry, producers who meet the more stringent standard increase their trade flows conditional on market entry (compare Panel A). Results on bilateral versus multilateral effects are robust as well (compare Panel B). SPS measures constitute a market entry barrier against all trade partners, while SPS intervention particularly raises bilateral trade flows conditional on meeting the stringent standard.

Regarding the applied tariffs, we find a slightly positive coefficient on the probability to trade, which suggests only a minor influence of tariffs on market entry fixed costs for agricultural and food trade, respectively. The positive minimal effect is in line with findings by Schlueter et al. (2009) for the meat sector. Further, the outcome equations suggest a minimal negative impact of tariffs on trade flows. This negative impact of tariffs on the trade volume stands in line with findings by Disdier et al. (2008) and Fontagné et al. (2005). Still, our results on the minor impact of tariffs on agricultural and food trade should be read with caution since we apply an interpolation rule, as discussed in section 2.2.2, and tariffs vary very little over time but rather across countries in the time period that we are looking at. Besides, keep in mind that the focus lies on the identification of the impact of SPS on the extensive and the intensive margin of trade. Tariffs are only included as a control variable for robustness reasons. Most importantly, the inclusion of applied tariffs does not alter our results on the impact of SPS measures.

Reverse Causality. A further concern is that reverse causality might be a problem in our estimated framework if actors are forward looking. However, the use of further instrumentation methods is not straightforward in the Heckman model. To give an indication that

forward looking actors are not an issue, we estimate a simple 2SLS model. As instruments for concerns over SPS measures, we use (i) the sum of SPS concerns of all other partner countries $k \neq i$ against the importer j in sector s , and (ii) the sum of SPS concerns raised by exporter i against importer j in sectors $l \neq s$ but $l, s \in \text{HS2}$ sector. The sum of SPS concerns of all other partner countries k against an importer is uncorrelated to bilateral trade between i and j , but is strongly correlated with SPS concerns of the exporter against the importer. Following similar reason, concerns over SPS measures in other HS4 product categories l within the same HS2 sector are unlikely to affect bilateral trade between the importer and the exporter in a specific HS4 product line s , but the sum of concerns related to other products l is strongly correlated to SPS concerns over a specific HS4 product s .

Table 2.5: Robustness: SPS and Trade (1996 - 2010)

Dependent Variable:	import _{ijts} > 0		ln(import _{ijts})		import _{ijts} > 0		ln(import _{ijts})	
	Probit		OLS		Probit		2SLS	
PANEL A: BILATERAL SPS								
	(A1)	(A2)	(A3)	(A4)	(A5)	(A6)		
SPS _{ij(t-1)s}	-0.140*** (0.05)	0.771*** (0.12)	0.598*** (0.14)					
SPSFreq _{ij(t-1)s}				-0.155*** (0.06)	0.763*** (0.13)	0.614*** (0.15)		
Observations	5,452,147	1,960,755	1,960,755	5,452,147	1,960,755	1,960,755		
Adjusted R ²		0.295	0.295		0.295	0.295		
Kleibergen-Paap Wald F stat.			645.57			692.10		
PANEL B: BILATERAL & MULTILATERAL SPS								
	(B1)	(B2)	(B3)	(B4)	(B5)	(B6)		
SPS _{ij(t-1)s}	-0.027 (0.05)	0.752*** (0.12)	0.563*** (0.15)					
SPS _{j(t-1)s} multilateral	-0.173*** (0.01)	0.031 (0.04)	0.054 (0.05)					
SPSFreq _{ij(t-1)s}				-0.024 (0.06)	0.736*** (0.14)	0.573*** (0.15)		
SPSFreq _{j(t-1)s} multilateral				-0.205*** (0.02)	0.046 (0.05)	0.066 (0.05)		
Observations	5,452,147	1,960,755	1,960,755	5,452,147	1,960,755	1,960,755		
Adjusted R ²		0.295	0.295		0.295	0.295		
Kleibergen-Paap Wald F stat.			386.53			416.89		

Note: ***, **, * denote significance at the 1%, 5%, and 10% level, respectively. Controls, constant, importer, exporter, HS4 product and time fixed effects and MR terms included but not reported. Gravity controls included but not reported. Country clustered robust standard errors reported in parenthesis. The instruments are the sum of concerns of all other countries $k \neq i$ against country j and the sum of bilateral SPS concerns in sectors $l \neq s$ with $s, l \in \text{HS2}$.

Table 2.5 Panel A reports the results for the SPS dummy variable and frequency measure, respectively. For comparison reasons, we first show a probit and an ordinary least squares

(OLS) model in columns (A1) to (A2) and (A4) to (A5). Columns (A3) and (A6) then report results for the 2SLS estimation. The probit results confirm our previous findings that SPS measures constitute a market entry barrier to trade. Even though OLS results are potentially biased due to reverse causality, censoring or sample selection, the simple OLS results also support our previous results. Again, we find a positive impact of SPS measures on trade flows. 2SLS results on the impact of SPS measures also confirm our previous findings. Instrumented coefficients are only slightly smaller than the coefficients from the Heckman outcome equation (compare Table 2.1 columns (2) and (4)). Hence, 2SLS results indicate that forward looking actors are not a problem in our setup. Our instruments are not only reasonable but also valid. The Kleibergen-Paap Wald F test on excluded instruments indicates that our F-Statistics range well above the 10% Stock and Yogo (2005) critical values, so that we can firmly reject the weak instrument hypothesis (Kleibergen and Paap, 2006). Since we have two instruments, we can also compute a test of overidentifying restrictions.¹⁵ The test fails to reject (p-value of 0.79) and thus indicates that not all the instruments are coherent.

When we dissociate the impact of bilateral SPS measures from that of multilateral SPS measures on trade, results remain generally in line. Table 2.5 Panel B reports the results. In terms of significance and magnitude, the probit models in columns (B1) and (B4) exhibit similar coefficients as those reported in the selection equations of Table 2.3. The only major change regarding the OLS and 2SLS models in columns (B2) to (B3) and (B5) to (B6), respectively, concerns the loss of significance of the coefficient associated with the multilateral SPS variable. Our results suggest that SPS measures exert a positive and significant effect on the trade flows of the reporting country, but do not affect the trade flows of other partner countries. This implies that the trade enhancing effect of SPS measures is a bilateral matter which could not be handled using notification-based data. In both 2SLS specifications, using either the dummy variable or the frequency index, instruments are valid and feasible with respect to the first stage F-Statistics. The Kleibergen-Paap Wald F Test on the excluded instruments is way above the 10% Stock and Yogo (2005) critical value.

¹⁵ Note that our results are robust when we use a just identified model using either of the two instruments.

2.4 Implementation

2.4.1 Benchmark Results

In the previous section, we point out that SPS measures pose a market entry barrier due to increased fixed costs. In addition, we find a positive effect on trade flows conditional on market entry due to the fact that the increase in market share is proportionally larger than the variable trade costs due to product adaptation. However, governments may choose from a range of SPS instruments to achieve certain policy goals related to animal, plant or human health. The ensuing heterogeneity in SPS intervention may cause ambiguous outcomes on trade, as different SPS instruments entail diverse costs. Measures related to testing, inspection and approval procedures are particularly costly and burdensome for the exporter proportional to the information they provide to the consumer. Such regulations may thus have a negative impact on market entry *and* the amount of trade. Conformity assessment-related measures entail fixed costs that relate to separate or redundant testing or certification of products for various export markets and to the time required to comply with administrative requirements and inspection by importer authorities. The latter may cause time delays that severely impact the profitability of a specific market. Other SPS measures directly related to product characteristics, such as quarantine requirements, pesticide residue levels, labeling or packaging, may pose a barrier to market entry, but once products meet higher standards, exporters gain market share (possibly even in several export markets) due to an increase in consumer trust through valuable product information. Accordingly, we expect that conformity assessment-related measures explain the negative effect on market entry, while concerns related to product characteristics explain the positive impact on trade flows conditional on entering the market.

To systematically compare the implied trade effects of different SPS instruments implemented to achieve a desired level of SPS safety, we distinguish concerns over SPS measure into requirements related to conformity assessment and concerns related to product characteristics. As expected, Table 2.6 column (1) shows that the extensive margin of trade is significantly negatively affected by conformity assessment-related measures (SPS Conformity_{ij(t-1)s}). The probability to trade bilaterally is lower by 8 percent in the presence

Table 2.6: The Impact of SPS on Trade, by Type of Concern (1996 - 2010)

Equation: Dependent Variable:	Heckman Selection Model (maximum likelihood)			
	Selection	Outcome	Selection	Outcome
	Pr(import _{ijts} > 0)	ln(import _{ijts})	Pr(import _{ijts} > 0)	ln(import _{ijts})
	(1)	(2)	(3)	(4)
SPS Conformity _{ij(t-1)s}	-0.258*** (0.07)	-0.402* (0.23)		
SPS Characteristic _{ij(t-1)s}	0.012 (0.07)	0.943*** (0.19)		
SPSFreq Conformity _{ij(t-1)s}			-0.290*** (0.09)	-0.461* (0.27)
SPSFreq Characteristic _{ij(t-1)s}			0.014 (0.07)	0.967*** (0.22)
Estimated correlation (rho)	0.460*** (0.01)		0.461*** (0.01)	
Estimated selection (lambda)	1.368*** (0.04)		1.370*** (0.04)	
Observations	5,452,147		5,452,147	

Note: ***, **, * denote significance at the 1%, 5%, and 10% level, respectively. Controls, constant, importer, exporter, HS4 product and time fixed effects and MR terms are included but not reported. Common religion is the selection variable and thus excluded in columns (2) and (4). Country clustered robust standard errors reported in parenthesis.

of a conformity assessment-related measure.¹⁶ SPS concerns related to product characteristics (SPS Characteristic_{ij(t-1)s}) have no significant impact on market entry. Hence, only conformity assessment-related measures constitute market entry barriers, probably due to the relatively high costs and burdensome procedures they impose on foreign producers.

In column (2), the intensive margin of trade is negatively and significantly affected by conformity assessment-related measures. This may result either from an increase in marginal costs or from a price effect in the case where producers pass through the costs of conformity assessment to consumers, thereby reducing the demand for their product. In contrast, concerns on product characteristics have a positive and significant impact on trade flows conditional on market entry. This suggests that SPS measures related to product characteristics provide information that enhance consumer trust in the quality of imported goods. The gain in market share is then relatively higher than the loss due to product adaption costs. This leads to enhanced trade flows for exporters that manage to overcome the fixed cost of market entry. The dummy measure indicates that conformity assessment-related factors decrease trade in agriculture and food products by 15.6 percent on average, while SPS measures related to product characteristics increase trade flows by 93 percent conditional on

¹⁶ Calculating the marginal effect, we get for SPS Conformity_{ij(t-1)s} a coefficient of -0.080 with a standard error of (0.02).

market entry.¹⁷ Estimates suggest qualitatively similar result when we use the normalized frequency index in Table 2.6 columns (3) and (4). The coefficient on conformity assessment is again negative and significant for the extensive *and* the intensive margin of trade, while the positive and significant impact of SPS concerns related to product characteristics on trade flows prevails.

2.4.2 Bilateral versus Multilateral Effects

In an attempt to dissociate the bilateral from the multilateral character of SPS measures, we again estimate the gravity model by including additional *multilateral* SPS variables. *Multilateral* variables are equal to one for all potential trading partners if at least one respective concern regarding a conformity assessment or a product characteristics measure has been raised against the importer j in sector s . Results are reported in Table 2.7.

In columns (1) and (3), the negative and significant coefficients on the bilateral and multilateral SPS Conformity variables point out that measures related to conformity assessment reduce the probability of bilateral trade between the country raising the concern and the one maintaining the measure, but also impedes market entry for all other exporters. The negative significant coefficient of the multilateral SPS Characteristic variable indicates that such measures hamper market entry as well. Yet, SPS measures related to product characteristics apply to all exporters similarly and are thus not discriminatory, in contrast to SPS measures related to conformity assessment. Most interesting, our results suggest that it is the bilateral component of SPS measures related to product characteristics that trigger a positive and significant effect on trade flows. Hence, depending on the type of regulatory instrument, policy makers may either discriminate against all potential trade partners or even benefit a specific partner that meets the stringent standard.

2.4.3 Sensitivity

We apply the same battery of robustness checks to the disaggregated SPS regulatory instruments than in section 2.3. Results remain generally in line.

¹⁷ Calculating marginal effects of the outcome equation according to Greene (2003), we get a coefficient of -0.156 with a standard error of (0.00) for SPS Conformity $_{ij(t-1)s}$ and a coefficient of 0.931 with a standard error of (0.00) for SPS Characteristic $_{ij(t-1)s}$.

Table 2.7: The Impact of Bilateral and Multilateral SPS on Trade, by Type of Concern (1996 - 2010)

Equation: Dependent Variable:	Heckman Selection Model (maximum likelihood)			
	Selection	Outcome	Selection	Outcome
	Pr(import _{ijts} > 0)	ln(import _{ijts})	Pr(import _{ijts} > 0)	ln(import _{ijts})
	(1)	(2)	(3)	(4)
SPS Conformity _{ij(t-1)s}	-0.188*** (0.07)	-0.348 (0.23)		
SPS Conformity _{j(t-1)s} multilateral	-0.107*** (0.02)	-0.090 (0.06)		
SPS Characteristic _{ij(t-1)s}	0.084 (0.07)	0.952*** (0.19)		
SPS Characteristic _{j(t-1)s} multilateral	-0.086*** (0.02)	0.016 (0.6)		
SPSFreq Conformity _{ij(t-1)s}			-0.222*** (0.08)	-0.435 (0.28)
SPSFreq Conformity _{j(t-1)s} multilateral			-0.109*** (0.02)	-0.040 (0.07)
SPSFreq Characteristic _{ij(t-1)s}			0.101 (0.07)	0.980*** (0.23)
SPSFreq Characteristic _{j(t-1)s} multilateral			-0.105*** (0.02)	-0.005 (0.07)
Estimated correlation (rho)	0.497*** (0.01)		0.497*** (0.01)	
Estimated selection (lambda)	1.091*** (0.01)		1.091*** (0.01)	
Observations	5,452,147		5,452,147	

Note: ***, **, * denote significance at the 1%, 5%, and 10% level, respectively. Controls, constant, importer, exporter, HS4 product and time fixed effects and MR terms are included but not reported. Common religion is the selection variable and thus excluded in columns (2) and (4). Country clustered robust standard errors reported in parenthesis.

Bilateral Tariffs. First, Table 2.8 Panel A provides evidence that our previous results are not affected by the inclusion of bilateral applied tariff protection. All coefficients on the probability and the amount of trade remain qualitatively and quantitatively similar compared to Table 2.6. Results still show that most of the negative effect on the probability of entering a market is due to conformity assessment-related SPS intervention, while concerns related to product characteristics explain the positive impact on trade flows. This applies to the frequency as well as to the SPS dummy variables. Regarding the effect of applied tariffs on market entry and on trade volumes, they show a minor impact on bilateral trade and interpretation should again be read with caution similar to results presented in Table 2.4. Panel B shows that results on bilateral versus multilateral effects are robust as well. Conformity assessment and measures related to product characteristics act as a market entry barrier against all trade partners, while intervention related to product characteristics increases bilateral trade flows conditional on entering the protected market.

Table 2.8: Robustness: SPS, Tariffs and Trade, by Type of Concern (1996 - 2010)

Heckman Selection Model (maximum likelihood)				
Equation:	Selection	Outcome	Selection	Outcome
Dependent Variable:	Pr(import _{ijts} > 0)	ln(import _{ijts})	Pr(import _{ijts} > 0)	ln(import _{ijts})
PANEL A: BILATERAL SPS (N = 5,452,147)				
	(A1)	(A2)	(A3)	(A4)
SPS Conformity _{ij(t-1)s}	-0.254*** (0.07)	-0.402* (0.23)		
SPS Characteristic _{ij(t-1)s}	0.015 (0.07)	0.941*** (0.19)		
SPSFreq Conformity _{ij(t-1)s}			-0.285*** (0.09)	-0.463* (0.27)
SPSFreq Characteristic _{ij(t-1)s}			0.017 (0.07)	0.965*** (0.22)
Tariff _{ijts} , weighted average	0.001*** (0.00)	-0.001** (0.00)	0.001*** (0.00)	-0.001** (0.00)
Estimated correlation (rho)	0.459*** (0.01)		0.459*** (0.01)	
Estimated selection (lambda)	1.365*** (0.04)		1.367*** (0.04)	
PANEL B: BILATERAL & MULTILATERAL SPS (N = 5,452,147)				
	(B1)	(B2)	(B3)	(B4)
SPS Conformity _{ij(t-1)s}	-0.189*** (0.07)	-0.347 (0.23)		
SPS Conformity _{ij(t-1)s} mult.	-0.098*** (0.02)	-0.093 (0.06)		
SPS Characteristic _{ij(t-1)s}	0.083 (0.07)	0.953*** (0.19)		
SPS Characteristic _{ij(t-1)s} mult.	-0.083*** (0.02)	0.013 (0.06)		
SPSFreq Conformity _{ij(t-1)s}			-0.224*** (0.09)	-0.435 (0.28)
SPSFreq Conformity _{ij(t-1)s} mult.			-0.098*** (0.02)	-0.044 (0.07)
SPSFreq Characteristic _{ij(t-1)s}			0.100 (0.07)	0.981*** (0.23)
SPSFreq Characteristic _{ij(t-1)s} mult.			-0.102*** (0.02)	-0.008 (0.07)
Tariff _{ijts} , weighted average	0.001*** (0.00)	-0.001** (0.00)	0.001*** (0.00)	-0.001** (0.00)
Estimated correlation (rho)	0.459*** (0.01)		0.459*** (0.01)	
Estimated selection (lambda)	1.364*** (0.04)		1.367*** (0.04)	

Note: ***, **, * denote significance at the 1%, 5%, and 10% level, respectively. Controls, constant, importer, exporter, HS4 product and time fixed effects and MR terms are included but not reported. Common religion is the selection variable and thus excluded in the outcome equations. Country clustered robust standard errors reported in parenthesis.

Table 2.9: Robustness: SPS and Trade, by Type of Concern (1996 - 2010)

Dependent Variable:	import _{ijts} > 0			ln(import _{ijts})		
	Probit	OLS	2SLS	Probit	OLS	2SLS
PANEL A: BILATERAL SPS						
	(A1)	(A2)	(A3)	(A4)	(A5)	(A6)
SPS Conformity _{ij(t-1)s}	-0.249*** (0.07)	-0.222 (0.20)	-0.570* (0.27)			
SPS Characteristic _{ij(t-1)s}	0.005 (0.06)	0.972*** (0.17)	0.999*** (0.22)			
SPSFreq Conformity _{ij(t-1)s}				-0.278*** (0.08)	-0.256 (0.24)	-0.509 (0.27)
SPSFreq Characteristic _{ij(t-1)s}				0.006 (0.07)	0.991*** (0.20)	0.981*** (0.22)
Observations	5,452,147	1,960,755	1,960,755	5,452,147	1,960,755	1,960,755
Adjusted R^2		0.295	0.295		0.295	0.294
Kleibergen-Paap Wald F stat.			167.37			139.43
PANEL B: BILATERAL & MULTILATERAL SPS						
	(B1)	(B2)	(B3)	(B4)	(B5)	(B6)
SPS Conformity _{ij(t-1)s}	-0.178** (0.07)	-0.217 (0.20)	-0.575** (0.27)			
SPS Conformity _{j(t-1)s mult.}	-0.107*** (0.02)	-0.016 (0.05)	0.021 (0.05)			
SPS Characteristic _{ij(t-1)s}	0.076 (0.06)	0.925*** (0.17)	0.943*** (0.23)			
SPS Characteristic _{j(t-1)s mult.}	-0.086*** (0.02)	0.088* (0.05)	0.086* (0.05)			
SPSFreq Conformity _{ij(t-1)s}				-0.208** (0.08)	-0.277 (0.24)	-0.531* (0.27)
SPSFreq Conformity _{j(t-1)s mult.}				-0.110*** (0.02)	0.037 (0.06)	0.064 (0.06)
SPSFreq Characteristic _{ij(t-1)s}				0.092 (0.07)	0.938*** (0.20)	0.920*** (0.23)
SPSFreq Characteristic _{j(t-1)s mult.}				-0.104*** (0.02)	0.078 (0.06)	0.078 (0.06)
Observations	5,452,147	1,960,755	1,960,755	5,452,147	1,960,755	1,960,755
Adjusted R^2		0.295	0.295		0.295	0.295
Kleibergen-Paap Wald F stat.			107.651			93.488

Note: ***, **, * denote significance at the 1%, 5%, and 10% level, respectively. Controls, constant, importer, exporter, HS4 product and time fixed effects and MR terms included but not reported. Gravity controls included but not reported. Country clustered robust standard errors reported in parenthesis. Instruments are the sum of SPS concerns related to conformity assessment or product characteristics of all other countries $k \neq i$ against the importer and the sum of SPS concerns related to conformity assessment or product characteristics raised by the exporter against the importer in sectors $l \neq s$ with $s, l \in HS2$, respectively.

Reverse Causality. Second, we estimate a probit and a 2SLS model separately. For comparison reasons, we again also report the OLS coefficients. In the 2SLS model, we deploy a similar instrumentation method as before.¹⁸

¹⁸ Instruments are (i) the sum of SPS concerns related to conformity assessment or product characteristics of all other countries $k \neq i$ against the importer and (ii) the sum of SPS concerns related to conformity assessment or product characteristics raised by the exporter against the importer in sectors $l \neq s$ but included within the same HS2 category, respectively.

Table 2.9 Panel A reports bilateral results. Probit, OLS and 2SLS results on the impact of SPS measures on trade confirm our findings from the Heckman model. Estimates exhibit expected signs, significance levels, and similar magnitudes as those reported in Table 2.6. Instruments in the 2SLS models are generally valid as the Kleibergen-Paap F-Statistics are way above the 10% Stock and Yogo (2005) critical values. The same applies to bilateral versus multilateral effects of SPS measures. Results hold and are reported in Table 2.9 Panel B. The positive impact of SPS measures on trade flows can be attributed to SPS measures related to product characteristics which mainly benefit the country raising the concern. But, there is evidence that SPS measures related to product characteristics also promote trade with all partners conditional on entering the market (compare columns (B2) and (B3)). Further, when using instrumentation methods, we find that SPS measures related to conformity assessment significantly reduce the bilateral exports of the country reporting the concern. Instruments are again valid and the first stage F-Tests on the excluded instruments pass the most stringent criterion of the Stock and Yogo (2005) critical values.

2.5 Concluding Remarks

This paper contributes to the literature by investigating the impact of SPS measures on the extensive and the intensive margin of agricultural and food trade. Using the database on specific trade concerns on SPS measures of the WTO, we deploy a Heckman selection model at the HS4 disaggregated level of trade that controls for a potential selection bias and zero trade flows using both a dummy variable and a normalized frequency measure on SPS concerns.

We find that aggregate SPS measures pose a negative effect on the probability to export to a protected market, but, conditional on market entry, trade flows to markets with SPS standards in place tend to be higher. This reveals two important issues: First, SPS measures pose a serious barrier to market entry by increasing the fixed costs of trading. Second, SPS standards provide information on product safety to consumers and thus exert a positive impact on the trade flows of those exporters that manage to overcome the fixed cost of entering the market. Hence, foreign producers who meet the stringent standard gain market share. The advantage from gaining market share outweighs the costs of product adaptation to meet the standard and leads to a positive effect on trade flows. The results are robust to the inclusion of applied bilateral tariff data and to instrumentation. In addition, we find

robust evidence that SPS measures pose market entry barriers to all potential exporters and are thus non-discriminatory. In contrast, conditional on market entry, SPS measures mostly increase trade flows of those countries that raise a concern over an SPS measure at the WTO SPS committee to the detriment of other exporters.

Further, we determine the trade outcomes on agricultural and food products of different SPS regulations implemented by policy makers to achieve certain health safety objectives. We distinguish concerns related to conformity assessment (i.e., certificate requirements, testing, inspection and approval procedures) and concerns related to product characteristics (i.e., requirements on quarantine treatment, pesticide residue levels, or labeling and packaging). Results indicate that conformity assessment-related SPS measures act as a barrier to market entry, while concerns related to product characteristics increase trade once exporters meet the stringent standard. This suggests that conformity assessment-related measures increase fixed costs due to often burdensome and separate certification, testing and inspection procedures in different export markets. In contrast, SPS measures related to product characteristics enhance consumer trust by providing safety information on imported products.

This result is particularly interesting for policy makers who often have to choose from a set of measures that equivalently reduce health risks but entail diverse trade costs. Even though SPS measures cover a relatively narrow area of health and safety measures that are often directly related to consumer protection, policy makers should be aware that policy substitution may be put at some expense. Hence, depending on the policy maker's choice between conformity assessment versus product characteristics measures, the implied impact on trade varies strongly. In particular, conformity assessment-related SPS measures increase the fixed costs of trade in agricultural and food products.

2.A Appendix

Table 2.10: Summary Table

Variable	Observations	Mean	Std. Dev.	Source
$\ln(\text{import}_{ijts})$	1,960,755	10.440	3.305	Comtrade (2011)
$\Pr(\text{import}_{ijts} > 0)$	5,452,147	0.360	0.480	Comtrade (2011)
$\text{SPS}_{ij(t-1)s}$	5,452,147	0.004	0.067	SPS IMS (2011)
$\text{SPSFreq}_{ij(t-1)s}$	5,452,147	0.004	0.062	SPS IMS (2011)
$\text{SPS}_{j(t-1)s}$ mult.	5,452,147	0.043	0.203	SPS IMS (2011)
$\text{SPSFreq}_{j(t-1)s}$ mult.	5,452,147	0.041	0.188	SPS IMS (2011)
$\text{SPS Conformity}_{ij(t-1)s}$	5,452,147	0.003	0.052	SPS IMS (2011)
$\text{SPSFreq Conformity}_{ij(t-1)s}$	5,452,147	0.003	0.049	SPS IMS (2011)
$\text{SPS Characteristic}_{ij(t-1)s}$	5,452,147	0.004	0.062	SPS IMS (2011)
$\text{SPSFreq Characteristic}_{ij(t-1)s}$	5,452,147	0.004	0.058	SPS IMS (2011)
$\text{SPS Conformity}_{j(t-1)s}$ mult.	5,452,147	0.031	0.174	SPS IMS (2011)
$\text{SPS Characteristic}_{j(t-1)s}$ mult.	5,452,147	0.039	0.193	SPS IMS (2011)
$\text{SPSFreq Conformity}_{j(t-1)s}$ mult.	5,452,147	0.031	0.164	SPS IMS (2011)
$\text{SPSFreq Characteristic}_{j(t-1)s}$ mult.	5,452,147	0.036	0.178	SPS IMS (2011)
$\ln \text{GDP}_{it} \times \text{GDP}_{jt}$	5,452,147	22.928	3.255	WDI (2011)
$\ln \text{POP}_{it} \times \text{POP}_{jt}$	5,452,147	6.210	2.759	WDI (2011)
$\ln \text{Distance}_{ij}$	5,452,147	8.511	0.949	CEPII (2005)
Adjacency_{ij}	5,452,147	0.080	0.271	CEPII (2005)
$\text{Common Language}_{ij}$	5,452,147	0.358	0.480	CEPII (2005)
Ever Colony_{ij}	5,452,147	0.094	0.293	CEPII (2005)
$\text{Common Colonizer}_{ij}$	5,452,147	0.159	0.366	CEPII (2005)
$\text{Colonizer post 1945}_{ij}$	5,452,147	0.061	0.240	CEPII (2005)
$\text{Common Religion}_{ij}$	5,452,147	0.251	0.298	Helpman et al. (2008)
Tariff_{ijts} , weighted average	5,452,147	2.977	15.071	IDB (2011) & TRAINS (2011)
MR Distance_{ijt}	5,452,147	9.512	0.835	own calculation, Baier & Bergstrand (2009)
$\text{MR Adjacency}_{ijt}$	5,452,147	-0.032	0.146	own calculation, Baier & Bergstrand (2009)
$\text{MR Common Language}_{ijt}$	5,452,147	0.297	0.400	own calculation, Baier & Bergstrand (2009)
$\text{MR Ever Colony}_{ijt}$	5,452,147	0.175	0.228	own calculation, Baier & Bergstrand (2009)
$\text{MR Common Colonizer}_{ijt}$	5,452,147	0.119	0.236	own calculation, Baier & Bergstrand (2009)
$\text{MR Colonizer post 1945}_{ijt}$	5,452,147	0.174	0.225	own calculation, Baier & Bergstrand (2009)
MR SPS_{ijts}	5,452,147	-1.779	4.358	own calculation, Baier & Bergstrand (2009)
$\text{IV SPS}_{ij(t-1)s}$	5,452,147	0.212	1.358	own calculation
$\text{IV SPSFreq}_{ij(t-1)s}$	5,452,147	0.200	1.262	own calculation
$\text{IV SPS Conformity}_{ij(t-1)s}$	5,452,147	0.104	0.780	own calculation
$\text{IV SPS Characteristic}_{ij(t-1)s}$	5,452,147	0.192	1.256	own calculation
$\text{IV SPSFreq Conformity}_{ij(t-1)s}$	5,452,147	0.098	0.674	own calculation
$\text{IV SPSFreq Characteristic}_{ij(t-1)s}$	5,452,147	0.182	1.202	own calculation

Table 2.11: List of Agricultural and Food Sectors and Products included in the Data

HS2 Code	Constraint	Specification
01		Live Animals
02		Meat and Edible Meat Offal
03		Fish and Crustaceans
04		Dairy, Eggs, Honey and Edible Products
05		Products of Animal Origin
06		Live Trees and other Plants
07		Edible Vegetables
08		Edible Fruits and Nuts, Peel of Citrus and Melons
09		Coffee, Tea, Mate and Spices
10		Cereals
11		Milling Industry Products
12		Oil Seeds, Miscellaneous Grains, Medical Plants and Straw
13		Lac, Gums, Resins, Vegetable Saps and Extracts Nes
14		Vegetable Plaiting Materials
15		Animal and Vegetable Fats, Oils and Waxes
16		Edible Preparations of Meat, Fish, Crustaceans
17		Sugars and Sugar Confectionery
18		Cocoa and Cocoa Preparations
19		Preparations of Cereals, Flour, Starch or Milk
20		Preparations of Vegetables, Fruits and Nuts
21		Miscellaneous Edible Preparations
22		Beverages, Spirits and Vinegar
23		Residues from Food Industries and Animal Feed
24		Tobacco and Manufacturing Tobacco Substitutes
29	includes 2905	Organic Chemicals
33	includes 3301	Essential Oils, Resinoids, Perfumery, Cosmetic or Toilet Preparations
35	includes 3501 to 3505	Albuminoidal Substances, Starches, Glues, Enzymes
38	includes 3809 and 3824	Miscellaneous Chemical Products
41	includes 4101 to 4103	Raw Hides and Skins (other than Furskins) and Leather
43	includes 4301	Furskins and Artificial Fur, Manufactures thereof
50	includes 5001 to 5003	Silk
51	includes 5101 to 5103	Wool, Animal Hair, Horsehair Yarn and Fabric thereof
52	includes 5201 to 5203	Cotton
53	includes 5301 and 5302	Vegetable Textile Fibers Nes, Paper Yarn, Woven Fabric

Note: This list follows the products listed in Annex 1 in the Agricultural Agreement of the WTO, yet, also including fish, fishing and seafood products. All HS4 product codes in an HS2 sector are included if not specified otherwise in the constraints column.

Chapter 3

Climate Change and the Relocation of Population

3.1 Introduction

Why people migrate and have migrated in the past is no big secret. They move to improve their lives. Annually, the number of migrants increases by about three million, drawing on falling migration costs that lead to an unprecedented potential of migrants from developing countries (Hatton and Williamson, 2005). At the same time, the amount of people affected by natural disasters stands at a staggering number of 243 million people per year.¹ If warming progresses, hundreds of millions of people face the threat of sea-level rise, extreme droughts, bigger storms, or changing rainfall patterns, so that the numbers of those needing to leave disaster-struck places will continue to rise (Stern, 2006; IPCC, 2012; Economist, 2012). Already, 135 million are estimated to be at risk of needing to migrate due to desertification alone (INCCCD, 1994; Myers, 2002), while 200 million are at risk due to sea-level rise (Myers, 2002). While not all of the affected move across borders, international migration provides one adaption mechanism in the presence of natural disasters (McLeman and Smit, 2006; Marchiori and Schumacher, 2011; IPCC, 2012).² On these grounds, the impact of global warming and increasingly extreme climate-related disasters on the worldwide relocation of people is one of the major potential problematic issues that mankind faces in the future.

¹ This number was calculated by Oxfam, 2009, "Forecasting the numbers of people affected annually by natural disasters up to 2015" for the period 1998-2007.

² Note that unfettered migration is, however, not always possible without further ado.

Historically, the vast bulk of relocation of people caused by disasters has occurred within nations. In this context, previous research found an effect of disasters in particular on migration from rural to urban areas within national boundaries (Barrios et al., 2006). But lately it has become clear that global migration is again³ on the rise due to the accelerating pace of globalization⁴ and also due to intensified disaster frequency and scale (see i.e., UNEP, 2002; Stern, 2006; Bailey and Wren-Lewis, 2009; IPCC, 2012 and World Bank, 2012)⁵.

The latest report by the IPCC (2012), a recent report on global warming by the World Bank (2012) and the Stern Review (Stern, 2006) particularly accentuate that climate change and associated extreme weather events have become serious issues that are global in their consequences. As disasters turn more intense, it will become difficult to sustain livelihoods in some regions (Marchiori and Schumacher, 2011; IPCC, 2012). As a consequence, people may migrate internationally (Tacoli, 2009; Barnett and Webber, 2010). Similarly, if by the end of this century extreme droughts double as estimated by Arnell (2004), more and more people will try to permanently relocate from already dry and poor areas, such as Sub-Saharan Africa, to fertile and rich regions, such as Europe or North America. But unfettered migration to northern countries is not always a possible adaption mechanism as industrialized nations confront migrants with even stricter immigration policies (Boeri and Brücker, 2005).

The aim of this paper is to assess the extent to which natural disasters affect bilateral migration from a macro perspective. While previous gravity equations used in empirical applications of bilateral migration are known for their strong fit to the data, the estimated equations typically do not have a theoretical gravity foundation. I motivate the empirical estimation by providing a stylized theoretical framework. I base the gravity model of migration on derivations by Anderson (2011) and extend it by introducing natural disasters as a shock to labor productivity. In the migration gravity model, bilateral migration depends on population stocks, on implicit migration frictions and on disaster events in the source and the destination country. I continue by empirically investigating the relation deploying a comprehensive dataset of a full matrix of countries of bilateral migration available in increments of 10 years from 1960 to 2010. I add to the empirical literature by allowing disasters in the origin *and* the destination country to vary in impact, while at the same

³ Hatton and Williamson (2005) note that a first wave of voluntary mass migration took place in the 19th century and the beginning of the 20th century, while a second shift took place after World War II.

⁴ The expansion of migration may be attributed to reductions in migration frictions, such as migration costs, migration policies or regional and global integration, which led to lower barriers to migration.

⁵ For example, Bailey and Wren-Lewis (2009) note that weather-related disasters doubled since the 1980s and Stern (2006) reports a three-fold increase since the 1960s.

time controlling for multilateral resistance (MR). Using explicit MR terms directly in the migration framework distinguishes this paper from previous approaches. It allows to control for disasters in the origin and in the destination country and for time-varying country characteristics, such as migration policies. MR terms are adapted to the setup from the derivations of Baier and Bergstrand (2009) using a Taylor series expansion. Empirical findings provide persuasive evidence that aggregated natural disasters increase international migration out of affected areas. I find incidental evidence that people migrate less often into disaster-affected areas. By decomposing natural disasters into climate-related and geophysical disasters, I show that these diverse types of disasters have different implications on migration dynamics. While geophysical disasters are episodic events, climate-related disasters occur more frequently, cause higher volatility, and lead to permanent and irreversible problems, such as land degradation, desertification, or sea-level rise, that remove the means of existence for people. My results confirm that large climate-related disasters occurring in the origin induce significantly higher numbers of migrants, while those happening in the destination prevent people from moving there. Migration increases by about 5 percent, on average, due to an increase in large climate-related disasters in the home country by one standard deviation, all else equal.

The paper relates to the literature on the determinants of migration⁶, to the general empirical literature on bilateral migration⁷, and to the more specific subcategory on the relation between migration and natural disasters or climate change. Theoretical work on the role of disasters for migration is scarce. A theoretical study by Marchiori and Schumacher (2011) uses an overlapping generations model for two countries with endogenous climate change. Two of their key findings state that climate change increases migration and that even small changes in climate have significant effects on the number of migrants. Empirical research includes work by Naudé (2010) and Drabo and Mbaye (2011), who investigate the relation between disasters and international migration from Sub-Saharan Africa or developing countries, respectively. Their key finding is that disasters cause outmigration. Using a gravity framework, Reuveny and Moore (2009) and Coniglio and Pesce (2011) analyze the role of source country climate anomalies on international migration to OECD countries. Their results suggest that an increase in weather-related disasters in the origin

⁶ Important contributions have been made by Sjaastad (1962); Borjas (1987, 1989); Mincer (1978); Stark (1991).

⁷ Studies include Lewer and Van den Berg (2008); Pedersen et al. (2008); Letouzé et al. (2009); Ortega and Peri (2009); Mayda (2010), to name only a few.

increases migration. In a similar manner, Beine and Parsons (2012) examine the impact of climate-related variables and natural disasters in the source country on migration. Using a comprehensive dataset of migration for 1960 to 2000, they find no direct effect of climate anomalies or disasters striking the origin on bilateral migration.

A shortcoming of preceding studies is that they deploy only one-directional disasters or climate anomalies in the source country. They disregard the possibility that disasters in the destination may affect migration differently.⁸ Following these considerations, Alexeev et al. (2011) estimate the impact of weather-related disasters in the origin and the destination on migration from 1986 to 2006. The authors find that an increase in weather-related events in the origin lead to higher outmigration and that an increase in disasters in the destination also triggers migration.⁹ Hence, a number of recent papers have come up with insightful answers. But, so far, empirical analysis has been hampered by the lack of adequate and comprehensive data on bilateral migration, which makes it difficult to generalize results and policy implications.¹⁰ This paper enhances and improves previous approaches in several ways: (i) it provides a stylized theoretical gravity framework of migration; and (ii) in the empirical analysis, it uses a comprehensive migration dataset, while it allows disasters in the origin *and* the destination country to vary in impact and at the same time controls for multilateral resistances.

The remainder of the paper is structured as follows. The second section provides a simple theoretical gravity model of migration. The third section describes details on the empirical strategy and on the data. The fourth section analyzes the impact of climate-related disasters on international migration patterns. The last section concludes and points to future work.

⁸ Beine and Parsons (2012), for instance, cannot make the effect of climate anomalies in the destination visible as they include combined destination country and time fixed effects to control for multilateral resistance of the destination country.

⁹ Note that their sample excludes South - South migration and that they use OECD outflows as inflows from OECD into non-OECD destinations. This might contain a large measurement error and bias their result. Even more crucial, they do not control for multilateral resistance.

¹⁰ According to the Global Migrant Origin Database, migration to non-OECD countries accounts for 51 percent of international migration. Piguet et al. (2011) note that disasters are unlikely to affect migration in rich and politically stable economies.

3.2 A Stylized Theoretical Framework

To provide a simple theoretical motivation for estimating bilateral migration in a gravity framework, I follow Anderson (2011). The decision to migrate is featured by the choice over a discrete number of alternative locations on a global scale. The costs of migration are common to all migrants within a particular bilateral link, albeit migration costs may have an idiosyncratic component reflecting individual costs or utility from moving.

Consider a multi-country framework where $i, j = 1, \dots, C$ denote countries, and $h = 1, \dots, H$ denotes individuals. Each individual h has an idiosyncratic component of utility from migrating, ξ_{ijh} , which is unobservable and independently distributed across individuals with an iid type-1 extreme value distribution. In addition, individuals face costs of migration, which are the same for all workers that migrate in a particular migration corridor, $\kappa_{ij} = \kappa_{ji}$. Migration costs constitute an iceberg cost factor $\kappa_{ij} \geq 1$ and $\kappa_{ii} = 1$. Migration costs are a function of several factors. They comprise time-invariant costs from the move, such as cultural proximity (common language, common colonizer), or geographic location (distance, common border), and time-variant factors, such as networks (stock of migrants), regional networks (free trade areas, regional trade agreements), immigration policies, political ties between country-pairs, or benevolence of welfare states in destination. In addition, migration costs may depend on unobservable bilateral determinants, such as historical affinity of country-pairs, ethnic or business networks. And, migration costs may also follow a common time trend.

When a natural disaster strikes it damages and destroys both physical and human capital. It follows that disasters affect the migration decision by reducing the productivity of labor. By this, they affect wages and eventually also the movement of people. I formally introduce natural disasters as random shocks D , where $D \geq 1$. The occurrence of disasters and the damage caused are assumed to be idiosyncratic across locations. Disasters have a transitive effect on labor productivity as they suddenly shift demand and/or supply structures. Let the wage net of migration costs and net of the shock a disaster bears on labor productivity at destination be $w_j/\kappa_{ij}D_j$, where w_j denotes the wage at destination j , and wage net of the labor productivity shock of a disaster at home is w_i/D_i , where w_i denotes the wage at origin

i and $\kappa_{ii} = 1$. Then, an individual h migrates if the utility for migrating to some destination j is larger than from staying at home, $(w_j/\kappa_{ij}D_j)\xi_{ijh} \geq w_i/D_i$.¹¹

To evaluate migration, suppose expected utility is given by the logarithmic of a power function (i.e., a constant relative risk aversion (CRRA) CES function).¹² Specifically, the observable component of log-linear utility from migrating is

$$\ln u_{ij} = \ln w_j - \ln \kappa_{ij} - \ln D_j - [\ln w_i - \ln D_i]. \quad (3.1)$$

Note that individual decisions can be aggregated up to a representative individual (McFadden, 1974), as migrants are assumed to be homogeneous except for the random term ξ_{ijh} that is iid extreme value distributed. To retrieve a tractable gravity equation, I assume that the aggregated level of the discrete choice probability is equal to migration flows from source i to destination j . Aggregate bilateral migration is then given as

$$M_{ij} = P(u_{ij})N_i, \quad (3.2)$$

where the population in the source country takes a decision on migration and, with ξ_{ijh} following an iid extreme value distribution, the probability $P(u_{ij})$ ¹³ is given by

$$P(u_{ij}) = P(u_{ij} = \max_k u_{ik}) = \frac{e^{u_{ij}}}{\sum_k e^{u_{ik}}}. \quad (3.3)$$

Since the D 's and κ 's enter the model multiplicatively through their effect on wages, they combine into a shock-cost measure θ_{ij} that represents both migration costs and shocks from natural disasters on labor productivity.¹⁴ Both migration costs and disaster shocks to labor productivity operate in combination with given wages to generate the allocation of migrants. The combined shock-cost measure is then given as $\theta_{ij} = \kappa_{ij}D_j/D_i$.

¹¹ Note that the average expected gain in utility from not migrating (remaining in i) is zero for individuals that choose to stay in the country of origin (Ortega and Peri, 2009).

¹² The CRRA CES utility function is given as $u_{ij} = \frac{1}{\sigma-1} \left(\frac{w_j/\kappa_{ij}D_j}{w_i/D_i} \right)^{\sigma-1}$, where σ is the elasticity of substitution for migrants from different locations (it may also be called the coefficient of relative risk aversion).

¹³ For examples of bilateral migration discrete choice models that build on a multinomial logit function, see Beine et al. (2011), Grogger and Hanson (2011), Gibson and McKenzie (2011) or Beine and Parsons (2012).

¹⁴ This useful simplification follows Anderson (2009) and is exploited in what follows. It can be decomposed at any point into its components.

With logarithmic utility, the structure of the migration equation corresponds to the CES demand shares that support the trade gravity specification¹⁵,

$$M_{ij} = \frac{(w_j/\theta_{ij})^{\sigma-1}}{\sum_k (w_k/\theta_{ik})^{\sigma-1}} N_i. \quad (3.4)$$

To derive a tractable gravity equation, define $\Gamma_i \equiv \sum_k (w_k/\theta_{ik})^{\sigma-1}$ and specify the aggregated labor market clearing condition as $N_j \equiv \sum_i M_{ij}$. The clearing condition is then $N_j = w_j^{\sigma-1} \sum_i (\theta_{ij}^{1-\sigma}/\Gamma_i) N_i$. In equilibrium, wages are

$$w_j^{\sigma-1} = \frac{N_j}{N\Gamma_j} \quad (3.5)$$

with total world population $N \equiv \sum_i N_i \equiv \sum_j N_j$ and $\Gamma_j = \sum_i \frac{\theta_{ij}^{1-\sigma}}{\Gamma_i} \frac{N_i}{N}$. Substituting for the equilibrium wage in equation (3.4) using equation (3.5) yields the tractable gravity specification of migration

$$M_{ij} = \frac{N_i N_j}{N} \left(\frac{\theta_{ij}}{\tilde{\Gamma}_i \tilde{\Gamma}_j} \right)^{1-\sigma}, \quad (3.6)$$

with the outward migration friction price index $\tilde{\Gamma}_i = \left[\sum_j \frac{N_j}{N} \left(\frac{\theta_{ij}}{\tilde{\Gamma}_j} \right)^{1-\sigma} \right]^{1/1-\sigma}$ and the inward migration friction price index of $\tilde{\Gamma}_j = \left[\sum_i \frac{N_i}{N} \left(\frac{\theta_{ij}}{\tilde{\Gamma}_i} \right)^{1-\sigma} \right]^{1/1-\sigma}$.

To make the impact of disasters visible in the gravity equation of migration, I decompose θ_{ij} . This gives

$$M_{ij} = \frac{N_i N_j}{N} \left(\frac{\kappa_{ij}}{\tilde{\Gamma}_i \tilde{\Gamma}_j} \right)^{1-\sigma} D_i^{\sigma-1} D_j^{1-\sigma}, \quad (3.7)$$

and multilateral resistance terms are $\tilde{\Gamma}_i = \left[\sum_j \frac{N_j}{N} \left(\frac{\kappa_{ij}}{\tilde{\Gamma}_j} \right)^{1-\sigma} \left(\frac{D_j}{D_i} \right)^{1-\sigma} \right]^{1/1-\sigma}$ and $\tilde{\Gamma}_j = \left[\sum_i \frac{N_i}{N} \left(\frac{\kappa_{ij}}{\tilde{\Gamma}_i} \right)^{1-\sigma} \left(\frac{D_j}{D_i} \right)^{1-\sigma} \right]^{1/1-\sigma}$. The first term of equation (3.7) denotes bilateral migration in a world without frictions, where migrants are found in equal shares relative to the population in all destinations. The second term denotes the impact of frictions in a world that entails costs to migration. The larger bilateral migration costs κ_{ij} , the lower are migration flows. Albeit, in a world in which migrants choose from a set of alternative destinations, migration also depends on multilateral resistance, which captures worldwide bilateral migration costs. The third term indicates that random shocks to labor productivity

¹⁵ See also Feenstra (2004), Appendix 3.B, Example 3 for derivations of CES demand systems for discrete choice models.

in the origin and in the destination country affect migration. The larger the disaster shock in the origin D_i , the higher are migration flows. While, the larger the disaster in the destination j , the lower are migration flows.

As in the traditional gravity model, price indexes are computable once migration costs κ_{ij} are constructed econometrically.¹⁶ I derive multilateral resistance terms from a Taylor series expansion of the gravity equation following the approach by Baier and Bergstrand (2009). Details are provided in Appendix 3.A. When MR terms are computed using the constructed θ 's, separate information is required on D 's to compute Γ 's that include disaster productivity effects.¹⁷ Multilateral resistance terms comprise time-variant unobservable migration costs, such as immigration policies or the benevolence of the welfare state at destination.

3.3 Empirical Strategy and Data

3.3.1 Empirical Strategy

To test whether natural disasters exert a significant effect on bilateral migration patterns, this section outlines a fully fledged gravity model on a panel of bilateral migration, where equation (3.7) provides the starting point. In the following, I ask two things: (i) how does the number of disasters in the origin ($D_{i,t}$) and the number of disasters in the destination ($D_{j,t}$) affect bilateral migration flows ($M_{ij,t}$); (ii) and, do climate-related disasters, such as extreme temperature events, floods, droughts, or storms, exert a different effect on bilateral migration than geophysical disasters? I embed the questions in an augmented gravity specification¹⁸ which I estimate by a conditional fixed effects (FE) Poisson Pseudo Maximum Likelihood (PPML) approach advocated by Santos Silva and Tenreyro (2006) to account for zero migration.¹⁹ In the early years of migration data, zeros make up more than

¹⁶ To model migration costs, I follow the literature and use a function of observables $\kappa_{ij} = border_{ij} dist_{ij}^{\rho}$, where $border_{ij}$ is a border dummy and $dist_{ij}^{\rho}$ is the bilateral distance between origin and destination. In the panel setup, time-invariant bilateral migration costs are controlled for by the inclusion of country-pair fixed effects. Time-variant costs are controlled for by including multilateral resistance terms directly in the empirical specification.

¹⁷ Anderson (2009) notes that, in absence of information, MR terms may be based on migration costs only. If I compute MR terms not using information on disasters, results in the empirical section are robust.

¹⁸ The augmented model includes controls which have been proven important in the empirical migration gravity literature, pair fixed effects and explicit multilateral resistance terms, which capture all bilateral migration frictions.

¹⁹ If there are zeros in the data and error terms are heteroscedastic, the ordinary least squares (OLS) estimator is inconsistent, while PPML estimation generates consistent estimates even when the underlying distribution is not strictly Poisson.

50% (see Özden et al., 2011) and remain important afterwards. I estimate a gravity equation of the form

$$M_{ij,t} = \exp[\alpha_1 \ln(N_{i,t}N_{j,t}) + \alpha_2 D_{i,t} + \alpha_3 D_{j,t} + \alpha_4 \mathbf{X}_{ij,t} + \alpha_5 \mathbf{MR}_{ij,t} + \nu_{ij} + \nu_t] + \varepsilon_{ij,t}, \quad (3.8)$$

where $N_{i,t}N_{j,t}$ is the product of the total population of i and j , $D_{i,t}$ ($D_{j,t}$) is the total number of natural disaster events in the origin (destination) in a given decade, and the vector of controls $\mathbf{X}_{ij,t} = [\ln(y_{j,t}/y_{i,t}); \ln(\text{POP Dens}_{i,t} \times \text{POP Dens}_{j,t}); \text{Polity}_{i,t}; \text{Polity}_{j,t}; \text{Civil War}_{i,t}; \text{Civil War}_{j,t}; \text{FTA}_{ij,t}; \text{RTA}_{ij,t} \ln(\text{Migration Stock}_{ij,t-1} + 1)]$. The variable $y_{j,t}/y_{i,t}$ is the ratio of destination to origin per capita GDP and proxies wage differences. The vector also contains the product of population densities, Polity indexes, and count variables of civil wars that took place in the source or the destination country within the last 10 years of observation, respectively, joint membership in a free trade area (FTA) or a regional trade agreement (RTA). To control for network effects, I include the migration stock that corresponds to the previous period.²⁰ I run a conditional fixed effects Poisson (FE PPML) model where I control for country-pair specific heterogeneity by including a complete collection of country-pair dummies, ν_{ij} , that account for all time-invariant bilateral determinants of trade, such as distance, adjacency, or historical ties. The country-pair effects nest country dummies specific to each origin or destination country, respectively, and capture time-invariant country characteristics, initial migration stocks and the time-invariant component of multilateral remoteness. ν_t is a year effect.

Over a long period of time, multilateral resistances (MR) do change. I approximate MR terms ($\mathbf{MR}_{ij,t}$) based on distance ($MRDIST_{ij,t}$) and adjacency ($MRADJ_{ij,t}$) following an approach by Baier and Bergstrand (2009). I derive MR indexes from a Taylor series expansion of the gravity equation.²¹ This innovative econometric approach allows to control

²⁰ The literature on networks identifies migrant networks to promote bilateral migration flows, trade and capital flows (Rauch and Trindade, 2002; Munshi, 2003; Kugler and Rapoport, 2007; Docquier and Lodigiani, 2010). In particular, Beine et al. (2011) find that migrant networks significantly increase migration flows to OECD countries.

²¹ For mathematical details, see Appendix 3.A. MR terms are calculated as

$$\begin{aligned} MRDIST_{ij,t} &= \left[\left(\sum_{k=1}^C \delta_{k,t} (\ln \text{Dist}_{ik} + D_{k,t} - D_{i,t}) \right) + \left(\sum_{m=1}^C \delta_{m,t} (\ln \text{Dist}_{mj} + D_{j,t} - D_{m,t}) \right) \right. \\ &\quad \left. - \left(\sum_{k=1}^C \sum_{m=1}^C \delta_{k,t} \delta_{m,t} (\ln \text{Dist}_{km} + D_{m,t} - D_{k,t}) \right) \right], \\ MRADJ_{ij,t} &= \left[\left(\sum_{k=1}^C \delta_{k,t} (\text{Adj}_{ik} + D_{k,t} - D_{i,t}) \right) + \left(\sum_{m=1}^C \delta_{m,t} (\text{Adj}_{mj} + D_{j,t} - D_{m,t}) \right) \right. \\ &\quad \left. - \left(\sum_{k=1}^C \sum_{m=1}^C \delta_{k,t} \delta_{m,t} (\text{Adj}_{km} + D_{m,t} - D_{k,t}) \right) \right]. \end{aligned}$$

δ denotes a states' shares of population over 'total' world population, $N_{k,t}/N_t$ and $N_{m,t}/N_t$.

for the direct effects of disasters in the source and the destination country and to control at the same time for time-varying country characteristics, such as immigration policies, which are absorbed in the MR terms. ε_{ij} is an additive error term.

Regarding the impact of disasters on migration, the presumption is that α_2 has a positive sign, such that disasters in the origin induce migration out of affected countries (i.e., due to higher utility elsewhere), while I expect α_3 to have a positive or a negative sign, indicating that disasters in a potential destination increase migration (i.e., due to increased demand for labor) or reduce migration (i.e., due to lower utility from migrating there).

3.3.2 Data Sources

Migration data combine two datasets. The Global Migrant Origin Database (Version 4, 2007) provided by the World Bank reports bilateral migration stocks in a 10-year interval matrix for 1960-2000 for 226 countries based primarily on the foreign-born concept. The dataset combines census and population register records to construct decennial matrices corresponding to the last five completed census rounds. Data for 2010 are also provided by the World Bank. The dataset updates data by Ratha and Shaw (2007) by incorporating the latest migration data as described in the Migration and Remittances Factbook 2011. The 2010 dataset uses the foreign-born concept and similar sources and methods as the 1960-2000 data. The migration datasets exclude refugees, so that the data capture permanent migration only. I proxy migration flows by taking the difference between the reported migration stocks of contiguous data, similarly to Beine and Parsons (2012). In some cases migration stocks shrink over the observed time period, which may be attributed to several reasons, such as return migration, migration to a third country or death. As no information on these cases exist, I first take only non-negative migration flows as the dependent variable. In a robustness check, I assume that negative values constitute return migration and recalculate migration flows by summing the absolute value of the negative flow from destination to origin and initial non-negative migration flows from the origin and the destination.

Data on total population, population density, nominal GDP in US dollars and GDP per capita come from the World Bank's World Development Indicators (WDI). The Polity Index stems from the Polity IV Project and is rescaled from 0 to 20, where 0 corresponds to autocracy and 20 indicates full democracy. Information on civil wars are taken from Intra-State War Data (v4.1) maintained by the Correlates of War Project. I work with the

total number of civil wars within the last 10 years of the reported migration observation. Geographic linkages – distance, and common border – are taken from CEPII’s Geographic and Bilateral Distance Database. Information on joint membership in a free trade area (FTA) or a regional trade agreement (RTA) come from the WTO.

Disaster data stem from the Emergency Events Database (EM-DAT) maintained by the Center for Research on the Epidemiology of Disasters (CRED). It should be clear from the outset that doubts exist on the accuracy of data on natural disasters, mainly due to the fact that the main source of information are national governments, which may have an incentive in inflating the measured effects for various reasons. But, using data from a single source should provide information on the relative size of disasters as biases should be systemic. I thus regard the data to be appropriate for the hypothesis I examine here. My approach is two-fold: I define large-scale disasters as events that (i) caused 1,000 or more dead; (ii) affected 100,000 or more persons; or (iii) caused a monetary damage of 1 billion or more US dollars. To make damages comparable over time, I convert dollar values into constant 2000 US dollars using the US GDP deflator from WDI. For robustness reasons, I use a lower threshold (large and medium-sized disasters) defined as (i) 500 or more dead, or (ii) 50,000 or more persons affected; or (iii) a monetary damage of 500 million US dollars or more. Both classifications follow the convention of Munich Re (2006).

As it matters at what point in time during a year a disaster occurs, I weight the disaster measure by the onset month, adapting a strategy used by Noy (2009):

$$\text{Disaster Onset}_{i,t} = \text{Disaster}_{i,t} * ((13 - \text{onset month})/12), \quad (3.9)$$

$$\text{Disaster Onset}_{i,t+1} = \text{Disaster}_{i,t+1} * (1 - ((13 - \text{onset month})/12)). \quad (3.10)$$

With this weighting scheme, I assume that disasters always exert a full year’s impact. For example, a disaster happening in November, affects variables in remaining months of the year of occurrence, but also exerts an impact on January to October in the subsequent year. This results in two measures, a simple disaster measure counting the number of large disasters happening within a year, and a disaster onset measure, where I weight disasters by the onset month for a full year’s circle.

In a robustness check, I use a novel and comprehensive dataset on pure physical disaster intensity measures. The dataset is compiled and described in detail in Felbermayr and

Gröschl (2012). The data is assembled by Felbermayr and Gröschl (2012) from primary sources on earthquakes, volcanic eruptions, precipitation, hurricane and wind speed. I use a combined measure, that counts disasters as large if they fall in the top 10 percent largest physical categories of each disaster type experienced by a country. Again, measures are weighted by onset month. Data are available for 1979-2010.²²

As migration stocks are reported in increments of 10 years and corresponding flows capture migration over 10 year intervals, I work with the number of disasters summed up over a given interval.²³ The decadal dataset considering EM-DAT data covers a total of 3,559 large natural disasters between 1960 and 2010. To be able to observe the particular impact of specific types of disasters, I distinguish disasters into sub-groups. Climate-related disasters comprise climatological events (extreme temperature events, droughts and wildfires), hydrological disasters (floods), and meteorological events (storms of any kind). Geophysical disasters group earthquakes, tsunamis, volcanic eruptions and landslides.²⁴ In total, I observe 3,320 climate-related and 239 geophysical disasters that are large in scale. Table 3.6 in Appendix 3.B reports summary statistics.²⁵

3.4 Natural Disasters and International Migration

3.4.1 Benchmark Results

This section presents results on the impact of large-scale aggregated disaster variables on migration patterns, as well as of climate-related and geophysical disasters separately. The presumption is that disasters related to climate change (i.e., extreme temperature events, droughts, wildfires, floods, or storms) play a substantial role with respect to their impact on international migration, while the effect of geophysical disasters (i.e., earthquakes, or volcanic eruptions) is ambiguous. This can be motivated by the nature of these events. While large geophysical disasters are known to occur repeatedly in specific regions due to

²² Note that this data is quite new and not yet fully explored. As it is available for a shorter time period and migration data is only available for six cross-sections, I here prefer the large disaster measure from EM-DAT over the otherwise in many ways preferable measure based on physical disaster intensity.

²³ When looking at migration stocks directly, I use one year lagged disasters to allow for delayed adjustment of migration patterns.

²⁴ A last category not considered in the estimations are biological disasters, such as epidemics and insect infestations, which are assumed to have a minor impact on bilateral migration and are thus disregarded in the paper.

²⁵ All disaster variables are rescaled by a factor 1/10.

the geological character of the earth, their frequency of occurrence is episodic but relatively rare. People might deliberately choose to live in these regions due to the particular richness of soil and willingly take their chances. They might even adapt to geophysical disasters in regions that are known to be susceptible to geophysical events, or migrate within the boundary of nations rather than to migrate internationally. In contrast, disasters related to climate change occur with a higher frequency and magnitude in recent decades and also strike regions that are not traditionally affected by catastrophic events (UNEP, 2002; Stern, 2006; Bailey and Wren-Lewis, 2009; IPCC, 2012 and World Bank, 2012). Extreme weather events cause higher vulnerability by irreversibly removing the subsistence possibility of people through persistent effects, such as land degradation, and by permanently lowering the productivity of assets (i.e., land). Those affected might be more prepared to move internationally in search of better conditions. While these considerations may also apply to disasters hitting potential destination countries, one could also think of a different scenario where disastrous episodic events (i.e., geophysical disasters) increase the demand for labor to promote reconstruction. This may lead to positive (or non-negative) international migration movements toward destination regions. Also, if disasters are not strong enough, I expect no measurable effect on international migration.

Table 3.1 reports benchmark results on the aggregated disaster measure in the origin and the destination country and for climate-related and geophysical disasters, respectively. All regressions are estimated by FE PPML and include country-pair fixed effects, year dummies and respective MR measures. Further, all regressions include a choice of controls derived from the migration literature. Columns (1) and (2) report results on the total number of disasters within a 10-year time span using the simple disaster measures. Columns (3) and (4) report findings deploying the disaster measure weighted by onset month.

In line with Lewer and Van den Berg (2008), the ratio of the two countries' GDP per capita levels, a proxy for relative wage differences, increases bilateral migration. Contrasting their cross-sectional results, migrants move less to countries more similar with respect to their population size. Countries more similar with respect to population density experience higher migration flows than those more different in population density. The political structure of the source country signals no direct effect on international migration, while more migration takes place toward democratic countries. Civil wars in the origin or destination have no significant impact on migration. Joint membership in a FTA significantly increases bilateral migration, while joint membership in an RTA has an adverse effect on migration patterns. Network

Table 3.1: Migration and Large Natural Disasters (1960-2010)

Dependent Variable: Δ bilateral migration from i in j				
Disaster Measure:	Simple		Onset	
	(1)	(2)	(3)	(4)
Disaster $_t^i$	0.026*** (0.01)		0.027*** (0.01)	
Disaster $_t^j$	-0.008 (0.02)		-0.010 (0.02)	
Climate-related Disaster $_t^i$		0.016** (0.01)		0.018** (0.01)
Climate-related Disaster $_t^j$		-0.029* (0.02)		-0.061** (0.03)
Geophysical Disaster $_t^i$		0.170 (0.11)		0.116 (0.11)
Geophysical Disaster $_t^j$		1.989** (0.84)		2.932** (1.23)
Controls				
$\ln(y_t^j/y_t^i)$	0.357*** (0.07)	0.359*** (0.07)	0.362*** (0.07)	0.350*** (0.07)
$\ln(\text{POP}_t^i \times \text{POP}_t^j)$	-7.961*** (2.53)	-8.076*** (2.47)	-8.008*** (2.54)	-8.113*** (2.54)
$\ln(\text{POP Dens}_t^i \times \text{POP Dens}_t^j)$	8.397*** (2.54)	8.518*** (2.48)	8.444*** (2.55)	8.541*** (2.55)
Polity Index $_t^i$	0.170 (0.19)	0.151 (0.19)	0.176 (0.19)	0.155 (0.19)
Polity Index $_t^j$	0.711*** (0.26)	0.667*** (0.25)	0.705*** (0.26)	0.665*** (0.26)
Civil War $_t^i$	0.159 (0.15)	0.107 (0.14)	0.147 (0.15)	0.083 (0.14)
Civil War $_t^j$	-0.265 (0.40)	-0.339 (0.41)	-0.261 (0.40)	-0.336 (0.41)
Free Trade Area $_t^{ij}$	0.465*** (0.15)	0.467*** (0.16)	0.464*** (0.15)	0.487*** (0.16)
Regional Trade Agreement $_t^{ij}$	-0.310* (0.18)	-0.306* (0.18)	-0.308* (0.18)	-0.323* (0.18)
\ln Migration Stock $_{t-1}^{ij}$	0.090** (0.04)	0.092** (0.04)	0.090** (0.04)	0.091** (0.04)
Observations	40,956	40,956	40,956	40,956

Note: ***, **, * denote significance at the 1%, 5%, and 10% level, respectively. Country pair and time fixed effects, and MR terms included in all regressions but not reported. Robust standard errors reported in parenthesis. Disasters are the number of large-scale disasters according to the decision rule.

effects, captured by the inclusion of the lagged bilateral migration stock, signal a positive and significant effect. This is in line with expectations and in accordance to research on migration networks (Docquier and Lodigiani, 2010; Beine et al., 2011). In particular, Table 3.1 reports that migration networks explain 9 percent of the variability in migration flows, all else equal. Coefficients on controls remain in line across different specifications.

Table 3.1 column (1) reports that disasters in the source country have a significant and positive impact on bilateral migration flows out of the region, while disasters in the destination have no measurable effect using the simple disaster measure. A one standard deviation increase in natural disasters in the origin leads, on average, to an increase in migration by 8.3 percent²⁶, all else equal. Results on disasters in the origin country are in line with expectations and with the literature (Drabo and Mbaye, 2011; Coniglio and Pesce, 2011; Reuveny and Moore, 2009). Column (2) reports that climatic events push migrants out of affected areas, while people less often move toward disaster affected economies. On average, a one standard deviation increase in climatic disasters in the origin leads to an increase in migration by 4.7 percent, all else equal. While we are mostly interested in the effect from climate-related disasters, geophysical disasters might exert an impact on migration as well. Column (2) reports that geophysical events that hit the source country have no significant effect on international migration, while geophysical disasters in the destination increase bilateral migration, possibly due to increased labor demand as reconstruction is often labor intensive. As the effect of disasters may relate to the month of the disaster onset and their spill over effects to the subsequent year, I use the disaster measures weighted by onset month in all following specifications. In column (3), I again find that natural disasters in the origin cause increased migration out of the affected country, while those in the destination are insignificant. A one standard deviation increase in natural disasters in the origin leads, on average, to an increase in migration by 8.5 percent. But, looking at climate-related and geophysical disasters separately in column (4), findings suggest that climatic disasters spur outmigration in the source country and reduce migration toward the destination, while geophysical disasters in the destination increase bilateral migration. A one standard deviation increase in climate-related disasters in the origin leads, on average, to an increase in migration by 5.2 percent, while a one standard deviation increase in large climate-related disasters in the destination leads, on average, to a drop in migration by 11.6 percent, all else equal. Results are broadly in line with findings by Reuveny and Moore

²⁶ The formula for this is $(\exp(0.026*3.061)-1)*100\%$.

(2009); Alexeev et al. (2011); Coniglio and Pesce (2011) and Drabo and Mbaye (2011), who find that climate-related disasters in the origin increase outmigration.

3.4.2 Heterogeneity Across Country Groups

According to Piguet et al. (2011) it is rather unlikely that disasters affect migration in rich and politically stable economies. To take a closer look, I first split the sample into OECD and non-OECD economies. Second, I distinguish rich, middle, and poor countries.

Table 3.2 Panel A reports results on OECD and non-OECD economies. As one might have presumed, previous findings are predominantly driven by migration from non-OECD countries to OECD states (compare columns (A1), (A4) and (A6)). While coefficient estimates on non-OECD countries mirror previous findings, results for migration from OECD economies are mainly insignificant (columns (A2), (A7) and (A8)). This supports the presumption by Piguet et al. (2011). Solely in column (A5), coefficients suggest that more international migration from non-OECD nations takes place if the destination is a non-OECD economy, despite the fact that it is hit by a disaster. Here, migrants might be attracted to non-OECD economies for labor intensive reconstruction purposes. To conclude, natural disasters affect international migration mainly from non-OECD to OECD economies, while migration from OECD countries is, on average, not affected. Table 3.7 in Appendix 3.B reports full results for Panel A.

In a next step, I distinguish rich, middle, and low income countries in Panel B. To determine the sets of poor, middle and rich economies, I follow the World Bank classification. While people from low income countries move to middle income countries in case of a disaster in the origin (column (B2)), people from middle income countries either move to low or to high income countries if their home country is struck by a large disaster (columns (B4) and (B6)). People from high income countries do not move if the origin is hit by a large disaster (columns (B7) to (B9)). Contrasting this, low income disaster struck destinations, are less attractive for migrants from low or middle income countries (columns (B1) and (B4)), but more so for migrants from high income nations (compare column (B7)). Disaster-affected middle income economies attract migrants from low and middle income countries (columns (B2) and (B5)), while high income destinations that are hit by a large disaster are less attractive, particularly for high income migrants (column (B9)). To conclude, the pattern that disasters are on average associated with migration out of affected areas, but negatively

Table 3.2: Summary: Development Status (1960-2010)Dependent Variable: Δ bilateral migration from i to j

PANEL A: OECD versus non-OECD									
Origin	non-OECD	OECD	all	all	non-OECD	non-OECD	OECD	OECD	
Destination	all	all	non-OECD	OECD	non-OECD	OECD	non-OECD	OECD	
	(A1)	(A2)	(A3)	(A4)	(A5)	(A6)	(A7)	(A8)	
Disaster Onset $_t^i$	0.022*** (0.01)	-0.000 (0.03)	0.017 (0.01)	0.034*** (0.01)	0.019 (0.01)	0.021*** (0.01)	0.023 (0.02)	0.007 (0.03)	
Disaster Onset $_t^j$	-0.037** (0.02)	-0.055 (0.04)	0.023 (0.02)	-0.062*** (0.02)	0.043* (0.02)	-0.081*** (0.02)	-0.036 (0.03)	-0.021 (0.07)	
Observations	33,808	7,148	29,390	11,566	23,814	9,994	5,576	1,572	
PANEL B: Income Groups									
Origin	low	low	low	middle	middle	middle	high	high	high
Destination	low	middle	high	low	middle	high	low	middle	high
	(B1)	(B2)	(B3)	(B4)	(B5)	(B6)	(B7)	(B8)	(B9)
Disaster Onset $_t^i$	-0.647 (0.69)	0.205** (0.09)	-0.051 (0.08)	0.044*** (0.02)	-0.009 (0.01)	0.029*** (0.01)	0.064 (0.05)	-0.001 (0.02)	0.026 (0.03)
Disaster Onset $_t^j$	-0.583 (0.40)	0.600*** (0.15)	-0.025 (0.04)	-0.115** (0.05)	0.104*** (0.03)	-0.032 (0.02)	0.409*** (0.13)	-0.016 (0.03)	-0.112*** (0.03)
Observations	2,107	4,042	3,875	3,551	8,725	7,979	2,112	4,756	3,809

Note: ***, **, * denote significance at the 1%, 5%, and 10% level, respectively. Controls, constant, country pair and time fixed effects, and MR terms included in all regressions but not reported. Robust standard errors reported in parenthesis. Disasters are the number of large-scale disasters according to the decision rule. Income group classification is according to the World Bank convention. Controls included as in Table 3.1. Full results for Panel A are reported in Table 3.7 and for Panel B in Table 3.8 in Appendix 3.B.

with migration into affected countries is mainly driven by middle income countries and lower migration flows toward disaster stuck low income economies. Table 3.8 in Appendix 3.B reports full results for Panel B.

The reason that people in middle (or low) income economies are more likely to relocate internationally in case of a disaster than people in high income countries might be attributable to the fact that property and life in high income economies are, on average, more often insured against damage or loss than in middle or low income countries. Hence, the insurance penetration²⁷ is, on average, much higher in rich economies than in middle or low income countries (compare Figure 3.1 in Appendix 3.B). Thus, people in high income countries see no need to migrate internationally in case of a disaster. Contrasting this, people in low and middle income countries are, on average, less often insured against damage and loss, but are at the same time more vulnerable to the irreversible and persistent effects

²⁷ The insurance penetration (life and non-life) is measured as life insurance premium volume as a share of GDP or non-life insurance premium volume as a share of GDP, respectively.

caused by climate change and concomitant disasters (i.e., land degradation) that remove their subsistence possibility.

3.4.3 Robustness Checks

In the robustness checks, I address six concerns. First, results could depend on the applied definition of 'large-scale' disasters. I conduct robustness checks pertaining to this choice. Second, if both the origin and the destination country are hit by a disaster, what happens to migration? To give an indication to this question, I introduce an interaction term that captures whether both countries in a pair were struck by a disaster. Third, the baseline regression accounts only for non-negative migration flows. In an alternative approach, I assume that all negative values relate to return migration and recalculate migration flows accordingly. Fourth, I conduct the estimation using a log-linear fixed-effects (FE) specification that disregards zero migration flows to show that results do not depend on the estimation strategy used. I compare linear FE results to FE PPML. Fifth, I turn to the discussion on flow adjustment problems. In empirical investigations of migration patterns it is not clear whether migration flow or stock data should be used. Accordingly, I conduct a robustness check using migration stocks instead of flows. Finally, data on natural disasters may be flawed by measurement and selection issues. As a robustness check, I thus turn to a new dataset on pure physical disaster intensity measures and conduct the analysis using the novel dataset.

Disaster Decision Rule. Table 3.3 summarizes results for the FE PPML specifications using medium-sized and large disasters, utilizing a lower threshold as specified in the data section. The lower threshold realizes an additional 1,327 natural disasters and thus increases the total number of disasters in the analysis by about 28 percent. As expected, results for origin country disasters remain qualitatively similar compared to those under the 'large-scale' decision rule, while destination effects vanish. This indicates that disasters in the destination need to be large and severe enough to exert a measurable effect on international migration. Still, findings confirm that natural disasters in the origin positively affect international migration flows in column (1). A one standard deviation increase in the aggregate disaster variable in the origin causes about 7.8 percent higher outmigration. Climate-related disasters in the origin cause, on average, higher migration out of affected areas in column (2).

Table 3.3: Summary: Migration and Medium and Large Natural Disasters (1960-2010)

Dependent Variable: Δ bilateral migration stocks from i in j				
	(1)	(2)	(3)	(4)
Disaster Onset $_t^i$	0.023***			
	(0.01)			
Disaster Onset $_t^j$	0.007			
	(0.02)			
Climate-related Disaster Onset $_t^i$		0.025***		0.020***
		(0.01)		(0.01)
Climate-related Disaster Onset $_t^j$		0.005		-0.033
		(0.02)		(0.02)
Geophysical Disaster Onset $_t^i$			0.209***	0.050
			(0.07)	(0.13)
Geophysical Disaster Onset $_t^j$			2.443***	3.011***
			(0.94)	(1.07)
Observations	40,956	40,956	40,956	40,956

Note: ***, **, * denote significance at the 1%, 5%, and 10% level, respectively. Country pair and time fixed effects, and MR terms included in all regressions but not reported. Robust standard errors reported in parenthesis. Disasters are the number of medium-sized and large disasters according to the decision rule. Controls included as in Table 3.1. Full results are reported in Table 3.9 in Appendix 3.B.

Geophysical disasters increase outmigration, but those occurring in destinations also attract migrants, possibly due to increased labor demand after an episodic disaster event in column (3). Column (4) reports that a one standard deviation increase in disasters related to climate change increase bilateral migration out of the affected country on average by 6 percent. Geophysical disasters in the origin have no measurable effect. Table 3.9 in Appendix 3.B reports full results.²⁸

Combined Disaster Effects. Do migration patterns change if both countries in a bilateral link are struck by disasters? I introduce an interaction term that is one if both countries in a pair are hit by a disaster and zero otherwise. I find that patterns for individual disasters remain robust. The interaction term, indicating that both the origin and the destination are

²⁸ As expected, if I use all natural disasters reported in the database, including small, medium and large disasters, I find no significant effect of disasters on migration. The effect of large disasters is then superposed by many small disasters, which are much more frequent but do not affect migration across borders. To conclude from this, a disaster has to be large and thus severe enough to exert a significant impact on international migration patterns. This could also be the reason why Beine and Parsons (2012) find no significant effect.

hit by natural disasters, is negative but insignificant. Table 3.10 in Appendix 3.B reports results.

Table 3.4: Summary: Robustness Checks

Dependent Variable: Δ bilateral migration stocks from i in j , including return migration								
	Return Migration				Log-Linear Model			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Disaster Onset $_t^i$	0.020*** (0.01)				0.020*** (0.00)			
Disaster Onset $_t^j$	-0.035*** (0.01)				-0.002 (0.01)			
Climate-related Disaster Onset $_t^i$		0.021*** (0.01)	0.009 (0.01)			0.021*** (0.01)	0.017*** (0.01)	
Climate-related Disaster Onset $_t^j$		-0.037** (0.01)	-0.038* (0.02)			-0.006 (0.01)	-0.048*** (0.01)	
Geophysical Disaster Onset $_t^i$			0.187 (0.12)	0.114 (0.13)			0.053 (0.07)	0.029 (0.08)
Geophysical Disaster Onset $_t^j$			-0.015 (0.15)	-0.118 (0.14)			0.536*** (0.07)	0.578*** (0.07)
Climatic Disaster Onset $_t^i$				0.236* (0.12)				-0.056 (0.05)
Climatic Disaster Onset $_t^j$				-0.304** (0.15)				0.002 (0.05)
Meteorological Disaster Onset $_t^i$				-0.011 (0.02)				0.039** (0.02)
Meteorological Disaster Onset $_t^j$				0.024 (0.03)				-0.022* (0.01)
Hydrological Disaster Onset $_t^i$				0.001 (0.01)				0.017** (0.01)
Hydrological Disaster Onset $_t^j$				-0.090** (0.04)				-0.181*** (0.03)
Observations	55,327	55,327	55,327	55,327	35,479	35,479	35,479	35,479
R^2 within					0.235	0.235	0.237	0.239

Note: ***, **, * denote significance at the 1%, 5%, and 10% level, respectively. Controls, country pair and time fixed effects, and MR terms included in all regressions but not reported. Robust standard errors reported in parenthesis. Disasters are the number of large-scale disasters according to the decision rule. Controls included as in Table 3.1. Estimations in columns (1) to (4) are conducted by FE PPML, while estimations in columns (5) to (8) use a linear fixed effects specification, where the dependent variable is loglinearized. Full results for return migration (columns (1) to (4)) are reported in Table 3.11, and for the log-linear model in Table 3.12 in Appendix 3.B.

Return Migration. The benchmark specifications consider only non-negative migration and disregard any information contained in negative migration flows resulting from differentiating migration stocks. In an alternative approach, I now assume all negative values to constitute return migration, as no information on negative flows exists. I recalculate migration flows by summing up the absolute value of the negative flow from destination to

origin and initial non-negative flows from source to destination country.²⁹ Table 3.4 columns (1) to (4) report FE PPML summary results for large disasters.

Controls signal similar results as before. Nicely, also results on disaster coefficients reaffirm previous findings. In particular, natural disasters in the origin increase migration out of affected countries, while disasters in the destination prevent people from permanently moving there (Table 3.4 column (1)). Coefficients on climate-related disasters fully confirm earlier patterns (compare column (2)), while geophysical events have no measurable effect considering return migration. Splitting climate disasters further into its sub-categories, I find that particularly source country droughts or extreme temperature events (climatic disasters) increase outmigration, while climatic events and flooding (hydrological) events in the destination prevent bilateral migration. Hence, results remain largely in line with previous findings even when considering the extreme hypothesis on return migration.

Linear Fixed Effects Estimation. Table 3.4 columns (5) to (8) summarize results using a log-linear fixed effects specification, disregarding zero bilateral migration. Results on the FE specification remain in line compared to the FE PPML estimations in Table 3.1. Controls signal qualitatively and quantitatively similar results as before. Estimates report that disasters are on average positively associated with migration out of affected areas (compare column (5)). Just like in previous regressions, the story is again mainly driven by climate-related disasters. Splitting them into sub-groups, column (8) shows that storms and floods play an important role in determining bilateral migration patterns. Table 3.12 in Appendix 3.B reports full results.

Migration Stocks. The empirical literature on migration faces important issues concerning data on bilateral migration. The migration dataset on the full matrix of countries is only available in 10 year steps for migrant stocks, as it relies on census data. But, one could think that 10-year differences are hard to interpret as migration flows due to adjustment problems. Also, from a theoretical perspective it is not clear whether to use migration flow or stock data. Accordingly, I conduct a robustness check using migration stocks instead of calculated flows to see whether results still hold.³⁰ Table 3.5 deploys

²⁹ Note that this approach most likely overstates real return flows, as the migration stock may as well shrink with some migrants dying and others moving on to a third country. But it will give an indication whether the baseline results hold.

³⁰ There are also several drawbacks from using migration stock data, i.e. controlling for network effects might introduce a Nickell bias. When error terms are serially correlated, coefficient estimates of the dynamic

Table 3.5: Migration Stocks and Large Natural Disasters (1960-2010)

Dependent Variable: bilateral migration stocks from i in j				
Disaster Variable:	One Year Lagged			
	(1)	(2)	(3)	(4)
Lagged Disaster Onset $_t^i$	0.100**			
	(0.04)			
Lagged Disaster Onset $_t^j$	0.034			
	(0.07)			
Lagged Climate-related Disaster Onset $_t^i$	0.100**	0.092**		
	(0.04)	(0.04)		
Lagged Climate-related Disaster Onset $_t^j$	0.031	0.054		
	(0.06)	(0.08)		
Lagged Geophysical Disaster Onset $_t^i$		-0.364	0.207	
		(0.57)	(0.69)	
Lagged Geophysical Disaster Onset $_t^j$		1.054	-1.299	
		(2.00)	(2.48)	
Lagged Climatic Disaster Onset $_t^i$			0.218	
			(0.20)	
Lagged Climatic Disaster Onset $_t^j$			0.215	
			(0.56)	
Lagged Meteorological Disaster Onset $_t^i$			-0.000	
			(0.04)	
Lagged Meteorological Disaster Onset $_t^j$			0.190**	
			(0.08)	
Lagged Hydrological Disaster Onset $_t^i$			0.234**	
			(0.10)	
Lagged Hydrological Disaster Onset $_t^j$			-1.051***	
			(0.23)	
Observations	67,484	67,484	67,484	67,484

Note: ***, **, * denote significance at the 1%, 5%, and 10% level, respectively. Controls, country pair and time fixed effects, and MR terms included in all regressions but not reported. Robust standard errors reported in parenthesis. Disasters are the number of large-scale disasters according to the decision rule, lagged by one year. Controls included as in Table 3.1. Full results are reported in Table 3.13 in Appendix 3.B.

migration stocks instead of flows using the FE PPML model as before. Results reaffirm previous findings. Controls remain in line and network effects are positive and significant, as expected. I use one year lagged disasters to allow for an adjustment process of migration patterns. In column (1), I find that a one standard deviation increase in natural disasters in the origin lead, on average, to an increase in the migration stock by 3.1 percent, all else equal. Columns (2) and (3) show that consistent with above findings, this pattern stems from disasters related to climate change. Geophysical disasters have no measurable effect. Splitting the climate-related disaster variable into further sub-categories, column (4) reports

equation suffer from a Nickell bias. The panel dataset is unbalanced and the inclusion of lagged variables reduces the time span covered. This limits possibilities considerably. I thus estimate the dynamic version only as a robustness check and coefficients should be treated with caution.

that outmigration is attributable to hydrological events, while large floods happening in the destination also prevent migrants from moving there. Table 3.13 in Appendix 3.B reports full results for migration stocks.

A Pure Physical Disaster Intensity Measure. As discussed in the data section and in the literature on natural disasters, data on disasters may be flawed by measurement and/or selection problems. As a last robustness check, I thus turn to a new dataset on pure physical disaster intensity measures and conduct the analysis pertaining to this choice. The dataset is compiled and described in detail in Felbermayr and Gröschl (2012). The data is assembled by Felbermayr and Gröschl (2012) from primary sources on earthquakes, volcanic eruptions, precipitation, hurricane and wind speed. To make measures comparable, I use a combined measure, that counts disasters as large if they fall in the top 10 percent largest physical categories of each disaster type experienced by a country. The disaster data based on pure physical measures is, however, only available for the shorter time period from 1979 to 2010. Thus, I lose quite a number of observations.

First, I use a simple disaster measure. Second, I employ the number of disasters weighted by respective onset months. Results remain in a similar range as the above findings when using the disaster measures constructed from physical disaster intensity variables. Controls signal similar results as before. Again, I find that large disasters in the origin country lead to more bilateral migration out of affected countries, while less people migrate toward disaster-struck destination countries. In column (3), a one standard deviation increase in disasters in the origin country leads, on average, to a 1.9 percent³¹ increase in bilateral migration for the 1980 to 2010 period, all else equal. Again, patterns are mainly driven by climate-related disasters, storms and precipitation differences in this case (compare columns (2) and (4)). Table 3.14 in Appendix 3.B reports results. Note that the data on the physical intensity of natural disasters are quite new and not yet fully explored. As a result, this topic needs further investigation and scrutiny in future research.

3.5 Concluding Remarks

This paper provides an answer to the pressing question on the impact of climate-related disasters on international migration. To motivate the empirical strategy, I construct a

³¹ The formula for this is $(\exp(0.029*0.632)-1)*100\%$.

stylized gravity framework of bilateral migration introducing disasters as random shocks to labor productivity. To test implications empirically, I deploy a matrix of international migration available for increments of 10 years from 1960 to 2010. Gravity estimations are augmented by the use of fixed effects and explicit MR terms to control for unobservable time-varying country variables, such as migration policies. Zeros are considered by using a conditional fixed-effects PPML specification.

The gravity model of migration implies that disasters in the origin increase migration, while disasters in the destination reduce migration. Empirically, I find supportive evidence particularly for climate-related disaster events. Findings suggest that, on average, a one standard deviation increase in large climate-related disasters in the source country leads to a 5% increase in international migration, all else equal. Large climate-related disasters happening in the destination decrease international migration. As suspected, findings are dominated by extreme weather events related to global warming, as those events irreversibly and persistently lower the productivity of land and thereby remove the subsistence possibility of a large number of people. As a result, large climate-related disasters lead to more international migration. Contrasting this, the effect of geophysical disasters is ambiguous. The latter are known to be episodic. Hence, people might willingly take their chances and adapt to the disaster risk in regions susceptible to geophysical events. By decomposing the sample, I show that results are particularly driven by migration from developing countries to industrialized economies. Overall, results are robust to the definition of disasters, when considering 'return' migration, when using a linear fixed effects specification, if migrant stocks are used instead of flows, or if disaster data based on pure physical disaster intensity measures are employed.

To conclude, findings yield supportive evidence that migration serves as an adaption strategy to disasters induced by climate change. This is particularly true for developing countries where large parts of the population are not insured against damage or loss, but are at the same time more vulnerable to the irreversible and persistent effects caused by climate change and concomitant disasters (i.e., land degradation) that remove their subsistence possibility.

Moreover, with respect to future research, I conjecture that the theoretical gravity model of migration may serve as a basis for structural estimation.³² It prepares the ground for

³² This implies a further course of action where, first, the structural model needs to be set out and, second, gravity variables, disaster shocks, migration costs, and the elasticity of substitution, will be estimated.

conducting counter-factual scenarios, such as to compute comparative static migration and welfare effects when abolishing migration costs, or to quantify the numbers of migrants induced by varying shocks to labor productivity triggered by extreme weather events caused by climate change.

The structural gravity equation may either be estimated in log-linear form or by PPML employing fixed effects. The line of action continues by estimating the simple gravity equation of migration using observable determinants of bilateral migration costs and shocks to labor productivity, such as common border, distance, cultural and historical ties, or natural disasters, that generate estimates for migration costs and shock measures. To continue, one would need to use data from a cross section (i.e., of the year 2010) to estimate the elasticity of substitution. Given estimates of migration costs, labor productivity shocks, data on N_i , and a value for σ , one may calculate baseline migration flows.

3.A Appendix

The Appendix provides details on the Taylor series expansion to obtain tractable MR terms used in empirical specifications. Derivations follow Baier and Bergstrand (2009). From the theoretical derivations in section 3.2, MR terms are given by

$$\tilde{\Gamma}_i = \left[\sum_j \delta_j \left(\frac{\theta_{ij}}{\tilde{\Gamma}_j} \right)^{1-\sigma} \right]^{\frac{1}{1-\sigma}}, \quad (3.11)$$

$$\tilde{\Gamma}_j = \left[\sum_i \delta_i \left(\frac{\theta_{ij}}{\tilde{\Gamma}_i} \right)^{1-\sigma} \right]^{\frac{1}{1-\sigma}}, \quad (3.12)$$

where δ is N_i/N or N_j/N , respectively.

The first order Taylor series expansion of any function $f(x_i)$, centered at x , is given by $f(x_i) = f(x) + [f'(x)](x_i - x)$. I follow Baier and Bergstrand (2009) and center around symmetric migration frictions $\theta_{ij} = \theta$. I start by dividing both sides of equation (3.11) by a constant $\theta^{1/2}$:

$$\begin{aligned} \tilde{\Gamma}_i/\theta^{1/2} &= \left[\sum_j \delta_j (\theta_{ij}/\theta^{1/2})^{1-\sigma} / \tilde{\Gamma}_j^{1-\sigma} \right]^{\frac{1}{1-\sigma}} \\ &= \left[\sum_j \delta_j (\theta_{ij}/\theta)^{1-\sigma} / (\tilde{\Gamma}_j/\theta^{1/2})^{1-\sigma} \right]^{\frac{1}{1-\sigma}} \end{aligned} \quad (3.13)$$

I define $\hat{\Gamma}_i = \tilde{\Gamma}_i/\theta^{1/2}$, $\hat{\theta}_{ij} = \theta_{ij}/\theta$, and $\hat{\Gamma}_j = \tilde{\Gamma}_j/\theta^{1/2}$. Substituting these in the previous equation, I obtain

$$\hat{\Gamma}_i = \left[\sum_j \delta_j \left(\frac{\hat{\theta}_{ij}}{\hat{\Gamma}_j} \right)^{1-\sigma} \right]^{\frac{1}{1-\sigma}}. \quad (3.14)$$

It will later be useful to rewrite equation (3.14) as

$$e^{(1-\sigma)\ln \hat{\Gamma}_i} = \sum_j e^{\ln \delta_j} e^{(\sigma-1)\ln \hat{\Gamma}_j} e^{(1-\sigma)\ln \hat{\theta}_{ij}}, \quad (3.15)$$

where e is the natural logarithm operator. In a world with symmetric migration costs $\theta_{ij} = \theta$, connoting $\hat{\theta}_{ij} = 1$, the latter implies

$$\hat{\Gamma}_i^{1-\sigma} = \sum_j \delta_j \hat{\Gamma}_j^{\sigma-1}. \quad (3.16)$$

Multiplying both sides by $\hat{\Gamma}_i^{\sigma-1}$ yields

$$1 = \sum_j \delta_j (\hat{\Gamma}_i \hat{\Gamma}_j)^{\sigma-1}. \quad (3.17)$$

As noted in Feenstra (2004, p.158, footnote 11), the solution to this equation is $\hat{\Gamma}_i = \hat{\Gamma}_j = 1$. For this reason, under symmetric migration costs $\hat{\theta}_{ij} = \hat{\Gamma}_i = \hat{\Gamma}_j = 1$ and $\Gamma_i = \Gamma_j = \theta^{1/2}$.

A first-order log-linear Taylor series expansion of $\hat{\Gamma}_i$ from equation (3.15), analogue for $\hat{\Gamma}_j$, centered at $\hat{\theta} = \hat{\Gamma}_i = \hat{\Gamma}_j = 1$ yields

$$\ln \tilde{\Gamma}_i = - \sum_j \delta_j \ln \tilde{\Gamma}_j + \sum_j \delta_j \ln \theta_{ij} \quad (3.18)$$

and

$$\ln \tilde{\Gamma}_j = - \sum_i \delta_i \ln \tilde{\Gamma}_i + \sum_i \delta_i \ln \theta_{ij}. \quad (3.19)$$

Using $d[e^{(1-\sigma)\ln \hat{x}}] / d[\ln \hat{x}] = (1-\sigma)e^{(1-\sigma)\ln \hat{x}}$, some mathematical manipulation and assuming symmetry of migration costs, a solution to the above equations is

$$\ln \tilde{\Gamma}_i = \left[\sum_j \delta_j \ln \theta_{ij} - \frac{1}{2} \sum_k \sum_m \delta_k \delta_m \ln \theta_{km} \right] \quad (3.20)$$

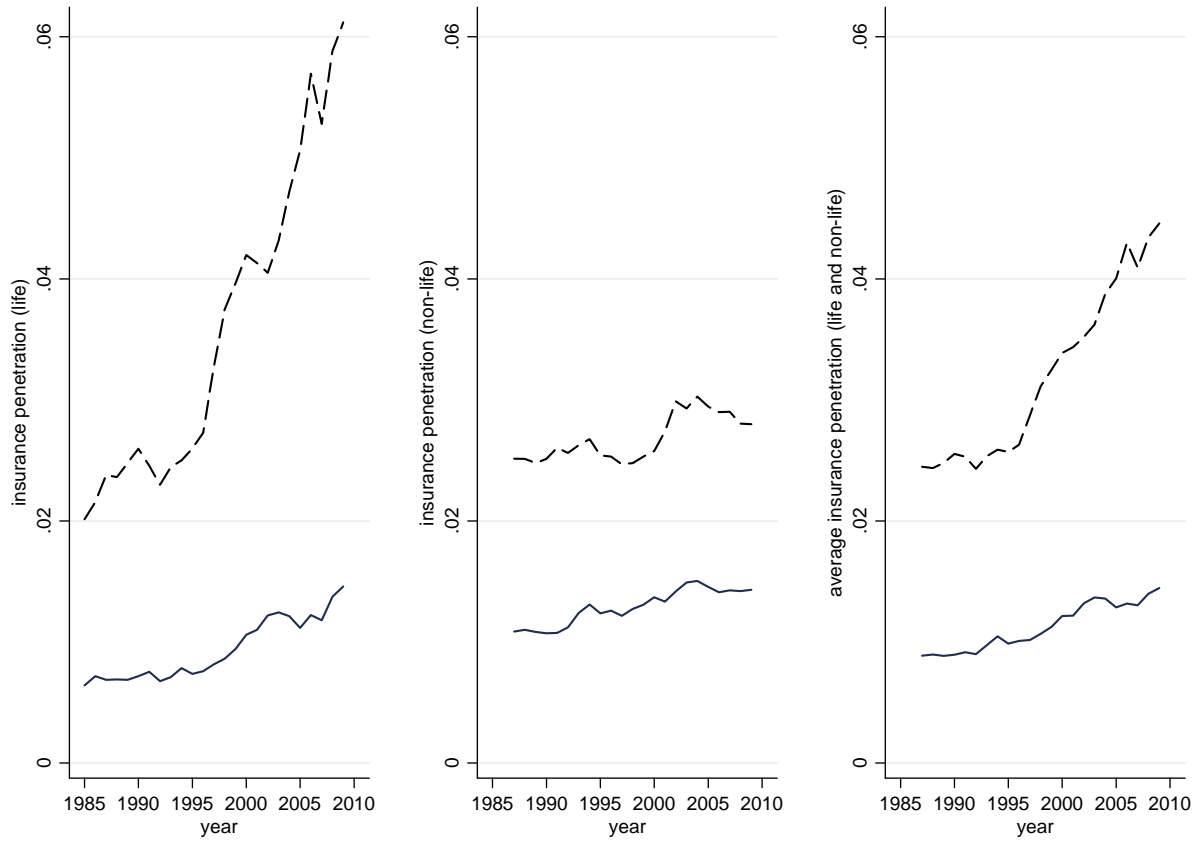
and

$$\ln \tilde{\Gamma}_j = \left[\sum_i \delta_i \ln \theta_{ij} - \frac{1}{2} \sum_k \sum_m \delta_k \delta_m \ln \theta_{km} \right], \quad (3.21)$$

where multilateral resistances are normalized by (the square root of) population weighted average migration frictions (the combined shock-cost measure).

3.B Appendix

Figure 3.1: Insurance Penetration by Income Level (1987-2009)



Note: The figure uses data from the World Bank Database on Financial Institutions for 1987 to 2009. The dotted line indicates the average insurance penetration for high income countries, while the solid line is for middle and low income countries. Insurance penetration is measured as life insurance premium volume as a share of GDP or nonlife insurance premium volume as a share of GDP, respectively.

Table 3.6: Summary Statistics and Data Sources

Variable	Obs.	Mean	St. Dev.	Data Source
Δ Migration $_t^{ij}$	40,956	3,341	38,541	Migration DRC (2007) & World Bank (2010)
Δ Migration $_t^{ij}$, return migration	40,956	4,026	40,639	Migration DRC (2007) & World Bank (2010)
Migration stocks $_t^{ij}$	40,956	8,788	101,204	Migration DRC (2007) & World Bank (2010)
Δ ln Migration $_t^{ij}$	32,430	3.947	2.961	Migration DRC (2007) & World Bank (2010)
ln Migration Stocks $_t^{ij}$	40,956	3.177	3.097	Migration DRC (2007) & World Bank (2010)
ln Migration Stocks $_{t-1}^{ij}$	35,037	4.541	3.080	Migration DRC (2007) & World Bank (2010)
ln (y_t^j/y_t^i)	40,956	0.415	2.184	WDI (2011)
ln (POP $_t^i \times$ POP $_t^j$)	40,956	32.307	2.143	WDI (2011)
ln (POP Dens $_t^i \times$ POP Dens $_t^j$)	40,956	7.544	2.074	WDI (2011)
Polity Index $_t^i$	40,956	0.544	0.370	Polity IV Project (2010)
Polity Index $_t^j$	40,956	0.617	0.378	Polity IV Project (2010)
Civil War $_t^i$	40,956	0.046	0.154	Intra-State War Data (v4.0)
Civil War $_t^j$	40,956	0.037	0.138	Intra-State War Data (v4.0)
Free Trade Area $_t^{ij}$	40,956	0.068	0.252	WTO
Regional Trade Agreement $_t^{ij}$	40,956	0.135	0.342	WTO
MRDist $_t^{ij}$	40,956	22.588	19.282	own calculation
MRAd $_t^{ij}$	40,956	-0.570	15.221	own calculation
Large Disaster $_t^i$	40,956	0.564	3.061	EM-DAT (2011)
Large Disaster $_t^j$	40,956	0.451	2.004	EM-DAT (2011)
Large Climate-related Disaster $_t^i$	40,956	0.524	2.841	EM-DAT (2011)
Large Climate-related Disaster $_t^j$	40,956	0.420	1.880	EM-DAT (2011)
Large Geophysical Disaster $_t^i$	40,956	0.040	0.264	EM-DAT (2011)
Large Geophysical Disaster $_t^j$	40,956	0.030	0.182	EM-DAT (2011)
Large Disaster Onset $_t^i$	40,956	0.559	3.034	EM-DAT (2011)
Large Disaster Onset $_t^j$	40,956	0.448	1.942	EM-DAT (2011)
Large Climate-related Disaster Onset $_t^i$	40,956	0.519	2.811	EM-DAT (2011)
Large Climate-related Disaster Onset $_t^j$	40,956	0.416	1.814	EM-DAT (2011)
Large Geophysical Disaster Onset $_t^i$	40,956	0.077	0.221	EM-DAT (2011)
Large Geophysical Disaster Onset $_t^j$	40,956	0.065	0.171	EM-DAT (2011)
Large Climatic Disaster Onset $_t^i$	40,956	0.204	1.355	EM-DAT (2011)
Large Climatic Disaster Onset $_t^j$	40,956	0.197	1.257	EM-DAT (2011)
Large Meteorological Disaster Onset $_t^i$	40,956	0.238	1.657	EM-DAT (2011)
Large Meteorological Disaster Onset $_t^j$	40,956	0.154	0.790	EM-DAT (2011)
Large Hydrological Disaster Onset $_t^i$	40,956	0.040	0.263	EM-DAT (2011)
Large Hydrological Disaster Onset $_t^j$	40,956	0.032	0.183	EM-DAT (2011)
Major Disaster Onset $_t^i$	40,956	0.496	3.284	EM-DAT (2011)
Major Disaster Onset $_t^j$	40,956	0.381	1.801	EM-DAT (2011)
Major Climate-related Disaster Onset $_t^i$	40,956	0.450	2.936	EM-DAT (2011)
Major Climate-related Disaster Onset $_t^j$	40,956	0.348	1.606	EM-DAT (2011)
Major Geophysical Disaster Onset $_t^i$	40,956	0.046	0.373	EM-DAT (2011)
Major Geophysical Disaster Onset $_t^j$	40,956	0.033	0.231	EM-DAT (2011)
Lagged Disaster Onset $_t^i$	40,956	0.062	0.302	EM-DAT (2011)
Lagged Disaster Onset $_t^j$	40,956	0.053	0.281	EM-DAT (2011)
Lagged Climate-related Disaster Onset $_t^i$	40,956	0.059	0.298	EM-DAT (2011)
Lagged Climate-related Disaster Onset $_t^j$	40,956	0.051	0.276	EM-DAT (2011)
Lagged Geophysical Disaster Onset $_t^i$	40,956	0.009	0.046	EM-DAT (2011)
Lagged Geophysical Disaster Onset $_t^j$	40,956	0.006	0.024	EM-DAT (2011)
Lagged Climatic Disaster Onset $_t^i$	40,956	0.029	0.206	EM-DAT (2011)
Lagged Climatic Disaster Onset $_t^j$	40,956	0.029	0.219	EM-DAT (2011)
Lagged Meteorological Disaster Onset $_t^i$	40,956	0.021	0.115	EM-DAT (2011)
Lagged Meteorological Disaster Onset $_t^j$	40,956	0.016	0.084	EM-DAT (2011)
Lagged Hydrological Disaster Onset $_t^i$	40,956	0.003	0.016	EM-DAT (2011)
Lagged Hydrological Disaster Onset $_t^j$	40,956	0.002	0.015	EM-DAT (2011)

Table 3.7: Migration and Large Natural Disasters, OECD versus non-OECD (1960-2010)

Dependent Variable: Δ bilateral migration from i to j

Origin	non-OECD	OECD	all	all	non-OECD	non-OECD	OECD	OECD
Destination	all	all	non-OECD	OECD	non-OECD	OECD	non-OECD	OECD
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Disaster Onset $_t^i$	0.022*** (0.01)	-0.000 (0.03)	0.017 (0.01)	0.034*** (0.01)	0.019 (0.01)	0.021*** (0.01)	0.023 (0.02)	0.007 (0.03)
Disaster Onset $_t^j$	-0.037** (0.02)	-0.055 (0.04)	0.023 (0.02)	-0.062*** (0.02)	0.043* (0.02)	-0.081*** (0.02)	-0.036 (0.03)	-0.021 (0.07)
Controls								
$\ln(y_t^j/y_t^i)$	0.191*** (0.07)	0.456*** (0.17)	0.404*** (0.09)	0.323** (0.14)	0.411*** (0.10)	0.050 (0.12)	0.749*** (0.17)	0.068 (0.29)
$\ln(\text{POP}_t^i \times \text{POP}_t^j)$	-6.289** (2.47)	-21.062* (11.14)	-7.617*** (2.85)	-6.522* (3.58)	-6.299** (3.11)	-5.291 (3.40)	-14.195*** (4.27)	-18.680 (71.63)
$\ln(\text{POP Dens}_t^i \times \text{POP Dens}_t^j)$	6.290** (2.48)	22.062** (11.19)	8.749*** (2.85)	7.533** (3.53)	7.595** (3.11)	4.992 (3.39)	14.343*** (4.29)	18.915 (71.90)
Polity Index $_t^i$	0.341* (0.18)	-0.418* (0.23)	0.131 (0.22)	0.244 (0.24)	0.197 (0.24)	0.404* (0.22)	0.380 (0.29)	-0.101 (0.39)
Polity Index $_t^j$	0.577* (0.34)	1.077*** (0.21)	0.896*** (0.35)	1.489*** (0.34)	0.878** (0.37)	1.229*** (0.44)	0.536 (0.34)	0.167 (0.29)
Civil War $_t^i$	0.110 (0.15)	-2.330** (1.10)	-0.137 (0.22)	0.390** (0.18)	-0.085 (0.22)	0.328** (0.14)	-0.725 (1.52)	-0.320 (1.20)
Civil War $_t^j$	-0.919** (0.44)	0.734* (0.43)	-0.820** (0.39)	3.195** (1.33)	-1.101** (0.45)	1.752 (1.48)	0.084 (0.38)	0.122 (2.07)
Free Trade Area $_{t-1}^{ij}$	0.547*** (0.19)	-0.842*** (0.28)	0.401** (0.16)	0.243 (0.25)	0.387** (0.17)	0.876*** (0.30)	-0.805 (0.70)	-0.132 (0.66)
Regional Trade Agreement $_{t-1}^{ij}$	-0.512*** (0.19)	1.501*** (0.41)	-0.281* (0.17)	0.113 (0.29)	-0.290* (0.17)	-0.799** (0.33)	0.873 (0.73)	0.281 (0.76)
\ln Migration Stock $_{t-1}^{ij}$	0.087*** (0.03)	-0.132** (0.06)	0.039 (0.05)	0.063 (0.06)	0.044 (0.05)	0.071* (0.04)	0.006 (0.05)	-0.031 (0.12)
Observations	33,808	7,148	29,390	11,566	23,814	9,994	5,576	1,572

Note: ***, **, * denote significance at the 1%, 5%, and 10% level, respectively. Country pair and time fixed effects, and MR terms included in all regressions but not reported. Robust standard errors reported in parenthesis. Disasters are the number of large-scale disasters according to the decision rule.

Table 3.8: Migration and Large Natural Disasters, by Income Group (1960-2010)

Dependent Variable: Δ bilateral migration from i to j

Origin	low	low	low	middle	middle	middle	high	high	high
Destination	low	middle	high	low	middle	high	low	middle	high
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Disaster Onset $_t^i$	-0.647 (0.69)	0.205** (0.09)	-0.051 (0.08)	0.044*** (0.02)	-0.009 (0.01)	0.029*** (0.01)	0.064 (0.05)	-0.001 (0.02)	0.026 (0.03)
Disaster Onset $_t^j$	-0.583 (0.40)	0.600*** (0.15)	-0.025 (0.04)	-0.115** (0.05)	0.104*** (0.03)	-0.032 (0.02)	0.409*** (0.13)	-0.016 (0.03)	-0.112*** (0.03)
Controls									
$\ln(y_t^j/y_t^i)$	0.674*** (0.22)	1.083*** (0.21)	0.110 (0.12)	0.606*** (0.20)	0.089 (0.19)	0.244* (0.13)	-0.091 (0.14)	0.627*** (0.20)	-0.189 (0.20)
$\ln(\text{POP}_t^i \times \text{POP}_t^j)$	3.356 (6.59)	2.565 (10.71)	-7.624* (4.38)	-9.228*** (2.63)	-12.274** (5.01)	-8.704** (3.86)	0.290 (5.35)	-10.740*** (2.08)	-15.018 (10.11)
$\ln(\text{POP Dens}_t^i \times \text{POP Dens}_t^j)$	-2.882 (6.21)	-1.591 (10.49)	7.061* (4.25)	12.637*** (2.78)	13.738*** (4.89)	8.773** (3.91)	-0.755 (5.20)	11.200*** (2.15)	16.116 (10.12)
Polity Index $_t^i$	-1.675*** (0.55)	-0.552* (0.34)	0.177 (0.26)	-0.537 (0.43)	1.386*** (0.41)	0.198 (0.26)	0.276 (0.37)	0.055 (0.28)	-0.090 (0.26)
Polity Index $_t^j$	-0.157 (0.29)	2.818*** (0.24)	-2.215*** (0.48)	0.532** (0.25)	1.098*** (0.42)	0.518 (0.79)	-0.870*** (0.29)	1.152*** (0.32)	1.656*** (0.32)
Civil War $_t^i$	0.305 (0.52)	-1.050** (0.44)	0.675 (0.64)	0.316 (0.29)	-0.449 (0.39)	0.174 (0.17)	0.155 (1.61)	-1.149 (1.42)	-2.596** (1.31)
Civil War $_t^j$	0.275 (0.83)	-1.075** (0.48)	0.529 (2.25)	-2.734*** (0.91)	-0.508 (0.60)	1.918 (1.51)	-0.073 (0.50)	0.438 (0.33)	2.586* (1.51)
Free Trade Area $_t^{ij}$	1.340*** (0.52)	0.949*** (0.32)	2.423*** (0.72)	-0.333 (0.25)	-0.164 (0.24)	0.455* (0.25)	-0.192 (0.66)	-0.411 (0.28)	0.164 (0.34)
Regional Trade Agreement $_t^{ij}$	0.062 (0.57)	-0.167 (0.20)	-0.221 (0.32)	-0.264 (0.31)	0.344 (0.33)	-0.389* (0.22)	0.466 (0.49)	0.740*** (0.28)	0.528 (0.33)
\ln Migration Stock $_{t-1}^{ij}$	-0.203*** (0.06)	0.249** (0.11)	-0.062 (0.04)	0.070 (0.10)	0.298*** (0.09)	0.083* (0.05)	0.188*** (0.07)	0.014 (0.05)	-0.111 (0.08)
Observations	2,107	4,042	3,875	3,551	8,725	7,979	2,112	4,756	3,809

Note: ***, **, * denote significance at the 1%, 5%, and 10% level, respectively. Country pair and time fixed effects, and MR terms included in all regressions but not reported. Robust standard errors reported in parenthesis. Disasters are the number of large-scale disasters according to the decision rule. Income group classification is according to the World Bank convention.

Table 3.9: Migration and Medium and Large Natural Disasters (1960-2010)

Dependent Variable: Δ bilateral migration stocks from i in j				
	(1)	(2)	(3)	(4)
Disaster Onset $_t^i$	0.023*** (0.01)			
Disaster Onset $_t^j$	0.007 (0.02)			
Climate-related Disaster Onset $_t^i$		0.025*** (0.01)		0.020*** (0.01)
Climate-related Disaster Onset $_t^j$		0.005 (0.02)		-0.033 (0.02)
Geophysical Disaster Onset $_t^i$			0.209*** (0.07)	0.050 (0.13)
Geophysical Disaster Onset $_t^j$			2.443*** (0.94)	3.011*** (1.07)
Controls				
$\ln(y_t^j/y_t^i)$	0.358*** (0.07)	0.358*** (0.07)	0.355*** (0.07)	0.368*** (0.07)
$\ln(\text{POP}_t^i \times \text{POP}_t^j)$	-7.605*** (2.64)	-7.622*** (2.62)	-7.143*** (2.77)	-7.472*** (2.63)
$\ln(\text{POP Dens}_t^i \times \text{POP Dens}_t^j)$	8.097*** (2.65)	8.109*** (2.64)	7.688*** (2.79)	7.989*** (2.65)
Polity Index $_t^i$	0.145 (0.18)	0.147 (0.19)	0.110 (0.18)	0.146 (0.19)
Polity Index $_t^j$	0.730*** (0.25)	0.728*** (0.25)	0.698*** (0.25)	0.640*** (0.25)
Civil War $_t^i$	0.070 (0.16)	0.078 (0.16)	-0.014 (0.16)	0.057 (0.15)
Civil War $_t^j$	-0.262 (0.40)	-0.262 (0.40)	-0.400 (0.41)	-0.432 (0.42)
Free Trade Area $_t^{ij}$	0.474*** (0.15)	0.475*** (0.15)	0.443*** (0.14)	0.457*** (0.15)
Regional Trade Agreement $_t^{ij}$	-0.353** (0.17)	-0.352** (0.17)	-0.360** (0.17)	-0.354** (0.17)
\ln Migration Stock $_{t-1}^{ij}$	0.089** (0.04)	0.089** (0.04)	0.080* (0.04)	0.081* (0.04)
Observations	40,956	40,956	40,956	40,956

Note: ***, **, * denote significance at the 1%, 5%, and 10% level, respectively. Country pair and time fixed effects, and MR terms included in all regressions but not reported. Robust standard errors reported in parenthesis. Disasters are the number of medium-sized and large disasters according to the decision rule.

Table 3.10: Migration and Combined Disaster Effects (1960-2010)

Dependent Variable: Δ bilateral migration from i in j		
	(1)	(2)
Disaster Onset $_t^i$	0.027*** (0.01)	
Disaster Onset $_t^j$	-0.010 (0.02)	
Climate-related Disaster Onset $_t^i$		0.018** (0.01)
Climate-related Disaster Onset $_t^j$		-0.061** (0.03)
Geophysical Disaster Onset $_t^i$		0.122 (0.11)
Geophysical Disaster Onset $_t^j$		2.941** (1.24)
$D_j^i \times D_t^j$ (dummy)	-0.104 (0.11)	-0.109 (0.11)
Controls		
$\ln(y_t^j/y_t^i)$	0.360*** (0.07)	0.349*** (0.07)
$\ln(\text{POP}_t^i \times \text{POP}_t^j)$	-8.014*** (2.56)	-8.126*** (2.57)
$\ln(\text{POP Density}_t^i \times \text{POP Density}_t^j)$	8.430*** (2.57)	8.535*** (2.58)
Polity Index $_t^i$	0.174 (0.19)	0.153 (0.19)
Polity Index $_t^j$	0.705*** (0.26)	0.664*** (0.26)
Civil War $_t^i$	0.141 (0.15)	0.074 (0.14)
Civil War $_t^j$	-0.290 (0.40)	-0.366 (0.40)
Free Trade Area $_t^{ij}$	0.465*** (0.15)	0.489*** (0.16)
Regional Trade Agreement $_t^{ij}$	-0.318* (0.17)	-0.334* (0.18)
\ln Migration Stock $_{t-1}^{ij}$	0.088** (0.04)	0.089** (0.04)
Observations	40,956	40,956

Note: ***, **, * denote significance at the 1%, 5%, and 10% level, respectively. Country pair and time fixed effects, and MR terms included in all regressions but not reported. Robust standard errors reported in parenthesis. Disasters are the number of large-scale disasters according to the decision rule.

Table 3.11: Return Migration and Large Natural Disasters (1960-2010)

Dependent Variable: Δ bilateral migration from i in j , return migration				
	(1)	(2)	(3)	(4)
Disaster Onset $_t^i$	0.020*** (0.01)			
Disaster Onset $_t^j$	-0.035*** (0.01)			
Climate-related Disaster Onset $_t^i$		0.021*** (0.01)	0.009 (0.01)	
Climate-related Disaster Onset $_t^j$		-0.037** (0.01)	-0.038* (0.02)	
Geophysical Disaster Onset $_t^i$			0.187 (0.12)	0.114 (0.13)
Geophysical Disaster Onset $_t^j$			-0.015 (0.15)	-0.118 (0.14)
Climatic Disaster Onset $_t^i$				0.236* (0.12)
Climatic Disaster Onset $_t^j$				-0.304** (0.15)
Meteorological Disaster Onset $_t^i$				-0.011 (0.02)
Meteorological Disaster Onset $_t^j$				0.024 (0.03)
Hydrological Disaster Onset $_t^i$				0.001 (0.01)
Hydrological Disaster Onset $_t^j$				-0.090** (0.04)
Controls				
$\ln(y_t^j/y_t^i)$	0.294*** (0.06)	0.292*** (0.06)	0.297*** (0.06)	0.284*** (0.06)
$\ln(\text{POP}_t^i \times \text{POP}_t^j)$	-8.398*** (2.22)	-8.399*** (2.22)	-8.493*** (2.21)	-7.945*** (2.40)
$\ln(\text{POP Dens}_t^i \times \text{POP Dens}_t^j)$	8.724*** (2.23)	8.720*** (2.22)	8.848*** (2.22)	8.339*** (2.40)
Polity Index $_t^i$	0.172 (0.20)	0.169 (0.19)	0.162 (0.20)	0.110 (0.19)
Polity Index $_t^j$	0.053 (0.28)	0.050 (0.28)	0.065 (0.27)	0.119 (0.26)
Civil War $_t^i$	-0.117 (0.25)	-0.116 (0.26)	-0.160 (0.26)	-0.222 (0.26)
Civil War $_t^j$	-0.342 (0.32)	-0.345 (0.32)	-0.336 (0.32)	-0.343 (0.32)
Free Trade Area $_t^{ij}$	0.579*** (0.19)	0.577*** (0.19)	0.607*** (0.19)	0.625*** (0.17)
Regional Trade Agreement $_t^{ij}$	-0.483** (0.21)	-0.478** (0.21)	-0.511** (0.22)	-0.557*** (0.20)
\ln Migration Stock $_{t-1}^{ij}$	0.144*** (0.04)	0.145*** (0.04)	0.143*** (0.04)	0.138*** (0.04)
Observations	55,327	55,327	55,327	55,327

Note: ***, **, * denote significance at the 1%, 5%, and 10% level, respectively. Country pair and time fixed effects, and MR terms included in all regressions but not reported. Robust standard errors reported in parenthesis. Disasters are the number of large-scale disasters according to the decision rule.

Table 3.12: Migration and Large Natural Disasters, Linear Specification (1960-2010)

Dependent Variable: $\Delta \ln$ bilateral migration from i in j				
	(1)	(2)	(3)	(4)
Disaster Onset _t ⁱ	0.020***			
	(0.00)			
Disaster Onset _t ^j	-0.002			
	(0.01)			
Climate-related Disaster Onset _t ⁱ		0.021***	0.017***	
		(0.01)	(0.01)	
Climate-related Disaster Onset _t ^j		-0.006	-0.048***	
		(0.01)	(0.01)	
Geophysical Disaster Onset _t ⁱ			0.053	0.029
			(0.07)	(0.08)
Geophysical Disaster Onset _t ^j			0.536***	0.578***
			(0.07)	(0.07)
Climatic Disaster Onset _t ⁱ				-0.056
				(0.05)
Climatic Disaster Onset _t ^j				0.002
				(0.05)
Meteorological Disaster Onset _t ⁱ				0.039**
				(0.02)
Meteorological Disaster Onset _t ^j				-0.022*
				(0.01)
Hydrological Disaster Onset _t ⁱ				0.017**
				(0.01)
Hydrological Disaster Onset _t ^j				-0.181***
				(0.03)
Controls				
$\ln(y_t^j/y_t^i)$	0.258***	0.258***	0.253***	0.256***
	(0.02)	(0.02)	(0.02)	(0.02)
$\ln(\text{POP}_t^i \times \text{POP}_t^j)$	-4.988***	-4.976***	-4.883***	-4.901***
	(0.86)	(0.86)	(0.85)	(0.85)
$\ln(\text{POP Density}_t^i \times \text{POP Density}_t^j)$	4.737***	4.726***	4.635***	4.660***
	(0.86)	(0.86)	(0.85)	(0.85)
Polity Index _t ⁱ	-0.030	-0.030	-0.030	-0.032
	(0.05)	(0.05)	(0.05)	(0.05)
Polity Index _t ^j	-0.082	-0.082	-0.067	-0.058
	(0.05)	(0.05)	(0.05)	(0.05)
Civil War _t ⁱ	-0.035	-0.033	-0.037	-0.025
	(0.06)	(0.06)	(0.06)	(0.06)
Civil War _t ^j	-0.136**	-0.138**	-0.149**	-0.102
	(0.07)	(0.07)	(0.07)	(0.07)
Free Trade Area _t ^{ij}	0.170***	0.169***	0.168***	0.159***
	(0.06)	(0.06)	(0.06)	(0.06)
Regional Trade Agreement _t ^{ij}	-0.117**	-0.117**	-0.116**	-0.106**
	(0.05)	(0.05)	(0.05)	(0.05)
\ln Migration Stock _{t-1} ^{ij}	0.089***	0.089***	0.090***	0.087***
	(0.01)	(0.01)	(0.01)	(0.01)
Observations	35,479	35,479	35,479	35,479
R ² within	0.235	0.235	0.237	0.239

Note: ***, **, * denote significance at the 1%, 5%, and 10% level, respectively. Constant, country pair and time fixed effects, and MR terms included in all regressions but not reported. Robust standard errors reported in parenthesis. Disasters are the number of large-scale disasters according to the decision rule. Estimations are conducted using a linear fixed effects specification.

Table 3.13: Migration Stocks and Large Natural Disasters (1960-2010)

Dependent Variable: bilateral migration stocks from i in j				
	(1)	(2)	(3)	(4)
Lagged Disaster Onset $_t^i$	0.100**			
	(0.04)			
Lagged Disaster Onset $_t^j$	0.034			
	(0.07)			
Lagged Climate-related Disaster Onset $_t^i$	0.100**	0.092**		
	(0.04)	(0.04)		
Lagged Climate-related Disaster Onset $_t^j$	0.031	0.054		
	(0.06)	(0.08)		
Lagged Geophysical Disaster Onset $_t^i$			-0.364	0.207
			(0.57)	(0.69)
Lagged Geophysical Disaster Onset $_t^j$			1.054	-1.299
			(2.00)	(2.48)
Lagged Climatic Disaster Onset $_t^i$				0.218
				(0.20)
Lagged Climatic Disaster Onset $_t^j$				0.215
				(0.56)
Lagged Meteorological Disaster Onset $_t^i$				-0.000
				(0.04)
Lagged Meteorological Disaster Onset $_t^j$				0.190**
				(0.08)
Lagged Hydrological Disaster Onset $_t^i$				0.234**
				(0.10)
Lagged Hydrological Disaster Onset $_t^j$				-1.051***
				(0.23)
Controls				
$\ln(y_t^j/y_t^i)$	0.094**	0.095**	0.096**	0.147***
	(0.05)	(0.05)	(0.04)	(0.03)
$\ln(\text{POP}_t^i \times \text{POP}_t^j)$	-3.450**	-3.449**	-3.562**	-3.849***
	(1.64)	(1.64)	(1.61)	(1.48)
$\ln(\text{POP Density}_t^i \times \text{POP Density}_t^j)$	3.503**	3.502**	3.619**	4.026***
	(1.65)	(1.65)	(1.61)	(1.47)
Polity Index $_t^i$	0.102	0.103	0.100	0.159
	(0.11)	(0.11)	(0.11)	(0.11)
Polity Index $_t^j$	0.091	0.090	0.091	0.107
	(0.13)	(0.13)	(0.13)	(0.12)
Civil War $_t^i$	-0.183	-0.178	-0.169	-0.063
	(0.13)	(0.13)	(0.13)	(0.08)
Civil War $_t^j$	-0.265	-0.264	-0.288	-0.458**
	(0.21)	(0.21)	(0.19)	(0.20)
Free Trade Area $_t^{ij}$	-0.053	-0.053	-0.041	0.155*
	(0.17)	(0.17)	(0.18)	(0.09)
Regional Trade Agreement $_t^{ij}$	0.054	0.054	0.043	-0.095
	(0.18)	(0.18)	(0.20)	(0.10)
\ln Migration Stock $_{t-1}^{ij}$	0.361***	0.361***	0.360***	0.330***
	(0.05)	(0.05)	(0.05)	(0.04)
Observations	67,484	67,484	67,484	67,484

Note: ***, **, * denote significance at the 1%, 5%, and 10% level, respectively. Country pair and time fixed effects, and MR terms included in all regressions but not reported. Robust standard errors reported in parenthesis. Disasters are the number of large-scale disasters according to the decision rule, lagged by one year.

Table 3.14: Pure Physical Disaster Measure and Migration (1980-2010)

Dependent Variable: Δ bilateral migration from i in j				
Disaster Measure:	Simple		Onset	
	(1)	(2)	(3)	(4)
Disaster $_t^i$	0.031** (0.01)		0.029** (0.01)	
Disaster $_t^j$	-0.027 (0.02)		-0.041*** (0.01)	
Climate-related Disaster $_t^i$		0.051*** (0.01)		0.036** (0.01)
Climate-related Disaster $_t^j$		-0.103*** (0.02)		-0.075*** (0.01)
Geophysical Disaster $_t^i$		-0.047* (0.03)		-0.106* (0.06)
Geophysical Disaster $_t^j$		0.174*** (0.03)		0.457*** (0.04)
Controls				
$\ln(y_t^j/y_t^i)$	0.196*** (0.02)	0.200*** (0.02)	0.196*** (0.02)	0.190*** (0.02)
$\ln(\text{POP}_t^i \times \text{POP}_t^j)$	-4.577*** (0.88)	-4.512*** (0.88)	-4.573*** (0.88)	-4.512*** (0.87)
$\ln(\text{POP Dens}_t^i \times \text{POP Dens}_t^j)$	4.231*** (0.88)	4.205*** (0.88)	4.231*** (0.88)	4.201*** (0.87)
Polity Index $_t^i$	-0.005 (0.05)	0.014 (0.05)	-0.005 (0.05)	0.006 (0.05)
Polity Index $_t^j$	-0.017 (0.06)	-0.061 (0.06)	-0.022 (0.06)	-0.052 (0.06)
Civil War $_t^i$	-0.092 (0.07)	-0.081 (0.07)	-0.090 (0.07)	-0.079 (0.07)
Civil War $_t^j$	0.025 (0.09)	0.030 (0.09)	0.026 (0.09)	0.047 (0.09)
Free Trade Area $_t^{ij}$	0.159** (0.06)	0.192*** (0.06)	0.160** (0.06)	0.188*** (0.06)
Regional Trade Agreement $_t^{ij}$	-0.128** (0.05)	-0.168*** (0.05)	-0.132** (0.05)	-0.165*** (0.05)
\ln Migration Stock $_{t-1}^{ij}$	0.021* (0.01)	0.018 (0.01)	0.021* (0.01)	0.016 (0.01)
Observations	29,988	29,988	29,988	29,988

Note: ***, **, * denote significance at the 1%, 5%, and 10% level, respectively. Country pair and time fixed effects, and MR terms included in all regressions but not reported. Robust standard errors reported in parenthesis. Disasters are the number of the 10% largest disasters experienced within a country. Physical disasters measures are available from 1979-2010. For details on the physical disaster intensity measure data set see Felbermayr and Gröschl (2012).

Chapter 4

Natural Disasters and the Effect of Trade on Income: A New Panel IV Approach*

4.1 Introduction

Does trade openness 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, particularly 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 simultaneous determination 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 and has been applied in many different contexts.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. In particular, the effect of openness is not robust to the inclusion of

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¹ Hall and Jones (1999); Chakrabarti (2000); Dollar and Kraay (2002); Irwin and Terviö (2002); Easterly and Levine (2003); Persson and Tabellini (2005); Alcalá and Ciccone (2004); Redding and Venables (2004); Noguera 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 3,467 times in research papers (November 11, 2012).

distance to the equator. Rodrik et al. (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 country characteristics – such as distance to the equator, historical factors going back to colonialism, or climatic conditions. However, the F&R-instrument is not applicable in the panel setup since geography does not vary across time.²

Gassebner et al. (2010) and Oh and Reuveny (2010) have observed empirically that natural disasters, such as volcanic eruptions, earthquakes, or storm floods affect countries' bilateral trade. Presumably, international trade allows countries to smooth out the effects of temporary output shocks. Building on this fact, we can follow F&R in using a gravity-type equation to predict exogenous variation in bilateral trade and aggregate this up to the country level to obtain an instrument for multilateral openness. Because GDP and domestic disasters must be excluded in the trade flow equation, we talk about a 'modified' gravity model.

To ensure the validity of the exclusion restriction, we use only natural disasters in *foreign* countries for the instrument. Geographical and historical determinants (and all other time-invariant country characteristics) are taken care of by fixed effects. The key identifying assumption is that – conditional on controls – foreign disasters have no effect on domestic GDP per capita other than through international transactions, i.e., the extent of openness.

These are our three key empirical results: First, using a theory-consistent gravity model, a large disaster increases the affected country's imports by 2 percent on average. The effect is stronger if the country is close to a major financial center and may be negative if the country is financially remote (i.e., if it cannot borrow internationally). Exports typically fall, but they fall by less if 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 shocks, we find a significant impact of foreign disasters on bilateral openness. Using this regression as a 'data reduction device', and aggregating over predicted bilateral openness, we construct an instrument for openness. The correlation of the instrument with observed

² Alternatively GMM-based approaches are used, e.g. Greenaway et al. (2002), or Lederman and Maloney (2003).

openness is high, signaling *relevance*. And, since its variation across time and countries is due to shocks unrelated to income, it is also *valid*.

Third, evaluated at the mean, the elasticity of income with respect to trade is about 0.33 in the non-instrumented and about 0.74 in the instrumented regressions. As in F&R, our results suggest that measurement error is substantial relative to the endogeneity bias. Alternatively, it is possible that the effect of openness on income is heterogeneous and that our instrument identifies the ‘local average treatment effect’ for a sub-group of countries (see Angrist and Pischke, 2009) for which openness has a large effect.³ Since our IV strategy relies on variation in the incidence of foreign disasters (and population) in a bilateral trade equation, we can include the direct effect of domestic disasters in the two stage least square (2SLS) regression. The direct effect of disasters on GDP per capita is zero on average, but strongly negative for small and/or remote countries. Interestingly, we find that the elasticity of openness on income is substantially larger than the average in the poor half of the sample and in non-OECD countries.

Related Literature. A large number of papers⁴ have applied and refined the instrument of F&R, while others have criticized the approach.⁵ Irwin and Terviö (2002) confirm F&R’s results but notice that the findings are not robust to the inclusion of distance from the equator. Noguer and Siscart (2005) also revisit F&R. In contrast to earlier studies, they 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 across German states due to the fall of the Berlin Wall. 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). The author proposes a time-varying geography-based instrument for trade. 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. The advantage of our approach over Feyrer’s is that natural disasters such as earthquakes, hurricanes or

³ We are grateful to an anonymous referee for pointing out this possible explanation.

⁴ 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.

⁵ See Rodrik et al. (2004), or Rodriguez and Rodrik (2001) for critical papers discussed above.

volcanic eruptions are beyond doubt exogenous to countries' GDP per capita, while the availability of airport infrastructure in a country pair not necessarily is.

A small, mostly empirical literature studies the effects of natural disasters on international transactions and economic outcomes. Yang (2008) reports that hurricanes lead to financial flows into developing countries, helping them to increase imports to buffer income losses. Skidmore and Toya (2007) and Noy (2009) document the importance of greater financial and trade openness for countries' capacity to overcome natural disasters. Sahin (2011) uses a CGE model. He finds that disasters affect bilateral and multilateral trade. To our knowledge, Gassebner et al. (2010) and Oh and Reuveny (2010) present the only empirical gravity studies of international trade that incorporates natural disasters. They find significant effects of natural disasters on *bilateral* trade. The present article builds on this finding and shows empirically that the incidence of disasters provides sufficient variation on the *multilateral* level to help identify the effect of openness on GDP per capita.

The paper is structured as follows. Section 4.2 highlights the various effects of natural disasters on trade. Section 4.3 describes the empirical strategy and the data. Section 4.4 uses a theory-consistent gravity equation to study the effects of disasters on trade. The 'modified' gravity model uses only exogenous variables to predict bilateral openness. Section 4.5 uses the instrument to estimate the effect of openness on income per capita. Section 4.7 concludes.

4.2 Natural Disasters and Trade

Before presenting our data and results, we briefly review theoretical mechanisms that link disasters and trade. For a more complete account, we refer the reader to Yang (2008)'s model of international risk-sharing in the presence of natural disasters.

The key mechanism is as follows. Let natural disasters represent *idiosyncratic* (i.e., country-specific) output shocks and let consumers be risk averse. Then, if there is a Pareto-efficient allocation of risk across individual countries in a risk-sharing agreement, individual consumption should not be affected by disasters. Rather, individual countries' consumption levels depend only on mean world output. In other words, countries face only *aggregate* output risk. Deviations between individual output and consumption are perfectly smoothed by international flows of goods (and, consequently, of finance): an affected country runs a

current account deficit for the period during which its output is temporarily depressed, and a surplus thereafter; its trade partners display mirror positions. If the situation of countries before a shock was one of balanced trade (zero current account surplus), the shock will have triggered intertemporal trade that would otherwise not have taken place, thereby making countries (both the affected and the non-affected) more open. The intertemporal budget constraint ensures that this openness effect is permanent.⁶

In practice, the effect of disasters on current accounts depends on whether idiosyncratic risk or aggregate risk dominates; this determines the extent to which consumption can be smoothed. Many of the events studied below – earthquakes, volcano eruptions, storms – are local phenomena, so that some consumption smoothing is expected. Moreover, even if ex ante risk-sharing arrangements are incomplete, countries can use ex post mechanisms such as international borrowing or asset sales to smooth consumption.⁷

Whether the smoothing mechanism can operate depends on countries' access to international goods and financial markets. Therefore, one would expect that, in a gravity model of trade, geographical frictions to the flow of goods (such as distance) or barriers to financial integration shape the effect of disasters on trade.⁸ Similarly, the extent of the affected country's home market also matters: the larger and more diverse it is, the more can internal trade help smooth consumption.

Summarizing, standard theory provides arguments for natural disasters to matter for bilateral and multilateral trade and openness. Whether that effect is negative or positive is, however, irrelevant for our empirical strategy. All we require is that disasters do have *some effect* on openness.

⁶ If a country runs a trade surplus before the disaster, it may achieve consumption smoothing by curtailing exports rather than increasing imports, depending, of course, on the degree of substitution between exported and imported goods. Also, as discussed by Gassebner et al. (2010), disasters may specifically destroy transport infrastructure, leading to reduced openness.

⁷ E.g., Frankel (2010) shows how migrants' remittances can help smooth consumption.

⁸ While Gassebner et al. (2010) do not provide evidence on these interactions, they show that democratic institutions – which correlate positively with the depth of financial markets – do indeed shape the trade effect of disasters in the expected way.

4.3 Empirical Strategy and Data

4.3.1 Second Stage Regression

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

$$\ln \bar{y}_\tau^i = \beta OPEN_\tau^i + \pi \ln POP_\tau^i + \sum_{s \leq \tau} \chi_s D_s^i + \nu^i + \nu_\tau + \varepsilon_\tau^i, \quad (4.1)$$

where we use τ to denote 5-year averages to purge the data from the influence of business cycles. The relationship explains log per capita income in purchasing power parity terms \bar{y}_τ^i as a function of openness to international trade ($OPEN_\tau^i$), 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 domestic natural disasters on per capita income.⁹ 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, i.e., distance to the equator or settler mortality). Common period effects are controlled for by including period dummies ν_τ .

It is well understood that $OPEN_\tau^i$ and the error term ε_τ^i in equation (4.1) 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 (4.1) by OLS will bias the estimate of β upwards. Instrumentation also solves a second issue, namely the fact that $OPEN_\tau^i$ is likely to be a noisy proxy for the true role that trade plays for the determination of per capita income. OLS estimates will therefore be biased downward. The third reason is *omitted variable bias*. F&R estimate a cross-section and instrument $OPEN_\tau^i$ by its geographical component. Rodriguez and Rodrik (2001) and others have shown that F&R's estimates are 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. However,

⁹ Note that we use only foreign disasters in the construction of the instrument and exclude domestic ones. The terms $\sum_{s \leq \tau} \chi_s D_s^i$, ν^i and ν_τ are absent in F&R's equation (4).

F&R's original instrument cannot be employed in a panel setup. The present section of this paper proposes an instrument for openness that has time variation. The starting point is that disasters affect countries' trade flows.

4.3.2 Standard Gravity

Before we describe the construction of our instrument, we use a standard gravity regression to show that disasters exert an economically significant effect on trade.¹⁰ We estimate the model using the Pseudo Poisson Maximum Likelihood (PPML) approach advocated by Santos Silva and Tenreyro (2006) to account for zero trade flows. Zeros make up more than 50 percent of observations in early years of our sample (1950s) and remain important afterwards. Noguer and Siscart (2005) have shown that out-of-sample predictions make the F&R instrument less precise. Thus, accounting for zeros is important.¹¹

Following the theoretical considerations of 4.2, the presumption is that the direct effect of a disaster in i on imports is positive, while the effect of a disaster in j is negative. However, that effect is conditioned by openness to international finance and trade. So, amongst other things, we include interaction terms with financial and geographical remoteness. We estimate a gravity equation of the form

$$M_t^{ij} = \exp [\delta_1 D_t^i + \delta_2 D_t^j + \gamma'_1 (\mathbf{\Gamma}_t^{ij} \times D_t^i) + \gamma'_2 (\mathbf{\Gamma}_t^{ij} \times D_t^j) + \boldsymbol{\xi}' \mathbf{X}_t^{ij} + \nu^{ij} + \nu_t] + \varepsilon_t^{ij}, \quad (4.2)$$

with $\mathbf{\Gamma}_t^{ij} = [\ln FINDIST^i, \ln FINDIST^j; \ln DIST^{ij}; \ln(y_t^i/y_t^j)]$. $FINDIST$ is Rose and Spiegel's (2009) measure of a country's international financial remoteness, $DIST^{ij}$ denotes geographical distance between countries i and j , y_t^i/y_t^j is the ratio of importer to exporter per capita GDP. $\mathbf{X}_t^{ij} = [\ln GDP_t^i, \ln GDP_t^j; \ln(y_t^i/y_t^j); FTA_t^{ij}, CU_t^{ij}, WTO_t^{ij}; MRDIST_t^{ij}, MRADJ_t^{ij}]$ contains the GDPs of country i and j , their ratio of GDP per capita, 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 distance ($MRDIST_t^{ij}$) and adjacency ($MRADJ_t^{ij}$). We run a conditional fixed effects Poisson (FE

¹⁰ We include covariates suggested by Gassebner et al. (2010), Oh and Reuveny (2010) or Sahin (2011). However, our specification is different: we use PPML methods and control for multilateral resistance by including country fixed-effects.

¹¹ Besides accounting for zeros, Santos Silva and Tenreyro (2006) show that the PPML model is resilient to rounding errors in the trade variable, that is, its inconsistency is very small. See Egger and Larch (2011) for a recent application in the panel context.

PPML) model where we include a dummy (ν^{ij}) for each country pair to account for all time-invariant bilateral determinants of trade. ν_t is a year effect.¹² The country pair effects nest country dummies and control for the time-invariant component of multilateral remoteness (MR); see Anderson and Van Wincoop (2003). However, over a long period of time, MR terms do change. We follow Baier and Bergstrand (2009), who derive theory-consistent MR indices from a Taylor series expansion of the Anderson and Van Wincoop (2003) gravity equation.¹³

4.3.3 Instrument Construction

The construction of the instrument follows F&R (1999): using a ‘modified’ gravity equation as a ‘data reduction device’,¹⁴ 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 country i ’s real per capita income, such as natural disasters in foreign countries and interaction of these disasters with bilateral geographical variables, or population. Then, we construct an exogenous proxy for multilateral openness Ω_t^i based on predicted bilateral openness

$$\Omega_t^i = \sum_{j \neq i} \hat{\omega}_t^{ij}. \quad (4.3)$$

Averaging over 5-year intervals, we obtain our instrument Ω_τ^i .

Our bilateral openness equation is based on equation (4.2). It differs from the standard gravity equation in that it excludes variables that would be correlated to income shocks such as GDP or domestic disasters. However, we continue to use PPML. Our preferred specification includes importer, exporter and year dummies.¹⁵ We estimate the relationship on yearly data, but will judge the validity of the resulting instrument based on five-year

¹² We estimate the variance-covariance matrix using a heteroskedasticity robust estimator that allows for clusters at the dyadic level. This is strongly recommended by Stock and Watson (2008) to avoid inconsistent estimates due to serial correlation.

¹³ Wooldridge (2002), p. 676, emphasizes that “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 in gravity models. See Liu (2009) for a recent example of a gravity model estimated using a conditional fixed effects PPML strategy. See Carrere (2006) for a more general discussion of the gravity equation and estimation issues.

¹⁴ We thank an anonymous referee for suggesting this terminology.

¹⁵ Alternatively, one could also include country-pair fixed effects. This does, however, not lead to a superior instrument but is computationally more expensive.

averages.¹⁶ We estimate

$$\omega_t^{ij} = \exp [\delta_3 D_t^j + \gamma'_3 (\Phi_t^{ij} \times D_t^j) + \zeta' \mathbf{Z}_t^{ij} + \nu^i + \nu^j + \nu_t] + \varepsilon_t^{ij}, \quad (4.4)$$

where $\Phi_t^{ij} = [\ln FINDIST^j; \ln AREA^j; \ln POP_t^j; ADJ^{ij}]$ contains financial remoteness, area, population and adjacency, while $\mathbf{Z}_t^{ij} = [\ln POP_t^i, \ln POP_t^j; \ln DIST_t^j; ADJ^{ij}]$ contains exogenous controls such as population and geographical variables (distance and adjacency) based on F&R (1999). As before, interactions of disasters with financial remoteness and geographical variables are motivated by the insights derived from the related literature.¹⁷ As a large part of countries' trade takes place with their immediate neighbors (F&R, 1999) and disasters might hit bordering countries alike, we include an interaction of disasters with adjacency.

The identifying assumption is that, conditional on second stage controls, *foreign* disasters, population, and bilateral geographic variables have no effect on *domestic* GDP per capita other than through openness. For the construction of the instrument Ω_t^i , we require exogeneity of regressors in (4.4). The quality of the instrument depends solely on its conditional correlation with observed openness. Hence, we design the bilateral openness equation to maximize conditional correlation between observed openness and constructed instrument.

4.3.4 Data

Data on natural disasters come from the *Emergency Events Database* EM-DAT maintained by the Center for Research on the Epidemiology of Disasters. While the database reports natural and technological disasters, 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 economic factors. These are 'large' earthquakes, volcanic eruptions, tsunamis, storms, storm floods, and droughts.¹⁸ The qualification 'large' makes sure that a disaster is of a sufficiently large dimension not to be caused by local determinants but rather by global phenomena. We define 'large' disasters as events that (i)

¹⁶ PPML is useful since 26 percent of our observations come with zero-trade flows. So, out-of-sample predictions are minimized (Noguer and Siscart, 2005). Using five-year averages in the gravity stage does not change results.

¹⁷ Interactions with surface area and population acknowledge the fact that economic density matters for the aggregate damage caused by a natural disaster.

¹⁸ Hence, we disregard extreme temperature, floods, insect infestations, (mud)slides, and wildfires. EM-DAT also classifies epidemics as disasters; we exclude them from our analysis.

caused 1,000 or more deaths; or (ii) injured 1,000 or more persons; or (iii) affected 100,000 or more persons. This leaves us with a total of 5,704 disasters between 1950 and 2008 in our dataset, 1,091 thereof are large in scale. In our robustness checks, we work with alternative definitions of disasters, such as a broader specification of disasters that includes *all* kinds of natural disasters¹⁹ or counting *all* sizes of disasters (large and small).

Over the period 1992-2008 (where the number of countries has been fairly stable), countries differ quite substantially with respect to the incidence of natural disasters. Normalizing by surface area, we observe that countries in Asia and at the Pacific rim are more strongly affected (compare Figure 4.1 in Appendix 4.A). This is not surprising, given the geological characteristics.

Data on nominal imports and exports measured in current US dollars come from the IMF's *Direction of Trade Statistics* (DoTS). Nominal GDP data in current US dollars 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, and common border – are taken from CEPII's *Geographic and Bilateral Distance Database*. As a measure of international financial remoteness, we use the logarithm of great-circle distance to the closest major financial hub (London, New York, or Tokyo) which is provided by Rose and Spiegel (2009).²⁰ Real GDP per capita data, aggregate openness, or population are taken from the Penn World Tables 7.0 database. Information on country pairs' joint membership in FTAs, the WTO, or in a currency union stem from the WTO. Tables 4.7 and 4.8 in Appendix 4.A contain summary statistics for the gravity and for the income regression, respectively.

We focus on three different samples: (i) a sample of 94 countries suggested by Mankiw et al. (1992), henceforth MRW, that excludes countries for which oil-production was the dominant industry 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 (162 countries).²¹ See Table 4.9 in Appendix 4.A for a list of countries.

¹⁹ Still excluding epidemics, though.

²⁰ We set financial remoteness to zero for countries where those financial centers are located.

²¹ The samples suggested by MRW are well established in the growth literature. The MRW sample has also been used by F&R (1999).

4.4 Gravity Results and Instrument Quality

4.4.1 Standard Gravity

To see the economic rationale for using natural disasters as a source of variation of trade openness, Table 4.1 presents results from estimating the standard gravity equation (4.2). The regressions include all usual gravity controls; to save space, we show only those of interest in the current context.²² Columns (1) to (3) report estimates for the MRW sample.²³ Column (1) shows that a disaster in the importer country increases its imports by about 2 percent on average. A disaster striking the exporter does not seem to adversely affect imports from that country. This picture changes in column (2), where we include the interaction between financial remoteness and the disaster variable. An importer that has maximum access to international financial markets experiences a surge of imports by 25 percent. If financial remoteness takes the mean value, the increase in imports drops to 1 percent. Disasters clearly reduce imports when financial distance is substantially larger than the sample average. Similarly, a financially central country sees a 22 percent fall in exports after a disaster hit the partner country j , but the effect vanishes when financial remoteness increases. Results are in line with intuition: a financially constrained importer cannot borrow against future output to increase imports when struck by a disaster.

Column (3) includes interactions of disasters with geographical distance to explore the possibility that the reaction of bilateral trade to disasters depends on bilateral trade costs. We do not find evidence for this hypothesis for the MRW sample. Further, we interact the ratio of importer to exporter per capita GDP with the disaster variables. When that ratio is high, relative capital abundance is supposedly high, too. If a relatively capital abundant country is struck by a disaster, its exports should drop 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

²² Table 4.10 in Appendix 4.A provides complete results. As in the cross-sectional model of Santos Silva and Teneyro (2006), elasticities on GDPs are below unity. The ratio of the two countries' GDP per capita levels increases bilateral imports. Joint FTA membership increases imports by 19 percent, while having a common currency boosts trade by 31 percent. Joint WTO membership increases trade by a slightly larger amount; see Liu (2009) for a comparable conditional fixed effects PPML model and corresponding results on the WTO effect.

²³ We show results obtained from using the broad categorization of large disasters as our key right-hand-side variable. Using the more narrow definition of the disaster variable leads to similar results but is unnecessarily restrictive in a standard gravity setup.

Table 4.1: Natural Disasters and Bilateral Imports (yearly data, 1950-2008)

Dependent Variable:	Bilateral import flows of i from j			
Sample:	MRW	MRW	MRW	FULL
Method:	FE PPML	FE PPML	FE PPML	FE PPML
	(1)	(2)	(3)	(4)
Disasters, importer (D_t^i)	0.020** (0.01)	0.255** (0.12)	0.329*** (0.10)	0.187** (0.09)
Disasters, exporter (D_t^j)	-0.009 (0.01)	-0.218* (0.12)	-0.250** (0.13)	-0.297*** (0.12)
Interactions				
$D_t^i \times \ln FINDIST^i$		-0.035** (0.02)	-0.034** (0.02)	-0.009 (0.01)
$D_t^i \times \ln DIST^{ij}$			-0.005 (0.01)	-0.008* (0.00)
$D_t^i \times \ln(y_t^i/y_t^j)$			0.010*** (0.00)	0.012*** (0.00)
$D_t^j \times \ln FINDIST^j$		0.041** (0.02)	0.044*** (0.02)	0.056*** (0.01)
$D_t^j \times \ln DIST^{ij}$			0.009 (0.01)	0.007* (0.00)
$D_t^j \times \ln(y_t^i/y_t^j)$			-0.011*** (0.00)	-0.003 (0.00)
Observations	407,114	407,114	407,114	821,177

Note: ***, **, * denote significance at the 1%, 5%, and 10% level, respectively. Additional controls (not reported) include the logarithm of GDP of country i and country j , the logarithm of the importer to exporter ratio of per capita GDP, a dummy for free trade agreements, currency unions and WTO membership. Constant, year and country-pair fixed effects and MR terms are similarly included but not reported. Full details in Table 4.10 of Appendix 4.A. Country-pair clustered robust standard errors reported in parentheses. Disasters are the number of large disasters in country i or country j , respectively. Column (1) to (3) use the sample suggested by Mankiw et al. (1992), while column (4) uses the full sample.

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 $\ln(y_t^i/y_t^j)$ should increase the effect of disasters on imports.

Column (4) uses the full sample. The positive effect of disasters on imports and the negative one on exports remain intact.

4.4.2 ‘Modified’ Gravity

The previous discussion supports our idea that disasters in the importer *and* the exporter country affect bilateral trade. In the next step, we modify the gravity equation to the specific

Table 4.2: Gravity as a data reduction device (1950-2008)

Dependent Variable:	Bilateral trade openness of i	
Estimation Method:	PPML	
Sample:	MRW	Full
	(1)	(2)
Disasters, exporter (D_t^j)	-0.818*** (0.26)	-0.836*** (0.12)
Interactions		
$D_t^j \times \ln FINDIST^j$	0.006 (0.01)	0.019*** (0.00)
$D_t^j \times \ln AREA^j$	-0.024 (0.02)	-0.017 (0.01)
$D_t^j \times \ln POP_t^j$	0.059** (0.03)	0.054*** (0.01)
$D_t^j \times ADJ^{ij}$	0.215** (0.09)	0.016 (0.03)
Controls		
$\ln POP_t^i$	-0.532*** (0.17)	-0.176*** (0.04)
$\ln POP_t^j$	0.160*** (0.05)	0.133*** (0.05)
$\ln DIST^{ij}$	-0.828*** (0.06)	-0.980*** (0.04)
ADJ^{ij}	0.503*** (0.15)	0.451*** (0.10)
Fixed Effects		
Importer, Exporter	YES	YES
Year	YES	YES
Observations	418,165	833,529

Note: ***, **, * denote significance at the 1%, 5%, and 10% level, respectively. Fixed effects included but not reported. Country-pair clustered robust standard errors in parenthesis. Disasters are the number of large disasters in i or j , respectively, according to the decision rule. Column (2) is our preferred specification.

needs of our instrumentation strategy.²⁴ Table 4.2 column (1) and (2) report estimates of the bilateral openness regressions. Column (1) draws on the MRW sample, while column (2) uses the full sample. Population size of the importer, introduced as a proxy for GDP, lowers imports while that of the exporter increases it. This is compatible with the idea that population size is a proxy for within-country trade. In both samples, disasters that hit the trade partner 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 significant and have the expected signs. Column (2) is our preferred specification for the construction of the instrument. Following F&R

²⁴ Remember that consistent estimates of disasters, population or trade costs are not required at this stage. What we need is the best possible fit based on exogenous regressors for the construction of the instrument. For that reason, we refrain from interpreting the results obtained in the ‘modified’ gravity equation.

(1999) and Feyrer (2009), we construct equation (4.3) on the full sample. This makes sure that we base the openness instrument on trade with all possible trading partners.

4.4.3 First Stage Regressions

Table 4.3: First-Stage (1950-2008) (fixed-effects estimates, 5-year averages)

Dependent variable:	Observed openness ($OPEN_{\tau}^i$)		
	MRW	MRW	Full
Sample:	Intermediate		
	(1)	(2)	(3)
Constructed openness (Ω_{τ}^i)	0.325*** (0.09)	0.330*** (0.12)	0.331*** (0.13)
$\Omega_{\tau-1}^i$	0.374*** (0.08)	0.366*** (0.10)	
$\ln POP_{\tau}^i$	-0.035 (0.07)	-0.030 (0.07)	-0.044 (0.07)
D_{τ}^i	0.026 (0.02)	0.027 (0.02)	0.014 (0.01)
$D_{\tau-1}^i$	0.061*** (0.02)	0.054*** (0.02)	0.038** (0.02)
Fixed Effects			
Country	YES	YES	YES
Period	YES	YES	YES
Observations	919	736	1,312
R^2	0.514	0.564	0.322
Partial R^2	0.18	0.21	0.04
F-Test on excl. Instrument	31.41	34.55	6.99

Note: ***, **, * denote significance at the 1%, 5%, and 10% level, respectively. Constant and fixed effects not reported. Robust clustered standard errors reported in parenthesis. Disasters are the number of large-scale disasters according to the decision rule. Column (1) uses the sample suggested by Mankiw et al. (1992), column (2) uses a smaller sample by Mankiw et al. (1992) that excludes countries likely to be subject to measurement error, while column (3) uses the full sample.

Table 4.3 assesses the quality of the instrument Ω_{τ}^i by reporting the first stage regressions for the three different country samples used in our 2SLS analysis.²⁵ Since Ω_{τ}^i is strictly exogenous to income $\ln \bar{y}_{\tau}^i$, $\Omega_{\tau-1}^i$ will be similarly exogenous and can be used as an additional instrument.²⁶ In column (1) and (2) the lag has a very similar effect on observed

²⁵ Robust standard errors that are clustered at the country level are reported. Note that bootstrapping standard errors actually makes no difference.

²⁶ When we work with the strongly unbalanced full sample, we only use the contemporaneous instrument.

openness than the contemporaneous value Ω_{τ}^i .²⁷ We control for the covariates of our second stage regression, namely population size, and natural disasters. We take care of the panel dimension by using the within-estimator on five-year averages. For all of our three samples, the estimated conditional correlation of the instrument with observed openness is statistically significant and positive. The marginal contribution of the instrument as measured by the partial R^2 statistics is satisfactory in columns (1) and (2); less so for the large country sample. A similar picture emerges from running an F-Test on the excluded instruments. The test statistics are well above the Stock-Yogo (2005) critical values so that we can firmly reject the weak instrument hypothesis for columns (1) and (2); for column (3) rejection is borderline. In all regressions, lagged disasters tend to increase the level of openness, the effect being economically substantial and significant.

4.5 The Effect of Openness on Income per Capita

In this section, we report estimated effects of openness on income, using constructed openness Ω_{τ}^i as an instrument for observed openness $OPEN_{\tau}^i$ in equation (4.1). Table 4.4 reports the results based on the fixed effects (within) estimator applied to 5-year averaged data. 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 percent. The cross-sectional exercise of F&R (based on 1985 data) yielded an effect of 0.82 percent. So, controlling for country heterogeneity substantially reduces the effect of openness on income per capita. This finding is robust to instrumentation and alternative samples. Unlike F&R, Feyrer (2009) uses the log of trade 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 0.33.²⁸ An increase of population by one percent *decreases* GDP per capita by about 0.69 percent. F&R have found a statistically only marginally positive effect of population. Controlling for country heterogeneity turns around the sign of

²⁷ Results are qualitatively similar and robust when we use the broader definition of large natural disasters, or the specification of the instrument where disasters in i and partner countries j and interactions are used to construct the instrument.

²⁸ 0.554×0.595 .

the population coefficient and makes it statistically significant in virtually all our regressions. Contemporaneous and lagged disasters have no measurable effect on per capita income.

Table 4.4: 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 ($\Omega_{\tau}^i, \Omega_{\tau-1}^i$)						
Sample:	MRW (N = 919)		MRW Intermediate (N = 736)		Full (N = 1,312)	
Estimation Method:	FE	2SLS	FE	2SLS	FE	2SLS
	(1)	(2)	(3)	(4)	(5)	(6)
$OPEN_{\tau}^i$	0.554*** (0.12)	1.245*** (0.18)	0.635*** (0.12)	1.268*** (0.16)	0.404*** (0.09)	1.763*** (0.49)
$\ln POP_{\tau}^i$	-0.689*** (0.10)	-0.651*** (0.11)	-0.608*** (0.10)	-0.585*** (0.11)	-0.590*** (0.10)	-0.500*** (0.13)
D_{τ}^i	0.003 (0.03)	-0.011 (0.03)	-0.017 (0.03)	-0.031 (0.03)	0.097** (0.04)	0.074 (0.05)
$D_{\tau-1}^i$	-0.043 (0.03)	-0.077** (0.04)	-0.054 (0.03)	-0.081** (0.04)	0.040 (0.03)	-0.006 (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	0.74	0.38	0.76	0.28	1.23
at median	0.29	0.64	0.33	0.66	0.24	1.06
Countries	94	94	72	72	162	162
R^2	0.944	0.933	0.956	0.949	0.923	0.861
Partial R^2		0.18		0.21		0.04
F-Test on excl. Instrument		31.41		34.55		6.99
Stock-Yogo weak ID test		19.93		19.93		6.66
Hansen p-value		0.85		0.88		

Note: ***, **, * denote significance at the 1%, 5%, and 10% level, respectively. Constant, country and period fixed effects not reported. Country clustered standard errors reported 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-Yogo (2005) critical values of 10% reported in column (2) and (4), while column (6) reports the 20% critical value. The weak instruments hypothesis is rejected with the most stringent criterion for the preferred samples and the third stringent criterion for the full sample.

Column (2) turns to the 2SLS regression (the corresponding first stage regression is displayed in Table 4.3 column (1)). Judged by the diagnostic statistics, the instrumentation strategy works well: the partial R^2 is 0.18 and the F-Test on the excluded instruments is 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.²⁹ The overidentifying restrictions test whether or not all the instruments are coherent (Parente and Santos Silva, 2012). The test fails to reject (p-value of 0.85). The 2SLS estimate implies that an increase in openness by one percentage point increases GDP per capita by 1.2 percent. F&R report an effect of 2.96 in the cross-section of 1985. In our exercise, instrumentation increases the effect of openness by the factor 2.2; in F&R it increases it by the factor 3.6. Evaluated at the mean openness value, our estimate implies an elasticity of 0.74.³⁰ Our specification implies that the elasticity is not constant: Countries with an openness level one standard deviation below the mean have an elasticity of 0.22, while countries with openness one standard deviation above the mean have an elasticity of 1.26.³¹ The effect of population remains negative and highly significant.

Columns (3) and (4) repeat the exercise for the smaller MRW intermediate sample. Both the OLS and the instrumented equations yield similar results to the MRW sample. Columns (5) and (6) turn to the full sample. Still, the instrumentation strategy works as the F-Test on the excluded instrument is above the Stock and Yogo (2005) 20% reference value and close to the 15% critical value. Since this large panel is strongly unbalanced, we restrict the set of instruments to the contemporaneous realization. Nonetheless, we still find a positive effect of openness on income per capita;³² the elasticity of income with respect to openness is about unity.

The literature has no clear cut predictions on the effect of disasters on GDP per capita. In our samples, contemporaneous domestic large natural disasters have no robust effect on per capita income. The statistically significant positive effect found in column (5) turns out to be spurious and disappears when the endogeneity of openness is accounted for. Lagged domestic disasters are more robustly related to GDP per capita and turn out negative in sign.

4.6 Robustness Checks

In our robustness checks, we address five concerns. First, our results could depend on the choice of the country sample. The literature has often pointed to the role of Sub-

²⁹ Note that our results are robust when using a just identified model.

³⁰ $1.245 \times 0.595 = 0.74$. Feyrer (2009) finds elasticities ranging from 0.42 to 0.59; see his Table 5.

³¹ $1.245 * (0.595 - 0.421) = 0.22; 1.245 * (0.595 + 0.421) = 1.26$.

³² Adding the lag of constructed openness the sample size shrinks, but point estimates and standard errors are comparable.

**Table 4.5: Alternative Samples and Definition of Disaster, Summary
(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)

PANEL A: Alternative Time Coverage and Country Samples

Sample:	Feyrer 1950-1995 (A1)	Balanced 1960-2008 (A2)	Balanced w/o Africa (A3)	50% rich (A4)	50% poor (A5)	OECD (A6)	NonOECD (A7)
$OPEN_{\tau}^i$	1.243*** (0.37) (0.06)	1.413*** (0.20) (0.04)	1.297*** (0.19) (0.05)	1.087*** (0.30) (0.07)	2.353** (0.96) (0.08)	0.396 (0.53) (0.11)	1.906*** (0.55) (0.06)
Observations	832	894	677	684	628	284	1,028
R^2	0.922	0.916	0.941	0.951	0.680	0.980	0.799
Partial R^2	0.05	0.11	0.12	0.07	0.03	0.19	0.04
F-Test on excl.Instrument	13.95	25.52	25.14	5.57	3.29	12.82	6.74

PANEL B: Alternative Definitions of Disasters, MRW

Disaster variable:	yearly			cumulated			
	narrow all (B1)	broad large (B2)	broad all (B3)	narrow large (B4)	narrow all (B5)	broad large (B6)	broad all (B7)
$OPEN_{\tau}^i$	1.285*** (0.18)	1.273*** (0.17)	1.312*** (0.17)	1.204*** (0.18)	1.272*** (0.18)	1.257*** (0.18)	1.292*** (0.18)
Observations	919	919	919	919	919	919	919
R^2	0.931	0.932	0.931	0.934	0.932	0.932	0.932
Partial R^2	0.18	0.18	0.18	0.18	0.18	0.18	0.18
F-Test on excl.Instrument	31.65	29.27	30.71	33.79	31.49	30.19	31.02

Note: ***, **, * denote significance at the 1%, 5%, and 10% level, respectively. Constant, controls, country and period fixed effects not reported. Country clustered standard errors in parenthesis. Full results can be found in Table 4.12 for Panel A and in Table 4.13 for Panel B in Appendix 4.A. In Panel A, the number of the disasters is the number of large natural disasters according to the decision rule. Panel A results base on the full sample, while Panel B depicts results for the Mankiw et al. (1992) sample. The number of the corresponding disaster according to the disaster definition in the heading for column (B1) to (B3). Disasters are the corresponding number of 5-year cumulated disasters according to the disaster definition in the heading for column (B4) to (B7). The weak instruments hypothesis is rejected with the most stringent criterion according to Stock-Yogo (2005) critical values of 10%.

Saharan Africa. Also the time period considered could matter. Second, when defining natural disasters, we had to take decisions. We conduct extensive sensitivity analysis pertaining to those choices. Third, in our baseline regressions, we predict the exogenous component of openness based on foreign disasters only. In an alternative approach, we include domestic disasters as well, but control for them in the second stage regression to mitigate concerns about the exclusion restriction. Fourth, we turn to the direct effect of disasters to address the finding that in some of our baseline regressions natural disasters have no measurable effect on GDP per capita. Finally, we report results from a first-differenced model and compare them to the fixed effects regressions reported in Table 4.4.

Sample Sensitivity. Panel A of Table 4.5 summarizes results from second stage 2SLS and associated first stage diagnostics for different samples.³³ Column (A1) follows Feyrer (2009) and restricts the sample to the years 1950-1995. Results are similar to the baseline of Table 4.4. The fact that we find a slightly larger elasticity of openness on GDP than Feyrer is therefore not due to our inclusion of more recent data. Column (A2) eliminates countries for which we have incomplete data for the period 1960-2008. Based on the resulting balanced sample, we support our earlier findings. Perhaps not surprisingly, first stage diagnostics improve when the balanced sample is used. A similar picture emerges if Sub-Saharan Africa is excluded from the balanced sample in column (A3).

In a next step, we split the sample into rich and poor countries, and into OECD and non-OECD economies. While we obtain a smaller positive effect for the 50 percent richest economies from 1950-2008 in column (A4), the effect for the 50 percent poorest countries increases as compared to the benchmark results. Moreover, the comparison between rich and poor reverses in the 2SLS regression compared to results under OLS. Splitting the sample in OECD and non-OECD member states in column (A6) and (A7), we find that openness does not 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. Comparing F statistics on excluded instruments to the critical values of Stock and Yogo (2005), the instrument remains technically valid for the OECD and the non-OECD sample. The conclusion from this sensitivity analysis is that poor countries benefit more from openness than rich ones.

Alternative Definition of Disasters. Next, we modify the definition of natural disasters. Remember that we selected large, narrowly defined disasters in our baseline regressions; i.e., we excluded disasters that may have a strong regional root. Panel B varies this choice. It reports second stage 2SLS results and first stage diagnostics. Column (B1) to (B3) report results obtained when using the narrow definition on large and small (i.e., all) disasters, when using the broad definition on large disasters only, or when including all broadly defined disasters.³⁴ In column (B1), we use the *total* number of disasters. Regression coefficients are essentially similar as to when applying the large disaster decision rule in Table 4.4 column (2). In column (B2) and (B3), we use a broader definition of disasters including all possible types

³³ Modifications are always relative to the full sample.

³⁴ Results on the gravity-type estimation from which the instrument is predicted can be found in Table 4.11, Appendix 4.A.

of natural disasters. Still, 2SLS results remain robust. In this sensitivity check, we work with the MRW sample. Modifying the definitions of disasters in the full samples similarly maintains results.

Further, we do not work with the number of disasters averaged over the 5-year interval, but with the number of disasters cumulated over the 5-year interval. Columns (B4) to (B7) in Table 4.5 Panel B report the coefficients. Again, we find a positive effect on per capita GDP comparable to our baseline findings, a comfortably high F-Test on the excluded instrument and a high partial R^2 .

Table 4.6: Alternative Instrument, Summary (fixed-effects estimates, 5-year averages)

Dependent Variable: ln real GDP per capita						
Dependent Variable (First-stage): Observed openness						
Instruments: Constructed openness ($\Omega_{\tau}^i, \Omega_{\tau-1}^i$)						
Sample:	MRW		MRW Intermediate		Full	
	(1)	(2)	(3)	(4)	(5)	(6)
$OPEN_{\tau}^i$	1.290*** (0.16)	1.269*** (0.17)	1.343*** (0.15)	1.363*** (0.16)	1.712*** (0.44)	1.299*** (0.19)
Observations	919	914	736	734	1,312	1,195
R^2	0.934	0.935	0.949	0.950	0.871	0.907
Partial R^2	0.18	0.18	0.21	0.20	0.04	0.11
F-Test on excl. Instrument	31.15	31.43	36.16	37.54	7.73	38.62

Note: ***, **, * denote significance at the 1%, 5%, and 10% level, respectively. Constant, controls, country and period fixed effects not reported. Country clustered standard errors in parenthesis. Full results can be found in Table 4.15 in Appendix 4.A. Disasters are the number of large disasters according to the decision rule. Column (1) to (2) use the sample suggested by Mankiw et al. (1992), column (3) to (4) use the intermediate sample by Mankiw et al. (1992), while column (5) to (6) use the full sample. Column (1), (3), and (5) include interactions between disasters and the log of population, area size and the lags thereof using a fixed effects approach, while column (2), (4), and (6) additionally include the interaction between disasters and the log of financial distance, the polity index (direct and interaction), squared disasters and the lags, respectively. All columns use the instrument constructed from Table 4.11 column (8) using disasters and interactions of country i and j . The weak instruments hypothesis is rejected with a Stock-Yogo (2005) critical value of 10% for all columns, except column (5) on the 20% value.

Alternative Instrument. In the baseline regressions, we excluded domestic disasters to avoid a possible violation of the exclusion restriction. Table 4.6 deviates from this strategy. It summarizes results obtained when using an instrument constructed from a gravity model that also uses *domestic* natural disasters along with foreign ones.³⁵ In the regressions, we include the direct effect of disasters, contemporaneous and lagged, as well as interactions of

³⁵ The underlying modified gravity equation is described in Table 4.11, column (8).

disasters with geographical or socio-economic variables. The exercise of column (1) contains the interactions between domestic disasters with size variables (ln population, ln area). Column (2) adds financial distance, the polity index, and squared disasters. Not surprisingly, compared to the baseline results of Table 4.4, the F statistic on excluded instruments increases. However, estimated openness coefficients do not change. This pattern holds true across our three samples.

Richer Accounting for Direct Effects of Disasters. Finally, we come back to the direct effect of domestic disasters on GDP per capita. Table 4.15 in Appendix 4.A contains detailed results. Here it suffices to describe the main results. Adding interactions between disasters and size variables such as population and area, we find a significant, strongly negative and robust effect of natural disasters on GDP per capita. For example, in the MRW sample, the direct effect of contemporaneous disasters is -1.18 and of first-lagged disasters -1.27. The interactions with population and area are positive. All point estimates are statistically significant (with one exception: the interaction between lagged disasters and area). The results suggest that large countries can cope more easily with natural disasters than small countries. Evaluated at means, the average effect of a large disaster turns out to be negative. These findings are intuitive. A given disaster destroys a smaller share of the total capital stock in a larger 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.³⁶

First Differenced Regressions. First differenced models are more efficient in the presence of strong serial correlation. We find that the 2SLS strategy still works fine and that qualitatively the same conclusions as those from fixed effects estimation are obtained: openness increases GDP per capita, population size decreases it, domestic disasters have no or a negative effect. However, two observations stand out: First, depending on the sample, 2SLS estimates of openness are between 54 and 72% of the point estimates under fixed

³⁶ The Polity Index is 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.

effects. So, they are even more strongly lower than the F&R baseline. Second, the difference in point estimates between the OLS and the 2SLS estimates is larger.³⁷

4.7 Conclusions

This paper generalizes the instrumental variables strategy of Frankel and Romer (1999) to a panel framework. Empirical papers such as Gassebner et al. (2010) or Oh and Reuveny (2010) as well as theoretical consumption-smoothing arguments suggest that natural disasters affect *bilateral* trade. They are therefore useful to predict the exogenous component of countries' *multilateral* trade, which provides a time-varying instrument for openness. To meet the exclusion restriction, our instrument is based only on the incidence of foreign disasters; moreover, domestic disasters are added to the income regressions as controls.

The setup allows to include country fixed-effects into the income regressions, thereby accounting for unobserved time-invariant determinants of growth linked to history or geography. We find that our instrument performs well: predicted openness conditionally correlates highly with observed openness. We confirm a positive, but smaller than hitherto reported, effect of openness on growth. That effect is larger in poor countries than in rich ones. It turns out robust to a battery of sensitivity checks.

We conjecture that our strategy 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 adoption, social, political, or economic institutions, economic inequality, environmental outcomes, and many more.

³⁷ Table 4.16 in Appendix 4.A provides results for first differenced models.

4.A Appendix

Figure 4.1: Average Number of Large Disasters by Surface Area (1992-2008)

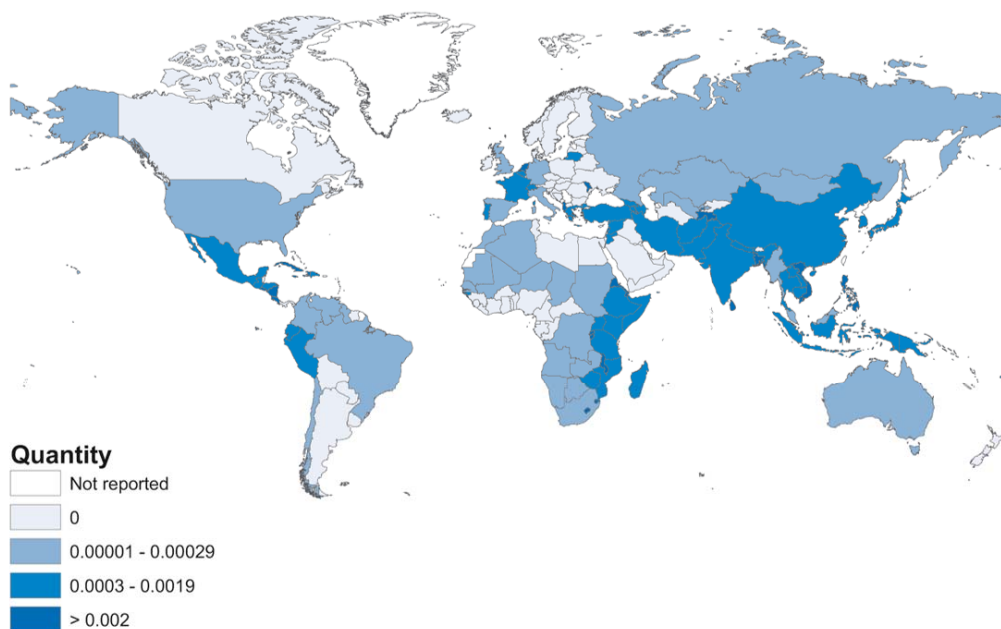


Table 4.7: Summary Statistics and Data Sources (Gravity Section)

Variable	MRW Sample		Full Sample		Data Source
	(N = 418,165)		(N = 833,529)		
	Mean	St. Dev.	Mean	St. Dev.	
$\omega^{ij} = (M_t^{ij} + M_t^{ji})/GDP_t^i$	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 4.8: Summary Statistics and Data Sources (Trade-Income Section)

Variable	MRW Sample (N = 1,007)			MRW Intermediate Sample (N = 804)			Full Sample (N = 1,448)			Data Source
	Median	Mean	St. Dev.	Median	Mean	St. Dev.	Median	Mean	St. Dev.	
$OPEN^i$	0.516	0.595	0.421	0.522	0.599	0.444	0.604	0.702	0.450	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
$\ln POP^i$	9.144	9.347	1.323	9.248	9.491	1.347	8.864	8.828	1.764	PWT 7.0
$\ln AREA^i$	12.701	12.552	1.717	12.688	12.555	1.725	12.375	12.012	2.140	CEPII (2005)
$\ln FINDIST^i$	7.740	7.349	1.594	7.616	7.165	1.732	7.710	7.414	1.372	Rose and Spiegel (2009)
$POLITY^i$	14	12.019	7.123	16.5	13.254	7.009	11.9	11.222	7.254	Polity IV Project (2010)
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	0.289	0.406	0.464	0.297	0.463	0.543	0.349	own calculations

Table 4.9: 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
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

Table 4.9 – continued

Country	MRW	MRW-I	Full	Country	MRW	MRW-I	Full
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

Table 4.10: Natural Disasters and Bilateral Imports (yearly data, 1950-2008)

Dependent Variable:	Bilateral import flows of i from j			
Sample:	MRW	MRW	MRW	FULL
Method:	FE PPML	FE PPML	FE PPML	FE PPML
	(1)	(2)	(3)	(4)
Disasters, importer (D_t^i)	0.020** (0.01)	0.255** (0.12)	0.329*** (0.10)	0.187** (0.09)
Disasters, exporter (D_t^j)	-0.009 (0.01)	-0.218* (0.12)	-0.250** (0.13)	-0.297*** (0.12)
Interactions				
$D_t^i \times \ln FINDIST^i$		-0.035** (0.02)	-0.034** (0.02)	-0.009 (0.01)
$D_t^i \times \ln DIST^{ij}$			-0.005 (0.01)	-0.008* (0.00)
$D_t^i \times \ln(y_t^i/y_t^j)$			0.010*** (0.00)	0.012*** (0.00)
$D_t^j \times \ln FINDIST^j$		0.041** (0.02)	0.044*** (0.02)	0.056*** (0.01)
$D_t^j \times \ln DIST^{ij}$			0.009 (0.01)	0.007* (0.00)
$D_t^j \times \ln(y_t^i/y_t^j)$			-0.011*** (0.00)	-0.003 (0.00)
Controls				
$\ln GDP_t^i$	0.496*** (0.11)	0.510*** (0.10)	0.513*** (0.10)	0.758*** (0.04)
$\ln GDP_t^j$	0.904*** (0.12)	0.906*** (0.11)	0.903*** (0.11)	0.753*** (0.04)
$\ln(y_t^i/y_t^j)$	0.193* (0.10)	0.190* (0.10)	0.187* (0.10)	-0.031 (0.03)
FTA_t^{ij}	0.183** (0.09)	0.188** (0.09)	0.188** (0.09)	0.227*** (0.07)
CU_t^{ij}	0.311*** (0.05)	0.304*** (0.05)	0.304*** (0.05)	0.326*** (0.03)
WTO_t^{ij}	0.236*** (0.07)	0.224*** (0.06)	0.233*** (0.06)	0.245*** (0.03)
Fixed Effects				
Pair	YES	YES	YES	YES
Year	YES	YES	YES	YES
Multilateral Resistance	YES	YES	YES	YES
Observations	407,114	407,114	407,114	821,177

Note: ***, **, * denote significance at the 1%, 5%, and 10% level, respectively. Constant, pair, and year fixed effects are not reported. Country-pair clustered robust standard errors reported in parentheses. Multilateral resistance terms and interactions are included but not reported. Disasters are the number of large disasters in country i or country j , respectively. Column (1) to (3) use the sample suggested by Mankiw et al. (1992), while column (4) uses the full sample.

Table 4.11: Robustness: PPML Specification to Construct Instrument (1950-2008)

Dependent Variable: Bilateral trade openness of i								
Disaster variable:	yearly			cumulated				yearly
Disaster definition:	narrow large	broad all	narrow all	broad large	narrow large	broad all	narrow all	both disasters
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Disasters, importer (D_t^i)								0.278** (0.13)
Disasters, exporter (D_t^j)	-0.905*** (0.18)	-0.131** (0.05)	-0.155*** (0.03)	0.031 (0.10)	-0.113* (0.07)	-0.007 (0.02)	-0.006 (0.01)	-0.843*** (0.12)
Interactions								
$D_t^i \times \ln FINDIST^i$								-0.020*** (0.01)
$D_t^i \times \ln AREA^i$								-0.022*** (0.01)
$D_t^i \times \ln POP_t^i$								0.010 (0.01)
$D_t^i \times \ln ADJ^{ij}$								0.143*** (0.06)
$D_t^j \times \ln FINDIST^j$	0.021*** (0.00)	0.011*** (0.00)	0.008*** (0.00)	0.006*** (0.00)	0.007*** (0.00)	0.002*** (0.00)	0.002*** (0.00)	0.018*** (0.00)
$D_t^j \times \ln AREA^j$	-0.000 (0.01)	0.002 (0.01)	0.001 (0.00)	-0.003 (0.01)	0.001 (0.01)	0.002 (0.00)	0.001* (0.00)	-0.017 (0.01)
$D_t^j \times \ln POP_t^j$	0.043*** (0.01)	0.004 (0.00)	0.007* (0.00)	0.002*** (0.00)	0.003 (0.00)	-0.001*** (0.00)	-0.001*** (0.00)	0.054*** (0.01)
$D_t^j \times \ln ADJ^{ij}$	0.008 (0.02)	0.017* (0.01)	0.014** (0.01)	0.006 (0.01)	0.003 (0.01)	0.003 (0.00)	0.003 (0.00)	0.007 (0.03)
Controls								
$\ln POP_t^i$	-0.178*** (0.04)	-0.173*** (0.04)	-0.175*** (0.04)	-0.177*** (0.04)	-0.176*** (0.04)	-0.170*** (0.04)	-0.170*** (0.04)	-0.176*** (0.04)
$\ln POP_t^j$	0.126*** (0.05)	0.123*** (0.04)	0.101** (0.04)	0.141*** (0.04)	0.134*** (0.05)	0.153*** (0.05)	0.153*** (0.05)	0.135*** (0.05)
$\ln DIST^{ij}$	-0.980*** (0.04)	-0.980*** (0.04)	-0.980*** (0.04)	-0.980*** (0.04)	-0.980*** (0.04)	-0.979*** (0.04)	-0.980*** (0.04)	-0.982*** (0.04)
ADJ^{ij}	0.452*** (0.10)	0.426*** (0.10)	0.411*** (0.10)	0.453*** (0.10)	0.454*** (0.10)	0.442*** (0.11)	0.426*** (0.11)	0.436** (0.10)
Fixed Effects								
Importer, Exporter	YES	YES	YES	YES	YES	YES	YES	YES
Year	YES	YES	YES	YES	YES	YES	YES	YES
Observations	833,529	833,529	833,529	833,529	833,529	833,529	833,529	833,529

Note: ***, **, * denote significance at the 1%, 5%, and 10% level, respectively. 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 heading.

Table 4.12: 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 (1)	2SLS (2)	2SLS (3)	2SLS (4)	2SLS (5)	2SLS (6)	2SLS (7)
$OPEN_\tau^i$	1.243*** (0.37)	1.413*** (0.20)	1.297*** (0.19)	1.087*** (0.30)	2.353** (0.96)	0.396 (0.53)	1.906*** (0.55)
$\ln POP_\tau^i$	-0.458*** (0.13)	-0.668*** (0.13)	-0.574*** (0.15)	-0.094 (0.13)	-0.960*** (0.34)	0.237 (0.38)	-0.638*** (0.18)
D_τ^i	0.047 (0.06)	0.107*** (0.04)	0.110*** (0.03)	-0.035 (0.05)	0.078 (0.05)	-0.133* (0.07)	0.074 (0.05)
$D_{\tau-1}^i$	-0.048 (0.06)	0.002 (0.04)	0.011 (0.05)	-0.076 (0.07)	-0.032 (0.08)	-0.172 (0.11)	-0.014 (0.06)
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	0.76	0.90	0.87	0.82	1.51	0.23	1.42
at median	0.66	0.78	0.74	0.71	1.31	0.21	1.19
Observations	832	894	677	684	628	284	1,028
R^2	0.922	0.916	0.941	0.951	0.680	0.980	0.799
Partial R^2	0.05	0.11	0.12	0.07	0.03	0.19	0.04
F-Test on excl. Instrument	13.95	25.52	25.14	5.57	3.29	12.82	6.74
Memo: FE OLS							
$OPEN_\tau^i$	0.434***	0.485***	0.446***	0.437***	0.394***	0.217	0.425***

Note: ***, **, * denote significance at the 1%, 5%, and 10% level, respectively. Constant, country and period fixed effects not reported. Country clustered standard errors reported in parenthesis. The number of the disaster is the number of large natural disasters according to the decision rule. Fixed effects OLS coefficients for observed openness are stated in the bottommost row for comparison reasons.

Table 4.13: 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 ($\Omega_{\tau}^i, \Omega_{\tau-1}^i$)

Sample:	MRW (N = 919)						
Disaster variable:	yearly			cumulated			
Disaster definition:	all	broad	broad	large	all	broad	broad
	large	large	all	large	all	large	all
Method:	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$OPEN_{\tau}^i$	1.285*** (0.18)	1.273*** (0.17)	1.312*** (0.17)	1.204*** (0.18)	1.272*** (0.18)	1.257*** (0.18)	1.292*** (0.18)
ln POP_{τ}^i	-0.658*** (0.11)	-0.667*** (0.11)	-0.656*** (0.11)	-0.653*** (0.11)	-0.658*** (0.11)	-0.668*** (0.11)	-0.656*** (0.11)
D_{τ}^i	0.004 (0.01)	0.030 (0.03)	0.004 (0.01)	-0.004 (0.01)	0.000 (0.00)	0.003 (0.01)	0.000 (0.00)
$D_{\tau-1}^i$	0.013 (0.01)	-0.031 (0.04)	0.014* (0.01)	-0.014* (0.01)	0.003 (0.00)	-0.003 (0.01)	0.003** (0.00)
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	0.76	0.75	0.78	0.71	0.75	0.74	0.76
at median	0.66	0.65	0.67	0.62	0.65	0.64	0.66
Countries	94	94	94	94	94	94	94
R^2	0.931	0.932	0.931	0.934	0.932	0.932	0.932
Partial R^2	0.18	0.18	0.18	0.18	0.18	0.18	0.18
F-Test on excl. Instrument	31.65	29.27	30.71	33.79	31.49	30.19	31.02
Hansen p-value	0.77	0.68	0.60	0.91	0.77	0.95	0.70
Memo: FE OLS							
$OPEN_{\tau}^i$	0.544***	0.544***	0.546***	0.554***	0.545***	0.544***	0.546***

Note: ***, **, * denote significance at the 1%, 5%, and 10% level, respectively. Constant, country and period fixed effects not reported. Country clustered standard errors reported in parenthesis. The number of the corresponding disaster according to the disaster definition in the heading for column (1) to (3). Disasters are the corresponding number of 5-year cumulated disasters according to the disaster definition in the heading for column (4) to (7). 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.

**Table 4.14: Robustness Checks: Alternative Definitions of Disasters
(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 = 1,312)						
Disaster variable:	yearly			cumulated			
Disaster definition:	broad		broad	broad		broad	
	all	large	all	large	all	large	all
Method:	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$OPEN_{\tau}^i$	1.775*** (0.43)	1.770*** (0.47)	1.760*** (0.40)	1.578*** (0.42)	1.853*** (0.50)	1.752*** (0.49)	1.857*** (0.49)
$\ln POP_{\tau}^i$	-0.478*** (0.13)	-0.502*** (0.13)	-0.482*** (0.13)	-0.512*** (0.12)	-0.473*** (0.13)	-0.504*** (0.13)	-0.475*** (0.13)
D_{τ}^i	0.008 (0.02)	0.068*** (0.03)	0.001 (0.01)	0.016* (0.01)	0.002 (0.00)	0.014** (0.01)	0.000 (0.00)
$D_{\tau-1}^i$	0.023* (0.01)	-0.014 (0.04)	0.020** (0.01)	0.001 (0.01)	0.005* (0.00)	-0.002 (0.01)	0.004** (0.00)
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	1.24	1.24	1.24	1.10	1.30	1.22	1.30
at median	1.07	1.06	1.06	0.95	1.11	1.05	1.12
Countries	162	162	162	162	162	162	162
R^2	0.860	0.861	0.862	0.877	0.853	0.863	0.853
Partial R^2	0.05	0.05	0.06	0.05	0.04	0.04	0.05
F-Test on excl.Instrument	9.85	7.59	11.47	7.48	7.80	6.70	8.23
FE OLS							
$OPEN_{\tau}^i$	0.411***	0.400***	0.409***	0.403***	0.411***	0.398***	0.409***

Note: ***, **, * denote significance at the 1%, 5%, and 10% level, respectively. Constant, country and period fixed effects are not reported. Country clustered standard errors reported in parenthesis. The number of the corresponding disaster according to the disaster definition in the heading for column (1) to (3). Disasters are the corresponding number of 5-year cumulated disasters according to the disaster definition in the heading for column (4) to (7). 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.

Table 4.15: Robustness Checks: Interactions Alternative Instrument (fixed-effects estimates, 5-year averages)

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

Sample: Method:	MRW		MRW Intermediate		Full	
	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS
	(1)	(2)	(3)	(4)	(5)	(6)
$OPEN_{\tau}^i$	1.290*** (0.16)	1.269*** (0.17)	1.343*** (0.15)	1.363*** (0.16)	1.712*** (0.44)	1.299*** (0.19)
$\ln POP_{\tau}^i$	-0.550 (0.45)	-0.579 (0.47)	-0.283 (0.73)	-0.338 (0.76)	-0.192 (0.37)	-0.164 (0.35)
$\ln POP_{\tau-1}^i$	-0.082 (0.42)	-0.071 (0.43)	-0.269 (0.69)	-0.244 (0.72)	-0.269 (0.37)	-0.315 (0.34)
D_{τ}^i	-1.185*** (0.31)	-1.126*** (0.44)	-1.376*** (0.36)	-1.277*** (0.46)	-0.821*** (0.20)	-0.743*** (0.28)
$D_{\tau-1}^i$	-1.278*** (0.29)	-1.673*** (0.39)	-1.373*** (0.30)	-1.769*** (0.39)	-0.706*** (0.21)	-1.090*** (0.36)
$POLITY_{\tau}^i$		-0.000 (0.00)		0.004 (0.00)		-0.002 (0.00)
$POLITY_{\tau-1}^i$		-0.002 (0.00)		-0.002 (0.00)		-0.006** (0.00)
$D_{\tau}^i \times \ln POP_{\tau}^i$	0.052** (0.02)	0.067** (0.03)	0.066** (0.03)	0.063** (0.03)	0.045* (0.03)	0.070*** (0.03)
$D_{\tau-1}^i \times \ln POP_{\tau-1}^i$	0.068*** (0.02)	0.103*** (0.03)	0.060*** (0.02)	0.087*** (0.03)	0.030 (0.02)	0.063*** (0.02)
$D_{\tau}^i \times \ln AREA_{\tau}^i$	0.045*** (0.02)	0.041** (0.02)	0.046*** (0.01)	0.058*** (0.02)	0.021 (0.02)	0.005 (0.02)
$D_{\tau-1}^i \times \ln AREA_{\tau-1}^i$	0.034 (0.03)	0.008 (0.03)	0.047* (0.02)	0.034 (0.04)	0.020 (0.03)	0.009 (0.02)
$D_{\tau}^i \times \ln FINDIST_{\tau}^i$		-0.011 (0.02)		-0.021 (0.02)		-0.005 (0.02)
$D_{\tau-1}^i \times \ln FINDIST_{\tau-1}^i$		0.062** (0.03)		0.052* (0.03)		0.041 (0.04)
$D_{\tau}^i \times POLITY_{\tau}^i$		-0.002 (0.01)		-0.004 (0.01)		0.000 (0.00)
$D_{\tau-1}^i \times POLITY_{\tau-1}^i$		-0.004 (0.00)		-0.007 (0.00)		-0.005 (0.00)
$D_{\tau}^{i^2}$		-0.021 (0.02)		-0.003 (0.02)		-0.031*** (0.01)
$D_{\tau-1}^{i^2}$		-0.006 (0.02)		0.009 (0.02)		-0.005 (0.01)
Observations	919	914	736	734	1,312	1,195
Countries	94	94	72	72	162	146
R^2	0.934	0.935	0.949	0.950	0.871	0.907
Partial R^2	0.18	0.18	0.21	0.20	0.04	0.11
F-Test on excl. Instrument	31.15	31.43	36.16	37.54	7.73	38.62
Hansen p-value	0.72	0.68	0.82	0.72		

Note: ***, **, * denote significance at the 1%, 5%, and 10% level, respectively. Constant, country and period fixed effects not reported. Country clustered standard errors reported in parenthesis. The number of the corresponding disaster according to the decision rule. All columns use the instrument constructed from Table 4.11 column (8) using disasters and interactions of country i and j . Column (1) to (2) use the sample suggested by Mankiw et al. (1992), column (3) to (4) use the intermediate sample by Mankiw et al. (1992), while column (5) to (6) use the full sample.

Table 4.16: Openness and real GDP per capita (1950-2008) (first-differenced estimates, 5-year averages)

Dependent Variable: $\Delta \ln$ real GDP per capita
 Dependent Variable (First-stage): Δ observed openness
 Instruments: $\Delta \Omega_{\tau}^i, \Delta \Omega_{\tau-1}^i$

Sample: Method:	MRW (N = 825)			MRW Intermediate (N = 644)			Full (N = 1,149)		
	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)
$\Delta OPEN_{\tau}^i$	0.131*** (0.05)		0.899*** (0.24)	0.133** (0.05)		0.894*** (0.24)	0.064 (0.04)		0.952** (0.48)
$\Delta \ln POP_{\tau}^i$	-0.463*** (0.15)	-0.063 (0.07)	-0.507*** (0.12)	-0.603*** (0.11)	-0.068 (0.08)	-0.544*** (0.11)	-0.451*** (0.13)	-0.102 (0.06)	-0.379*** (0.12)
ΔD_{τ}^i	-0.023 (0.02)	0.011 (0.01)	-0.028 (0.02)	-0.026 (0.02)	0.008 (0.01)	-0.029* (0.02)	0.007 (0.03)	0.020** (0.01)	0.001 (0.03)
$\Delta D_{\tau-1}^i$	-0.024 (0.02)	0.017 (0.01)	-0.034 (0.03)	-0.023 (0.02)	0.010 (0.01)	-0.029 (0.02)	0.016 (0.03)	0.014 (0.01)	0.015 (0.03)
$\Delta \Omega_{\tau}^i$		0.190*** (0.03)			0.178*** (0.03)			0.185*** (0.07)	
$\Delta \Omega_{\tau-1}^i$		0.218** (0.08)			0.242** (0.09)				
Fixed Effects									
Period	YES	YES	YES	YES	YES	YES	YES	YES	YES
Countries	94	94	94	72	72	72	162	162	162
Partial R^2		0.04		0.05			0.02		
F-Test on excl. Inst.		21.81		14.52			7.55		
Hansen p-value		0.57		0.34					

Note: ***, **, * denote significance at the 1%, 5%, and 10% level, respectively. Constant and period fixed effects not reported. Country clustered standard errors reported 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 a smaller sample by Mankiw et al. (1992) that excludes 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 (2), critical values of 15% reported in column (5), and critical values of 20% reported in column (8). 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.

Chapter 5

Economic Effects of Natural Disasters: New Insights from New Data*

5.1 Introduction

How do natural disasters affect economic activity? How does a country's integration into global financial and goods markets, or the quality of its institutions, shape this effect? In a global environment, in which climate-related natural disasters are thought to be on the rise due to global warming, these are important questions. Using standard tools of growth empirics, a number of recent papers (Kahn, 2005; Noy, 2009; Strobl, 2011; Cavallo et al., 2012) have come up with insightful answers. However, up to now the analysis has been hampered by inadequate data on natural disasters.

So far, most studies have used information about disasters from the Emergency Events Database (EM-DAT), provided by the Centre for Research on the Epidemiology of Disasters (CRED) at Université Catholique de Louvain. "The database is compiled from various sources, including UN agencies, non-governmental organizations, insurance companies, research institutes and press agencies".¹ It contains outcome data: it reports the number of people killed or affected (i.e., injured or rendered homeless) or the estimated monetary damage. Mainly for two reasons, these features of EM-DAT may lead to biased results in regression analyses: first, selection into the database may systematically depend on country

* This chapter is based on joint work with Gabriel Felbermayr. It is based on the draft "Economic Effects of Natural Disasters: New Insights from New Data", Ifo Institute, 2012.

¹ <http://www.emdat.be/>.

characteristics; second, regressing EM-DAT outcome variables on economic aggregates such as GDP per capita may induce further correlation between the disaster variable and the error term. Presumably, this may bias estimated coefficients upwards.

In the present paper, we compile an alternative database which contains information on the physical strength of all natural disasters – earthquakes, volcanic eruptions, storms, floods, droughts – that have been recorded by geophysicists or climatologists from 1979 to 2010, essentially covering all countries of the world. Information about the exact coordinates of the events allows us to use geographic information system software and to map disasters to countries. We show that the likelihood of some disaster with given physical magnitude being reported in EM-DAT depends strongly on the affected country's GDP per capita. This finding warns that results based on EM-DAT may suffer from selection bias.

Next, we investigate the effect of natural disasters on per capita GDP. Standard neoclassical theory predicts a temporary drop in economic activity as physical capital is destroyed or the average productivity of productive assets (e.g., land) is reduced. Subsequently, increased investment replenishes the capital stock again and, asymptotically, puts it back to its steady state level. In terms of growth rates, theory predicts growth to be lower than trend on impact and higher than trend thereafter. Alternative hypotheses revolve around the idea that the destruction of physical capital allows the affected economy to replace outdated equipment and structures faster than along the balanced growth path (Caballero and Hammour, 1994). This may add to a temporary boom following the disaster. However, a lack of appropriate institutions, inadequate financing conditions, or limited access to international markets may hamper the catching-up process. Hence, the prediction is that a natural disaster lowers GDP per capita on impact. If the event is not strong, its output effect may not be measurable. A positive disaster response of GDP per capita would be hard to imagine, at least on impact. It would require that the investment boom raises the productive capacity of the economy so fast and strongly that the flow value of output accumulated over the year in question (i.e., GDP) turns out higher with the disaster than without.²

Our regressions provide very pervasive evidence that natural disasters do indeed lower GDP per capita temporarily. A disaster whose physical strength belongs to the top decile of the country-level disaster distribution reduces growth by about 3 percentage points. This effect is halved when looking at disasters belonging to the top 15 percentile. It is again

² Recall that, at constant total factor productivity and given capital stock, an investment boom needs to be exactly matched by lower consumption and/or net exports.

halved when looking at the top 20%. This intuitive pattern, well in line with theory, cannot be produced using the EM-DAT data. Finally, we investigate whether institutional quality and international openness helps spur the economic reconstruction process so that, even upon impact, the negative growth effect of a natural disaster is mitigated. We find that this is indeed the case and that, depending on the disaster type, better institutional quality, higher openness to trade, higher financial openness, and lower ODA inflows determine a country's ability to better cope with natural disaster shocks.

The remainder of this paper is structured as follows: the second section contains a brief review of the empirical literature on the disaster-growth relationship, the third section describes our data collection effort and compares the resulting data set to the EM-DAT data. The fourth section analyzes the growth impact of natural disasters and the fifth section studies how that impact is shaped by institutions or openness. Finally, the sixth section concludes.

5.2 Related Literature

Table 5.1 summarizes the related literature on natural disasters. To compare, the table lists the source of disaster data, disaster variables and methods used, as well as results for each paper. Most of the empirical literature uses available disaster data from EM-DAT and finds inconclusive and sometimes even counterintuitive results.³⁴

Table 5.1: Disaster Effects in the Literature

Author	Data	Disaster Measure	Method	Result
Albala-Bertrand (1993)	ECLA Statistical Yearbook for Latin America	Economic effects of disasters.	Before and after statistical analysis.	Neutral or positive effect of disasters on economic growth.

³ Other studies look into further consequences of disasters, for instance, Yang (2008) examines the consequences of disasters on international financial flows using Hurricane track data. Miguel et al. (2004) use precipitation data to investigate the impact of economic shocks on civil conflict in Sub-Saharan Africa, and Brückner and Ciccone (2011) examine the democratic consequences of economic shocks caused by exogenous weather variation in Sub-Saharan Africa.

⁴ Theoretical applications on the impact of natural disaster occurrence on output dynamics do not provide a clear cut prediction either (see, i.e., Soretz, 2007; Hallegatte and Dumas, 2009; Narita et al., 2010; Ikefuji and Horii, 2012).

Table 5.1 – continued

Author	Data	Disaster Measure	Method	Result
Skidmore and Toya (2002)	EM-DAT, Davis (1992)	Total number of disasters reported.	OLS.	Geophysical disasters negatively or neutrally, climatic events positively affect long-run economic growth.
Raddatz (2007)	EM-DAT	Count variable base on decision rule by the IMF: affects at least half a percent of a country's population, or causes damages of at least half a percent of national GDP, or results in more than one fatality every 10,000 people.	Panel VAR.	Climatic and humanitarian disasters reduce per capita GDP.
Hochrainer (2009)	EM-DAT, Munich Re NatCat Service	Monetary losses.	ARIMA.	Disasters adversely affect GDP.
Noy (2009)	EM-DAT	Killed over population, affected over population, damage over GDP.	Hausman-Taylor.	Negative impact of disasters measured by monetary damage on GDP growth, no effect if other measures are used.
Leiter et al. (2009)	EM-DAT	Flooding events by region.	OLS, 2SLS	Firms in flood-affected areas show higher growth rates than those in unaffected regions.
Noy and Nualsri (2011)	EM-DAT	Killed over population, affected over population, damage over GDP.	Panel VAR.	Negative impact of disasters measured by killed on long-run growth, no effect with monetary damage.
Strobl (2011)	Hurricane track data	Index of hurricane destructiveness.	Bias correction LSDV.	Negative effect on output.
Loayza et al. (2012)	EM-DAT	Affected over population.	GMM.	Positive impact of floods, negative effect of droughts, no effect of earthquakes and storms on GDP growth.
Fomby et al. (2012)	EM-DAT	Count variable base on constructed threshold on killed, affected, damage.	Panel VARX.	Positive impact of floods, negative effect of droughts, earthquakes adversely affect agricultural growth but positively affect non-agricultural growth, storms have a small and transient negative impact on GDP growth.
Cavallo et al. (2012)	EM-DAT	Percentile-based global definition of large disaster using killed/population.	Compare actual evolution of post-disaster GDP with counterfactual.	No effect on GDP per capita.
Felbermayr and Gröschl (2013)	EM-DAT	Count variable base on decision rule by Munich Re: 1,000 or more killed, 100,000 or more affected, or at least 1 billion US\$ monetary damage.	PPML gravity, FE/FD 2SLS	Positive effect of importer disaster, negative effect of exporter disaster on imports. Neutral, positive or negative effect of disaster on GDP p.c., depending on the sample.

Table 5.1 – continued

Author	Data	Disaster Measure	Method	Result
The Role of other Factors				
Kahn (2005)	EM-DAT	Killed over population, affected over population, damage over GDP.	Probit, OLS	Advanced economies and countries with qualitatively better institutions face lower costs from disaster.
Skidmore and Toya (2007)	EM-DAT	Disaster-related deaths, damage/GDP.	OLS	Countries with better institutions, higher per capita income, higher educational attainment, higher degree of openness, more complete financial systems, and higher levels of government spending better cope with disaster shocks.
Disasters in the Gravity Model				
Gassebner et al. (2010)	EM-DAT	Count variable base on decision rule by Munich Re: 1,000 or more killed, 100,000 or more affected, or at least 1 billion US\$ monetary damage.	FE gravity.	Neutral or positive effect of importer disaster, neutral or negative effect of exporter disaster on imports.
Oh and Reuveny (2010)	EM-DAT	Total number of disasters.	FE gravity.	Negative effect of (importer/exporter) climatic disasters, positive effect of (importer/exporter) geophysical disasters on imports.

5.3 Data

5.3.1 Disaster Data

Data on the impact of natural disasters obtained from existing databases, such as the Emergency Events Database (EM-DAT), are a function of economic development itself (Kahn, 2005; Skidmore and Toya, 2007).⁵ However, estimating causal relations between disasters and economic variables, such as income per capita, migration, or trade, requires appropriate measures on the disaster impact.⁶ Noy (2009) and Cavallo et al. (2012) even directly mention that comprehensive physical disaster intensity measures would be preferable for causal empirical analysis. As a result, this section proposes a novel and comprehensive dataset on exogenous measures of physical disaster intensity, such as Richter scale and wind

⁵ EM-DAT reports the outcome of disasters, such as how many people were killed or affected and how much monetary damage was done.

⁶ Nonetheless, EM-DAT measures of facts and disaster outcomes are often treated as exogenous variables in the literature.

speed. We utilize data from five primary sources mainly used in geophysics or climatology. We build two datasets. First, an event related database, which comprises information on the intensity of the disaster, the month, year and country of occurrence. Second, we collapse the data on a country-year level to be able to conduct an analysis on the impact of disasters on economic activity.

Earthquakes. Seismic data stem from the Incorporated Institute for Seismology (IRIS), which lists the Richter scale of every earthquake on the planet by the position (latitude and longitude) of the epicenter. Using information on the epicenter, we map the data to the corresponding countries using Flinn-Engdahl codes⁷ and geographic information system (GIS) software⁸. We consider earthquakes on land and those happening right off the coastline of a specific country. The maximum Richter scale that occurs in our data is 8.8, which was measured on the Philippines in February 2006. Two measures are obtained from this data. First, we use the maximum Richter scale that occurs within a certain country in a year as a measure of physical disaster intensity of earthquakes. Second, Cavallo et al. (2012) report that only sufficiently large events have a measurable impact on income. To obtain a measure of large-scale events, we calculate the density of earthquake intensity by country on daily data. We construct a cutoff value taking into account only the top 10 (15, 20) percent.⁹ We then count events per country per year which fall in the 10 (15, 20) percent largest earthquakes experienced by the country over the period 1979 to 2010. In addition, we also calculate the distribution of Richter scale values on global daily data and construct the cutoff according to the global level. As it matters at what point in time during a year a disaster occurred, we weight our count variables of disasters by the onset month, adapting a procedure used by Noy (2009):

$$\text{Quake Onset}_{i,t} = \text{Quake}_{i,t} * ((13 - \text{onset month})/12), \quad (5.1)$$

⁷ The Flinn-Engdahl seismic and geographical categorization of regions breaks the world into regions based on geographic and political boundaries, where each region is assigned a unique number. Regional boundaries are defined at one-degree intervals and may thus differ from national boundaries.

⁸ GIS uses geographical location as the key index variable to relate information to specific countries, or regions. Any variable that can be located spatially can be referenced using GIS software. Information, such as precipitation in a certain region, storm or epicenter location anywhere on the globe may be mapped to countries using information on their geographical occurrence, such as longitude and latitude, respectively.

⁹ Mean and maximum values of Richter scale, as well as cutoff levels at top 10, 15 and 20 percent for earthquakes by country are reported in Table 5.20 in Appendix 5.A.

$$\text{Quake Onset}_{i,t+1} = \text{Quake}_{i,t+1} * (1 - ((13 - \text{onset month})/12)). \quad (5.2)$$

Equation (5.1) takes into account the impact a disaster has in the year of occurrence. As the impact of a disaster that happens in December might spill over into the following year, we use equation (5.2) to account for spill over effects.¹⁰

Volcanic Eruptions. The Global Volcanism Program of the Smithsonian Institution records volcanic eruptions and specifies the magnitude by the Volcanic Explosivity Index (VEI).¹¹ The Global Volcanism Program documents the eruption start date and stop date of all – small and large – volcanic eruptions, if available. The listed VEI is the highest VEI reached during the entire eruptive episode. The highest reported VEI in our dataset is 6, which was recorded for an outbreak by the Pinatubo on the Philippines in 1991. We obtain two measures, the Volcanic Explosivity Index as the disaster intensity measure for volcanic eruptions, and a count variable of large volcanic eruptions on the country and the global level, respectively. We count large events where the divergence in the experienced VEI belongs to the top 10 (15, 20) percentile of the country-level (global-level) disaster distribution.¹² We weight the measure by onset month.

Storms. For wind speed, we use two primary data sources, the International Best Track Archive for Climate Stewardship and Global Surface Summary of Day data. The International Best Track Archive for Climate Stewardship (IBTrACS data version v03r03) maintained by the National Climatic Data Center of the National Oceanic and Atmospheric Administration (NOAA) records data of individual hurricane events, positions (latitude and longitude) of hurricane centers at 6-hourly intervals, combined with intensity information (wind speed in knots and barometric pressure). The data incorporate information from a variety of sources, such as reconnaissance aircraft, ships, and satellites.¹³ The raw 'best track' data give no indication on affected countries. We use geographic information system (GIS) software to map hurricane position data to affected countries. Not only do we consider

¹⁰ The weighting scheme assumes that disasters always exert a full year's impact. Hence, a disaster happening in May, affects variables in remaining months of the year of occurrence, but also exerts an impact on January to April in the subsequent year.

¹¹ The Volcanic Explosivity Index is coded in a logarithmic scale and reaches from 0 to 8. The VEI combines the volume of tephra ejected and the height of the eruption cloud.

¹² Table 5.20 in Appendix 5.A reports mean and maximum values of VEI, as well as cutoff levels at top 10, 15 and 20 percent for volcanoes by country level.

¹³ Since the 1960s most of the data stem from satellites (Chu et al., 2002; Yang, 2008).

positions (latitude and longitude) on land, but we also consider those positions which are off the coastline of a country.¹⁴ To capture also tornadoes, winter and summer storms (not captured by the data), the hurricane track data is matched to daily data of the Global Surface Summary of Day (GSOD) data (version 7) on maximum wind speed and wind gust. This dataset includes records of wind speed from over 9000 worldwide stations and is produced by the National Climatic Data Center (NCDC). GSOD uses daily summaries of hourly observations contained in the Integrated Surface Data (ISD). We collapse daily extremes on wind speed and wind gust over all stations on a country basis. Combining both datasets, we obtain a combined measure that brings together wind speed from the hurricane track data and wind speed from GSOD. We use the maximum wind speed in knots on a country basis as our disaster intensity measure for storms. Following previous considerations, we construct count measures of large storm events taking into account the top 10 (15, 20) percent of wind speeds at the country (global) level.¹⁵ Storm count measures are weighted by onset month.

Precipitation. Precipitation data are recorded by the Goddard Space Flight Center of the National Aeronautics and Space Administration (NASA) in the Global Precipitation Climatology Project (GPCP). The GPCP combines weather station rainfall gauge measures and satellite information. Average monthly precipitation are provided in millimeters (mm) for 2.5 latitude and longitude degree grid nodes. Following Miguel et al. (2004) and Brückner and Ciccone (2011), we bring the data to the country level by matching each rainfall estimate per node to the corresponding country using GIS software. We average rainfall across nodes to produce an estimate of total monthly rainfall per country.¹⁶ Our principal measure of exogenous weather variation is the difference in monthly rainfall in mm, which we define as the proportional change in monthly rainfall from the monthly mean over the total available time period (1979-2010), $(R_{i,t} - R_{i,1979-2010})/R_{i,1979-2010}$. We distinguish two disaster types, floods and droughts. We measure flooding events by the positive difference in monthly precipitation. Droughts are, however, different in the sense that only one dry month usually does not cause a drought, but several months in a row or within the year might do so. For

¹⁴ As tropical storms and hurricanes can be relatively wide, tropical storm-force and hurricane-force winds may cause destruction even though they did not make landfall (National Hurricane Center, 1999).

¹⁵ Table 5.21 in Appendix 5.A reports mean and maximum values of wind speed, as well as cutoff levels at top 10, 15 and 20 percent for storms at the country level.

¹⁶ If no degree node fell within the national boundaries of a country, we assigned the rainfall measures from the nearest node(s) to their borders. This is similar to the approach taken by Miguel et al. (2004) and Brückner and Ciccone (2011).

this reason, we create a indicator variable for droughts, which is one if at least three months in a row fall below 50% of the long-run average monthly mean, or if at least five months within a year are 50% below the long-run monthly mean, and zero otherwise. In a second step, we identify large events if the divergence in monthly precipitation belongs to the top decile (15 or 20 percentile) of the country-level (global-level) disaster distribution.¹⁷ We count large events on a country-year basis weighted by onset month.

In the following, we focus on two datasets: (i) an event based set, which merges physical disaster intensity measures on a monthly basis to disaster occurrence and disaster outcome variables reported in EM-DAT¹⁸, and (ii) a panel dataset aggregated to country-year observations. The latter comprises information on country-year maximum physical disaster intensity measures and on aggregate large disaster measures. Note that large disasters combine the 10 (15, 20) percent largest earthquakes, storms, volcanic eruptions, and absolute rainfall differences into a simple count measure that accounts for the onset month, $\text{Disaster Onset}_{i,t} = \text{Quake Onset}_{i,t} + \text{Storm Onset}_{i,t} + \text{Volcano Onset}_{i,t} + \text{Precipitation Onset}_{i,t}$. As the impact of a disaster on the economy might depend on the disaster intensity relative to the size of the economy, we follow the literature (i.e., Skidmore and Toya, 2002) and scale disaster variables by land area. Both datasets are available for the period 1979 to 2010.

5.3.2 Other Data

To assess the quality of our novel comprehensive dataset, we take data on people killed, affected, or monetary damage from EM-DAT provided by the Centre for Research on the Epidemiology of Disasters (CRED). We construct a large disaster variable by deploying the decision rule based on the convention of 'great natural disasters' by Munich Re (2006). Disasters are defined as large if events (i) killed 1,000 or more persons; (ii) affected 100,000 or more persons; or (iii) caused a monetary damage of 1 billion or more US dollars. To compare damage over time, we convert dollar values into constant 2000 US dollars using the US GDP deflator from the World Development Indicators (WDI). We count the number of large disasters in EM-DAT and scale them by onset month and land area accordingly.

¹⁷ Table 5.21 in Appendix 5.A reports mean and maximum values of absolute precipitation difference in mm, as well as cutoff levels at top 10, 15 and 20 percent for precipitation differences at the country level.

¹⁸ Observations in EM-DAT and in the new database do sometimes not match perfectly. Even though many more observations are available in our physical disaster measures dataset, a small part of observations in EM-DAT has no counterpart in the new database. A list of observations and the reason they did originally not match can be found in Table 5.22 in Appendix 5.A.

To estimate the disaster impact on economic activity, we use real income per capita, population, and trade openness from the Penn World Tables (PWT, 7.1). Further controls include the Polity index from the Polity IV Project. The polity index is rescaled and normalized between 0 and 1, with 0 being the most autocratic state and 1 being the most democratic nation. Data on institutional quality come from Acemoglu et al. (2001), originally provided by the Political Risk Services in the International Country Risk Guide (ICRG). The protection against expropriation risk is scaled from 0 to 10, where a higher value means less risk. We follow Acemoglu et al. (2001) and use the average value for each country, as values for several years are missing. As a measure of international financial openness, we use the updated Chinn-Ito index, which measures the degree of capital account openness of a country. For a more detailed description on the index see Chinn and Ito (2008). Alternative to trade openness, we use the index of trade policy openness constructed by Sachs and Warner (1995) and extended by Wacziarg and Welch (2008). Data on net official development assistance (ODA) received by a country as a share of gross national income (GNI) and other macroeconomic control variables, such as the domestic credit in banking sector as a share of GDP, gross capital formation as a share of growth, foreign direct investment as a share of GDP, consumer price inflation, the interest rate, or current account balance as a share of GDP stem from WDI. On geography, we use the absolute value of the distance from the equator (latitude) and surface area in squared kilometers from CEPII (2005).

5.3.3 Stylized Facts on Disasters

To give a summary of our novel data on disaster intensity, we show in Figure 5.1 where physical disaster measures on earthquakes, volcanic eruptions, wind, and positive and negative differences in monthly precipitation cluster.

Looking only at large disasters, as coded in EM-DAT, in our event based dataset in Table 5.2, we find that large earthquakes have a mean Richter scale of 6.2, while large volcanic eruptions have a mean VEI of 3.44. Storms are listed as large-scale with a mean wind speed of 72.66 knots, a flood is large with a mean divergence in monthly precipitation of 0.389 mm and droughts are large in scale with a negative mean difference in monthly precipitation of 0.360 mm. While droughts cause the highest death toll and affected population, storms are, on average, responsible for the largest monetary damages.

Figure 5.1: Observations by Event and Disaster Type

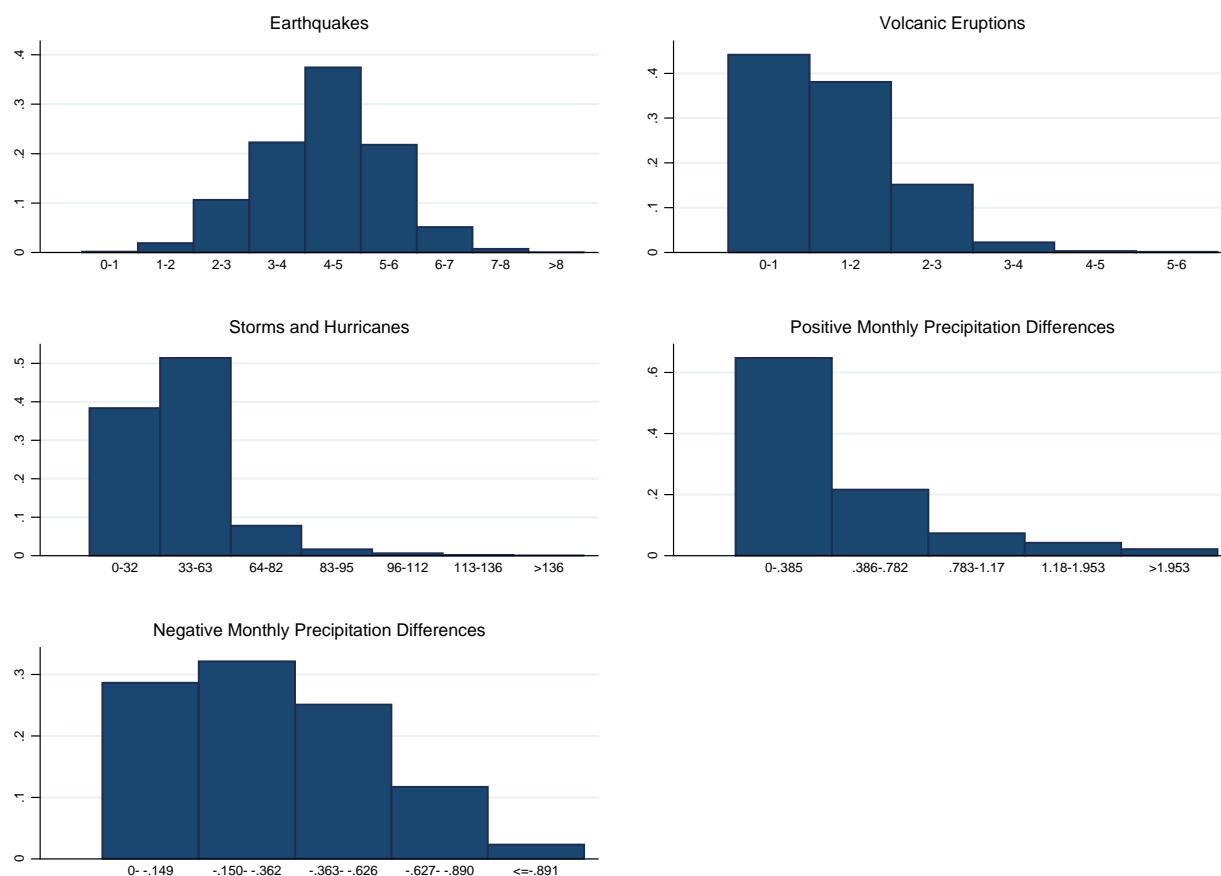


Table 5.2: Large Disaster, Event Based Data (1979-2010)

Large Disaster Type	Physical Magnitude					Costs Reported in EMDAT (Mean in %)		
	Obs.	Mean	Std. Dev.	Minimum	Maximum	Killed over population	Affected over population	Damage over GDP
Earthquake	146	6.2	0.986	3.7	8.6	0.023	1.401	1.404
Volcano	9	3.44	1.424	1	6	0.009	1.609	0.542
Storm	494	72.66	25.884	12	150	0.002	3.644	11.465
Flood	467	0.389	0.783	0.003	13.549	0.001	3.092	0.359
Drought	253	-0.360	0.266	-0.983	-0.001	0.143	18.027	1.526

Note: The decision rule on large disasters in EM-DAT builds on the convention of Munich Re (2006). Large-scale disasters are defined as events that (i) caused 1,000 or more injured or dead; (ii) affected 100,000 or more persons; or (iii) caused a monetary damage of 1 billion or more US dollars.

Table 5.3 reports that far less observations of a particular disaster intensity category (i.e., Richter scale of 6-7) are reported in EM-DAT. In other words, we have many more observations of the same physical strength in our new database. Further, on average, disaster costs (killed, affected, monetary damage) increase with the physical strength of a disaster.

In a next step, we examine how well our event data on Richter scale, wind speed, VEI, or absolute precipitation difference do in predicting natural disasters reported in EM-DAT. Similar to Kahn (2005), but using event based data, we construct a dummy variable that

Table 5.3: Costs by Physical Magnitude, Event Based Data (1979-2010)

Physical Magnitude Scale				
PANEL A: EARTHQUAKES				
Richter scale	5 - 6	6 - 7	7 - 8	≥ 8
Total observations	8,065	2,058	303	22
Reported in EM-DAT	281	252	110	9
Killed over pop	0.001	0.001	0.028	0.011
Affected over pop	0.271	0.281	0.730	1.074
Damage over GDP	0.402	0.375	1.499	1.841
PANEL B: VOLCANOES				
Volcanic Explosivity	2 - 3	3 - 4	4 - 5	≥ 5
Total observations	370	147	22	4
Reported in EM-DAT	35	32	10	3
Killed over pop	0.00003	0.008	0.0001	0.001
Affected over pop	0.094	0.958	0.577	0.698
Damage over GDP	0.003	0.282	0.444	0.082
PANEL C: STORMS				
Storm Category	Tropical	Scale 1	Scale 2	\geq Scale 3
Wind Speed in knots	33 - 63	64 - 82	83 - 95	≥ 96
Total observations	38,798	5,890	1,232	600
Reported in EM-DAT	856	556	247	230
Killed over pop	0.0004	0.0002	0.0002	0.003
Affected over pop	1.021	0.680	2.266	4.188
Damage over GDP	11.261	0.413	0.604	1.678
PANEL D: FLOODS				
Positive Δ Precipitation	0.387 - 0.782	0.783 - 1.17	1.18 - 1.953	≥ 1.954
Total observations	7,206	2,449	1,406	709
Reported in EM-DAT	448	150	96	34
Killed over pop	0.0002	0.0003	0.001	0.003
Affected over pop	0.837	1.096	0.992	4.546
Damage over GDP	0.181	0.547	0.409	0.578
PANEL E: DROUGHTS				
Negative Δ Precipitation	-0.150 - -0.362	-0.363 - -0.626	-0.627 - -0.890	≤ -0.890
Total observations	13,722	10,723	5,008	1,004
Reported in EM-DAT	140	126	77	26
Killed over pop	0.088	0.088	0.098	0.670
Affected over pop	12.252	18.559	24.110	18.895
Damage over GDP	0.287	0.436	0.806	0.029

equals one if a disaster is reported in EM-DAT and zero otherwise. We then estimate the probability of a disaster being reported in EM-DAT using a linear probability specification, such that

$$\Pr(\text{Report}_{k,i,t}) = \beta_1 \text{Intensity}_{k,i,t} + \beta_2 \mathbf{X}_{i,t} + \nu_i + \nu_t + \varepsilon_{k,i,t} \quad (5.3)$$

where, $\text{Report}_{k,i,t}$ is one if a specific disaster event k is reported in a specific country i at time t in EM-DAT, and zero otherwise. $\text{Intensity}_{k,i,t}$ is the maximum Richter scale value matched to EM-DAT for earthquake events, it is the maximum wind speed for storm events, it is the maximum VEI reported for volcanic eruptions, and it is the maximum absolute difference in monthly precipitation in mm reported for drought or flooding events. Vector $\mathbf{X}_{i,t}$ contains time-varying controls, such as the logarithm of total population, an index for democracy, and five bins for GDP per capita according to the 1-20th, 21-40th, 41-60th, 61-80th, an 81-100th percentile of the income distribution. These are equivalent to the logarithmic GDP per capita of $[0, 6.198)$, $[6.198, 7.596)$, $[7.596, 8.994)$, $[8.994, 10.392)$, $[10.392, \text{max})$. To control for time-invariant country specific characteristics, we include country fixed effects. Time fixed effects control for time trends common to all disaster events.

Table 5.4 reports results. Looking at earthquakes in column (1), we find that the likelihood that an earthquake is reported in EM-DAT increases with our physical measure. We also find that the probability of reporting an earthquake increases with income (the reference group are countries in the lowest income bin). In column (2), we restrict the analysis to the probability of reporting volcanic eruptions. Compared to other disaster types, we have relatively few observations for volcanic eruptions. Still, VEI strongly explains volcanic eruption events reported to EM-DAT, but income seems not to matter. Column (3) reports that the higher the wind speed, the higher the probability of storms being reported in EM-DAT. The likelihood of reporting storms to EM-DAT increases in the second income bin relative to the poorest nations. Column (4) reports that the higher the difference in monthly precipitation, the higher the probability that a flood is reported to EM-DAT. Richer nations report more flooding events compared to the poorest countries. Finally, the probability of reporting droughts to EM-DAT increases with the divergence in monthly precipitation¹⁹, while income seems not to play a role. To conclude, we find that our physical disaster intensity measures strongly predict the reporting of respective disasters to EM-DAT, while income plays a role for earthquakes, storms and floods.

¹⁹ Note that droughts are constituted by negative precipitation differences. However, for convenience, we here rescale the measure for droughts to the absolute difference in monthly precipitation.

Table 5.4: Probability of Disaster Reporting, Event Based (1979-2010)

Dependent Variable:	Variable equals one if disaster is reported in EM-DAT				
	Earthquakes (1)	Volcano (2)	Storms (3)	Floods (4)	Droughts (5)
Richter scale	0.051*** (0.00)				
Volcanic Explosivity Index		0.099*** (0.02)			
Wind speed			0.002*** (0.00)		
Δ precipitation				0.031*** (0.00)	0.002* (0.00)
ln GDP per capita [6.198,7.596)	0.018* (0.01)	-0.298 (0.24)	0.016*** (0.01)	0.012** (0.01)	0.003 (0.00)
ln GDP per capita [7.596,8.994)	0.029** (0.01)	-0.388 (0.24)	0.010 (0.01)	0.023*** (0.01)	0.004 (0.00)
ln GDP per capita [8.994,10.392)	0.034*** (0.01)	-0.354 (0.25)	0.012 (0.01)	0.017** (0.01)	0.003 (0.00)
ln GDP per capita [10.392,max)	0.037** (0.01)	-0.364 (0.25)	0.010 (0.01)	0.012 (0.01)	0.002 (0.00)
Country Fixed Effects	YES	YES	YES	YES	YES
Year Fixed Effects	YES	YES	YES	YES	YES
Observations	27,820	870	53,166	57,168	57,168
Adjusted R^2	0.126	0.171	0.195	0.090	0.018

Note: ***, **, * denote significance at the 1%, 5%, and 10% level, respectively. Constant, country and time fixed effects included but not reported. Controls included but not reported comprise the logarithm of population and an index of democracy. All estimations use a linear probability specification.

We further investigate the quality of our data by examining the role of disaster intensity in experienced costs as recorded in EM-DAT. We use a similar setup as Kahn (2005), but build our analysis on event based data. We estimate

$$\ln \text{Outcome}_{k,i,t} = \alpha_1 \text{Intensity}_{k,i,t} + \alpha_2 \mathbf{X}_{i,t-1} + \nu_i + \nu_t + \varepsilon_{k,i,t} \quad (5.4)$$

where $\text{Outcome}_{k,i,t}$ is the logarithm of killed over population, affected over population, or monetary damage over GDP from EM-DAT for a specific event k in country i at time t , respectively. $\text{Intensity}_{k,i,t}$ is defined as above, the vector $\mathbf{X}_{i,t}$ contains the logarithm of GDP per capita, of total population, and an index of democratization. Again, we include time and country fixed effects.²⁰ We separately conduct the cost analysis for earthquakes, volcanic eruptions, storms, and floods.²¹

²⁰ Country fixed effects control for country specific characteristics, but also for the mean vulnerability of a country and the mean level of being at risk of disasters.

²¹ For volcanic eruptions, we consider only killed and affected over population, as too few observations are available on monetary damage over GDP to conduct an empirical analysis. We do not consider droughts

Table 5.5: Costs Caused by Geophysical Disasters, Event Based (1979-2010)

Disaster Type:	Earthquakes			Volcanoes	
	Killed over population (log) (1)	Affected over population (log) (2)	Damage over GDP (log) (3)	Killed over population (log) (4)	Affected over population (log) (5)
Richter scale	0.857*** (0.17)	0.824*** (0.15)	0.633*** (0.20)		
Volcanic Eruption Index				2.172 (2.74)	0.365* (0.20)
ln GDP per capita	0.784 (0.53)	1.477*** (0.52)	0.640 (0.88)	2.256 (3.11)	-1.764 (2.70)
Country Fixed Effects	YES	YES	YES	YES	YES
Year Fixed Effects	YES	YES	YES	YES	YES
Observations	512	686	282	29	89
Adjusted R^2	0.333	0.251	0.361	0.336	0.567

Note: ***, **, * denote significance at the 1%, 5%, and 10% level, respectively. Constant, country and time fixed effects included but not reported. Further controls included but not reported comprise the logarithm of population and an index of democracy. Monetary Damage of volcanic eruptions have too little (18) observations and are thus not considered in the analysis.

Table 5.5 reports results on the costs caused by earthquakes or volcanic eruptions. As can be seen in columns (1) to (3), Richter scale has a high predictive power in explaining the cost of an earthquake event for either of the three outcome measures. GDP per capita only increases the number of affected over population. For volcanic eruptions, we have very little observations in EM-DAT. Nonetheless, column (5) reports that the larger the volcanic eruption the higher the number of people affected relative to total population.

Table 5.6 reports results on the costs caused by storms or floods. The death toll over population, affected over population and monetary damage over GDP of storm events are, on average, strongly explained by wind speed observed in our new database (compare columns (1) to (3)). In addition, income significantly increases affected over population, while it decreases monetary damage over GDP for storm events. Similarly, the costs from flooding events are well explained by differences in monthly precipitation in columns (4) to (6), and damage over GDP decreases with GDP per capita.

Overall, our results show that physical disaster intensity measures have a strong prediction power in explaining the likelihood of a disaster being reported and the costs caused by natural

in this context, as they are hard to identify with monthly precipitation in the events based setup. As explained above, droughts may be caused by longer periods of little or no rain compared to the long run mean. In the following, we thus define droughts as an indicator variable that is one if at least three months in a row fall below 50% of the long-run average monthly mean, or if at least five months within a year are 50% below the long-run monthly mean, and zero otherwise.

Table 5.6: Costs Caused by Climatological Disasters, Event Based (1979-2010)

Disaster Type:	Storms			Floods		
	Killed over population (log) (1)	Affected over population (log) (2)	Damage over GDP (log) (3)	Killed over population (log) (4)	Affected over population (log) (5)	Damage over GDP (log) (6)
Wind speed	0.016*** (0.00)	0.033*** (0.00)	0.029*** (0.00)			
Δ precipitation				0.200*** (0.06)	0.407*** (0.11)	0.525*** (0.12)
ln GDP per capita	-0.205 (0.19)	0.849* (0.46)	-0.999*** (0.37)	-0.110 (0.16)	0.408 (0.29)	-0.761** (0.37)
Country Fixed Effects	YES	YES	YES	YES	YES	YES
Year Fixed Effects	YES	YES	YES	YES	YES	YES
Observations	1,565	1,367	1,087	2,218	2,628	1,096
Adjusted R^2	0.539	0.408	0.309	0.461	0.329	0.317

Note: ***, **, * denote significance at the 1%, 5%, and 10% level, respectively. Constant, country and time fixed effects included but not reported. Further controls included but not reported comprise the logarithm of population and an index of democracy.

disasters as recorded in EM-DAT. The role income plays in the probability of being reported in EM-DAT and the impact of GDP on disaster costs measures both warn that EM-DAT disaster variables are jointly determined with economic development and may suffer from selection bias.

5.4 Empirical Strategy

The following section examines the impact of natural disasters on GDP per capita. Following Mankiw et al. (1992), Islam (1995), and most subsequent empirical models on economic performance, we include lagged GDP per capita to estimate a dynamic model. As the dynamic model contains a potential unit root, we follow the recommendation of the literature and differentiate the left-hand side variable. The specification that follows is

$$\Delta \ln y_{i,t} = \alpha_1 \ln y_{i,t-1} + \alpha_2 D_{i,t} + \alpha_3 \mathbf{X}_{i,t-1} + \nu_i + \nu_t + \varepsilon_{i,t}, \quad (5.5)$$

where $\Delta \ln y_{i,t} = \ln y_{i,t} - \ln y_{i,t-1}$ is the difference in logarithmic real GDP per capita, which corresponds to a growth rate, and $\ln y_{i,t-1}$ is the lagged log of GDP per capita. $D_{i,t}$ is either the number of large-scaled disasters weighted by onset month and scaled by land area, or our physical disaster intensity measures for various types of disasters over land

area. The vector of controls $\mathbf{X}_{i,t-1} = [\ln POP_{i,t-1}, POLITY_{i,t-1}, OPEN_{i,t-1}, OTHER_{i,t-1}]$ includes the log of total population, the polity index rescaled from 0 to 1, and openness to international trade, namely imports plus exports divided by GDP. $OTHER_{i,t-1}$ comprises further macroeconomic factors, such as domestic credit by the banking sector, gross capital formation, foreign direct investment, consumer price inflation, real interest rates, and current account balance. To allow for a delayed adjustment of income per capita on economic developments, we use the lag of control variables. Time-invariant country characteristics are accounted for by country-specific fixed effects, while year fixed effects control for common time trends.

Including lagged GDP per capita as an explanatory variable, may introduce a potential bias.²² However, as Nickell (1981) predicts, the bias diminishes with large T.²³ Following this, the bias of estimates in our panel with a rather long time dimension (T=30) is supposedly relatively small (Judson and Owen, 1999). Still, for robustness, we estimate the equation by two-step feasible generalized method of moments (GMM) techniques and a method proposed by Anderson and Hsiao (1982). We instrument the lagged real GDP per capita accordingly.²⁴

For α_2 , we expect to see economic growth to be lower than trend on impact, as GDP per capita at the time of the disaster (t) has to decrease as physical capital is destroyed or the average productivity of productive assets (e.g., land) is reduced. The adverse effect thus captures the productivity gap or forgone production due to the disaster. In subsequent periods, increased investment replenishes the capital stock again and either puts income below, back to or above its steady state level in the long-run.²⁵

The second part of the empirical analysis examines factors that possibly mitigate the shock from a disaster. Specifically, we estimate

$$\Delta \ln y_{i,t} = \alpha_1 \ln y_{i,t-1} + \alpha_2 D_{i,t} + \alpha_3 \mathbf{X}_{i,t} + \alpha_4 D_{i,t} \times Z_i + \nu_i + \nu_t + \varepsilon_{i,t}, \quad (5.6)$$

²² With our fixed-effects model, estimates are biased downwards if $y_{i,t-1}$ and ν_i are correlated.

²³ In his paper, Nickell (1981) shows that the bias from including a lagged dependent variable approaches zero as T approaches infinity. Hence, he shows that the fixed effects estimator performs well in cases where the panel has a large time dimension.

²⁴ As T gets large, computational requirements increase substantially. For this reason, we reduce the number of lagged values used as instruments to a subset of available lagged values. We use just one instrument (see also Judson and Owen, 1999). This increases the computational efficiency without significantly detracting from effectiveness.

²⁵ See Hochrainer (2009) Figure 1 for a graphical illustration of possible effects.

where all variables are as above and $D_{i,t} \times Z_i$ captures an interaction term between the disaster intensity variable and geography (latitude) or economic factors (risk of expropriation, Sachs-Warner trade policy openness, multilateral openness to trade, Chinn-Ito financial openness, and ODA received). While variables capturing the disaster intensity are exogenous by construction, some economic factors might be jointly determined with income per capita. As instruments are not readily available for most variables in the panel framework, we use the initial value of economic variables in 1979, respectively.

Controlling for geography and other sources of economic stability, we expect to find evidence that natural disasters decrease per capita income growth on impact (a negative α_2). For α_4 , we presume that countries more open to financial transactions and trade, and countries with superior institutions can better cope with consequences from natural disasters (a positive α_4).²⁶

5.5 Natural Disasters and Growth

5.5.1 Benchmark Results

This section analyzes the impact of natural disasters on per capita GDP growth.²⁷ Table 5.7 reports results for large disasters from EM-DAT and disasters from the physical disaster intensity database on the 10, 15 and 20 percent largest disasters at the country and at the global level, respectively. All specifications in Table 5.7 use disasters measures weighted by onset month and scaled by land area.

Table 5.7 does provide pervasive evidence that natural disasters do indeed lower GDP per capita temporarily, but not when large disasters are coded from EM-DAT. A disaster whose physical strength belongs to the top decile of the country-level disaster distribution reduces growth by about 3 percentage points (column (2)). This effect is halved when looking at disasters belonging to the top 15% (column (3)). It is again halved when looking at the top 20% (column (4)). But, a disaster has to be large enough to have a substantial impact

²⁶ Some disaster types may also show up to have a positive effect on economic activity. This revolves around the idea that the destruction of physical capital allows the affected economy to replace outdated equipment and structures faster than along the balanced growth path (Caballero and Hammour, 1994). This may add to a temporary boom following the disaster.

²⁷ Summary results including the mean and standard deviations, as well as a description of variables are reported in Table 5.14 in Appendix 5.A.

Table 5.7: EMDAT versus NEW Database, Various Cutoff levels, Growth Rates (1979-2010)

Dependent Variable: $\Delta \ln$ GDP per capita		NEW Disaster Measure						
Disaster Variable	EM-DAT	10% largest at country level	15% largest at country level	20% largest at country level	10% largest at global level	15% largest at global level	20% largest at global level	
Cutoff Level	(1)	(2)	(3)	(4)	(5)	(6)	(7)	
EMDAT Disaster Onset _{<i>t,t</i>}	-0.060 (0.10)							
NEW Disaster Onset _{<i>t,t</i>}		-0.032*** (0.01)	-0.016** (0.01)	-0.008** (0.00)	-2.204* (1.15)	-0.717** (0.30)	-0.351 (0.22)	
Controls								
\ln GDP per capita _{<i>t,t-1</i>}	-0.084*** (0.01)	-0.085*** (0.01)	-0.085*** (0.01)	-0.085*** (0.01)	-0.084*** (0.01)	-0.084*** (0.01)	-0.084*** (0.01)	
Country Fixed Effects	YES	YES	YES	YES	YES	YES	YES	
Year Fixed Effects	YES	YES	YES	YES	YES	YES	YES	
Observations	1,861	1,861	1,861	1,861	1,861	1,861	1,861	
Countries	112	112	112	112	112	112	112	
R ² within	0.236	0.238	0.238	0.237	0.237	0.237	0.237	

Note: ***, **, * denote significance at the 1%, 5%, and 10% level, respectively. Constant, country and time fixed effects included but not reported. The decision rule for large-scale disasters from EMDAT bases on the convention of 'great natural disasters' by Munich Re (2006) and the United Nations. Disasters are defined as large if (i) 1,000 or more were killed, or (ii) 100,000 or more affected, or (iii) at least 1 billion US dollar monetary damage (made comparable over time using the deflator on US dollars from WDI). Columns (2) to (4) use country cutoff levels, while columns (5) to (7) use global cutoff levels as defined in the data section.

on economic growth (Table 5.7 column (7)). The adverse effects of large-scale disasters on growth are in line with findings by Noy (2009) and Loayza et al. (2012).

Table 5.8: EMDAT versus New Database and Controls, Growth Rates (1979-2010)

Dependent Variable:	$\Delta \ln$ GDP per capita		
Disaster Variable	EM-DAT	NEW Disaster Measure	
Cutoff Level	large disaster	10% largest at country level	15% largest at global level
	(1)	(2)	(3)
EMDAT Disaster Onset $_{i,t}$	-0.124 (0.09)		
NEW Disaster Onset $_{i,t}$		-0.019** (0.01)	-0.804*** (0.28)
Controls			
\ln GDP per capita $_{i,t-1}$	-0.092*** (0.02)	-0.093*** (0.02)	-0.092*** (0.02)
\ln population $_{i,t-1}$	-0.065* (0.03)	-0.067* (0.03)	-0.065* (0.03)
Polity index $_{i,t-1}$	0.016 (0.02)	0.017 (0.02)	0.016 (0.02)
Trade openness $_{i,t-1}$	0.039*** (0.01)	0.038*** (0.01)	0.039*** (0.01)
Interest rate $_{i,t-1}$	-0.024 (0.18)	-0.022 (0.18)	-0.021 (0.18)
Domestic credit $_{i,t-1}$	-0.034*** (0.01)	-0.034*** (0.01)	-0.034*** (0.01)
Gross capital formation $_{i,t-1}$	0.022** (0.01)	0.022** (0.01)	0.022** (0.01)
Foreign direct investment $_{i,t-1}$	0.399 (0.47)	0.375 (0.47)	0.406 (0.47)
\ln Inflation $_{i,t-1}$	-0.003* (0.00)	-0.003* (0.00)	-0.003* (0.00)
Current account balance $_{i,t-1}$	0.062 (0.04)	0.059 (0.04)	0.062 (0.04)
Country Fixed Effects	YES	YES	YES
Year Fixed Effects	YES	YES	YES
Observations	1,861	1,861	1,861
Countries	112	112	112
R^2 within	0.274	0.274	0.274

Note: ***, **, * denote significance at the 1%, 5%, and 10% level, respectively. Constant, country and time fixed effects included but not reported. The decision rule for large-scale disasters from EMDAT bases on the convention of 'great natural disasters' by Munich Re (2006) and the United Nations. Disasters are defined as large if (i) 1,000 or more were killed, or (ii) 100,000 or more affected, or (iii) at least 1 billion US dollar monetary damage (made comparable over time using the deflator on US dollars from WDI). Column (2) uses the 10% country cutoff level, while column (3) use the 15% global cutoff level.

Various Controls. Next, we include control variables employing a choice of variables used in Skidmore and Toya (2002); Noy (2009) and Loayza et al. (2012). These comprise a set of structural, domestic policy, and external factors. Structural factors include a variable of the size of the economy (total population), an index of democratization (Polity index), and a measures of openness to trade (imports plus exports divided by GDP). Domestic policy variables comprise inflation, domestic credit, gross capital formation, and the current account balance. External factors are foreign direct investment and real interest rates. Table 5.8 reports results for the large disaster measure from EM-DAT, and large disasters as defined by the 10% country cutoff or the 15% global cutoff, respectively. We choose the slightly lower threshold for global cutoffs to take into account that some countries might experience a lot of heavy disasters, while others experience disasters of a lesser magnitude that might still be destructive with respect to the country's prerequisites.

Still, we observe no significant effect of large disasters as reported in EM-DAT in column (1), while either specification of the aggregated large disaster measure from the new database reports a negative effect on economic activity (compare columns (2) and (3)). The impact when measuring large disasters according to the global cutoff increases strongly, presumably, as only very large disasters are coded by this cutoff. In addition, we find that the lag of GDP per capita negatively affects growth rates, as does population. The level of democracy does not play a role, but being more open to international trade and higher gross capital formation have a significant positive effect on economic activity. A high level of domestic credit provided by the banking sector or high inflation rates adversely affect growth rates.

Physical Intensity Measures. In subsequent specifications, we use the physical disaster intensity measures. Hence, no specific cutoff levels or decision rules are applied. Table 5.9 deploys two different samples, the MRW sample suggested by Mankiw et al. (1992)²⁸ in columns (1) to (3), and the full sample of 112 countries in columns (4) to (6).

For both samples, Table 5.9 reports that larger earthquakes and droughts adversely affect growth rates, while volcanic eruptions, storms and floods show no significant effect. Results are robust when we include structural, domestic policy, and external control variables in columns (2) to (3) and columns (5) to (6). Lower previous GDP per capita, lower total population size, higher openness to trade, lower domestic credit rates, higher gross capital

²⁸ This sample excludes countries for which oil-production was the dominant industry and states that formerly were part of the Soviet Union or Soviet satellite states and is well established in the growth literature.

Table 5.9: GDP per capita and Natural Disasters, Growth Rates (1979-2010)

Dependent Variable: Sample	$\Delta \ln$ GDP per capita					
	MRW			Full		
	(1)	(2)	(3)	(4)	(5)	(6)
Richter scale $_{i,t}$	-0.027** (0.01)	-0.036** (0.01)	-0.028** (0.01)	-0.021 (0.01)	-0.032** (0.01)	-0.026** (0.01)
VEI $_{i,t}$	3.149* (1.80)	3.048 (1.86)	2.112 (1.90)	3.286 (1.99)	3.287 (2.00)	2.452 (2.04)
Wind speed $_{i,t}$	-0.006 (0.01)	-0.007 (0.01)	-0.004 (0.01)	-0.003 (0.01)	-0.005 (0.01)	-0.004 (0.01)
Positive Δ rain $_{i,t}$	-0.063 (0.08)	-0.047 (0.07)	-0.061 (0.06)	-0.112 (0.09)	-0.086 (0.08)	-0.090 (0.07)
Drought $_{i,t}$ (dummy)	-0.316** (0.14)	-0.302** (0.14)	-0.258** (0.10)	-0.380** (0.15)	-0.359** (0.14)	-0.339** (0.13)
Controls						
\ln GDP per capita $_{i,t-1}$	-0.103*** (0.02)	-0.111*** (0.02)	-0.110*** (0.02)	-0.084*** (0.01)	-0.095*** (0.02)	-0.093*** (0.02)
\ln population $_{i,t-1}$		-0.061 (0.04)	-0.083** (0.04)		-0.059* (0.03)	-0.068** (0.03)
Polity index $_{i,t-1}$		0.027 (0.02)	0.024 (0.02)		0.022 (0.02)	0.017 (0.02)
Trade openness $_{i,t-1}$		0.037*** (0.01)	0.032*** (0.01)		0.041*** (0.01)	0.039*** (0.01)
Interest rate $_{i,t-1}$			0.077 (0.22)			-0.014 (0.18)
Domestic credit $_{i,t-1}$			-0.035*** (0.01)			-0.034*** (0.01)
Gross capital formation $_{i,t-1}$			0.020** (0.01)			0.022** (0.01)
Foreign direct investment $_{i,t-1}$			0.101 (0.07)			0.036 (0.05)
\ln inflation $_{i,t-1}$			-0.002 (0.00)			-0.003* (0.00)
Current account balance $_{i,t-1}$			0.104** (0.04)			0.058 (0.04)
Country Fixed Effects	YES	YES	YES	YES	YES	YES
Year Fixed Effects	YES	YES	YES	YES	YES	YES
Observations	1,470	1,470	1,470	1,861	1,861	1,861
Countries	79	79	79	112	112	112
Adjusted R^2	0.180	0.193	0.221	0.223	0.236	0.258

Note: ***, **, * denote significance at the 1%, 5%, and 10% level, respectively. Country-pair clustered robust standard errors reported in parentheses. Constant, country and time fixed effects included but not reported. Columns (1) to (3) use the sample suggested by Mankiw et al. (1992).

formation rates, lower inflation rates, and higher current account balance are all positively associated with growth in GDP per capita. The index of democracy, real interest rates and foreign direct investment do not significantly affect economic growth.²⁹

²⁹ For robustness reasons, we report parsimonious results on the direct effect of natural disasters on the logarithm of GDP per capita using a non-dynamic model in Table 5.15 in Appendix 5.A. Findings suggest a direct adverse effect of earthquakes and storms on income per capita, while droughts report a positive

Heterogeneity across Samples. This paragraph explores the impact of natural disasters on the economic activity of various country groups. Table 5.10 summarizes results. All regressions include the full set of control variables, country and time fixed effects. First, we separate developing from industrialized economies in columns (1) and (2). We find that larger earthquakes and droughts negatively affect economic growth in non-OECD countries in column (1). In line with expectations, we find no significant impact on growth in OECD economies. Solely droughts exert a very strong negative effect in column (2).

Table 5.10: GDP per capita and Natural Disasters, Samples (1979-2010)

Dependent Variable: $\Delta \ln$ GDP per capita							
Sample :	Non-OECD	OECD	Low Income	Middle Income	High Income	Democracy	Autocracy
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Richter scale _{<i>i,t</i>}	-0.031** (0.02)	0.037 (0.80)	-2.482*** (0.78)	-0.138* (0.08)	-0.014 (0.01)	-0.040 (0.03)	-0.019 (0.02)
VEI _{<i>i,t</i>}	1.907 (2.22)	4.480 (5.02)	159.218 (217.19)	0.552 (2.05)	2.818 (6.04)	1.792 (2.37)	2.626 (27.73)
Wind speed _{<i>i,t</i>}	-0.006 (0.01)	-0.048 (0.10)	-0.972*** (0.27)	0.003 (0.01)	-0.004 (0.01)	0.007** (0.00)	-0.014** (0.01)
Positive Δ rain _{<i>i,t</i>}	-0.108 (0.09)	-1.664 (2.01)	2.505 (1.86)	-0.156 (0.15)	-0.023 (0.04)	-0.269** (0.12)	-0.005 (0.06)
Drought _{<i>i,t</i>} (dummy)	-0.245* (0.14)	-15.218*** (5.30)	-1.079 (1.55)	-0.300** (0.12)	-3.675** (1.52)	-0.382*** (0.11)	1.597 (6.56)
Country Fixed Effects	YES	YES	YES	YES	YES	YES	YES
Year Fixed Effects	YES	YES	YES	YES	YES	YES	YES
Observations	1,327	534	223	988	650	1,418	443
Adjusted R^2	0.257	0.548	0.177	0.309	0.444	0.351	0.142

Note: ***, **, * denote significance at the 1%, 5%, and 10% level, respectively. Country clustered standard errors reported in parenthesis. Constant, controls, country and time fixed effects included but not reported. Controls included as of Table 5.9 column (6). Columns (3) to (5) use the classification of income groups of the World Bank. Column (6) includes countries with a polity index above 0.5, while column (7) includes those with a polity index below or equal to 0.5.

Columns (3) to (5) decompose the sample in low, middle, and high income countries according to the convention of the World Bank. Results show that particularly low and middle income countries experience a strong negative effect from earthquakes on income growth. The stronger the storm to more adverse the effect on economic activity in low income countries, while droughts are particularly bad for economic growth in middle and high income countries.

Finally, we dissect democracies (polity index > 0.5) from autocracies (polity index ≤ 0.5) in columns (6) and (7), respectively. Findings in column (6) suggest that stronger storms

effect, but not for OECD countries where the effect is strongly negative in column (4) including the full set of controls.

have a positive, while floods and droughts have a negative impact on democratic nations' income per capita. Column (7) reports that autocracies suffer, on average, most from the adverse effect of storms on income growth.

Alternative Estimation Techniques. To control for a potential Nickell bias, we use alternative estimation techniques even though the bias in our setup is supposedly small as T is rather large.³⁰ To show that our estimates are not largely biased by the inclusion of the one year lagged GDP per capita, we estimate equation (5.5) using two-step feasible GMM or a technique proposed by Anderson and Hsiao (1982).³¹ Table 5.11 reports results. All estimations include but not report the full set of controls, country and time fixed effects.

Table 5.11: GDP per capita and Natural Disasters, Instrumented (1979-2010)

Dependent Variable: $\Delta \ln$ GDP per capita				
Method:	IV GMM		Anderson-Hsiao	
Sample:	MRW	FULL	MRW	FULL
	(1)	(2)	(3)	(4)
Richter scale $_{i,t}$	-0.024** (0.01)	-0.026** (0.01)	-0.041* (0.02)	-0.040* (0.02)
VEI $_{i,t}$	0.716 (1.98)	1.175 (2.24)	-2.342 (1.87)	-2.308 (1.74)
Wind speed $_{i,t}$	-0.004 (0.01)	-0.005 (0.01)	-0.006 (0.01)	-0.006 (0.01)
Positive Δ rain $_{i,t}$	-0.064* (0.04)	-0.073* (0.04)	-0.128 (0.09)	-0.094 (0.09)
Drought $_{i,t}$ (dummy)	-0.184* (0.10)	-0.284* (0.15)	-0.298* (0.18)	-0.364* (0.19)
Controls				
\ln GDP per capita $_{i,t-1}$	-0.126*** (0.02)	-0.124*** (0.02)	0.186 (0.27)	0.139 (0.23)
Observations	1,300	1,620	1,183	1,464
F-Test on excl. Inst.	876.52	852.35	17.87	21.63

Note: ***, **, * denote significance at the 1%, 5%, and 10% level, respectively. Country clustered standard errors reported in parenthesis. Controls, country and time fixed effects included but not reported. Controls included as of Table 5.9 column (6). Lagged GDP per capita treated as endogenous in IV specifications. Third lag used as instrument. Estimations are conducted by two-step feasible GMM estimation in columns (1) and (2). Estimations are conducted by Anderson-Hsiao first-differenced estimation in columns (3) and (4), where the dependent variable is double differenced.

³⁰ Note, that the fixed effects estimator performs well if the panel has a large time dimension (Nickell, 1981).

³¹ As T gets large, computational requirements in GMM setups increase substantially. As T is relatively large in our panel ($T=30$), differenced or systems GMM estimates are not preferable, as GMM estimates with many instruments might behave in a nonstandard manner with issues on the limit distribution of the estimator yet to be determined. Hence, we focus on two-step feasible GMM and Anderson-Hsiao.

By applying two-step feasible GMM or Anderson-Hsiao, we confirm previous results. For both the MRW and the full sample, we find that stronger earthquakes and more droughts decrease growth rates. In addition, using two-step feasible GMM in columns (1) and (2), we find a negative effect of floods on income growth. Treating the lag of GDP per capita as endogenous, we find that lagged GDP per capita still adversely affects GDP growth. We use the third lag as an instrument and find comfortably high F-Tests on the excluded instrument that are 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). Applying Anderson-Hsiao first-differenced estimation, where the dependent variable is double differenced, first-differenced lagged GDP per capita has no impact. Again, we find perfectly high F-Tests on the excluded instrument in columns (3) and (4). To conclude, we can show that our previously applied fixed effects method produces estimates on the physical disaster intensity variables with correct signs and a relatively small, even negligible bias.

5.6 The Influence of other Factors

This section examines the importance of geography, institutional quality, and measures of openness in determining a country's ability to mitigate the consequences of natural disasters on economic growth. For convenience, we decompose geophysical and climatological types of disasters into separate tables.

Geophysical Disasters. Table 5.12 reports results on geophysical disasters (earthquakes and volcanic eruptions) interacted with macroeconomic factors. The full set of country and time fixed effects and controls is included but not reported to conserve space. In all specifications, we find that stronger earthquakes adversely affect GDP per capita on impact (except column (2) and (6)), while we find ambiguous effects of volcanic eruptions.³²

Gallup et al. (1999); Hall and Jones (1999) and Acemoglu et al. (2001) document a strong correlation between latitude and economic performance. Thus, we investigate whether geography (absolute latitude) plays a role in determining a nation's ability to cope with the

³² Note that positive effects may be explained by the idea that the destruction of physical capital allows the affected economy to replace outdated equipment and structures faster than along the balanced growth path (Caballero and Hammour, 1994). This may add to a temporary boom following the disaster. In addition, volcanic eruptions, especially those with a large VEI, are very rare episodic events (compare Figure 5.1) and may thus be special with respect to their measurable impact on economic activity.

Table 5.12: Geophysical: Macroeconomic Factors (1979-2010)

Dependent Variable:	$\Delta \ln$ GDP per capita					
	Geography	Institution	Trade policy	Trade	Financial	ODA
	(1)	(2)	openness (3)	openness (4)	openness (5)	received (6)
Richter scale $_{i,t}$	-0.015*	0.369	-0.118**	-0.183**	-0.147**	-0.012
	(0.01)	(0.78)	(0.05)	(0.09)	(0.06)	(0.01)
VEI $_{i,t}$	1.344	-9.586	-38.864	65.025**	3.642	8.841***
	(3.84)	(10.50)	(34.90)	(31.90)	(4.65)	(1.73)
Richter scale $_{i,t} \times$ Latitude $_i$	-0.007*					
	(0.00)					
VEI $_{i,t} \times$ Latitude $_i$	0.099					
	(0.21)					
Richter scale $_{i,t} \times$ Institution $_i$		-0.042				
		(0.08)				
VEI $_{i,t} \times$ Institution $_i$		1.700				
		(1.61)				
Richter scale $_{i,t} \times$ Trade policy $_i$			0.091*			
			(0.05)			
VEI $_{i,t} \times$ Trade policy $_i$			41.762			
			(34.87)			
Richter scale $_{i,t} \times$ Trade openness $_{i,1979}$				0.043*		
				(0.02)		
VEI $_{i,t} \times$ Trade openness $_{i,1979}$				-109.480*		
				(55.15)		
Richter scale $_{i,t} \times$ Financial openness $_{i,1979}$					0.172**	
					(0.08)	
VEI $_{i,t} \times$ Financial openness $_{i,1979}$					-3.138	
					(7.96)	
Richter scale $_{i,t} \times$ ODA received $_{i,1979}$						-0.110*
						(0.06)
VEI $_{i,t} \times$ ODA received $_{i,1979}$						-4.538***
						(1.07)
Country Fixed Effects	YES	YES	YES	YES	YES	YES
Year Fixed Effects	YES	YES	YES	YES	YES	YES
Observations	1,861	1,568	1,771	1,638	1,463	1,039
Adjusted R^2	0.258	0.247	0.255	0.219	0.218	0.214

Note: ***, **, * denote significance at the 1%, 5%, and 10% level, respectively. Country clustered standard errors reported in parenthesis. Constant, controls, country and time fixed effects included but not reported. Controls included as of Table 5.9 column (6).

shock from natural disasters in column (1). We find that national geography plays a role in determining economic growth after a shock from earthquakes, but not for volcanic eruptions. The shock from an earthquake is, on average, even stronger for countries with higher latitude values. According to Acemoglu et al. (2001), a country's geographic location is a significant determinant of economic performance as it is strongly correlated with institutions. Column (2) examines whether nations with higher quality institutions suffer, on average, less in terms of income loss from natural disaster. Nations with higher quality institutions, hence, with

a higher value on the protection against the risk of expropriation, may be better suited to cope with shocks from natural disasters. All else equal, we find no significant impact using the simple fixed effects specification.³³

Next, countries more open to trade may also receive larger international capital inflows in the aftermath of a disaster. For this reason, a nation's openness level may help to reduce the impact of natural disasters on income growth. We deploy two measures of trade openness. In column (3), we use the Sachs-Warner trade policy openness dummy (updated by Wacziarg and Welch (2008)).³⁴ We find that a one standard deviation increase in the trade policy openness measure mitigates the consequences of an earthquake of similar size, on average, by 4.6%³⁵, all else equal. Using another standard measure of trade openness (imports plus exports divided by GDP), we find that a one standard deviation increase in initial openness to trade alleviates the negative impact of earthquakes on per capita GDP by 2.2%^{36, 37}

Yang (2008) shows that financial flows play a role in recovering from disasters. Here, we use two proxies. In column (5), we include initial values on the measure of financial openness (Chinn-Ito capital account openness index). We find that a one standard deviation increase in financial openness alleviates the shock from an earthquake by 4.7%, all else equal. In column (6), we use initial values of ODA inflows to test whether this mitigates a disaster shock. We find that ODA further increases the adverse effects of earthquakes. A one standard deviation increase in initial ODA increases the shock from equal-sized earthquakes by 55%. In the case of volcanoes, receiving more ODA lowers the positive effect on growth. Note that data on ODA is only available for the sub-group of developing countries. This might drive effects.

³³ Using two-step feasible GMM, Table 5.16 in Appendix 5.A reports that institutional quality mitigates the loss in per capita GDP caused by an earthquake.

³⁴ The measure classifies an economy as open if: (i) its average tariff rate was below 40%, (ii) its non-tariff barriers covered less than 40% of imports, (iii) it had no socialist economic system (iv) it had no state monopoly of major exports, or (v) its black-market premium was under 20% during either the 1970s or the 1980s.

³⁵ The formula for this is $(\exp(0.091*0.489)-1)*100$.

³⁶ The formula for this is $(\exp(0.043*0.509)-1)*100$.

³⁷ To circumvent endogeneity issues, we use the initial value of trade openness in 1979.

Table 5.13: Climatological: Macroeconomic Factors (1979-2010)

Dependent Variable:	$\Delta \ln$ GDP per capita					
	Geography	Institution	Trade policy	Trade	Financial	ODA
	(1)	(2)	openness (3)	openness (4)	openness (5)	received (6)
Wind speed $_{i,t}$	-0.014*** (0.00)	0.272* (0.15)	0.008* (0.00)	0.011 (0.01)	0.009* (0.00)	-0.002 (0.01)
Positive Δ rain $_{i,t}$	-0.066 (0.05)	-8.011** (3.33)	-0.393** (0.19)	-0.347 (0.21)	-0.246** (0.12)	-0.178 (0.16)
Drought $_{i,t}$ (dummy)	-1.802 (3.45)	-58.844* (34.70)	-0.439*** (0.15)	-10.558 (7.81)	-0.213** (0.09)	-1.849*** (0.53)
Wind speed $_{i,t} \times$ Latitude $_i$	0.001*** (0.00)					
Positive Δ rain $_{i,t} \times$ Latitude $_i$	-0.008 (0.01)					
Drought $_{i,t} \times$ Latitude $_i$	0.071 (0.17)					
Wind speed $_{i,t} \times$ Institution $_i$		-0.030* (0.02)				
Positive Δ rain $_{i,t} \times$ Institution $_i$		0.856** (0.36)				
Drought $_{i,t} \times$ Institution $_i$		7.521 (4.61)				
Wind speed $_{i,t} \times$ Trade policy $_i$			-0.022*** (0.01)			
Positive Δ rain $_{i,t} \times$ Trade policy $_i$			0.363* (0.20)			
Drought $_{i,t} \times$ Trade policy $_i$			10.687*** (1.32)			
Wind speed $_{i,t} \times$ Trade openness $_{i,1979}$				-0.006** (0.00)		
Positive Δ rain $_{i,t} \times$ Trade openness $_{i,1979}$				0.083 (0.06)		
Drought $_{i,t} \times$ Trade openness $_{i,1979}$				10.745 (8.19)		
Wind speed $_{i,t} \times$ Financial openness $_{i,1979}$					-0.029*** (0.01)	
Positive Δ rain $_{i,t} \times$ Financial openness $_{i,1979}$					0.274* (0.15)	
Drought $_{i,t} \times$ Financial openness $_{i,1979}$					-5.167 (4.06)	
Wind speed $_{i,t} \times$ ODA received $_{i,1979}$						-0.003 (0.01)
Positive Δ rain $_{i,t} \times$ ODA received $_{i,1979}$						0.097 (0.12)
Drought $_{i,t} \times$ ODA received $_{i,1979}$						0.640*** (0.19)
Country Fixed Effects	YES	YES	YES	YES	YES	YES
Year Fixed Effects	YES	YES	YES	YES	YES	YES
Observations	1,861	1,568	1,771	1,638	1,463	1,039
Adjusted R^2	0.258	0.251	0.257	0.216	0.218	0.208

Note: ***, **, * denote significance at the 1%, 5%, and 10% level, respectively. Country clustered standard errors reported in parenthesis. Constant, controls, country and time fixed effects included but not reported. Controls included as of Table 5.9 column (6).

Climatological Disasters. Table 5.13 reports results on climatological events. Larger floods and droughts adversely affect GDP per capita, while the impact of storms is ambiguous.³⁸

In column (1), we find that geographic location plays a role in determining economic growth combined with storm events, but not for floods or droughts. Stronger storms cause, on average, higher losses to income growth, but these losses are less severe for countries with higher latitude values. A one standard deviation increase in latitude alleviates the shock from a storm of similar strength by 17.9%. In column (2), we test whether higher quality institutions mitigate the income loss from natural disasters. All else equal, we find that institutional quality lowers positive effects from storms, but alleviates adverse effects on income growth from flooding. No significant impact of institutions are found in combination with droughts.

Next, column (3) reports that a one standard deviation increase in trade policy openness mitigates the consequences induced by floods, on average, by 19%, and even more so for droughts, all else equal. Using initial trade openness in column (4), we find a negative effect of the interaction on GDP growth for storms, but no effect on floods or droughts. Column (5) reports that a one standard deviation increase in financial openness alleviates the negative effect from floods of equal size by 7.5%. Finally, higher initial ODA strongly decreases the shock from droughts in column (6).

Alternative Estimation Techniques. To test whether results are robust with respect to a potential Nickell bias, we again use two-step feasible GMM and Anderson-Hsiao first-differencing to instrument for the one year lagged GDP per capita. Tables 5.16 and 5.17 in Appendix 5.A report results for geophysical disasters using two-step feasible GMM and Anderson-Hsiao, respectively. Results on the mitigating effects of macroeconomic factors hold under both specifications. F-Tests on the excluded instrument are in both specifications comfortably high. In addition, we find that institutions play a mitigating role in the case of earthquakes in Table 5.16 column (2). And, that trade policy openness alleviates negative effects from volcanic eruptions in Table 5.17 column (3).

³⁸ Again, positive effects may be explained by the idea that the destruction of physical capital allows the affected economy to replace outdated equipment and structures faster than along the balanced growth path Caballero and Hammour (1994). This may add to a temporary boom following the disaster.

Tables 5.18 and 5.19 in Appendix 5.A report results for climatological disaster types using two-step feasible GMM and Anderson-Hsiao, respectively. Generally, result on the mitigating effects of geography, institutional quality, and measures of openness are robust. F-Tests on the excluded instrument are in both estimation frameworks comfortably high. Moreover, we find that openness to trade plays a particular role in mitigating adverse effect from droughts using either two-step feasible GMM or Anderson-Hsiao in column (4), respectively.

5.7 Concluding Remarks

In the present paper, we compile a novel and comprehensive database on natural disasters from 1979 to 2010 based on primary sources from geophysics and climatology, essentially covering all countries of the world. Our alternative data suits a causal analysis of the impact of natural disasters on economic growth, as it builds on pure physical measures of disaster intensity, such as Richter scale, VEI, wind speed and precipitation.

We first use an event based dataset to show that the probability of a disaster with given physical magnitude being reported in EM-DAT depends strongly on the affected country's GDP per capita. In a next step, we examine the impact of natural disasters on per capita GDP. Our findings provide very pervasive evidence that natural disasters do indeed lower GDP per capita temporarily. Our key findings are: A disaster whose physical strength belongs to the top decile of the country-level disaster distribution reduces growth by about 3 percentage points. This effect is halved when looking at disasters belonging to the top 15% percentile. It is again halved when looking at the top 20%. Further, we show that, depending on the disaster type, better institutional quality, higher openness to trade, higher financial openness, and lower ODA inflows help spur the economic reconstruction process so that, even upon impact, the adverse growth effect of a natural disaster is mitigated.

We believe that our comprehensive dataset on pure physical disaster intensity measures can be fruitfully applied to many other frameworks, including the relation between disasters and trade patterns, migration patterns, poverty, inequality, consumption, and investment decisions, and many more.

5.A Appendix

Table 5.14: Summary Table, Full Sample

Variable	Observations	Mean	Std. Dev.	Source	Description
$\Delta \ln \text{GDP per capita}_{i,t}$	1,861	0.050	0.0576	PWT (7.0)	Per capita GDP growth.
$\ln \text{GDP per capita}_{i,t-1}$	1,861	8.522	1.2165	PWT (7.0)	GDP per capita.
$\ln \text{population}_{i,t}$	1,861	9.561	1.4943	PWT (7.0)	Total population in thousands.
Polity index $_{i,t}$	1,861	0.738	0.3140	Polity IV (2010)	Polity index, normalized between 0 and 1.
Trade openness $_{i,t-1}$	1,861	0.777	0.5062	PWT (7.0)	Imports plus exports over GDP.
Interest rate $_{i,t-1}$	1,861	0.007	0.0115	WDI (2012)	Real interest rate.
Domestic credit $_{i,t-1}$	1,861	0.628	0.4923	WDI (2012)	Domestic credit in banking sector (share of GDP).
Gross capital formation $_{i,t-1}$	1,861	0.055	0.1699	WDI (2012)	Gross capital formation (share of growth).
Foreign direct investment $_{i,t-1}$	1,861	0.029	0.0446	WDI (2012)	Foreign direct investment, net inflows (share of GDP).
$\ln \text{inflation}_{i,t-1}$	1,861	-5.154	1.1969	WDI (2012)	Inflation, consumer prices.
Current account balance $_{i,t-1}$	1861	-0.019	0.0723	WDI (2012)	Current account balance (share of GDP)
Absolute latitude $_i$	1,861	28.767	17.7846	CEPII (2005)	Absolute Latitude.
Institution $_i$	1,568	7.559	1.6166	ICRG, Acemoglu et al. (2004)	Protection against expropriation risk.
Trade policy openness $_i$	1,823	0.607	0.4886	Sachs & Warner (1995), Wacziarg & Welch (2008)	Initial index of trade policy openness in 1979 constructed by Sachs and Warner (1995), extended by Wacziarg and Welch (2008).
Trade openness $_{i,1979}$	1,638	0.648	0.5093	PWT (7.0)	Imports plus exports over GDP in 1979.
Financial openness $_{i,1979}$	1,463	0.340	0.2654	Chinn & Ito (2010)	Financial (capital account) openness in 1979.
ODA $_{i,1979}$	1,039	5.198	7.2957	WDI (2012)	Net official development assistance received (share of GNI) in 1979 .
Richter scale $_{i,t}$ (direct)	1,861	4.049	2.2450	Own calculations from primary sources	Maximum Richter scale experienced.
VEI $_{i,t}$ (direct)	1,861	0.267	0.7738	Own calculations from primary sources	Max. Volcanic Eruption Index.
Wind speed $_{i,t}$ (direct)	1,861	63.178	21.9785	Own calculations from primary sources	Max. wind speed in knots.
Positive Δ rain $_{i,t}$ (direct)	1,861	0.809	0.7950	Own calculations from primary sources	Max. positive difference in monthly rainfall from long run monthly mean.
Drought $_{i,t}$ (direct)	1,861	0.052	0.2212	Own calculations from primary sources	One if precipitation in three months in a row, or at least in 5 months a year 50% below long run monthly precipitation mean, zero otherwise.
Richter scale $_{i,t}$ (by area $_i$)	1,861	0.012	0.0661	Own calculations from primary sources	Max. Richter scale by land area.
VEI $_{i,t}$ (by area $_i$)	1,861	0.000	0.0003	Own calculations from primary sources	Max. Volcanic Eruption Index by land area.
Wind speed $_{i,t}$ (by area $_i$)	1,861	0.182	0.7758	Own calculations from primary sources	Max. wind speed in knots by land area.
Positive Δ rain $_{i,t}$ (by area $_i$)	1,861	0.003	0.0143	Own calculations from primary sources	Max. positive difference in monthly rainfall from long run monthly mean by land area.
Drought $_{i,t}$ (by area $_i$)	1,861	0.000	0.0033	Own calculations from primary sources	One if precipitation in three months in a row, or at least in 5 months a year 50% below long run monthly precipitation mean, zero otherwise, by land area.
Disaster Onset $_{i,t}$, country 10% (by area $_i$)	1,861	0.006	0.0714	Own calculations from primary sources	large disasters defined as 10% largest disasters in a country by land area.
Disaster Onset $_{i,t}$, country 15% (by area $_i$)	1,861	0.012	0.1316	Own calculations from primary sources	large disasters defined as 15% largest disasters in a country by land area.
Disaster Onset $_{i,t}$, country 20% (by area $_i$)	1,861	0.021	0.2122	Own calculations from primary sources	large disasters defined as 20% largest disasters in a country by land area.
Disaster Onset $_{i,t}$, global 10% (by area $_i$)	1,861	0.00001	0.0004	Own calculations from primary sources	large disasters defined as global 10% largest disasters by land area.
Disaster Onset $_{i,t}$, global 15% (by area $_i$)	1,861	0.00006	0.0010	Own calculations from primary sources	large disasters defined as global 15% largest disasters by land area.
Disaster Onset $_{i,t}$, global 20% (by area $_i$)	1,861	0.00035	0.0027	Own calculations from primary sources	large disasters defined as global 20% largest disasters by land area.
EMDAT Disaster Onset $_{i,t}$ (by area $_i$)	1,861	0.002	0.0078	EM-DAT (2012)	Large scale disasters by land area, as defined by decision rule.

Table 5.15: GDP per capita and Natural Disasters, Levels (1979-2010)

Dependent Variable:	ln GDP per capita			
Sample :	MRW	Full	Non-OECD	OECD
	(1)	(2)	(3)	(4)
Richter scale _{<i>i,t</i>}	-0.195*** (0.05)	-0.199*** (0.05)	-0.195*** (0.06)	2.116 (2.14)
VEI _{<i>i,t</i>}	-7.575 (6.34)	-6.810 (5.57)	-8.008 (8.39)	-6.948 (19.63)
Wind speed _{<i>i,t</i>}	-0.094** (0.04)	-0.095** (0.05)	-0.087* (0.05)	0.037 (0.44)
Positive Δ rain _{<i>i,t</i>}	0.367 (0.24)	0.228 (0.25)	0.042 (0.25)	-0.344 (9.87)
Drought _{<i>i,t</i>} (dummy)	1.694*** (0.25)	1.675*** (0.25)	1.494*** (0.48)	-80.179** (35.47)
Controls				
ln population _{<i>i,t-1</i>}	-0.588*** (0.18)	-0.803*** (0.15)	-1.150*** (0.19)	-0.184 (0.38)
Polity index _{<i>i,t-1</i>}	0.015 (0.09)	0.046 (0.08)	-0.073 (0.06)	0.914*** (0.14)
Trade openness _{<i>i,t-1</i>}	0.020 (0.09)	0.036 (0.07)	0.045 (0.07)	0.059 (0.09)
Interest rate _{<i>i,t-1</i>}	-0.773* (0.43)	-0.410 (0.55)	-0.525 (0.59)	-2.616 (2.82)
Domestic credit _{<i>i,t-1</i>}	-0.030 (0.05)	-0.005 (0.05)	0.033 (0.07)	-0.003 (0.04)
Gross capital formation _{<i>i,t-1</i>}	0.025 (0.03)	0.029 (0.02)	0.025 (0.02)	0.126** (0.05)
Foreign direct investment _{<i>i,t-1</i>}	0.017 (0.20)	-0.126 (0.19)	-0.088 (0.30)	0.017 (0.10)
ln inflation _{<i>i,t-1</i>}	0.011 (0.01)	0.004 (0.01)	0.003 (0.01)	0.005 (0.01)
Current account balance _{<i>i,t-1</i>}	0.263 (0.19)	0.269 (0.18)	0.331 (0.20)	-0.117 (0.30)
Country Fixed Effects	YES	YES	YES	YES
Year Fixed Effects	YES	YES	YES	YES
Observations	1,470	1,861	1,327	534
Countries	79	112	86	26
Adjusted R^2	0.886	0.875	0.870	0.966

Note: ***, **, * denote significance at the 1%, 5%, and 10% level, respectively. Country clustered standard errors reported in parenthesis. Constant, controls, country and time fixed effects included but not reported. Controls included as of Table 5.9 column (6).

Table 5.16: Geophysical: Macroeconomic Factors, two-step feasible GMM (1979-2010)

Dependent Variable:	$\Delta \ln$ GDP per capita					
	Geography	Institution	Trade policy	Trade	Financial	ODA
	(1)	(2)	openness (3)	openness (4)	openness (5)	received (6)
Richter scale _{<i>i,t</i>}	-0.016*	-3.237*	-0.134**	-0.195**	-0.135***	-0.013*
	(0.01)	(1.80)	(0.06)	(0.09)	(0.05)	(0.01)
VEI _{<i>i,t</i>}	-1.474	-3.952	-46.638	61.317*	2.025	8.235***
	(4.09)	(10.91)	(31.96)	(32.60)	(4.79)	(1.86)
Richter scale _{<i>i,t</i>} × Latitude _{<i>i</i>}	-0.007*					
	(0.00)					
VEI _{<i>i,t</i>} × Latitude _{<i>i</i>}	0.229					
	(0.19)					
Richter scale _{<i>i,t</i>} × Institution _{<i>i</i>}		0.345*				
		(0.19)				
VEI _{<i>i,t</i>} × Institution _{<i>i</i>}		0.662				
		(1.71)				
Richter scale _{<i>i,t</i>} × Trade policy _{<i>i</i>}			0.107*			
			(0.06)			
VEI _{<i>i,t</i>} × Trade policy _{<i>i</i>}			48.687			
			(31.94)			
Richter scale _{<i>i,t</i>} × Trade openness _{<i>i,1979</i>}				0.047**		
				(0.02)		
VEI _{<i>i,t</i>} × Trade openness _{<i>i,1979</i>}				-104.830*		
				(57.18)		
Richter scale _{<i>i,t</i>} × Financial openness _{<i>i,1979</i>}					0.159**	
					(0.07)	
VEI _{<i>i,t</i>} × Financial openness _{<i>i,1979</i>}					-2.416	
					(8.06)	
Richter scale _{<i>i,t</i>} × ODA received _{<i>i,1979</i>}						-0.079**
						(0.04)
VEI _{<i>i,t</i>} × ODA received _{<i>i,1979</i>}						-4.867***
						(1.11)
Country Fixed Effects	YES	YES	YES	YES	YES	YES
Year Fixed Effects	YES	YES	YES	YES	YES	YES
Observations	1,620	1,385	1,552	1,434	1,290	909
F-Test on excl. Inst.	876.34	732.77	854.76	707.55	900.38	666.10

Note: ***, **, * denote significance at the 1%, 5%, and 10% level, respectively. Country clustered standard errors reported in parenthesis. Constant, controls, country and time fixed effects included but not reported. Controls included as of Table 5.9 column (6). Lagged GDP per capita treated as endogenous in IV GMM regressions. Third lag used as instrument. Estimations are conducted by two-step feasible GMM estimation.

Table 5.17: Geophysical: Macroeconomic Factors, Anderson-Hsiao (1979-2010)

Dependent Variable:	$\Delta \ln$ GDP per capita					
	Geography	Institution	Trade policy	Trade	Financial	ODA
	(1)	(2)	openness	openness	openness	received
	(1)	(2)	(3)	(4)	(5)	(6)
Richter scale $_{i,t}$	-0.026** (0.01)	-2.468 (2.60)	-0.143** (0.06)	-0.223** (0.10)	-0.211** (0.08)	-0.037* (0.02)
VEI $_{i,t}$	-3.303 (3.67)	-3.592 (10.97)	-79.653*** (22.75)	41.398 (32.27)	1.338 (4.69)	8.114* (4.60)
Richter scale $_{i,t} \times$ Latitude $_i$	-0.007** (0.00)					
VEI $_{i,t} \times$ Latitude $_i$	0.089 (0.24)					
Richter scale $_{i,t} \times$ Institution $_i$		0.261 (0.28)				
VEI $_{i,t} \times$ Institution $_i$		0.093 (1.71)				
Richter scale $_{i,t} \times$ Trade policy $_i$			0.107** (0.05)			
VEI $_{i,t} \times$ Trade policy $_i$			78.515*** (22.89)			
Richter scale $_{i,t} \times$ Trade openness $_{i,1979}$				0.052** (0.03)		
VEI $_{i,t} \times$ Trade openness $_{i,1979}$				-76.429 (57.53)		
Richter scale $_{i,t} \times$ Financial openness $_{i,1979}$					0.237** (0.10)	
VEI $_{i,t} \times$ Financial openness $_{i,1979}$					-9.535 (8.13)	
Richter scale $_{i,t} \times$ ODA received $_{i,1979}$						-0.083* (0.05)
VEI $_{i,t} \times$ ODA received $_{i,1979}$						-6.671** (3.16)
Year Fixed Effects	YES	YES	YES	YES	YES	YES
Observations	1,464	1,260	1,368	1,300	1,176	822
F-Test on excl. Inst.	21.44	12.93	27.43	22.74	11.56	10.44

Note: ***, **, * denote significance at the 1%, 5%, and 10% level, respectively. Country clustered standard errors reported in parenthesis. Constant, controls, and time fixed effects included but not reported. Controls included as of Table 5.9 column (6). Lagged GDP per capita treated as endogenous in regressions. Third lag used as instrument. Estimations are conducted by Anderson-Hsiao first-differenced estimation. Dependent variable is double differenced.

Table 5.18: Climatological: Macroeconomic Factors, two-step feasible GMM (1979-2010)

Dependent Variable:	$\Delta \ln$ GDP per capita					
	Geography	Institution	Trade policy	Trade	Financial	ODA
	(1)	(2)	openness	openness	openness	received
	(1)	(2)	(3)	(4)	(5)	(6)
Wind speed $_{i,t}$	-0.017*** (0.01)	0.103 (0.15)	0.010*** (0.00)	0.017** (0.01)	0.012*** (0.00)	-0.007 (0.01)
Positive Δ rain $_{i,t}$	-0.079*** (0.03)	-4.919* (2.66)	-0.282** (0.12)	-0.178 (0.16)	-0.160* (0.09)	-0.186 (0.13)
Drought $_{i,t}$ (dummy)	0.396 (6.32)	-66.659 (52.23)	-0.305** (0.13)	-12.184** (5.53)	-0.080 (0.11)	-2.132*** (0.62)
Wind speed $_{i,t} \times$ Latitude $_i$	0.001*** (0.00)					
Positive Δ rain $_{i,t} \times$ Latitude $_i$	-0.002 (0.01)					
Drought $_{i,t} \times$ Latitude $_i$	-0.035 (0.32)					
Wind speed $_{i,t} \times$ Institution $_i$		-0.012 (0.02)				
Positive Δ rain $_{i,t} \times$ Institution $_i$		0.523* (0.29)				
Drought $_{i,t} \times$ Institution $_i$		8.429 (6.89)				
Wind speed $_{i,t} \times$ Trade policy $_i$			-0.026*** (0.01)			
Positive Δ rain $_{i,t} \times$ Trade policy $_i$			0.230* (0.13)			
Drought $_{i,t} \times$ Trade policy $_i$			10.053*** (1.79)			
Wind speed $_{i,t} \times$ Trade openness $_{i,1979}$				-0.008*** (0.00)		
Positive Δ rain $_{i,t} \times$ Trade openness $_{i,1979}$				0.034 (0.04)		
Drought $_{i,t} \times$ Trade openness $_{i,1979}$				12.553** (5.80)		
Wind speed $_{i,t} \times$ Financial openness $_{i,1979}$					-0.034*** (0.01)	
Positive Δ rain $_{i,t} \times$ Financial openness $_{i,1979}$					0.139 (0.11)	
Drought $_{i,t} \times$ Financial openness $_{i,1979}$					-11.145 (7.02)	
Wind speed $_{i,t} \times$ ODA received $_{i,1979}$						0.004 (0.01)
Positive Δ rain $_{i,t} \times$ ODA received $_{i,1979}$						0.138 (0.11)
Drought $_{i,t} \times$ ODA received $_{i,1979}$						0.763*** (0.21)
Country Fixed Effects	YES	YES	YES	YES	YES	YES
Year Fixed Effects	YES	YES	YES	YES	YES	YES
Observations	1,620	1,385	1,552	1,434	1,290	909
F-Test on excl. Inst.	893.26	691.50	800.48	662.42	911.66	618.15

Note: ***, **, * denote significance at the 1%, 5%, and 10% level, respectively. Country clustered standard errors reported in parenthesis. Constant, controls, country and time fixed effects included but not reported. Controls included as of Table 5.9 column (6). Lagged GDP per capita treated as endogenous in IV GMM regressions. Third lag used as instrument. Estimations are conducted by two-step feasible GMM estimation.

Table 5.19: Climatological: Macroeconomic Factors, Anderson-Hsiao (1979-2010)

Dependent Variable:	$\Delta \ln$ GDP per capita					
	Geography	Institution	Trade policy openness	Trade openness	Financial openness	ODA received
	(1)	(2)	(3)	(4)	(5)	(6)
Wind speed $_{i,t}$	-0.020*** (0.00)	-0.376 (0.26)	0.004 (0.01)	0.013 (0.01)	0.012* (0.01)	-0.021*** (0.01)
Positive Δ rain $_{i,t}$	-0.067 (0.06)	-11.000** (4.29)	-0.665* (0.40)	-0.628 (0.45)	-0.444* (0.25)	-0.259 (0.19)
Drought $_{i,t}$ (dummy)	9.294** (4.22)	-61.200 (56.78)	-0.301** (0.12)	-16.507*** (6.16)	-0.364** (0.17)	-1.473 (1.34)
Wind speed $_{i,t} \times$ Latitude $_i$	0.001*** (0.00)					
Positive Δ rain $_{i,t} \times$ Latitude $_i$	-0.009 (0.01)					
Drought $_{i,t} \times$ Latitude $_i$	-0.480** (0.22)					
Wind speed $_{i,t} \times$ Institution $_i$		0.039 (0.03)				
Positive Δ rain $_{i,t} \times$ Institution $_i$		1.174** (0.46)				
Drought $_{i,t} \times$ Institution $_i$		8.340 (7.40)				
Wind speed $_{i,t} \times$ Trade policy $_i$			-0.020* (0.01)			
Positive Δ rain $_{i,t} \times$ Trade policy $_i$			0.642 (0.41)			
Drought $_{i,t} \times$ Trade policy $_i$			11.286*** (2.09)			
Wind speed $_{i,t} \times$ Trade openness $_{i,1979}$				-0.008** (0.00)		
Positive Δ rain $_{i,t} \times$ Trade openness $_{i,1979}$				0.158 (0.12)		
Drought $_{i,t} \times$ Trade openness $_{i,1979}$				17.098*** (6.43)		
Wind speed $_{i,t} \times$ Financial openness $_{i,1979}$					-0.044*** (0.01)	
Positive Δ rain $_{i,t} \times$ Financial openness $_{i,1979}$					0.515 (0.32)	
Drought $_{i,t} \times$ Financial openness $_{i,1979}$					1.444 (10.20)	
Wind speed $_{i,t} \times$ ODA received $_{i,1979}$						0.014*** (0.00)
Positive Δ rain $_{i,t} \times$ ODA received $_{i,1979}$						0.197 (0.17)
Drought $_{i,t} \times$ ODA received $_{i,1979}$						0.442 (0.43)
Year Fixed Effects	YES	YES	YES	YES	YES	YES
Observations	1,464	1,260	1,368	1,300	1,176	822
F-Test on excl. Inst.	21.63	13.08	25.20	21.84	11.49	9.41

Note: ***, **, * denote significance at the 1%, 5%, and 10% level, respectively. Country clustered standard errors reported in parenthesis. Constant, controls, and time fixed effects included but not reported. Controls included as of Table 5.9 column (6). Lagged GDP per capita treated as endogenous in regressions. Third lag used as instrument. Estimations are conducted by Anderson-Hsiao first-differenced estimation. Dependent variable is double differenced.

Table 5.20: Cutoff Levels by Country, Earthquakes and Volcanoes

Country	Earthquakes					Volcanoes				
	mean	maximum	10%	15%	20%	mean	maximum	10%	15%	20%
Afghanistan	6.1	6.7	7.2	6.9	6.6	0	0	0	0	0
Albania	5.0	5.9	5.5	5.2	4.9	0	0	0	0	0
Algeria	5.2	7.7	7.2	6.9	6.5	0	0	0	0	0
Angola	1.9	5.4	5.4	5.3	5.2	0	0	0	0	0
Argentina	6.1	7.0	6.6	6.3	6.0	0	2	2	2	2
Armenia	5.1	8.1	7.5	7.2	6.8	0	0	0	0	0
Australia	5.2	6.9	6.4	6.1	5.7	0	2	2	2	2
Austria	3.9	6.0	5.6	5.3	4.9	0	0	0	0	0
Azerbaijan	5.1	8.1	7.6	7.3	6.9	0	0	0	0	0
Bahrain	0.0	0.0	0.0	0.0	0.0	0	0	0	0	0
Bangladesh	5.1	6.4	6.1	5.9	5.7	0	0	0	0	0
Belarus	0.0	0.0	0.0	0.0	0.0	0	0	0	0	0
Belgium	2.6	5.0	4.8	4.6	4.3	0	0	0	0	0
Benin	0.0	0.0	0.0	0.0	0.0	0	0	0	0	0
Bolivia	6.0	7.2	6.7	6.3	6.0	0	0	0	0	0
Bosnia and Herzegovina	0.0	0.0	0.0	0.0	0.0	0	0	0	0	0
Botswana	2.8	4.8	4.7	4.5	4.3	0	0	0	0	0
Brazil	5.7	7.0	6.8	6.6	6.3	0	0	0	0	0
Bulgaria	4.5	7.0	6.5	6.2	5.8	0	0	0	0	0
Burkina Faso	0.0	0.0	0.0	0.0	0.0	0	0	0	0	0
Burundi	5.3	7.0	6.6	6.3	6.0	0	0	0	0	0
Cambodia	0.0	0.0	0.0	0.0	0.0	0	0	0	0	0
Cameroon	0.0	0.0	0.0	0.0	0.0	0	2	2	2	2
Canada	6.0	7.2	6.7	6.4	6.1	0	0	0	0	0
Central African Republic	0.6	4.9	4.9	4.9	4.8	0	0	0	0	0
Chad	0.0	0.0	0.0	0.0	0.0	0	0	0	0	0
Chile	6.5	7.5	7.0	6.7	6.3	2	5	5	5	4
China	6.7	8.0	7.4	7.0	6.6	0	0	0	0	0
Colombia	5.9	8.2	7.6	7.3	6.9	1	3	3	3	3
Congo, Dem. Rep.	5.6	7.0	6.6	6.3	5.9	1	3	3	3	3
Congo, Rep.	0.6	5.5	5.7	5.6	5.5	0	0	0	0	0
Costa Rica	5.8	7.6	7.1	6.7	6.4	1	2	3	3	3
Cote d'Ivoire	0.4	3.4	3.5	3.4	3.3	0	0	0	0	0
Croatia	5.0	5.9	6.6	6.2	5.8	0	0	0	0	0
Cuba	3.9	7.1	6.8	6.6	6.3	0	0	0	0	0
Cyprus	4.8	5.3	6.2	5.9	5.6	0	0	0	0	0
Czech Republic	2.8	4.4	4.7	4.4	4.2	0	0	0	0	0
Denmark	2.0	4.3	4.2	4.0	3.8	0	0	0	0	0
Dominican Republic	5.2	6.9	6.6	6.3	6.1	0	0	0	0	0
Ecuador	5.8	8.2	7.6	7.2	6.8	1	4	4	4	4
Egypt, Arab Rep.	4.6	7.1	6.6	6.2	5.9	0	0	0	0	0
El Salvador	5.4	8.0	7.5	7.2	6.8	0	3	3	3	3
Eritrea	0.0	0.0	0.0	0.0	0.0	0	0	0	0	0
Estonia	0.3	2.6	2.7	2.6	2.5	0	0	0	0	0
Ethiopia	5.1	6.8	6.5	6.2	5.9	0	0	0	0	0
Finland	3.2	4.3	4.0	3.7	3.5	0	0	0	0	0
France	4.0	5.0	4.6	4.4	4.1	0	0	0	0	0
Gabon	0.0	0.0	0.0	0.0	0.0	0	0	0	0	0
Gambia, The	0.0	0.0	0.0	0.0	0.0	0	0	0	0	0
Georgia	5.0	8.4	7.8	7.4	7.1	0	0	0	0	0
Germany	4.0	5.8	5.4	5.1	4.8	0	0	0	0	0
Ghana	1.2	4.4	4.4	4.2	4.1	0	0	0	0	0
Greece	6.1	7.2	6.6	6.3	5.9	0	0	0	0	0
Guatemala	5.7	7.1	6.7	6.4	6.0	0	3	4	4	4
Guinea	1.4	6.8	6.9	6.7	6.5	0	0	0	0	0
Guinea-Bissau	0.0	0.0	0.0	0.0	0.0	0	0	0	0	0
Haiti	3.1	7.3	6.9	6.5	6.2	0	0	0	0	0
Honduras	4.9	6.7	6.4	6.2	5.9	0	0	0	0	0
Hungary	2.2	5.1	5.0	4.7	4.5	0	0	0	0	0
India	6.6	8.6	7.9	7.5	7.0	0	0	0	0	0
Indonesia	7.2	8.4	7.8	7.5	7.1	3	4	5	5	4
Iran, Islamic Rep.	6.2	7.5	7.0	6.6	6.2	0	0	0	0	0
Iraq	5.1	5.8	5.5	5.2	5.0	0	0	0	0	0
Ireland	0.2	2.1	2.1	2.0	1.9	0	0	0	0	0
Israel	3.6	5.3	4.9	4.6	4.4	0	0	0	0	0

Table 5.20 – continued

Country	Earthquakes					Volcanoes				
	mean	maximum	10%	15%	20%	mean	maximum	10%	15%	20%
Italy	5.4	7.2	6.6	6.2	5.9	1	3	3	3	3
Jamaica	2.7	5.6	5.2	5.0	4.7	0	0	0	0	0
Japan	6.8	8.1	7.5	7.0	6.6	2	3	3	3	3
Jordan	4.5	6.0	5.5	5.2	4.9	0	0	0	0	0
Kazakhstan	4.9	5.8	6.8	6.5	6.1	0	0	0	0	0
Kenya	3.7	5.8	5.5	5.2	4.9	0	0	0	0	0
Korea, Rep.	3.8	6.2	5.8	5.6	5.3	0	0	0	0	0
Kuwait	0.2	2.2	2.2	2.1	2.0	0	0	0	0	0
Kyrgyz Republic	5.4	6.2	6.8	6.5	6.1	0	0	0	0	0
Lao PDR	4.4	7.4	7.1	6.8	6.5	0	0	0	0	0
Latvia	0.0	0.0	0.0	0.0	0.0	0	0	0	0	0
Lesotho	2.0	4.9	4.8	4.6	4.4	0	0	0	0	0
Liberia	0.5	4.7	4.7	4.5	4.4	0	0	0	0	0
Libya	2.0	4.9	5.4	5.2	5.0	0	0	0	0	0
Lithuania	0.0	0.0	0.0	0.0	0.0	0	0	0	0	0
Macedonia, FYR	4.9	5.7	6.6	6.2	5.8	0	0	0	0	0
Madagascar	2.6	5.8	5.7	5.5	5.3	0	0	0	0	0
Malawi	4.3	6.6	6.3	6.0	5.7	0	0	0	0	0
Malaysia	2.2	6.0	6.0	5.8	5.6	0	0	0	0	0
Mali	0.0	0.0	0.0	0.0	0.0	0	0	0	0	0
Mauritania	1.7	4.6	4.5	4.4	4.4	0	0	0	0	0
Mauritius	5.0	6.3	6.3	6.1	6.0	0	0	0	0	0
Mexico	6.5	8.3	7.7	7.3	6.9	1	5	5	5	4
Moldova	3.8	5.6	6.3	6.0	5.7	0	0	0	0	0
Mongolia	5.3	6.4	6.1	5.9	5.6	0	0	0	0	0
Morocco	4.3	5.2	4.9	4.7	4.5	0	0	0	0	0
Mozambique	4.7	7.4	7.0	6.6	6.3	0	0	0	0	0
Namibia	2.5	5.3	5.3	5.1	4.8	0	0	0	0	0
Nepal	5.3	7.2	6.9	6.6	6.3	0	0	0	0	0
Netherlands	1.7	5.6	5.5	5.2	4.9	0	0	0	0	0
New Zealand	6.6	8.2	7.5	7.1	6.7	1	3	3	3	3
Nicaragua	5.3	6.6	6.3	6.0	5.7	1	3	3	3	3
Niger	0.0	0.0	0.0	0.0	0.0	0	0	0	0	0
Nigeria	0.5	4.5	4.6	4.5	4.5	0	0	0	0	0
Norway	3.9	5.4	5.0	4.7	4.4	0	0	0	0	0
Oman	3.8	6.2	5.9	5.6	5.2	0	0	0	0	0
Pakistan	6.1	7.7	7.2	6.9	6.5	0	0	0	0	0
Panama	5.6	6.9	6.6	6.3	6.0	0	0	0	0	0
Papua New Guinea	6.9	8.0	7.5	7.2	6.9	2	4	4	4	4
Paraguay	0.7	5.8	5.9	5.8	5.8	0	0	0	0	0
Peru	6.4	7.9	7.3	6.9	6.5	0	3	3	3	3
Philippines	7.0	8.8	8.1	7.6	7.2	2	6	6	6	5
Poland	4.7	5.3	5.0	4.7	4.5	0	0	0	0	0
Portugal	3.7	5.4	5.0	4.7	4.4	0	0	0	0	0
Romania	5.1	7.4	6.9	6.6	6.3	0	0	0	0	0
Russian Federation	6.3	8.4	7.8	7.3	6.9	3	4	5	5	4
Rwanda	3.0	5.4	5.3	5.0	4.8	0	0	0	0	0
Saudi Arabia	4.8	6.5	6.0	5.7	5.4	0	0	0	0	0
Senegal	0.0	0.0	0.0	0.0	0.0	0	0	0	0	0
Serbia	0.5	5.5	5.7	5.4	5.1	0	0	0	0	0
Sierra Leone	0.7	6.8	6.8	6.6	6.3	0	0	0	0	0
Singapore	2.2	6.0	6.0	5.8	5.6	0	0	0	0	0
Slovak Republic	3.0	4.4	4.7	4.4	4.2	0	0	0	0	0
Slovenia	4.9	5.7	6.6	6.2	5.8	0	0	0	0	0
Somalia	1.3	6.0	6.0	5.8	5.6	0	0	0	0	0
South Africa	5.0	5.9	5.5	5.2	4.9	0	0	0	0	0
Spain	4.9	6.3	5.8	5.5	5.2	0	0	0	0	0
Sri Lanka	0.0	0.0	0.0	0.0	0.0	0	0	0	0	0
Sudan	3.8	7.2	6.9	6.5	6.2	0	0	0	0	0
Swaziland	0.4	3.9	4.7	4.6	4.4	0	0	0	0	0
Sweden	3.8	5.1	4.7	4.5	4.2	0	0	0	0	0
Switzerland	3.7	5.0	4.7	4.4	4.1	0	0	0	0	0
Syrian Arab Republic	4.3	6.0	5.5	5.2	4.9	0	0	0	0	0
Tajikistan	5.8	6.9	6.4	6.1	5.7	0	0	0	0	0
Tanzania	5.6	7.0	6.6	6.3	5.9	0	3	3	3	3
Thailand	4.0	6.3	6.1	5.9	5.6	0	0	0	0	0

Table 5.20 – continued

Country	Earthquakes					Volcanoes				
	mean	maximum	10%	15%	20%	mean	maximum	10%	15%	20%
Timor-Leste	5.7	7.2	7.2	6.9	6.7	0	0	0	0	0
Togo	0.0	0.0	0.0	0.0	0.0	0	0	0	0	0
Trinidad and Tobago	4.5	6.7	6.3	6.0	5.7	0	0	0	0	0
Tunisia	4.5	6.0	5.8	5.6	5.3	0	0	0	0	0
Turkey	5.8	7.7	7.1	6.8	6.4	0	0	0	0	0
Turkmenistan	5.3	7.4	7.1	6.8	6.5	0	0	0	0	0
Uganda	4.5	6.0	5.8	5.5	5.2	0	0	0	0	0
Ukraine	4.1	5.6	6.3	6.0	5.7	0	0	0	0	0
United Arab Emirates	3.4	5.8	5.9	5.6	5.2	0	0	0	0	0
United Kingdom	3.4	5.7	5.3	5.0	4.7	0	0	0	0	0
United States	6.6	8.0	7.3	6.9	6.5	3	5	5	5	4
Uruguay	0.0	0.0	0.0	0.0	0.0	0	0	0	0	0
Uzbekistan	4.5	5.4	6.9	6.6	6.3	0	0	0	0	0
Venezuela, RB	5.4	6.8	6.4	6.1	5.8	0	0	0	0	0
Vietnam	3.3	6.9	6.6	6.3	6.0	0	0	0	0	0
Yemen, Rep.	3.2	5.2	5.6	5.5	5.3	0	0	0	0	0
Zambia	4.4	5.7	5.4	5.1	4.8	0	0	0	0	0
Zimbabwe	3.4	6.4	0.0	0.0	0.0	0	0	0	0	0

Table 5.21: Cutoff Levels by Country, Storms and Precipitation Differences

Country	Storms					Precipitation				
	mean	maximum	10%	15%	20%	mean	maximum	10%	15%	20%
Afghanistan	37	50	61	58	54	1.25	2.04	2.27	2.14	2.01
Albania	37	66	61	57	54	1.12	2.23	2.09	1.97	1.85
Algeria	68	82	76	72	67	1.21	2.32	2.17	2.05	1.92
Angola	18	52	48	45	42	1.17	7.07	6.48	6.11	5.75
Argentina	76	97	89	84	80	0.54	1.18	1.11	1.04	0.98
Armenia	44	54	56	53	50	0.72	1.93	1.81	1.70	1.60
Australia	95	135	125	119	113	0.73	1.52	1.44	1.36	1.27
Austria	81	105	98	93	87	0.81	1.57	1.48	1.39	1.31
Azerbaijan	52	82	76	71	67	0.91	1.71	1.62	1.52	1.43
Bahrain	39	42	56	53	50	1.55	2.81	7.10	6.70	6.30
Bangladesh	54	115	105	99	94	1.64	4.62	4.29	4.04	3.80
Belarus	52	58	56	53	50	0.84	1.69	1.59	1.50	1.41
Belgium	63	90	84	79	75	0.95	1.77	1.68	1.58	1.48
Benin	45	80	73	69	65	2.83	18.81	17.22	16.26	15.29
Bolivia	59	80	74	70	66	0.69	1.22	1.15	1.08	1.01
Bosnia and Herzegovina	63	91	90	85	80	0.86	1.00	1.32	1.24	1.16
Botswana	51	61	56	53	50	2.15	14.79	13.54	12.79	12.03
Brazil	63	76	71	68	64	0.25	0.43	0.41	0.39	0.36
Bulgaria	79	126	118	111	105	1.08	2.46	2.30	2.17	2.04
Burkina Faso	48	70	64	61	57	2.18	10.77	9.90	9.34	8.79
Burundi	32	60	55	52	49	0.79	2.75	2.54	2.40	2.25
Cambodia	29	50	46	44	41	1.20	2.77	2.58	2.43	2.28
Cameroon	45	58	53	50	47	0.94	2.60	2.40	2.27	2.13
Canada	99	117	110	106	101	0.21	0.40	0.38	0.36	0.33
Central African Republic	40	52	48	45	43	0.93	2.37	2.21	2.08	1.95
Chad	44	90	83	78	74	1.38	5.36	4.96	4.67	4.39
Chile	76	95	88	84	80	0.48	0.93	0.87	0.82	0.77
China	85	117	108	102	97	0.55	1.04	0.97	0.92	0.86
Colombia	56	78	72	68	64	0.43	0.88	0.82	0.77	0.73
Congo, Dem. Rep.	38	50	46	43	41	0.26	0.53	0.50	0.47	0.44
Congo, Rep.	45	80	73	69	65	0.48	1.08	1.01	0.95	0.90
Costa Rica	49	67	62	59	56	0.74	1.58	1.47	1.38	1.30
Cote d'Ivoire	46	62	57	54	51	0.99	3.71	3.42	3.23	3.03
Croatia	67	84	97	92	87	0.85	1.58	1.49	1.41	1.32
Cuba	75	155	142	135	127	0.93	3.29	3.04	2.87	2.69
Cyprus	53	56	61	57	54	1.79	2.64	2.76	2.60	2.44
Czech Republic	81	97	91	86	81	0.90	1.63	1.54	1.45	1.36
Denmark	83	100	93	89	84	0.84	1.75	1.65	1.55	1.46
Dominican Republic	57	130	119	112	106	0.74	1.25	1.18	1.11	1.04
Ecuador	45	62	57	54	51	0.47	1.12	1.04	0.98	0.93
Egypt, Arab Rep.	58	67	62	59	56	1.59	4.34	4.03	3.80	3.57
El Salvador	42	60	55	52	49	0.90	1.90	1.78	1.68	1.57
Eritrea	12	40	56	53	50	1.50	4.80	4.44	4.19	3.94
Estonia	56	74	69	65	61	0.83	1.47	1.39	1.31	1.23
Ethiopia	47	70	64	61	57	1.23	2.95	2.73	2.58	2.42
Finland	57	82	76	72	68	0.64	1.04	0.99	0.93	0.87
France	97	114	106	100	95	0.76	1.17	1.12	1.05	0.99
Gabon	49	73	67	63	60	0.60	1.23	2.37	2.23	2.10
Gambia, The	43	80	73	69	65	3.68	13.41	12.30	11.61	10.92
Georgia	70	113	104	98	93	0.79	2.02	1.89	1.79	1.68
Germany	84	119	111	105	99	0.77	1.56	1.47	1.39	1.30
Ghana	37	58	53	50	47	0.97	3.64	3.36	3.17	2.98
Greece	67	87	81	77	73	0.95	2.26	2.12	2.00	1.88
Guatemala	41	65	60	57	54	1.02	2.64	2.45	2.31	2.17
Guinea	38	78	72	68	64	0.80	1.92	1.78	1.68	1.58
Guinea-Bissau	22	59	55	52	49	2.33	11.86	10.88	10.27	9.66
Haiti	45	140	128	121	114	0.84	2.00	1.86	1.76	1.65
Honduras	54	140	128	121	114	0.95	2.96	2.75	2.60	2.44
Hungary	61	107	99	94	88	1.01	1.85	1.74	1.64	1.54
India	67	140	129	121	114	0.79	3.21	2.97	2.80	2.63
Indonesia	60	76	70	66	62	0.45	0.78	0.74	0.70	0.65
Iran, Islamic Rep.	45	68	63	60	56	1.06	2.33	2.18	2.06	1.93
Iraq	34	60	61	58	55	1.29	3.31	3.08	2.90	2.73
Ireland	73	96	89	85	80	0.72	1.37	1.29	1.22	1.14
Israel	53	64	60	57	54	1.71	3.69	3.45	3.25	3.05

Table 5.21 – continued

Country	Storms					Precipitation				
	mean	maximum	10%	15%	20%	mean	maximum	10%	15%	20%
Italy	85	97	91	86	82	0.67	1.10	1.05	0.98	0.92
Jamaica	62	125	115	109	103	0.95	2.86	2.65	2.50	2.35
Japan	93	110	102	97	92	0.50	0.97	0.92	0.87	0.81
Jordan	58	72	66	63	59	2.03	5.57	5.15	4.86	4.57
Kazakhstan	71	80	91	86	82	0.66	1.04	1.17	1.11	1.04
Kenya	61	74	68	65	61	1.23	5.02	4.62	4.36	4.10
Korea, Rep.	72	98	91	86	82	0.99	2.25	2.12	1.99	1.87
Kuwait	44	57	61	57	54	1.87	4.03	3.77	3.55	3.33
Kyrgyz Republic	52	64	61	58	55	1.02	1.91	1.89	1.79	1.68
Lao PDR	42	55	51	48	45	0.92	2.02	1.89	1.78	1.67
Latvia	53	66	62	58	55	0.75	1.09	1.04	0.98	0.92
Lesotho	31	55	51	48	45	1.56	5.58	5.17	4.87	4.58
Liberia	19	45	41	39	37	0.83	3.45	3.18	3.00	2.82
Libya	63	73	74	70	66	1.70	4.66	4.31	4.07	3.82
Lithuania	54	74	69	65	61	0.75	1.06	1.02	0.96	0.90
Macedonia, FYR	62	80	74	70	66	1.00	2.20	2.07	1.95	1.83
Madagascar	77	120	110	104	98	0.77	2.04	1.90	1.79	1.68
Malawi	34	62	57	54	51	1.21	2.62	2.45	2.31	2.17
Malaysia	46	51	47	45	42	0.55	1.13	1.06	1.00	0.94
Mali	54	87	80	76	71	1.71	8.58	7.88	7.44	6.99
Mauritania	62	90	83	78	74	2.50	8.00	7.37	6.95	6.54
Mauritius	64	100	92	87	82	1.14	2.35	2.20	2.07	1.95
Mexico	104	165	153	145	137	0.70	2.11	1.96	1.85	1.74
Moldova	43	58	59	56	53	1.13	2.04	1.93	1.81	1.70
Mongolia	69	86	79	75	71	0.70	1.52	1.43	1.34	1.26
Morocco	62	80	74	71	67	1.54	4.38	4.06	3.83	3.59
Mozambique	60	105	96	91	86	0.92	2.07	1.94	1.82	1.71
Namibia	53	66	62	59	55	1.55	4.05	3.77	3.55	3.34
Nepal	34	55	51	48	45	1.53	5.15	4.77	4.50	4.23
Netherlands	70	88	82	78	73	0.86	1.41	1.34	1.26	1.18
New Zealand	71	95	88	84	79	0.46	1.06	0.99	0.93	0.88
Nicaragua	47	140	128	121	114	0.71	2.85	2.63	2.48	2.33
Niger	56	70	65	61	58	1.98	5.63	5.22	4.93	4.63
Nigeria	31	64	59	56	53	1.06	3.38	3.13	2.95	2.77
Norway	89	120	112	106	101	0.61	1.17	1.11	1.04	0.98
Oman	59	84	77	73	69	2.68	6.72	6.18	5.84	5.49
Pakistan	62	105	97	91	86	1.31	4.46	4.13	3.89	3.66
Panama	44	60	55	52	49	0.90	1.95	1.82	1.72	1.61
Papua New Guinea	25	89	82	77	73	0.46	0.67	0.64	0.60	0.56
Paraguay	56	70	65	61	58	0.89	2.36	2.20	2.07	1.95
Peru	55	72	66	63	60	0.33	0.52	0.49	0.47	0.44
Philippines	90	125	115	109	103	0.68	1.54	1.44	1.36	1.28
Poland	86	95	89	85	80	0.78	1.38	1.31	1.23	1.15
Portugal	68	84	78	74	70	1.07	2.17	2.04	1.92	1.80
Romania	87	117	109	103	98	0.80	1.36	1.29	1.22	1.14
Russian Federation	89	113	106	101	96	0.24	0.36	0.34	0.32	0.30
Rwanda	30	52	48	45	43	0.55	1.56	1.45	1.37	1.29
Saudi Arabia	57	68	63	60	56	1.69	5.01	4.64	4.38	4.12
Senegal	53	61	56	53	51	2.98	14.79	13.55	12.79	12.03
Serbia	59	72	74	70	66	1.02	1.57	1.50	1.41	1.32
Sierra Leone	22	51	47	44	42	1.27	5.95	5.47	5.16	4.85
Singapore	37	50	46	43	41	0.68	1.57	1.47	1.39	1.30
Slovak Republic	82	97	94	89	84	0.92	1.85	1.74	1.64	1.54
Slovenia	61	87	84	79	75	0.81	1.58	1.49	1.40	1.31
Somalia	34	90	83	78	74	1.64	6.67	6.13	5.79	5.44
South Africa	70	103	96	91	86	1.04	3.23	2.99	2.82	2.65
Spain	74	93	87	83	78	0.99	2.15	2.01	1.89	1.78
Sri Lanka	48	90	83	78	73	0.98	3.07	2.85	2.69	2.52
Sudan	55	93	85	81	76	0.94	2.54	2.37	2.23	2.10
Swaziland	39	50	46	44	41	1.26	2.82	6.85	6.47	6.08
Sweden	69	95	88	84	79	0.59	0.95	0.91	0.86	0.80
Switzerland	97	119	111	105	99	0.90	2.14	2.00	1.89	1.77
Syrian Arab Republic	52	70	64	61	57	1.43	3.79	3.52	3.32	3.12
Tajikistan	48	58	65	61	57	0.89	1.76	2.89	2.73	2.56
Tanzania	57	73	67	64	60	0.64	2.21	2.05	1.93	1.82
Thailand	50	73	68	64	61	0.75	1.73	1.62	1.52	1.43

Table 5.21 – continued

Country	Storms					Precipitation				
	mean	maximum	10%	15%	20%	mean	maximum	10%	15%	20%
Timor-Leste	0	0	0	0	0	1.65	3.64	3.37	3.18	2.99
Togo	44	56	51	49	46	1.10	4.79	4.42	4.17	3.92
Trinidad and Tobago	39	61	56	53	50	0.97	2.50	2.32	2.19	2.06
Tunisia	61	75	70	66	63	1.24	2.73	2.55	2.40	2.26
Turkey	61	80	74	71	67	0.77	1.64	1.54	1.45	1.36
Turkmenistan	53	66	83	78	74	1.24	2.68	3.36	3.16	2.97
Uganda	33	60	55	52	49	0.80	2.61	2.41	2.27	2.14
Ukraine	58	78	73	69	65	0.75	1.19	1.13	1.07	1.00
United Arab Emirates	51	64	59	56	53	2.69	9.54	8.75	8.26	7.77
United Kingdom	100	134	125	119	113	0.63	1.17	1.11	1.04	0.98
United States	118	145	136	130	124	0.28	0.40	0.38	0.36	0.34
Uruguay	57	76	71	67	63	1.00	2.01	1.89	1.78	1.67
Uzbekistan	54	66	61	58	54	1.22	2.26	2.13	2.01	1.88
Venezuela, RB	53	68	63	59	56	0.58	1.44	1.34	1.27	1.19
Vietnam	61	80	74	70	66	0.76	1.59	1.48	1.40	1.31
Yemen, Rep.	64	97	90	85	80	1.37	3.53	5.01	4.73	4.44
Zambia	45	65	60	56	53	1.30	4.67	4.31	4.07	3.82
Zimbabwe	49	68	63	59	56	1.92	5.15	4.76	4.49	4.22

Table 5.22: Corrections on Disaster Data, EM-DAT versus New Data

Type	Country	Year	Month	Comment
Earthquake	Bulgaria	1990	5	no magnitude recorded in IRIS
Earthquake	Congo, Democratic Republic of	1992	1	other month coded in EMDAT
Earthquake	Congo, Democratic Republic of	2008	1	other month coded in EMDAT
Earthquake	Congo, Republic of	2005	12	not listed in IRIS
Earthquake	Dominican Republic	2003	1	other month coded in EMDAT
Earthquake	Malaysia	2004	12	Tsunami
Earthquake	Maldives	2004	12	Tsunami
Earthquake	Rwanda	2002	1	not listed in IRIS
Earthquake	Rwanda	2008	2	not listed in IRIS
Earthquake	Saint Lucia	2007	1	not listed in IRIS
Earthquake	Saint Lucia	2007	11	not listed in IRIS
Earthquake	Somalia	2004	12	Tsunami
Earthquake	Sri Lanka	2004	12	Tsunami
Earthquake	Tanzania	2000	1	not listed in IRIS
Earthquake	Tanzania	2004	1	not listed in IRIS
Earthquake	Thailand	1995	7	not listed in IRIS
Earthquake	Thailand	2004	12	Tsunami
Earthquake	Uganda	1990	5	other month coded in EMDAT
Earthquake	Uzbekistan	1992	5	other month coded in EMDAT
Storm	Bhutan	1994	5	no wind speed listed in Weather Stations
Storm	Bhutan	2009	5	no wind speed listed in Weather Stations
Storm	Burundi	2010	10	no wind speed listed in Weather Stations
Storm	Dominican Republic	1995	8	no wind speed listed in Weather Stations
Storm	Gambia, The	2008	1	no wind speed listed in Weather Stations
Storm	Guinea-Bissau	1993	1	no wind speed listed in Weather Stations
Storm	Haiti	2001	10	no wind speed listed in Weather Stations
Storm	Haiti	2007	8	no wind speed listed in Weather Stations
Storm	Iran	1981	1	no wind speed listed in Weather Stations
Storm	Iran	1981	10	no wind speed listed in Weather Stations
Storm	Liberia	1995	8	no wind speed listed in Weather Stations
Storm	Malawi	2005	12	no wind speed listed in Weather Stations
Storm	Papua New Guinea	2007	11	no wind speed listed in Weather Stations
Storm	Sierra Leone	1984	7	no wind speed listed in Weather Stations
Storm	Sierra Leone	1999	6	no wind speed listed in Weather Stations
Volcano	Argentina	1991	8	Outbreak on Chilean national territory
Volcano	Argentina	2008	5	Outbreak on Chilean national territory
Volcano	Cameroon	1984	8	no eruption listed in Global Volcanism Programme
Volcano	Cameroon	1986	8	no eruption listed in Global Volcanism Programme
Volcano	Cameroon	1999	4	other month coded in EMDAT
Volcano	Cape Verde	1995	1	other month coded in EMDAT
Volcano	Chile	1993	4	other month coded in EMDAT
Volcano	Colombia	1985	11	other month coded in EMDAT
Volcano	Colombia	1988	3	no eruption listed in Global Volcanism Programme
Volcano	Colombia	2006	3	other month coded in EMDAT
Volcano	Colombia	2006	7	no eruption listed in Global Volcanism Programme
Volcano	Colombia	2007	4	other month coded in EMDAT
Volcano	Colombia	2008	4	other month coded in EMDAT
Volcano	Colombia	2008	11	other month coded in EMDAT
Volcano	Congo, Democratic Republic of	2001	1	other month coded in EMDAT
Volcano	Costa Rica	1991	8	other month coded in EMDAT
Volcano	Costa Rica	1998	5	other month coded in EMDAT
Volcano	Ecuador	1999	9	other month coded in EMDAT
Volcano	El Salvador	2005	10	other month coded in EMDAT
Volcano	Guatemala	1983	7	no eruption listed in Global Volcanism Programme

Table 5.22 – continued

Type	Country	Year	Month	Comment
Volcano	Guatemala	1984	7	no eruption listed in Global Volcanism Programme
Volcano	Guatemala	1992	1	other month coded in EMDAT
Volcano	Guatemala	1996	11	no eruption listed in Global Volcanism Programme
Volcano	Guatemala	1998	5	no eruption listed in Global Volcanism Programme
Volcano	Guatemala	2000	1	other month coded in EMDAT
Volcano	Guatemala	2002	2	other month coded in EMDAT
Volcano	Guatemala	2010	5	other month coded in EMDAT
Volcano	Iceland	1996	10	other month coded in EMDAT
Volcano	Indonesia	1983	6	other month coded in EMDAT
Volcano	Indonesia	1984	6	other month coded in EMDAT
Volcano	Indonesia	1984	9	other month coded in EMDAT
Volcano	Indonesia	1991	10	other month coded in EMDAT
Volcano	Indonesia	1994	11	other month coded in EMDAT
Volcano	Indonesia	1994	11	other month coded in EMDAT
Volcano	Indonesia	1997	1	other month coded in EMDAT
Volcano	Indonesia	2004	9	other month coded in EMDAT
Volcano	Indonesia	2006	4	other month coded in EMDAT
Volcano	Italy	1979	9	other month coded in EMDAT
Volcano	Japan	1991	6	other month coded in EMDAT
Volcano	Japan	1993	6	other month coded in EMDAT
Volcano	Japan	2000	8	other month coded in EMDAT
Volcano	Mexico	1997	7	other month coded in EMDAT
Volcano	Mexico	1998	11	other month coded in EMDAT
Volcano	Mexico	1999	2	other month coded in EMDAT
Volcano	Mexico	2000	12	other month coded in EMDAT
Volcano	Mexico	2002	2	other month coded in EMDAT
Volcano	New Zealand	1995	9	other month coded in EMDAT
Volcano	Nicaragua	1995	11	other month coded in EMDAT
Volcano	Papua New Guinea	1983	10	other month coded in EMDAT
Volcano	Papua New Guinea	1992	10	other year coded in EMDAT
Volcano	Papua New Guinea	1996	10	other year coded in EMDAT
Volcano	Papua New Guinea	1996	12	other year coded in EMDAT
Volcano	Papua New Guinea	2002	7	other month coded in EMDAT
Volcano	Papua New Guinea	2005	6	other month coded in EMDAT
Volcano	Papua New Guinea	2006	7	other month coded in EMDAT
Volcano	Peru	1990	6	other month coded in EMDAT
Volcano	Philippines	1991	6	other month coded in EMDAT
Volcano	Philippines	1992	2	no eruption listed in Global Volcanism Programme
Volcano	Philippines	1995	7	no eruption listed in Global Volcanism Programme
Volcano	Philippines	2000	2	other month coded in EMDAT
Volcano	Philippines	2001	5	other month coded in EMDAT
Volcano	Philippines	2001	7	no eruption listed in Global Volcanism Programme
Volcano	Philippines	2006	8	other month coded in EMDAT
Volcano	Philippines	2007	7	other month coded in EMDAT
Volcano	Philippines	2009	12	other month coded in EMDAT
Volcano	Saint Vincent and the Grenadines	1979	1	no eruption listed in Global Volcanism Programme
Volcano	Trinidad and Tobago	1997	2	no eruption listed in Global Volcanism Programme

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