
On the Consequences of Trade: Inequality, the Environment, and Aid

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Introduction

Backdrop

Over the last half century, international trade has become increasingly important for the world economy. As a share of global GDP, the sum of exports and imports went from around 25% in 1970 to a peak of 63% in 2022 (World Bank, 2024a). This rise has been particularly pronounced since the last decade of the 20th century, as the latest wave of globalization—or hyperglobalization even (Rodrik, 2019)—gained momentum. By encouraging specialization and leveraging comparative advantage (Grossman and Helpman, 2015), trade has contributed to raising productivity levels and ultimately accelerating economic growth (Frankel and Romer, 1999; Dollar and Kraay, 2002).

The case of China is a good example of such a process. In a few decades, the country went from a rather low income level to becoming one of the largest economies in the world. To a considerable degree, this success is attributable to China’s growing engagement with international markets, more precisely as an exporter of manufactured goods and a recipient of foreign investment—even if with an important participation of the Chinese state (Brandt and Rawski, 2008; Zhu, 2012). Total factor productivity in the country grew by almost 30% between 1977—the year before Deng Xiaoping kicked off the process of “reform and opening-up”—and 2011, a decade after the country’s accession to the World Trade Organization (FRED, 2024). In the same period, real GDP per capita grew more than 17-fold (World Bank, 2024a), while extreme poverty, which affected more than 90% of the population in 1977, was virtually eliminated (World Bank, 2024b).

However, China’s rapid economic rise has had significant—and not always positive—international implications. In particular, in the process of becoming the “global factory,” China induced a far-reaching shift in trade patterns all over the world—the so-called “China shock” (Autor et al., 2013; Autor et al., 2016). Many firms in other countries began to struggle as they faced tougher competition from China, not rarely leading to a lasting blow to employment and wage levels (Autor

et al., 2021). In line with the predictions of a simple Heckscher-Ohlin model, low-skilled workers—particularly in richer countries, which tend to have a comparative advantage in the production of goods with higher value added—were hit the hardest as trade exposure to China increased. The consequences of this process have been not only economic, such as a surge in unemployment and higher income and wealth inequality within countries, but also political, with rising ideological polarization (Autor et al., 2020) and nationalism (Colantone and Stanig, 2018).

China’s ascent served as a timely reminder that trade creates not only “winners,” but—if not properly compensated—also “losers,” such that the long-held “consensus that trade could be strongly redistributive in theory but was relatively benign in practice has not stood up well to these new developments” (Autor et al., 2016, p. 208). In fact, the share of “losers” can be considerably high, especially in the absence of countervailing monetary and fiscal policies (Stiglitz, 2017). In other words, although trade liberalization may have the potential to make everyone better off, the many that inevitably lose out are rarely compensated, an element of central importance to explain the current backlash against globalization (e.g., Stiglitz, 2017; Rodrik, 2019; Guriev and Papaioannou, 2022; Colantone et al., 2024).

In addition to its troubled relationship with economic inequality, trade liberalization also carries ambiguous consequences for the environment (Gallagher, 2009). On the one hand, there is some evidence of an inverted-U shaped relationship between national income and environmental degradation—referred to as the environmental Kuznets curve—which suggests that countries may pollute less as they become richer (Grossman and Krueger, 1995; Antweiler et al., 2001). Insofar as trade contributes to economic growth (Frankel and Romer, 1999), it should then also lead to better environmental indicators as well as to stricter environmental regulation, as richer societies tend to value a clean environment more highly (Dasgupta et al., 2002). A particularly relevant way in which trade may contribute to better environmental quality is via the transmission of more efficient and “greener” technologies (Frankel and Rose, 2005; Antràs, 2020).

These improvements in technique notwithstanding, the overall scale of pollution and resource depletion is likely to increase with the expansion of economic activity (Grossman and Krueger, 1991; Copeland and Taylor, 1994; World Bank, 2020). In fact, as indicated by the Jevons paradox, efficiency gains may actually amplify the damage to the environment. To make matters worse, international competition may prompt countries to relax their environmental standards, thus leading to a race to the bottom (Vogel, 1997; Esty, 2001). As some countries—usually the poorer ones—tend to have particularly weak environmental regulation, this process may

lead to the creation of pollution havens (Eskeland and Harrison, 2003; Copeland and Taylor, 2004), in which firms specialize in the production of pollution-intensive goods.

This pattern of production specialization according to the quality of environmental standards evokes a broader issue associated with international trade, namely the fact that countries around the world are in very different positions in terms of their ability to compete on the international market (Stiglitz, 2017). That is, there is also substantial inequality *between* countries, not only with respect to income levels,¹ but also in terms of production factors, which affect the overall pattern of production specialization (Hidalgo et al., 2007). Despite being challenged, as of late, by a few emerging economies, most notably China, these deep imbalances in wealth and power are also visible in the architecture of the international order (Mansfield, 2014). In fact, more influential countries often exploit these imbalances to further cement their advantages (e.g., Dreher et al., 2009; Kersting and Kilby, 2014).

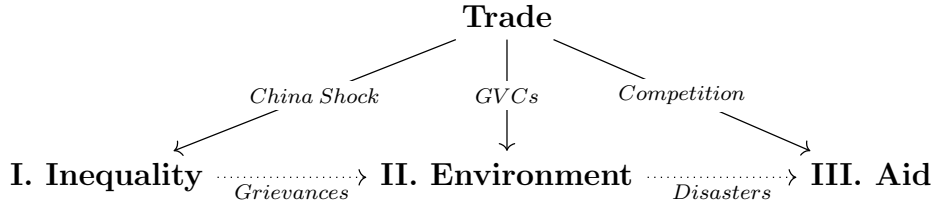
A notorious manifestation of such power play occurs through the provision of foreign aid as powerful donors allocate aid not only based on recipient need, but also to further their own interests (e.g., Alesina and Dollar, 2000; Kuziemko and Werker, 2006; Bueno de Mesquita and Smith, 2009; Fuchs et al., 2015). Overwhelmingly, these strategic considerations revolve around either commercial interests or geopolitics—if not both. Therefore, what a country trades not only reflects, at least partially, its position in the international order, but also exerts a considerable influence on what kind of foreign policy it engages in, of which aid is a crucial part (Baldwin, 1985; Milner and Tingley, 2010).

Overview

It is from this backdrop that this dissertation departs. It analyzes, empirically, the consequences of international trade along the three dimensions outlined above: economic inequality (Chapter 1), the environment (Chapter 2), and international aid (Chapter 3). In addition to their methodological similarities, the chapters also highlight how these dimensions are interconnected, as illustrated by Figure 1.

Building upon the argument that factor endowments influence distributional outcomes (Sokoloff and Engerman, 2000), Chapter 1, entitled *The (Other) China Shock and the Brazilian Soy Boom: Cui Bono?*, examines the consequences of the “other”

¹For instance, while the countries in the G7 (Canada, France, Germany, Italy, Japan, United Kingdom, and the United States) account for about 10% of the world’s population, their economies represent roughly 30% of the global GDP—though this mismatch has been declining over time (World Bank, 2024a).

Figure 1 – Dissertation Overview

China shock—that is, the repercussions of the surge in Chinese demand, mostly for agricultural commodities (Hansen and Wingender, 2023)—for economic inequality in Brazilian municipalities from 1985 to 2020. It proposes a new identification strategy that exploits plausibly exogenous variation in demand for soybeans based on fluctuations in the size of the pig stock in China and shows that the proceeds of this China-driven agricultural bonanza have been rather unequally distributed.

The soy boom has fueled land consolidation and economic inequality, especially in places dominated by large-scale mechanized agriculture. Income gains have been mostly limited to the top deciles of the distribution, while the poorest segments of the population have become worse off. Additionally, there is evidence that the more unequal a municipality, the more deforestation and rural conflict increase as soy expands. Hence, as Figure 1 shows, economic inequality can have important implications for environmental conservation, in particular due to social grievances that often arise as a result of an unequal distribution of resources (Albertus et al., 2018). More broadly, this suggests that one cannot fully grasp the impact on the environment without taking the underlying socioeconomic forces into account.

These findings add a new layer to the broader empirical literature on the consequences of trade for economic inequality as they fill an important gap with regard to the global consequences of China’s rapid economic rise. Although there is a rich body of work on the supply-side of this shock—which has hit mostly high-income countries through import competition (Autor et al., 2013; Autor et al., 2016; Autor et al., 2020)—far less is known about the consequences of surging Chinese demand, which has been especially strong for raw materials and soybeans in particular. Due to the telecoupled nature of the soy trade (Pendrill et al., 2019; Meyfroidt et al., 2020), the explicit inclusion of China in the analytical framework is of particular importance.

Moreover, especially due to its focus on Brazil—an agricultural powerhouse and the world’s largest net exporter of food (Klein and Luna, 2019)—Chapter 1 speaks to the literature on commodity booms and agricultural modernization (e.g., Gollin et al., 2007; Cisneros et al., 2021; Farrokhi and Pellegrina, 2023). In the case of

the Brazilian soy boom, previous studies have shown that it has ushered a structural transformation of the economy, with productivity gains not restricted to the agricultural sector (Bustos et al., 2016; Bustos et al., 2020). There is also (mostly descriptive) evidence of benefits in terms of income gains (Weinhold et al., 2013; Garrett and Rausch, 2016; Martinelli et al., 2017) as well as higher well-being indicators and poverty reduction (VanWey et al., 2013; Weinhold et al., 2013; Piras et al., 2021). However, the consequences for the distribution of income and land have not received much attention.²

Chapter 1 contributes to this debate by bringing the distributive issue to the center of the analysis. This is of crucial importance not only because of the sheer magnitude of soy production in Brazil (by far the most important crop in the country), but in particular due to the specific characteristics of modern soy cultivation.³ To be economically viable, it usually requires substantial investments in machinery, pesticides, and fertilizers as well as large plots of land (Fearnside, 2001; Weinhold et al., 2013; Bustos et al., 2016). Furthermore, the analysis introduces a new instrumental variable to deal with the well-known issues of endogeneity in this setting. This approach is shown to provide robust results for analyses both in the medium as well as in the long run. Therefore, it pushes the literature on the distributional outcomes of the soy boom beyond descriptive evidence and, more broadly, puts forward an empirical strategy for causal identification that can be used to assess a myriad of other outcomes related to the Brazilian commodity boom and the “other” China shock.⁴

Chapter 2, *Global Value Chains and Firms’ Environmental Performance* (co-authored with Inmaculada Martínez-Zarzoso and Leila Baghdadi), (re)turns to the environmental consequences of trade. Based on new survey data for firms in Central Asia, Eastern Europe, Middle East, and North Africa, it examines the link between global value chains (GVCs) and firms’ environmental performance. Using propensity score matching (PSM) to compare similar companies that differ in terms of GVC participation, the analysis shows that firms that join GVCs perform better across several environmental indicators. In particular, these firms are more likely to

²Weinhold et al. (2013) and Martinelli et al. (2017) show descriptive evidence that soybean production is correlated with higher economic inequality. Nevertheless—as the analysis in Chapter 1 shows—these correlations significantly underestimate the size of the effect.

³Soy has become the most relevant agricultural activity in Brazil both in terms of planted area as well as production amount, accounting for almost half of the total value of agricultural production (IBGE, 2023a) and roughly 14% of the country’s export revenues (MDIC, 2022).

⁴So far, most of this literature relies on the differential gains in yields, across municipalities, in the aftermath of the adoption of genetically engineered seeds in Brazil (e.g., Bustos et al., 2016; Bustos et al., 2020; Falcone and Rosenberg, 2022). A limitation of this approach is that it only allows for an analysis in first differences—before and after the adoption.

adopt environmentally friendly production technologies, comply with more demanding standards and regulations and, in the case of manufacturing firms, monitor CO₂ emissions along their supply chains. Moreover, by combining PSM with difference-in-differences, it provides evidence that becoming active in GVCs is also linked to more efficient energy use. The results are stronger for firms in countries outside the European Union and underscore the importance of environmental regulation to ensure that global supply chains go hand in hand with greener production. In fact, the analysis indicates that environmental regulations and standards may be a necessary condition to guarantee that international trade pushes firms to operate in a more environmentally friendly manner.

To date, there has been little evidence—if at all—on how joining GVCs affects firms’ environmental footprint. Most studies at the firm level focus on a single country and usually disregard the importance of two-way trading and the productivity advantage that could emerge when firms have easier access to imported inputs (e.g., Cole et al., 2008; Batrakova and Davies, 2012; Richter and Schiersch, 2017). This is of particular importance as it could also generate a double dividend if the engagement of firms with GVCs implies that they must comply with stricter product standards and environmental regulations.

Chapter 2 is therefore the first study to examine the link between GVC participation and a wide range of environmental indicators at the firm level. By taking advantage of detailed information on firms’ environmental records, recently made available in the Green Economy Module of the World Bank Enterprise Surveys (World Bank, 2021), it presents a comprehensive picture, across several dimensions, of how joining GVCs is linked to firms’ behavior with respect to the environment. This includes (i) pollution monitoring, (ii) the adoption of several more climate-friendly production technologies, as well as (iii) the external constraints that firms face, such as environmental regulation and customer standards.

Moreover, the focus on GVCs reflects a broader trend in international trade as companies have been spreading across the globe in search of lower costs, thus making production chains increasingly geographically fragmented. The share of international trade that takes place within GVCs went from about 40% in 2009 to more than 70% a decade later (OECD, 2013; 2020). Due to the use of firm-level data on a broad set of countries that stretch across different survey waves, the analysis in Chapter 2 is also able to identify important channels that influence the link between GVCs and the environment. It therefore adds more nuance to the broader literature on the trade-environment nexus, which is mostly based on data at higher levels of aggregation (e.g., Grossman and Krueger, 1991; Copeland and

Taylor, 1994; Antweiler et al., 2001; Managi et al., 2009).

Finally, Chapter 3, *The Speed of Aid: Strategic Urgency in International Emergency Relief* (co-authored with Andreas Fuchs), investigates the effects of trade and geopolitical competition for the provision of humanitarian assistance in the aftermath of natural disasters. Based on a trilateral panel of daily humanitarian aid decisions of 43 donor countries following more than 500 fast-onset natural disasters between 2000 and 2022, it shows that donor countries take faster aid decisions if they have stronger strategic interests at stake in a given recipient country.

The analysis, which relies on daily variation in donor responses and a series of multidimensional fixed effects for identification, reveals a bandwagon effect—as donors follow their peers’ commitments—which is largely explained by commercial competition: the more donors compete over export and import markets, the faster they react to each other. The results are driven by government-to-government aid and underscore the importance of recipient-specific lead donors, who are natural first movers. In sharp contrast to the UN Resolution 46/182 (OP2), which states that “humanitarian assistance must be provided in accordance with the principles of humanity, neutrality and impartiality” (OCHA, 2009), these findings suggest that trade competition can distort emergency relief and highlight that strategic interests shape even ostensibly altruistic behavior in international humanitarian aid.

Chapter 3 provides the first empirical analysis of the speed of foreign aid decisions. Moreover, by focusing on emergency relief, it focuses on a context often presumed to be less politicized than that of other types of aid and targets the kind of assistance for which speed is particularly decisive for effectiveness. Chapter 3 therefore contributes to the literature on the economics of natural disasters (e.g., Kahn, 2005; Strobl, 2012; Cavallo et al., 2013; Felbermayr and Gröschl, 2013; Felbermayr and Gröschl, 2014) as it adds a crucial dimension to the understanding of how the international community reacts to these events. It highlights how strategic interests—and trade competition in particular—may compromise the speed and thus arguably the effectiveness of such efforts. In fact, as climate change continues to increase the frequency and the intensity of extreme weather events (IPCC, 2023), the urgency and relevance of timely emergency aid after natural disasters are likely to become even more important in the future—which also reiterates the interconnectedness between environmental and climatic conditions and foreign aid, as indicated by the dashed arrow in Figure 1.

More broadly, Chapter 3 advances the economics literature on international humanitarian assistance (Strömberg, 2007; Raschky and Schwindt, 2012; Becerra et al., 2014; Nunn and Qian, 2014; Becerra et al., 2015; Raschky and Schwindt, 2016)

and, in particular, the strand concerned with the determinants of its allocation decisions (Eisensee and Strömberg, 2007; Annen and Strickland, 2017; Fuchs and Öhler, 2021; Mogge et al., 2023). It does so by shifting the focus from the traditional questions of “to whom?” and “how much aid?” to the crucial question of “how fast?,” demonstrating that strategic donor interests not only influence aid volumes but also response times.

Furthermore, Chapter 3 is embedded in a rich literature of research on aid donor interactions, which includes studies of coordination (Djankov et al., 2009; Aldasoro et al., 2010; Bourguignon and Platteau, 2015) as well as competition (Fuchs et al., 2015; Steinwand, 2015; Davies and Klasen, 2019) between donors. Even though there is a virtual consensus on the widespread lack of coordination, this literature still lacks a proper understanding of the underlying causes of donor fragmentation (Steinwand, 2015). Chapter 3 contributes to this debate by analyzing on-going aid decisions after a clearly identified event. By watching the response to disaster emergencies as it unfolds, day after day, the analysis in Chapter 3 is able to identify new patterns of interaction and competition among donors—the implications of which are not restricted to humanitarian aid.

Chapter 1

The (Other) China Shock and the Brazilian Soy Boom: Cui Bono?

Abstract

Building upon the argument that factor endowments influence distributional outcomes, this chapter examines the consequences of the China shock to global food markets for economic inequality in Brazilian municipalities from 1985 to 2020. I propose a new identification strategy that exploits plausibly exogenous variation in demand for soybeans based on fluctuations in the size of the pig stock in China and show that the proceeds of this China-driven agricultural bonanza have been rather unequally distributed. The soy boom has fueled land consolidation and economic inequality, especially in places dominated by large-scale mechanized agriculture. Income gains have been mostly limited to the top deciles of the distribution, while the poorest segments of the population have become worse off. Additionally, there is evidence that the more unequal a municipality, the more deforestation and rural conflict increase as soy expands.

Keywords: inequality, soybeans, Brazil, China shock, deforestation, conflict, sustainability, supply chains

JEL Codes: D63, F63, O13, Q17, Q56

1.1 Introduction

One of the most significant implications of the recent rise of China as an economic power has been a large positive shock in demand for raw materials—which has been labeled the “other” China shock (Hansen and Wingender, 2023). Featuring prominently on the receiving end of this shock, particularly via growing demand for soybeans, is Brazil, a country in which the production of this crop more than tripled in two decades, from around 33 Mt in 2000 to almost 122 Mt in 2020 (IBGE, 2023a). With China as its main buyer, Brazil has recently overtaken the United States to become the world’s largest producer and exporter of soybeans (FAO, 2023).¹

To what extent has this newfound prosperity trickled down to broader swaths of the Brazilian population and at what costs? This chapter examines the socio-economic and environmental consequences of this demand shock and the boom in soybean production that ensued. Following Sokoloff and Engerman’s (2000) seminal contribution, the underlying premise is that factor endowments play an important role in determining (structural) inequality. Or, in the case of China and Brazil, I argue that, by its sheer scale, the former’s economic rise and its growing need for certain agricultural commodities—especially after China’s accession to the WTO in 2001—have significantly influenced the use of factor endowments in the latter. Not only has Chinese demand been a major force behind Brazil’s consolidation as an agricultural powerhouse, but it has favored the production of certain crops more than others. In particular, it has helped making this model of large-scale, industrialized soybean cultivation—or “neo-developmentalism” (Hopewell, 2017; Sauer and Mészáros, 2017)—viable and highly profitable. Therefore, the China shock might have had unintended, but profound implications for the distribution of land and income in Brazil. More precisely, for reasons discussed below, this study’s main hypothesis is that soy expansion fuels economic inequality.

The focus on soy is justified for at least two reasons. First, although exports of other agricultural commodities produced in Brazil have also increased in the last few decades, soybeans are by far the most important crop in the country, corresponding to more than 40% of the total value of agricultural production (IBGE, 2023a) and taking up a larger area than any other crop (Martinelli et al., 2017).² Second, (modern) soybean cultivation possesses certain features that should make its expansion

¹Soybeans are one of the world’s most commercialized commodities, reaching a total trade volume of USD 64 billion in 2020 (Escobar et al., 2020). Among its many uses, this legume is demanded, to a large extent, to serve as animal feed.

²What is more, soybeans account for approximately 14% of Brazil’s export revenues (MDIC, 2022) and are planted by more than 250,000 producers in over 1,500 municipalities all over the country’s territory (Martinelli et al., 2017).

particularly worrying in terms of distributional consequences. Differently from traditional, subsistence agriculture, soy is a huge business, highly capital intensive and reliant on constant technological innovation (Hopewell, 2017; Navarro and Pedroso, 2018). Importantly, it is “one of the least labor-intensive agricultural activities” (Bustos et al., 2016, p. 1328) and usually the “domain of wealthy agribusiness entrepreneurs” (Fearnside, 2001, p. 28). With soy as one of its most demanded agricultural goods (Hansen and Wingender, 2023), China’s rapid economic rise has been an enormous incentive for the expansion of this crop in Brazil.³

To arrive at causally credible estimates, I propose a new identification strategy that exploits fluctuations in the size of the pig stock in China over time. As I explain in detail below, this is a major driver of Chinese demand for soy and is affected to a large extent by sporadic episodes of disease outbreak. For cross-sectional variation, I interact this variable with soil suitability for soybean cultivation in Brazilian municipalities, which yields a powerful instrument for municipal soy production.

The empirical analysis consists of two parts. First, using data for all Brazilian municipalities from 1985 to 2017 at (roughly) ten-year intervals, I provide evidence that, in the long run, land inequality has increased as soy expands. In a difference-in-differences-like setup, the results indicate that China’s WTO accession has constituted an inflection point in the relationship between inequality and soy production. After 2001, the more suitable a municipality is for soy cultivation, the more unequal it has become.

In the second (and main) part of the analysis, for a more rigorous examination of the soy-inequality nexus I construct a yearly panel (2000–2020), again at the municipal level in Brazil, in which the main dependent variable is a grid-based measure of economic inequality (Galimberti et al., 2023; Weidmann and Theunissen, 2021).⁴ This is a considerable improvement with regard to the literature concerned with inequality in Brazilian municipalities, which at best relies on census-based data, available only once every decade.

Also in this case I find that soy production, fueled by Chinese demand, has substantially increased inequality in Brazilian municipalities—although with notable regional differences. These results are robust to series of tests. In particular, the effect is indeed driven primarily by soybeans, as taking other important crops into consideration barely changes the main conclusions. Furthermore, I show evidence that the instrumental variable approach does not seem to be driven by spurious

³Soybeans, together with other oilseeds as well as fats and oils, account for roughly half of China’s agricultural import value (Gale et al., 2015a).

⁴Because the measure of spatial inequality relies on gridded population data, it can only be constructed from 2000 onward.

correlation (Christian and Barrett, 2023). I also examine whether the reported rise in inequality is in fact driven by the (other) China shock. This does seem to be the case, as replicating the analysis for demand from the European Union (EU)—the second most important market for Brazilian soybeans—does not yield comparable results.

In line with the Sokoloff-Engerman argument, the results indicate that soy on its own is not to blame, but rather its modern—i.e., highly mechanized, large-scale—cultivation. Importantly, I provide evidence that the surge in inequality is linked to a broader process of land consolidation, as the median size of rural properties has increased as soy expands, while the growth in the total number of these properties has slowed down post China’s WTO accession. Furthermore, based on a quantile analysis of household-level microdata, I show that it does not seem to be the case that soy has been lifting all boats, but only unevenly. Rather, it tends to benefit only those at the top deciles of the income distribution. For most of the population, soy expansion is even detrimental to household income—and the reduction in income increases the poorer one is.

Higher economic inequality has not been the only negative aspect of this process. In the Brazilian Midwest, in particular, it has also been accompanied by rising deforestation and rural conflict. In these cases, too, inequality plays an important role, as the more (initially) unequal the place, the more harmful the soy boom is in terms of vegetation loss and conflict.

Taken together, these findings add an important dimension to the broader empirical literature on agricultural modernization and commodity booms. Existing evidence indicates that, on the one hand, the soy boom has ushered a structural transformation of the Brazilian economy, with productivity gains not only in agriculture, but also positive spillovers to the manufacturing sector and increases in capital accumulation (Bustos et al., 2016; Bustos et al., 2020).⁵ Besides, it has also contributed to improvements in local development and macroeconomic conditions via, e.g., tax and export revenues (Garrett and Rausch, 2016).

In terms of the socioeconomic impact of the soy boom in Brazil and who stands to gain from these transformations, the evidence so far is much less clear. Many descriptive studies have documented potential benefits, ranging from income gains (Weinhold et al., 2013; Garrett and Rausch, 2016; Martinelli et al., 2017) to higher well-being indicators and poverty reduction (VanWey et al., 2013; Weinhold et al.,

⁵Note that the structural transformation that Bustos et al. (2016) and Bustos et al. (2020) refer to was driven in particular by the introduction of genetically engineered soybean seeds in Brazil in 2003—and is not necessarily tied to the China shock.

2013; Piras et al., 2021).⁶ However, consistent with the results presented here, there is also evidence that soybean production is correlated with higher economic inequality (Weinhold et al., 2013; Martinelli et al., 2017). Nevertheless—as I report below—correlations significantly underestimate the size of the effect. Therefore, an important contribution of this study is to devote special attention to the issue of endogeneity, in particular by proposing a new identification strategy.

What is more, Falcone and Rosenberg (2022) show that the introduction of genetically engineered (GE) soybean seeds in Brazil benefited large-scale farmers to a much greater extent than smallholders and led to a reduction in rural employment between the agricultural census waves of 1996 and 2006. Yet, a more rigorous assessment of who benefits and who loses from the expansion of soy has been lacking. More broadly, research has paid little attention to the consequences for local communities and the channels governing this relationship.⁷

In addition to examining the soy-inequality nexus, this work contributes to the literature on the global consequences of China’s rapid economic ascent. Although there is a rich body of work on the supply-side of this shock, which has hit mostly rich countries through import competition (Autor et al., 2013; Autor et al., 2016; Autor et al., 2020), far less is known about the consequences of China’s surging demand, which has been particularly strong for agricultural commodities. Notable exceptions are Costa et al. (2016) and Hansen and Wingender (2023). The latter show that this demand shock, especially after China’s WTO accession, led to significant expansion in cropland and deforestation in exporting countries. Not only is the explicit inclusion of China in the analytical framework conceptually important—in particular given the telecoupled nature of the soy trade (Pendrill et al., 2019; Meyfroidt et al., 2020)—but, as I will argue below, it also allows for a considerable improvement in the empirical identification.⁸

I proceed as follows: in Section 1.2, I discuss the theoretical framework upon which this chapter is based. Section 1.3 sheds light on the long-run dynamics of land inequality in Brazil against the backdrop of China’s WTO accession. In the main part of the analysis, Section 1.4, I examine the consequences of soy expansion

⁶Importantly, a major issue associated with large-scale soybean plantation is its environmental impact. See, for instance, Fearnside (2001), Nepstad et al. (2006), Barona et al. (2010), Hargrave and Kis-Katos (2013), Dreoni et al. (2021), Song et al. (2021), and Carreira et al. (2024).

⁷In particular, there is a growing concern that this agricultural boon may be contributing to further (premature) deindustrialization (and “re-primarization”) in Brazil (Escher et al., 2020).

⁸To date, most of the literature concerned with identifying causal effects (on different outcome variables) of the soybean expansion in Brazil relies on the differential gains in yields, across space, in the aftermath of the adoption of GE seeds in Brazil (e.g., Bustos et al., 2016; Bustos et al., 2020; Falcone and Rosenberg, 2022). A caveat of this strategy is that it only allows for an analysis in first differences—before and after the adoption.

for spatial inequality. I present robustness tests in Section 1.5 and assess potential mechanisms in Section 1.6. Section 1.7 offers some concluding remarks.

1.2 Framework

1.2.1 Brazil and the “Other” China Shock

Since the 1980s, China has embarked on a process of opening up its economy, which included its application for admission to the General Agreement on Tariffs and Trade (GATT) in 1986 and, after a lengthy procedure, culminated with its accession to the World Trade Organization (WTO) in December 2001. In a decade, from 1995 to 2005, trade (exports plus imports) as a share of China’s GDP jumped from less than 39% to over 63% (Brandt and Rawski, 2008).

Internationally, not all consequences of this process have been positive. In the United States, for instance, there is evidence that the rise in exposure to Chinese manufactured goods has increased unemployment and reduced wages in affected industries (Autor et al., 2013; Autor et al., 2016). What is more, the reverberations of the China shock are not limited to the economic realm. Trade exposure to China—and particularly to Chinese exports—has increased ideological polarization in US electoral districts (Autor et al., 2020) and fueled nationalism across Western Europe (Colantone and Stanig, 2018).

The liberalizing reforms in China have also led to a surge in the country’s imports of agricultural goods, although these demand-side effects have received far less attention in the literature. China went from a net exporter of agricultural goods prior to WTO accession to a net importer afterward and is now the destination of more than 10% of global agricultural exports (Hansen and Wingender, 2023). With soybean tariffs slashed to 3% in 2001 (Gale et al., 2015b), Chinese imports of this crop jumped from 6.7 Mt in 1999 to 23.2 Mt in 2003—an increase of more than 246% (FAO, 2023).

These changes in the composition of Chinese imports also reflect a shift in the country’s dietary habits—something that will be important for the identification strategy later on. In particular, meat consumption has increased substantially as Chinese citizens become more affluent. The average meat consumption per year in urban parts of China went from roughly 25 kg per person in 1990 to more than 40 kg in 2021. In rural parts of the country, this figure more than tripled over the sample period, reaching more than 30 kg per person a year in 2021 (Ye and Leeming, 2023). This growing appetite for meat—and for pork in particular, which has long been a staple of Chinese cuisine (Grimmelt et al., 2023)—has been one of the main reasons

for the rise in demand for soybeans (Da Silva et al., 2017), which are used primarily for animal feed.

Whereas the increase in exposure to Chinese exports has hit the manufacturing sector in high-income countries particularly hard, the growing Chinese demand for agricultural and mineral commodities has been felt primarily in low and middle-income countries, where the production of these goods usually accounts for a larger share of the economy. Brazil is arguably one of the countries that have been most affected by—and to some extent benefited from—this additional demand. The country rapidly became China’s largest supplier of many of these commodities, including soybeans, which have become Brazil’s main export crop (Waroux et al., 2019). The share of Brazilian soybean production exported to China rose from 12% in 2004 to more than 48% in 2020 (Trase, 2022).

This boost from international trade has accelerated the process of agricultural modernization in Brazil (Farrokhi and Pellegrina, 2023), which has its roots in the 1960s, and firmly established the country as an agricultural powerhouse and a global leader in agribusiness—Brazil has become the world’s largest net exporter of food (Klein and Luna, 2019).

This transformation has been accompanied by several advantages for Brazil. At the macro level, this boom in external demand has created a new stream of tax and export revenues (Garrett and Rausch, 2016) and, despite being an activity of low value added, generated positive spillovers to the rest of the economy (Bustos et al., 2016). There is also descriptive evidence that it is correlated with higher HDI values across Brazilian municipalities (Martinelli et al., 2017; Piras et al., 2021), and contributed to reducing poverty and raising median rural incomes in the Amazon region (Weinhold et al., 2013).

1.2.2 Factor Endowments and Inequality

Who has benefited and who has lost from the rapid expansion of soybean production is a more complex question, however. That is, whether these gains are reflected throughout the distribution and to what extent this applies to the country as whole. An important caveat of agricultural modernization is that frequently not all landowners are in a position to benefit from this process, as it may require a certain operational scale and capacity to access (subsidized) credit lines, and invest in machinery and agricultural inputs. Those who do not fulfill these conditions are often squeezed out of the market (Pompeia, 2021).

There is indeed some evidence that this may be the case. Weinhold et al. (2013) document a positive correlation between soy production and rural inequality in mu-

municipalities in the Brazilian Amazon, a finding that is corroborated by Martinelli et al. (2017), who observe that Brazilian municipalities with higher soy production tend to also have higher Gini coefficients of income inequality. These studies, however, cover only part of Brazilian municipalities and/or a limited time period, and look solely at Gini indices of income inequality at 10-year intervals. Moreover, they do not aim to identify causal relationships nor underlying mechanisms and do not directly take the role of China into account.

The distributional consequences of the recent transformation of Brazilian agriculture are arguably a central feature of this process for at least two reasons. First, because of the importance of international demand in this case, the trade-induced effects on inequality cannot be neglected (Goldberg and Pavcnik, 2007; Antràs et al., 2017). Second, as put forward by Sokoloff and Engerman (2000), factor endowments play a decisive role in shaping (structural) inequality. This implies, in particular, that the properties of the crops that a certain place produces have a significant influence on who is set to benefit the most from their cultivation. Sokoloff and Engerman's stylized historical comparison is between land endowments that favored commodities featuring economies of scale in Latin America versus an environment more suitable for crops such as wheat and corn in North America. The former is associated with plantation agriculture and land concentration, whereas the latter has been arguably more conducive to more equitable production in family farms and the like.

Adapting Sokoloff and Engerman's (2000) framework to modern Brazil, my argument is that certain key transformations in technological, political, and international conditions—and the rise of China in particular—that took place since the late 20th century have fundamentally altered the use of factor endowments in Brazil and therefore may have had important consequences for socioeconomic inequality in the country. The clearest manifestation of these changes is soy. In the span of a few decades, it went from a niche crop, grown mostly in family farms in the temperate Pampas of Southern Brazil, to spearheading the country's agricultural transformation. It is now mainly cultivated in much larger rural properties in the acidic soils of the Cerrado in Midwest Brazil, where it has reached global productivity records.

What initially enabled this metamorphosis was the significant effort in agricultural research, especially in plant genetics, seed varieties, and soil correction, led by the Brazilian Enterprise for Agricultural Research (Embrapa), established in 1973. This created the conditions for the adoption of large-scale agriculture in a vast part of the Brazilian territory that was once considered unsuitable for modern

agriculture.⁹ As I began to argue above, however, what ultimately propelled soy production in Brazil to the heights seen today has been the seemingly unwavering Chinese demand.¹⁰

In this case, therefore, the typical effect of international trade on distributional outcomes is potentially magnified by its influence on the relative attractiveness of different land uses, which have tilted in favor of a particularly wealth-concentrating crop: soy cultivation is often a highly capital-intensive (and yet low value-added) activity that occupies vast swaths of land, such that its benefits may be largely restricted to a small group of large-scale producers. According to Weinhold et al. (2013, p. 141), “soy is a relatively expensive crop to grow, needing a high level of investment in fertilizers, pesticides and machinery—it is generally held that it is not economically viable to grow soybeans on plots of less than 500 hectares.”

Given the characteristics of modern soybean cultivation in Brazil, this chapter’s main objective is to test the hypothesis that its expansion has led to an increase in economic inequality in the country and examine which segments of society benefited the most from it. More broadly, however—although it is crucial to distinguish between the soy boom having simply lifted some boats more than others versus actually making considerable parts of the population worse off—there is a case to be made that, especially in a country such as Brazil, more inequality is *intrinsically* detrimental (Satz and White, 2024). As one of the world’s most unequal countries, the richest 1% of the population in Brazil account for almost 20% of the country’s income and 49% of its wealth (World Inequality Lab, 2024). In terms of land inequality, less than 1% of rural properties take up almost half of the country’s territory (Oxfam, 2016) and a significant share of these *latifundia* remain uncultivated (Deininger and Byerlee, 2012).

In fact, Brazil’s current land configuration still owes much to its colonial heritage of hereditary captaincies (Naritomi et al., 2012; Holanda, 2015). Despite the pressure of powerful peasant movements, political efforts toward a democratizing agrarian reform have been mostly watered down in the wake of the 1964 military coup. What followed was a process of “conservative modernization” (Klein and Luna, 2019), that is “rural modernization ‘within law and order’, [...] ‘destroying’

⁹The agronomist Norman Borlaug once stated, in reference to the Cerrado, that “nobody thought these soils were ever going to be productive, but Embrapa was able to put all the pieces together” (Rohter, 2007). In fact, until the late 1960s, before Embrapa was created, “more than half of Brazil’s territory remained untouched by agriculture” (Hopewell, 2017, p. 10).

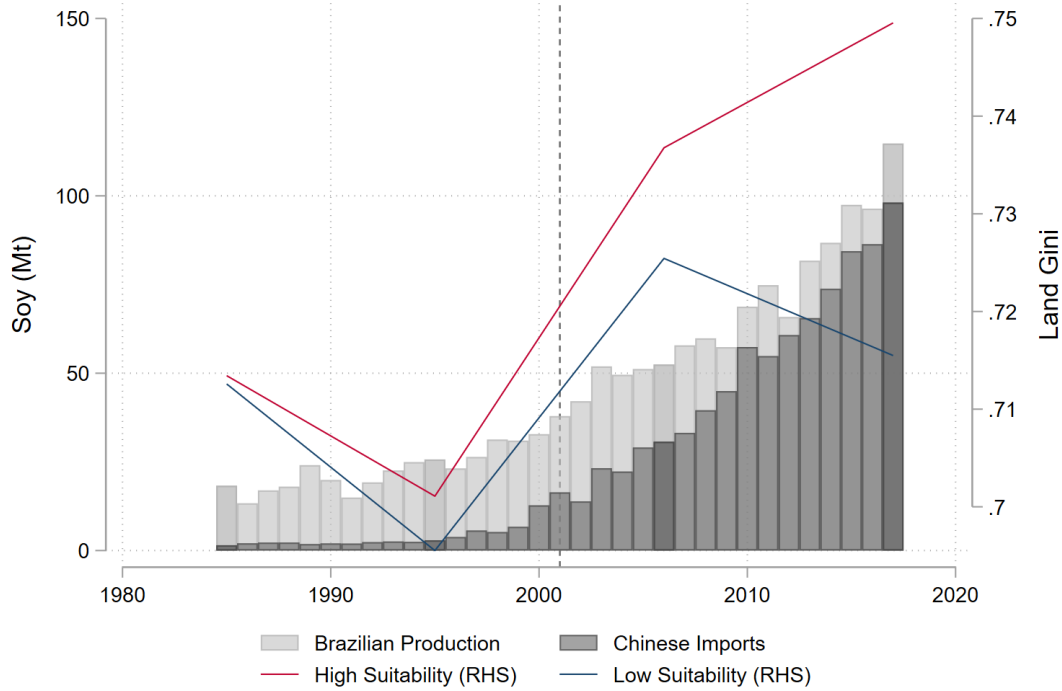
¹⁰An important additional ingredient that created the conditions for Brazil’s agricultural transformation were the country’s own liberalizing reforms adopted during the 1990s. Domestic policies such as the Kandir Law, passed in 1996 and which exempted exports from the interstate movement tax, significantly contributed to soy’s expansion.

peasant organizations, [...] clearly benefiting the large landowners,” who provided critical support for the military regime (Reydon et al., 2015, p. 511). Furthermore, in spite of a significant “march to the West” that picked up steam since the mid-twentieth century (Pellegrina and Sotelo, Forthcoming), access to land is still restricted—and perhaps increasingly so—to a small agrarian elite, and land regulation remains sketchy (Reydon et al., 2015).¹¹

Increasing (land) inequality tends to translate into increasingly concentrated political power. The rural caucus, which represents the interests of large landowners in the Brazilian Congress, went from 73 seats in 2003 (or 14% of the total) to 324 (63%) in 2024, making it a critical force for the realization of presidents’ legislative agendas, regardless of their political orientation (Helfand, 1999; Bragança and Dahis, 2022; Milmanda, 2022). This grouping has been crucial to defend the interests of their members and direct government investment toward agribusiness, ultimately consolidating the sector’s hold on the economy and national politics (Sauer and Mészáros, 2017). More precisely, as has been the case through most of Brazilian history (Reydon et al., 2015), these entrenched agrarian interests often block the enactment of policies that, e.g., promote human capital formation or would make the country less dependent on exporting primary goods. In short, high inequality—and land inequality in particular—may prevent the emergence of more inclusive, human-capital promoting institutions that are crucial for long-run economic development (Sokoloff and Engerman, 2000; Acemoglu and Robinson, 2008; Ziblatt, 2008; Galor et al., 2009).

Figure 1.1 provides a first visual indication that the economic rise of China—especially after the country’s accession to the WTO in 2001—and the ensuing surge in demand for soybeans may indeed be related to rising land inequality in Brazil. By splitting municipalities by the median value of soil suitability for soy, it becomes clear that the gap in the Gini coefficient between these two groups increases in the period, with above-median places becoming significantly more unequal post 2001 ($p = 0.000$). Moreover, as the bars indicate, soybean production in Brazil and imports from China have followed a trend not too dissimilar to that of inequality in the more suitable half of municipalities over the same period. Therefore, I now turn

¹¹As Reydon et al. (2015, p. 510) explain, “[t]he legal and institutional mechanisms developed in the 21st century to deal with the critical agrarian situation in Brazil have been based on the historical pattern of occupation and development in Brazil, and seem to be insufficient to improve this situation. Formal regulations, which have never been completely enforced, make land access in Brazil both fragile and inchoate. [...] If legislation has never been able to regulate land ownership, it has much less been able to regulate land use. Brazil lacks a register of both privately owned and public unclaimed land (as defined in the 1850 Land Law) and the existing social regulation is inadequate. Land reform in Brazil has not been able to eradicate landlessness or poverty.”

Figure 1.1 – Land Inequality and Soil Suitability for Soybeans

Notes: Figure shows the evolution of the average Gini coefficient of land inequality in Brazilian municipalities through four waves of the agricultural census (IBGE, 1985; 1996; 2006; 2017). Municipalities are split into two groups: below (Low Suitability) and above-median (High Suitability) soil suitability for soybean cultivation (data from FAO, 2022). Brazilian yearly soybean production (data from IBGE, 2023a) and Chinese total imports of soybeans (data from FAO, 2023) are in megatonnes (Mt). The vertical dashed line marks the year of China's accession to the WTO, 2001.

to a more rigorous examination of inequality in Brazil following China's economic rise.

1.3 WTO Accession: Land Inequality

1.3.1 Main Data and Empirical Strategy

To assess how the surge in demand from China influenced land inequality in Brazil, the first part of the empirical analysis leverages longer-run data on municipal soybean production in Brazil.¹² I rely on municipal agricultural surveys (PAM), conducted yearly by the Brazilian statistical office (IBGE, 2023a), to compile soy production in each Brazilian municipality from 1974 to 2020.¹³ Appendix Figure A.1

¹²Because the number (and thus the borders) of Brazilian municipalities changed substantially during this period—from 3,959 municipalities in 1970 to 5,570 since 2013—I make use of “minimum comparable areas” (MCAs) to ensure that the units of analysis are consistent over time (Ehrl, 2017). Throughout this section, what I refer to as municipalities are 1970 MCAs.

¹³Roughly half of Brazil's municipalities produced a positive amount of soybeans at least during some point in the sample period.

illustrates the expansion of soybean production across the Brazilian territory between 1985 and 2020.

Because the main dependent variable of interest, the Gini coefficient of land inequality, is only available in the agricultural census (IBGE, 1985; 1996; 2006; 2017), which is conducted roughly every ten years (in four waves from 1985 to 2017), I use the average (or total) soy production in each municipality over the period of ten years that precedes the census year. Additional data sources are introduced upon use. Descriptive statistics, including all variables used in this chapter, are shown in Appendix Table A.1.

I begin by examining the direct relationship between the Gini coefficient of land inequality in municipality m and period p ($landgini_{mp}$) and China's accession to the WTO:

$$landgini_{mp} = \beta_1 WTO_p + \omega_m + u_{mp} \quad (1.1)$$

where WTO_p is a binary variable that takes the value 1 if period p is after 2001.¹⁴ All specifications include municipality fixed effects (ω_m) and some also period fixed effects (ξ_p).¹⁵ Standard errors are clustered at the municipality level.

Given that China's accession to the WTO may have affected inequality in Brazil through several different channels and that this dummy variable is likely capturing also other events that took place during the same period, there is no particular expectation with regard to β_1 . To get closer to the connection to the soy boom, the next specification allows for municipalities to be differentially impacted by China's WTO accession based on their degree of involvement with the soy business, as they include an interaction term between WTO_p and a measure of such involvement, $\bar{\Theta}_{mp}$. More precisely, I estimate the following equation:

$$landgini_{mp} = \beta_1 WTO_p + \beta_2 \bar{\Theta}_{mp-1} + \beta_3 WTO_p \times \bar{\Theta}_{mp-1} + \omega_m + \xi_p + u_{mp} \quad (1.2)$$

Here, $\bar{\Theta}_{mp-1}$ stands for either the (log) production of soybeans in municipality m averaged over the ten years preceding the census year ($\overline{soy}_{mp-1}$), or municipality's m soil suitability for soy ($soil_m$), which is a function of weather and soil characteristics and is widely used in the literature.¹⁶ While the former has the advantage of varying

¹⁴In the baseline models, due to the paucity of data available for such a long period at the municipal level and the fact that many of the most relevant variables—such as GDP per capita, population etc.—would most likely be “bad controls” (Angrist and Pischke, 2009), I refrain from including a vector of control variables.

¹⁵Because period fixed effects absorb the WTO dummy, they are not always included.

¹⁶I use suitability values that consider soy cultivation under high levels of input. See FAO

over time and capturing each municipality’s exact involvement with the soy business, the latter has the advantage of being less susceptible to endogeneity concerns. In either case, β_3 estimates the difference-in-differences between more or less exposed municipalities in relation to the WTO accession. Importantly, the point is not to think of the year of 2001 as a watershed moment in which China went from complete autarky to being totally integrated into world markets. As mentioned earlier, this was a gradual process that did not happen overnight. For example, in the particular case of soy, another important event took place already in 1995, when China exempted the crop from import quotas (Gale et al., 2015b). Hence, the year of the accession to the WTO should not be seen as a sudden shock, but rather as the pinnacle of a bumpy and lengthy process.

Therefore, although informative, the model above is unlikely to yield causal parameters. More broadly, omitted variables and the possibility of reverse causality would compromise the interpretation of the parameters of interest. For instance, it may be the case that soy expands faster in places that are, on average, initially more equal, even if poorer, because there is not so much economic activity going on to begin with. This would be consistent with the evidence that indicates that municipalities, at least on or close to the deforestation frontier, tend to go through different stages of land use before large-scale soy plantation really kicks in (Song et al., 2021). These typically include logging followed by cattle grazing. Moreover, measurement error is another issue that could lead to underestimating the effect of soy expansion on inequality (attenuation bias).

To overcome these issues, I propose a new instrumental variable that leverages arguably exogenous variation in an important driver of Chinese demand for soybeans: the size of the pig stock in China. More than 80% of the country’s soybean imports are used for animal feed—and particularly to feed pigs (Anand, 2021). This makes hog feeding the main reason for China, which is home to about half of the world’s pig population (Reuters, 2019a), to import soybeans.¹⁷ Crucially, an important part of the variation in the size of the Chinese pig stock is explained by sporadic episodes of disease outbreak, which are orthogonal to economic conditions in Brazil. In 2019, for instance, China lost around half of its herd to African swine fever (Reuters, 2019b). In this first part of the analysis, given that land inequality data are only available (roughly) every ten years, I use the variation in the Chinese pig stock between census waves. That is, $\Delta pigstock_p = pigstock_p - pigstock_{p-1}$.

(2022) for more details. Note that, because this measure does not vary over time, the uninteracted term is absorbed by the municipality fixed effects.

¹⁷As a senior animal protein analyst at Rabobank puts it, “[t]he expectation of rapid hog restocking has been the key reason for strong imports” (Lee, 2021).

Appendix Figure A.2 illustrates the evolution of China’s pig stock over time. Data on the size of the Chinese pig stock come from FAO (2023).

To map how temporal variation in Chinese pig stock affects soybean production at the municipal level in Brazil, I employ FAO’s index of agricultural suitability for soy cultivation ($soil_m$).¹⁸ More precisely, I define $Z_{mp} = \Delta pigstock_p \times soil_m$, which I use as an instrument for (log) Brazilian municipal soybean production averaged over the preceding period, $\overline{soy}_{mp-1}$.¹⁹ Thus, the first-stage regression in a 2SLS setup is as follows:

$$\overline{soy}_{mp} = \lambda(\Delta pigstock_p \times soil_m) + \omega_m + \xi_p + \nu_{mt} \quad (1.3)$$

Alternatively, to allow for differences before and after China’s accession to the WTO, the second-stage regression may include not only $\widehat{\overline{soy}}_{mp}$, but also its interaction with WTO_p . In this case, $WTO_p \times \Delta pigstock_p \times soil_m$ enters the model as an additional exogenous variable.

Therefore, the identification strategy is again akin to a difference-in-differences comparison. In response to fluctuations in the Chinese pig stock, the first-stage regression compares within variation in Brazilian municipal soy production in places more or less suitable for cultivating this crop. Hence, the exclusion restriction is that, conditional on the control variables and fixed effects, the interaction between fluctuations in the total number of pigs in China and municipal soil suitability for soy affects inequality in Brazilian municipalities only through soy production.

Potential threats to the identification, such as the role of other crops and neighboring municipalities as well as spurious correlation (Christian and Barrett, 2023), are discussed in detail in Section 1.5. Importantly, I show evidence that the size of the pig stock in China affects soybean production in Brazil, and not the other way around.

1.3.2 Results

Table 1.1 reports the OLS results for the link between China’s WTO accession and the Gini coefficient of land inequality in Brazilian municipalities. Column 1 shows a positive and significant association between the two variables. Yet, this does not say much in terms of the role of soy. Including municipal soy production, in

¹⁸By its use of this kind of interacted instrument, the empirical analysis is similar to, e.g., Nunn and Qian (2014), Bustos et al. (2016), Cisneros et al. (2021), and Dreher et al. (2021a). More broadly, this one-dimensional shift-share approach is related to the use of exogenous shock variables for identification (Borusyak et al., 2022).

¹⁹Using total soy production in the period preceding the census year, instead of the average, is of little practical consequence.

column 2, shows a positive correlation with land inequality. Interestingly, once the relationship between the two variables is allowed to vary with respect to China's WTO accession, the results indicate a negative association before 2001, which turns positive afterward. The effect is nevertheless rather small in magnitude. These findings remain largely unchanged with the introduction of census wave fixed effects in column 4.²⁰

Columns 5 and 6 use soil suitability for soy (instead of actual production) as the measure of exposure to the China shock. Again, the results indicate that more exposed municipalities have suffered more in terms of increases in land inequality. In this case, a standard deviation gain in soil suitability for soybeans implies an increase in the land inequality Gini of approximately 0.01 unit—or roughly a tenth of a standard deviation. Here too the inclusion of period fixed effects is of little consequence.

Table 1.1 – Land Inequality (OLS)

	<i>Dependent Variable: Land Gini</i>							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
WTO	0.030*** (0.001)	0.029*** (0.001)	0.021*** (0.001)		-0.004 (0.003)			
Soy (log)		0.001*** (0.000)	-0.002*** (0.000)	-0.002*** (0.000)				
WTO × Soy (log)			0.004*** (0.000)	0.004*** (0.000)				
WTO × Soil					0.007*** (0.001)	0.007*** (0.001)		
Imports (log)							-0.005*** (0.001)	
Imports (log) × Soil							0.003*** (0.000)	0.003*** (0.000)
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Period FE	No	No	No	Yes	No	Yes	No	Yes
N	15,149	15,137	15,137	15,137	15,149	15,149	15,149	15,149

Notes: *WTO* is a binary variable that takes the value 1 if period *p* is after 2001. *Soy (log)* is the natural logarithm of municipal soybean production. *Soil* is the average soil suitability for soybeans in the municipality. *Imports (log)* stands for the natural logarithm of Chinese annual soybean imports. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors, clustered at the level of 1970 MCAs, in parentheses.

These results should be interpreted with caution. As discussed above, China's accession to the WTO is hardly unanticipated and may be correlated with other concomitant shocks that affect both soy production and inequality in Brazil. Therefore, it is not surprising that these findings are not robust to a rigorous test of parallel trends. That is, using a placebo dummy variable that takes the value 1 already in the previous census wave (1995) produces slightly weaker, but overall similar results (not reported). Somewhat reassuringly, however, a simple test of difference in mean inequality between below and above-median municipalities in terms of soil

²⁰Note that the wave fixed effects absorb the uninteracted WTO dummy.

suitability does not reject the null hypothesis in the first two periods of the sample, 1985 and 1995. It is only in the first census wave after China's accession to the WTO, in 2006, that these two groups become significantly different in terms of land inequality ($p = 0.000$). This difference persists in the fourth and final wave of the sample (2017).

A way to investigate the long-run influence of Chinese demand on inequality in Brazil in a more flexible manner, that is, without having to rely on a clear cut-off point, is to use temporal variation in China's total soybean imports.²¹ Once again, this provides additional descriptive evidence that the detrimental effect to inequality levels in Brazil is mediated by the soy boom. The more suitable for soy a municipality's soil, the more unequal it is expected to become as Chinese demand increases (columns 7 and 8).

Next, I turn to the results from the 2SLS approach (Table 1.2), in which soybean production is instrumented as described in equation 1.3. Columns 1 and 2 split the sample into periods before and after China's WTO accession, respectively. Consistent with the argument presented above, the results show that soybean cultivation leads to higher land inequality only in the post-WTO period. Accordingly, only for this second period is the instrument relevant (Stock and Yogo, 2005). That is, before 2001, the variation in the size of the Chinese pig stock does not seem to explain soybean production in Brazil, which is in line with the argument that the China shock has had a considerable influence on factor endowments in Brazil. The results imply that, post WTO, a (within) standard-deviation increase in municipal soy production (which represents a 274% increase at the mean) is expected to raise land inequality by approximately 1.6 (within) standard deviations—much larger than the previous OLS results suggest. I come to similar conclusions if, instead of splitting the sample, I allow the effect of soybean production on land inequality to vary according to China's WTO membership status (column 3). In this case, as explained previously, both municipal soy production as well as its interaction with the WTO dummy are instrumented.

To understand to what extent the increase in inequality is indeed due to soy—and not to other crops or perhaps agriculture in general—I examine the role of corn.²² There are at least three reasons to consider that corn may be a confounding factor: (i) it is the second (after soy) most important crop in Brazil in terms of production

²¹Similarly to the way in which Brazilian soybean production has been defined, I use the (log) total Chinese imports of soybeans averaged over the ten years preceding the census year. Using the sum instead of the average makes little difference (not reported). Data on Chinese soybean imports come from FAO (2023).

²²Data on corn production also come from IBGE (2023a).

Table 1.2 – Land Inequality (2SLS)

	<i>Dependent Variable: Land Gini</i>						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Soy (log)	0.012 (0.018)	0.122*** (0.022)	0.006** (0.003)	0.001 (0.003)		0.201 (0.665)	0.264** (0.134)
WTO × Soy (log)			0.006*** (0.001)	0.010*** (0.001)		-0.272 (0.916)	-0.040* (0.023)
WTO			-0.001 (0.002)	0.047*** (0.008)	-0.277*** (0.052)	-5.654 (18.632)	0.182** (0.083)
Corn (log)				-0.000 (0.001)	-0.131*** (0.034)	-1.548 (4.992)	
WTO × Corn (log)				-0.008*** (0.001)	0.046*** (0.008)	0.933 (3.071)	
Imports (log)							-0.075** (0.036)
Period	Pre WTO	Post WTO	Full	Full	Full	Full	Full
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Period FE	Yes	Yes	No	No	No	No	No
N	7,556	7,556	15,133	15,133	15,133	15,133	15,133
KP F-stat	6.63	35.66	145.72	153.86	10.14	0.05	2.09

Notes: *WTO* is a binary variable that takes the value 1 if period *p* is after 2001. *Soy (log)* is the natural logarithm of municipal soybean production. *Corn (log)* is the natural logarithm of municipal corn production. *Imports (log)* stands for the natural logarithm of Chinese annual soybean imports. First-stage Kleibergen-Paap F-statistic reported. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors, clustered at the level of 1970 MCAs, in parentheses.

value (IBGE, 2023a), (ii) it is often planted in rotation with soybeans, and (iii) it is commonly used for animal feed as well. However, differently from soybeans, China still keeps a policy of self-sufficiency in corn production (Hansen and Wingender, 2023). In any case, controlling for municipal corn production and its interaction with the WTO dummy (column 4) barely affects the main coefficient of interest—in fact, the magnitude of the soy interaction term even slightly increases. It is nevertheless interesting that the corn interaction term, although it should not be interpreted causally, indicates a reduction in inequality post 2001.

If I instrument the corn variables instead (columns 5 and 6), the F statistic drops dramatically, indicating that fluctuations in the size of the pig stock in China are not that relevant to explain variation in corn production in Brazil. Once the soy variables are added as (non-instrumented) control variables (column 6), the F statistic becomes even smaller and all coefficients of interest become indistinguishable from zero.

Column 7 includes total Chinese imports of soybeans as a control variable. Not surprisingly, the instruments turn irrelevant. This shows that, in line with the exclusion restriction, variation in the Chinese pig stocks affect soybean production in Brazil through Chinese soy imports. Once this channel is shut down, the pig stock does not have any additional influence on Brazilian production.

Furthermore, first-stage results, reported in Appendix Table A.2, indicate that,

post WTO, when the Chinese pig stock increases—and demand for soy presumably goes up—soybean production in Brazil is able to expand in areas that are less suitable for cultivation. As discussed above, this changes only if Chinese soybeans imports are included as an additional control variable (columns 5 and 6).

1.4 Post WTO Accession: Spatial Inequality

I now proceed to analyze the distributional effects and the potential underlying mechanisms in a more granular and rigorous empirical setting using annual panel data for all Brazilian municipalities from 2000 to 2020.

1.4.1 Spatial Inequality

As already mentioned, conventional measures of economic inequality at the municipality level in Brazil, upon which most of the related literature relies, are based on data collected in the national census, which is conducted only every ten years or so. In order to assess inequality on a yearly basis, I follow Weidmann and Theunissen (2021) to calculate a measure of spatial inequality for each Brazilian municipality for each year in the sample. This is done as follows: I first calculate (log) average nighttime light emissions per capita in each cell. Similarly to Weidmann and Theunissen (2021), I define light per capita as follows: $lpc_{it} = \frac{\ln(ntl_{it}+1)}{\ln(pop_{it}+1)}$, where, for cell i in year t , ntl_{it} stands for the average nightlight radiance and pop_{it} refers to population.²³ This relies on high-resolution data on nightlights, which come from DMSP and VIIRS (harmonized as in Li et al., 2020), and gridded population estimates, taken from WorldPop (2023).²⁴ Both light as well as population data are available at a spatial resolution of 30 arcsec (approximately 1 km at the equator).²⁵

From the distribution of light per capita in a given area, I calculate a yearly Gini index of spatial inequality at the municipal level (*spatialgini*), which yields a measure that is positively and significantly correlated with census-based Gini indices of income and land inequality in Brazil in the years in which these are

²³Note that, for cells with no inhabitants, the denominator would be zero. In these cases, light per capita is set to missing. This also ensures that the measure of spatial inequality is not affected by unpopulated areas.

²⁴Average (log) nightlight radiance by municipality-year is also used on its own, as a measure of economic activity, as luminosity per se has been shown to be a good proxy for economic activity and regional development even at small unit of analysis, such as municipalities, including in rural areas in low and middle-income countries (Pérez-Sindín et al., 2021). Gridded population data are only available from 2000, which sets the lower bound for the period of analysis in this part.

²⁵Given that the size of the median (average) rural property in Brazil in 2017 was 0.43 km² (0.95 km²), it seems reasonable to keep this resolution.

available.²⁶ Moreover, Weidmann and Theunissen (2021, p. 9) validate this measure on a sample of African countries and conclude that it is a “strong predictor of actual local [economic] inequality” for both urban as well as rural locations.

Importantly, this measure has the advantage of going beyond mere income inequality and also of capturing the spread of economic activity (and its distribution) within municipalities. This is particularly valuable given soy’s pattern of expansion throughout the Brazilian territory, which tends to create pockets of high light emission, but little population—given the low demand for workers. This is exemplified by the construction of soy-specific infrastructure, such as drying, storage, and crushing facilities, which generates areas of high light per capita that can be captured from space. Finally, an additional benefit of measuring inequality using remote sensing data is that it does not suffer from reporting biases, which often distort more common measures of inequality that rely on survey data (Lakner et al., 2016; Sánchez-Ancochea, 2021).²⁷

Figure 1.2 illustrates the construction of the Gini index of spatial inequality for Sorriso, one of the top soy-producing municipalities in Brazil. Combining grid-cell measures of (log) nighttime light and (log) population as explained above, it arrives at estimates of light per capita for every 1 km² pixel. Upon aggregation at the municipal level this yields a Gini of spatial inequality of 0.796 for Sorriso in 2020.

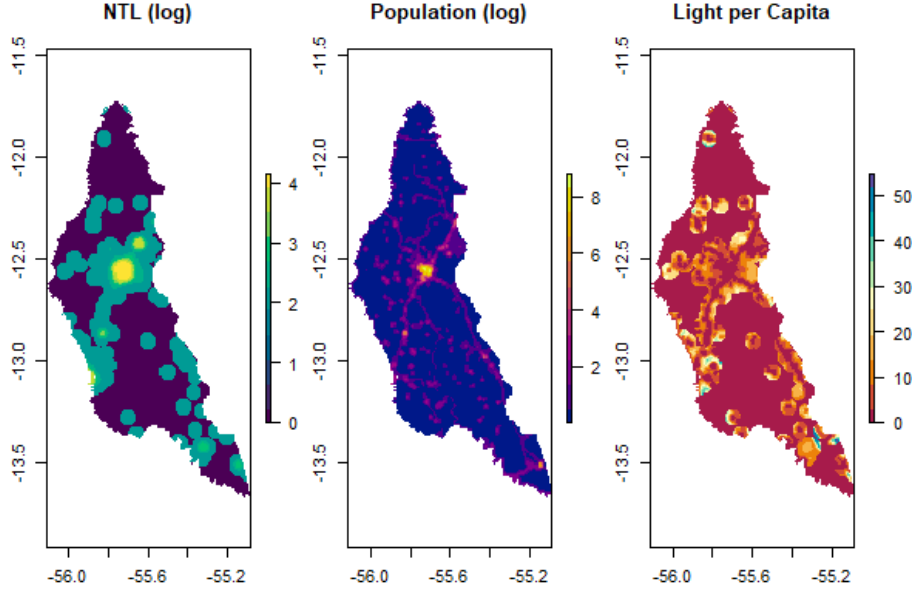
To get a sense of what this measure is capturing, one can use satellite images to further inspect the areas of high light per capita revealed by the figure. For instance, zooming into the high light per capita pocket in the southern tip of Sorriso shows that this area is occupied by a collection of seemingly large farms. One such farm, for which online information could be found, is Fazenda Angela Tereza, which produces soybeans and corn on an area of roughly 3,000ha.²⁸ One can clearly see the infrastructure for storing, drying and processing grains, surrounded by vast tracts of cropland (Appendix Figure A.4). Roughly speaking, another way of seeing this is to think of light and population as capital and labor. Soy brings a lot of the former, but not as much of the latter, which is consistent with the observed high light per capita pockets.²⁹

²⁶Across all Brazilian municipalities, the correlation of the measure of spatial Gini is 0.24 ($p = 0.000$) with the demographic census-based Gini of income inequality in 2010 and 0.26 ($p = 0.000$) with the agricultural census-based Gini of land inequality in 2017. These are years for which each of these censuses is available.

²⁷See Galimberti et al. (2023) for a more detailed discussion of the advantages of using geospatial data to measure inequality.

²⁸See Gazeta Digital (2023) for more information (in Portuguese).

²⁹Soy expansion may create jobs, but these tend to be low-skill occupations in services and not directly related to the soy business.

Figure 1.2 – Spatial Gini: Sorriso

Notes: Figure shows nighttime light (NTL), population, and light per capita for the municipality of Sorriso (MT) in 2020. Author's calculations based on nighttime light data from VIIRS, harmonized as in Li et al. (2020), and gridded population estimates from WorldPop (2023).

1.4.2 Empirical Strategy

I then proceed to estimate the following baseline OLS model:

$$spatialgini_{mrt} = \beta soy_{mrt-1} + \mathbf{X}_{mrt-1}'\gamma + \omega_{mr} + \phi_{rt} + u_{mrt} \quad (1.4)$$

where $spatialgini_{mrt}$ is the measure of inequality, introduced above, for municipality m in region r and year t . The main explanatory variable of interest, soy_{mrt-1} , denotes the (log) total amount of soybeans produced in municipality m in the previous year, $t - 1$, which allows for some adjustment time.³⁰ The data source for the production of soybeans (and other crops) remains the same, the municipal agricultural surveys (PAM), conducted by the Brazilian statistical office (IBGE, 2023a). This time, however, I can use (log) yearly municipal production directly (instead of using the average over the 10-year period before each census wave, as in Section 1.3). $\mathbf{X}_{mrt-1}$ is a vector of (lagged) control variables, including municipality's GDP (log), net tax revenues (log), value added in agriculture (log), value added in industry (log), value added in services (log)—all taken from IBGE (2023b)—and population (log), made

³⁰Increasing the time lag (up to $t - 3$) does not significantly change the results (not reported).

available by IBGE (2021).³¹ Finally, ω_{mr} and ϕ_{rt} are municipality and region-year fixed effects, respectively. The former absorb all municipal characteristics that are constant over time and the latter account for region-specific time-varying shocks.³² Finally, u_{mrt} is the error term. Once again, the standard errors are clustered at the municipality level.

Similarly to Section 1.3, I again use variation in the size of the Chinese pig stock, interacted with municipal soil suitability for soy, to instrument municipal soy production and circumvent issues of endogeneity. This time, however, I can use *annual* variation in the pig stock, i.e., $\Delta pigstock_{t-1} = pigstock_{t-1} - pigstock_{t-2}$, such that the first-stage equation takes the following form:³³

$$soy_{mrt} = \lambda(\Delta pigstock_{t-1} \times soil_{mr}) + \mathbf{X}_{mrt-1}'\pi + \omega_{mr} + \phi_{rt} + \nu_{mrt} \quad (1.5)$$

1.4.3 Results

Table 1.3 reports the main results. In panel A, the main explanatory variable of interest is the (log) total amount of soy produced in each municipality. Column 1 starts by reporting OLS coefficients. It shows a positive and significant—although rather small in magnitude—relationship between soy production and the spatial Gini index. It nevertheless turns insignificant upon the inclusion of region-year fixed effects (column 2). Once soy production is instrumented according to the strategy described above in equation 1.5 (columns 3 and 4), the coefficient’s magnitude increases substantially, especially once region-year fixed effects are included. This indicates that regional heterogeneity might play an important role, which is something that is addressed in more detail in Section 1.6. The coefficient in column 4 implies that a (within) standard-deviation increase in municipal soy production (about 178% at the mean) is expected to lead to more than a third of a (within) standard deviation hike in spatial inequality in the following year—no small feat given that inequality tends to change slowly over time.³⁴

Furthermore, the comparison to the OLS model suggests a downward bias if soy

³¹In my preferred 2SLS specifications, I do not include these control variables because they are likely “bad controls” (Angrist and Pischke, 2009). In any case, their inclusion does not fundamentally alter any result.

³²Brazil has five geographic regions: North, Northeast, Midwest, Southeast, and South.

³³Note that $soil_{mr}$ is absorbed by municipality fixed effects and $\Delta pigstock_{t-1}$ by region-year fixed effects.

³⁴As discussed previously, most other variables that are relevant for the soy-inequality nexus are arguably “bad controls” and I therefore prefer not to include them in the baseline 2SLS specifications. I do so in the robustness section below, and the coefficient of interest barely changes.

Table 1.3 – Spatial Inequality

<i>Dependent Variable: Spatial Gini</i>				
<i>Panel A – Soy Production</i>				
	(1)	(2)	(3)	(4)
Soy (log)	0.001*** (0.000)	-0.000 (0.000)	0.027*** (0.005)	0.067*** (0.011)
Municipality FE	Yes	Yes	Yes	Yes
Year FE	Yes	-	Yes	-
Region-Year FE	No	Yes	No	Yes
Controls	Yes	Yes	No	No
Model	OLS	OLS	2SLS	2SLS
N	115,994	115,994	116,203	116,203
KP F-stat			335.99	114.53
<i>Panel B – Soy Exports to China</i>				
	(1)	(2)	(3)	(4)
Soy CHN (log)	0.001*** (0.000)	0.000 (0.000)	0.045*** (0.008)	0.079*** (0.015)
Municipality FE	Yes	Yes	Yes	Yes
Year FE	Yes	-	Yes	-
Region-Year FE	No	Yes	No	Yes
Controls	Yes	Yes	No	No
Model	OLS	OLS	2SLS	2SLS
N	88,662	88,662	88,783	88,783
KP F-stat			101.94	56.41

Notes: *Soy (log)* is the natural logarithm of municipal soybean production. *Soy CHN (log)* is the natural logarithm of municipal soybean exports to China. First-stage Kleibergen-Paap F-statistic reported. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors, clustered at the municipality level, in parentheses.

is not instrumented. As discussed previously, this is consistent with the argument that soybeans tend to expand faster in places that are initially less unequal and the fact that the OLS models likely suffer from attenuation bias.³⁵ Importantly, the instrumental variable far exceeds usual relevance thresholds across all specifications, as indicated by the first-stage F statistics.³⁶

Panel B shows results for municipal soybean exports to China instead of total production.³⁷ The pattern is comparable to the one reported in panel A. Given the distribution of each explanatory variable, the magnitude of the effect, in terms of

³⁵Another potential explanation is that, because the 2SLS model estimates a local average treatment effect, the part of soy production that responds to demand shocks in China has a different (more severe) effect on inequality.

³⁶Appendix Table A.3 shows first-stage results.

³⁷Data on municipal soybean exports, which contains the market of destination, come from Trase (2022)—it is however only available since 2004. Restricting the period of analysis of production data (panel A) to the same range does not substantially alter any conclusion (not reported).

standard deviations, is also remarkably similar. This shows that the Chinese demand for soybeans affect inequality dynamics in Brazil not only in an indirect manner, but also directly via soybeans that are actually imported by China. Whether this means that the brunt of the inequality increase is driven by Chinese demand is a question that will receive more attention in Section 1.6.

Importantly, Appendix Table A.4 (panel A) shows that the previous results apply to most, but not all Brazilian regions. While soy production is linked to higher inequality in the Midwest, Northeast, and Southeast of the country, this is not the case in the North nor in the South, where the instrument proves to be quite weak. Although little soybean is planted in the North, the South is, after the Midwest, the second most important soy-producing region in the country.³⁸ However, as already hinted at above, soybean cultivation tends to differ greatly in the Midwest and in South. Soybeans have been cultivated in the more temperate areas of Southern Brazil for over a century, where properties are usually smaller and family farms are responsible for a significant share of total production. In Rio Grande do Sul, they account for over a third of the state's soy production (Vander Venet et al., 2015).³⁹ The history of soy production in the Midwest, which is mostly characterized by the acidic soils of the Cerrado biome, is much more recent. Still virtually insignificant in the 1970s, it only consistently overtook the South in terms of total production in the early 2000s.

Even though other factors—notably technological progress—contributed to the transformation of the Midwest into Brazil's own “soy belt,” Chinese demand is a crucial part of the explanation. Given the different nature of production in this region—usually more mechanized and on larger properties—it is not surprising that inequality has increased as soy harvests grew. The contrasting results for Brazil's two most important growing regions therefore fit remarkably well into Sokoloff and Engerman's (2000) explanation of the importance of factor endowments as a determinant of inequality.

The Northeast region, in turn, although responsible for a smaller share of production, is where most of the “Matopiba” is located.⁴⁰ This area, which has a long history of poverty and deprivation, has become (in)famous as one of the most important frontiers of soybean expansion, where speculative investments in real estate

³⁸Appendix Figure A.5 shows the evolution of soybean production in all five Brazilian regions over time.

³⁹For comparison, in Brazil as a whole, smallholders account for about 15% of total soybean production (Raleira et al., 2022).

⁴⁰Matopiba is a portmanteau of the abbreviations of four Brazilian states: Maranhão (MA), Tocantins (TO), Piauí (PI), and Bahia (BA). With the exception of Tocantins, all of these states are located in the Northeast region of Brazil.

are rampant (Pitta et al., 2017; Lopes et al., 2021). These reasons may help explain the strikingly large coefficient for the Northeast, well above the national average.⁴¹

Furthermore, dividing the sample of Brazilian municipalities in quintiles by GDP per capita shows that soy expansion is detrimental for those in the poorest three quintiles (Table A.4, panel B). For the richest group of municipalities, it may even lead to lower inequality.⁴² This corroborates the interpretation that the issue seems to be not simply the expansion of soy, but rather the way it amplifies pre-existing socioeconomic deprivations and institutional frailties, such as the lack of proper land tenure regulation and enforcement.

1.5 Robustness

Table 1.4 tests the robustness of the main results (from Section 1.4) in several ways. First, columns 1 to 4 repeat the baseline models (without and with region-year fixed effects), but this time include a vector with the following control variables: municipal GDP, tax revenues, agriculture value added, industry value added, service value added, and population size (all in logarithmic scale). Although these might be “bad controls” (Angrist and Pischke, 2009), in the sense that they may be a channel through which soy production affects inequality, the main findings barely change.

Second, as explained above, to construct the measure of spatial inequality I rely on harmonized nightlight data (Li et al., 2020), taken from two different sources: DMSP and VIIRS. Given that the latter provides more accurate data, I show results for an alternative measure of spatial inequality that relies only on VIIRS data for light emission (column 5).⁴³ An important drawback is that these data are only available since 2012. Regardless, this also yields a positive—although smaller in magnitude and less precisely estimated—coefficient.⁴⁴ The instrument remains highly relevant for this shorter period of analysis.

Column 6 shows results for municipal soy production in megatonnes, instead of using the logarithmic form. Also in this case there is a positive and significant effect on spatial inequality. Moreover, using other variables to measure soy cultivation, such as the area planted or harvested instead of the production amount, does not

⁴¹For a detailed account of these differences in soy production and the characteristics of its supply chains across Brazilian regions, see Dos Reis et al. (2024).

⁴²It should be noted, however, that the instrument is rather weak at the extremes of the distribution.

⁴³For a broader comparison between DMSP and VIIRS data, see Gibson (2020).

⁴⁴Restricting the sample period to 2012–2020 for the baseline measure of spatial inequality yields a positive and highly significant coefficient, but also of smaller magnitude. This indicates that the effect of soy on inequality seems to have been stronger in the first decade of the 21st century (not reported).

Table 1.4 – General Robustness

	<i>Dependent Variable: Spatial Gini</i>							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Soy (log)	0.026*** (0.005)	0.063*** (0.011)			0.002* (0.001)		0.087*** (0.015)	0.158*** (0.028)
Soy CHN (log)			0.043*** (0.008)	0.077*** (0.015)				
Soy (mt)						6.528*** (1.361)		
Corn (log)							-0.014*** (0.002)	
Sugarcane (log)							-0.000 (0.001)	
Soy Ngb. (log)								-0.004*** (0.001)
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	-	Yes	-	Yes	-	-	-
Region-Year FE	No	Yes	No	Yes	No	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Model	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS
N	115,994	115,994	88,662	88,662	46,240	115,994	114,208	110,558
KP F-stat	308.01	108.85	98.47	53.88	204.49	50.55	69.55	44.06

Notes: *Soy (log)* is the natural logarithm of municipal soybean production. *Soy CHN (log)* is the natural logarithm of municipal soybean exports to China. *Soy (mt)* is municipal soybean production in megatonnes. *Corn (log)* is the natural logarithm of municipal corn production. *Sugarcane (log)* is the natural logarithm of municipal sugarcane production. *Soy Ngb. (log)* is the natural logarithm of the total soybean production in the neighborhood of a municipality. The neighborhood consists of all neighboring municipalities. First-stage Kleibergen-Paap F-statistic reported. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors, clustered at the municipality level, in parentheses.

affect the findings in any meaningful way (not reported).

Similarly to what has been done for the longer-run analysis above, column 7 examines the role of other crops. Controlling for the municipal production of the most important products in Brazilian agriculture after soybeans, corn and sugarcane, barely affects the main coefficient of interest.

A potential concern for the exclusion restriction is that Chinese demand for soybeans affects inequality in a given Brazilian municipality through soy production in its neighboring areas. The specification in column 8 thus controls for total soy production (in logarithmic scale) in neighboring municipalities.⁴⁵ Reassuringly, the coefficient of interest remains highly significant and even increases in magnitude.

Because soybean production is correlated across space, I also rerun the main baseline specification allowing for spatial correlation within different distance cut-offs (Conley, 1999), ranging from 50 km to 250 km. As Appendix Table A.5 shows, the results are robust to this adjustment.

⁴⁵In the baseline specifications I prefer not to add this control variable because its correlation with municipal soy production is rather high (0.54).

1.5.1 Instrumental Variable

A potential concern with instrumental-variable approaches that, like mine, interact a time series variable with a measure of cross-sectional exposure is spurious correlation (Christian and Barrett, 2023). In particular, this could be caused by a co-movement between the exogenous time-varying component and the endogenous variable. Following Christian and Barrett’s (2023, p. 1098) recommendations, throughout this chapter I use the *variation* in the size of the Chinese pig stock, because “[f]irst differencing to render the instrument [...] stationary appears a reasonably promising way to address the spurious regression problem in panel IV estimation.”⁴⁶ As recommended, I also visually inspect my proposed source of exogenous variation and, reassuringly, there is no clear trend in $\Delta pigstock$ (Appendix Figure A.3). The stationarity of the instrument’s temporal component is further corroborated by an augmented Dickey-Fuller test for unit root, which convincingly rejects the null hypothesis ($p = 0.000$).

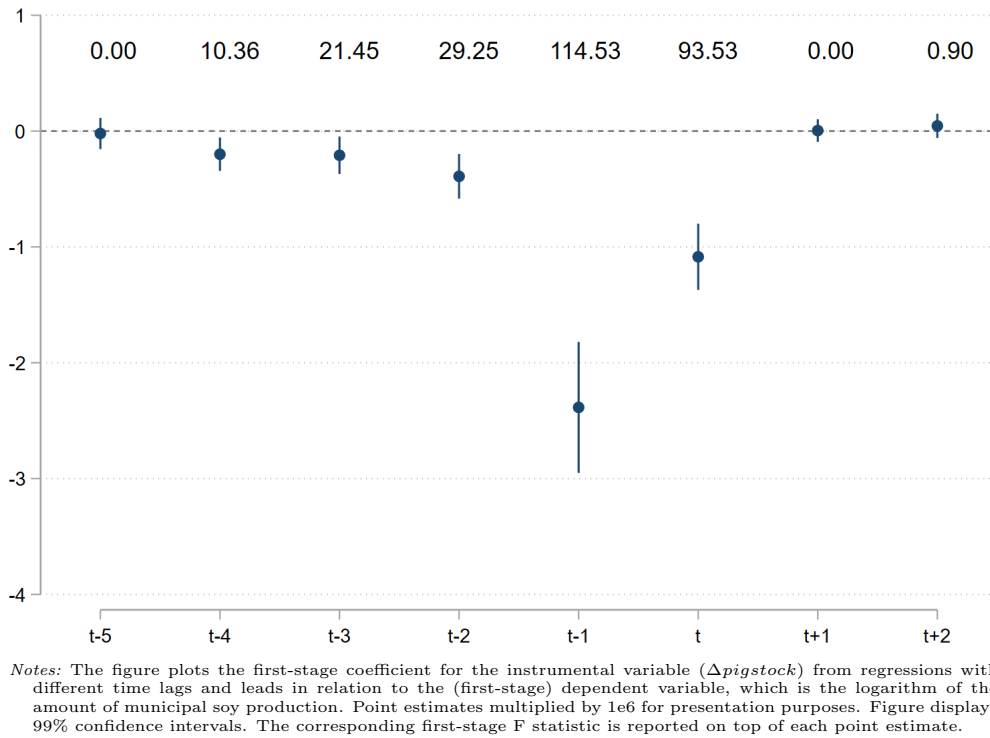
To further examine the validity of the instrument, Figure 1.3 plots the first-stage coefficient associated with the instrumental variable ($\Delta pigstock_{t-1} \times soil_{mr}$) with different lags and leads—that is, instead of instrumenting soy production in t with the variation in the size of the Chinese pig stock between $t - 2$ and $t - 1$ (interacted with time-invariant municipal soil suitability for soy cultivation), I try also different yearly intervals ($t - 1$ is the baseline specification).

As expected, the figure shows that going back in time reduces the magnitude of the coefficient (which becomes insignificant by $t - 5$ and beyond) and decreases the instrument’s relevance, indicated by the first-stage F-statistics, displayed above each point estimate. That is, intuitively, the importance of variations in the pig stock as a determinant of soy production seems to wane over time.

Besides, although still powerful, instrumenting production using the current (t) change in the pig stock—that is, from $t - 1$ to t —produces a considerably weaker instrument and smaller first-stage coefficient. This indicates that producers might need some time to adjust production in response to demand shocks. This is also consistent with the fact that many farmers heavily rely on forward contracts to market their harvest.

Finally, in a sort of falsification test, I show results for future changes in the size of the pig stocks in China. Reassuringly, this yields extremely weak instruments and coefficients that are indistinguishable from zero, giving support to the argument of

⁴⁶Using the size of the Chinese pig stock itself, instead of its change over time, also yields a powerful instrument (not reported). Nevertheless, I refrain from doing so given Christian and Barrett’s (2023) concerns.

Figure 1.3 – Instrument Leads and Lags

shifter exogeneity (Borusyak et al., 2022), as variation in the pig stock (partially) drives soybean production abroad, and not the other way around. This is also in line with the evidence that, to a large extent, variation in the Chinese pig stock is explained by sporadic episodes of disease outbreak (You et al., 2021).

Alternative Instruments

Appendix Table A.6 further investigates whether the findings above are driven by a “spurious regression” problem (Christian and Barrett, 2023). To do so, it shows results using different sources of temporal variation as instruments for municipal soy production. In this case, to make sure the instrument’s relevance does not come solely from soil suitability for soybean cultivation, I use uninteracted instruments that vary only over time.⁴⁷

The table shows that using $\Delta pigstock$ on its own (i.e., uninteracted) still yields a positive and significant coefficient of interest in the second-stage regression (column 1). Moreover, despite losing some power, it still generates a relevant instrument and, considering the other alternatives, the variation in Chinese pig stocks produces by

⁴⁷Note that, in this case, region-year fixed effects would absorb all of the instrument’s variation. I therefore include region-5-year-period fixed effects instead.

far the highest F statistic. Relying on variation in the Chinese cattle stock—which are also mostly fed with soy-based feed but are a less preferred protein source in the Chinese diet—yields a much weaker first stage (column 2). As a falsification test, I employ, alternatively, variation in the Chinese production of crude steel, which is considered by the Chinese government to be “a strategically important commodity” (Dreher et al., 2021a, p. 18) and a good proxy for economic activity in the country—and in particular for its “capacity to provide physical project inputs ” (Bluhm et al., 2020, p. 18).⁴⁸ This exercise shows, however, that it seems to be indeed the variation in the size of the Chinese pig stock—as theory would have it—that matters to explain soy production in Brazil, and not just any cyclical fluctuation related to the status of the Chinese economy. Finally, using variation in the size of the pig stock in the European Union—the world’s second largest stock, after China’s—does not yield a powerful instrument on its own (column 4). Even though the role of the EU is discussed in more detail below, this already hints at the fact that, although it is the second most important buyer of Brazilian soybeans, feeding pigs is a less prominent reason for EU demand.

1.5.2 Is it really about China?

I have so far highlighted the role of the China shock as a major engine behind soy expansion in Brazil and therefore as an indirect cause of the observed increase in inequality. This begs the question of whether there is anything specific to Chinese demand of soybeans that leads to higher inequality. In fact, the existing literature on commodity shocks devotes little attention to how the consequences for the producing (origin) country might vary according to the country to which commodities are exported. In the case of soy, it becomes crucial to acknowledge the heterogeneous nature of its demand and the potential segmentation of supply chains.

Because soy trade is telecoupled by nature (Pendrill et al., 2019; Meyfroidt et al., 2020), this means that producers in Brazil may adopt different practices depending on, e.g., the environmental awareness of their final consumers and the standards required by the importing country (Nepstad et al., 2006; Garrett et al., 2013b). The fact that the two largest buyers of Brazilian soybeans, China and the European Union, differ in some important aspects makes this issue all the more relevant.

Therefore, to benchmark the findings above, I repeat the analysis for the EU. I

⁴⁸The temporal variation in the Chinese steel production (at times combined with that of other products or raw materials) was proposed by Dreher et al. (2021a) as source of (arguably) exogenous variation driving Chinese foreign aid and has since been used more widely in the literature (e.g., Humphrey and Michaelowa, 2019; Bluhm et al., 2020; Dreher et al., 2021b; Cruzatti et al., 2023; Zeitz, 2020).

do so in two ways. First, I use variation in the size of the European pig stock (now interacted with soil suitability for soy) as the instrumental variable. Second, I use soy exported to the EU (instead of total production) as the instrumented variable.

Importantly, as the previous results in Table A.6 attest, the scale of the Chinese pig stock (more than half of the pigs worldwide) has no peer in terms of influencing the demand for soy. Besides, feeding pigs does not seem to be as important a reason behind European soy imports. Therefore, the identification strategy is probably less pertinent in the case of EU demand. Nevertheless, although less powerful than for China, the (interacted) instrument is still fairly relevant (Appendix Table A.7).

In this case, there is no evidence that total soy production is linked to higher municipal inequality. This indicates that the response in production triggered by demand fluctuations in the EU do not seem to have considerable distributional implications. Increases in soybean exports to the EU, on the other hand, do lead to higher spatial inequality. A potential explanation for these contrasting results is that soy farmers who export (part of) their harvests tend to operate on larger farms. Therefore, although the EU may not possess the scale—differently from China—to influence the soy-inequality nexus as a whole, it does so via its imports. Even so, the magnitude of this effect is significantly smaller than that of exports to China.⁴⁹ As the findings from Section 1.3 also indicate, given the unprecedented scale—in size and tempo—of China’s economic rise, it is perhaps not surprising that its consequences are rather distinct.

1.6 The Soy-Inequality Nexus

The analysis above presents evidence that soy expansion has increased land and spatial inequality in Brazil. This section aims to understand why this is the case.

1.6.1 Land Consolidation

A common consequence of processes of agricultural modernization is that farmers (or peasants), usually smallholders, who are less well positioned to exploit economies of scale and “ride the wave” of booming demand might be squeezed out of the market and end up selling their land to wealthier farmers. To understand to what extent this may be the case, Table 1.5 repeats the 2SLS specification in column 3 of Table 1.2, but now with different outcome variables, all at the municipal level and in

⁴⁹Although, as discussed previously, regional differences play an important role, it does not seem to be the case that they account for the contrasting results between China and the EU, as the share of soy exported to each of these two markets is rather similar across Brazilian regions.

logarithmic scale: median area of rural properties, the number of rural properties, the number of agricultural cooperatives, and population size.⁵⁰

Table 1.5 – Land Consolidation

<i>Dependent Variable:</i>	Property Area (log) (1)	No. Properties (log) (2)	No. Coop (log) (3)	Population (log) (4)
Soy (log)	0.048*** (0.018)	9.662*** (0.548)	16.673*** (0.959)	0.204*** (0.014)
WTO × Soy (log)	0.023*** (0.005)	-1.792*** (0.158)	-3.015*** (0.266)	-0.030*** (0.004)
WTO	-0.151*** (0.014)	0.669 (0.494)	-3.407*** (0.835)	0.105*** (0.012)
Municipality FE	Yes	Yes	Yes	Yes
Period FE	No	No	No	No
N	15,133	15,171	15,171	15,133
KP F-stat	145.72	145.91	145.91	145.72

Notes: WTO is a binary variable that takes the value 1 if period p is after 2001. *Soy (log)* is the natural logarithm of municipal soybean production. Dependent variables: *Property Area (log)* is the median size of rural properties in a municipality in logarithmic scale (column 1); *No. Properties (log)* is the natural logarithm of the number of rural properties in a municipality (column 2); *No. Coop (log)* is the natural logarithm of the number of agricultural cooperatives in a municipality (column 3); and *Population (log)* is the natural logarithm of municipal population (column 4). First-stage Kleibergen-Paap F-statistic reported. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors, clustered at the level of 1970 MCAs, in parentheses.

First, column 1 indicates that the median size of rural properties in municipalities in Brazil increases with the amount of soy produced and even more so after 2001. This is consistent with the interpretation that, due to the crop’s characteristics, soybean cultivation has a tendency to monoculture and land consolidation. Column 2 provides further evidence of land consolidation, as it indicates that, although soy expansion (perhaps mechanically) increases the number of rural properties, the effect subsides in the post-WTO period. Column 3 shows a similar pattern for the number of agricultural cooperatives, potentially reflecting the increasing financialization of the agricultural sector in Brazil and the rise of agribusiness—a model in which big trading companies dominate and cooperatives play a less important role. Finally, to assess, even if crudely, whether (intramunicipal) displacement might explain the increases in inequality, the specification in column 4 has municipal population as the outcome variable. Rather, the expansion of soybean production seems to lead to increases in population in the municipality, although the magnitude of the effect goes down after China’s accession to the WTO.

Next, I analyze the components that make up the measure of spatial inequality—that is, income and population—separately. First off, Table 1.6 shows that there is no clear evidence that the soy boom has increased average income per capita.⁵¹

⁵⁰The first three variables are taken from the four agricultural census waves between 1985 and 2017. Municipal population data have already been introduced in Section 1.4.

⁵¹If run separately for each Brazilian region, the 2SLS models indicate that, perhaps unsurprisingly, soy production increases GDP per capita only in the Midwest. The coefficient is statistically

The OLS coefficient is small in magnitude and only marginally significant (column 1). In the 2SLS setup, it is far from achieving statistical significance and even turns negative (column 2).⁵² This makes it unlikely that the increase in inequality, reported previously, occurs simply because some benefit more than others. Rather, it suggests that some may have become worse off. This question is examined in more detail in Section 1.6.2.

Columns 3 and 4 indicate in addition that average (log) cell luminosity even goes down as soy expands. As is the case in the main section of results, once again OLS significantly underestimates the effect. Finally, there is additional evidence that expanding soy production leads to (small) increases in total municipal population, also when it is measured using the gridded population data (column 6).

Table 1.6 – Income, Lights, and Population

<i>Dependent Variable:</i>	GDP pc (log)		NTL		Population (log)	
	(1)	(2)	(3)	(4)	(5)	(6)
Soy (log)	0.003* (0.002)	-0.003 (0.036)	-0.002* (0.001)	-0.106*** (0.022)	-0.001 (0.001)	0.049*** (0.008)
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Region-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	No	No	No	No	No
Model	OLS	2SLS	OLS	2SLS	OLS	2SLS
N	116,774	116,774	116,858	116,858	116,780	116,780
KP F-stat		113.97		118.14		113.96

Notes: *Soy (log)* is the natural logarithm of municipal soybean production. Dependent variables: *GDP pc (log)* is municipal GDP per capita in logarithmic scale (columns 1 and 2); *NTL* is the natural logarithm of municipal average nightlight radiance (column 3 and 4); and *Population (log)* is the natural logarithm of municipal population (columns 5 and 6). First-stage Kleibergen-Paap F-statistic reported. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors, clustered at the municipality level, in parentheses.

Taken together, these results paint a picture that is consistent with a process of land consolidation. While the increase in soy production brings benefits to the agricultural sector, these do not seem to extend to other parts of the economy.⁵³ The reduction in average light emission is in line with the anecdotal evidence that indicates that small-holder farmers, who tend to live on the outskirts of municipalities, are pushed to the urban center (or elsewhere) as agriculture consolidates. This would concentrate light both at the rural part (as land consolidates) as well as in the urban center. Given that the latter is usually already well lit, this movement may decrease luminosity in the rural part without necessarily creating more (observable) light in the center. This does not necessarily mean that the soy business

insignificant for the other four regions (not reported).

⁵²Using (log) GDP, instead of (log) GDP per capita, yields similar findings (not reported).

⁵³Appendix Table A.8 shows that soy production increases the share of value added in agriculture, but has no influence on the industrial or service sector.

does not create additional light through its machinery and dedicated facilities, but rather that this seems to be more than offset by intra-municipal flows and the lack of activity being created in other capital-intensive sectors of the economy.

The increase in population, on the other hand, is in line with the creation of (potentially low value-added) activity in services, such as small shops and restaurants. Moreover, this is embedded in a deeper process of reorganization of Brazil's population. Mirroring the growing dominance of agribusiness in the Brazilian economy, millions of people have been moving toward the interior of the country after centuries of concentration along the coastal areas. In any case, it is important to note that an increase in total population does not mean that soy is not linked to displacement, a phenomenon widely discussed in the related literature, "as large-scale mechanized agriculture uses less labor per hectare than small-scale farming techniques" (Weinhold et al., 2013, p. 133). This process could still be taking place within municipalities, something which intermunicipal migration data would not be able to capture.⁵⁴

Not only is luminosity decreasing—or at least growing at a slower pace—as soybean expands, but it seems to do so in such a way that light becomes more concentrated. Again, this is consistent with the pattern of land consolidation and the displacement of small-holder farmers. As Appendix Table A.9 shows, the increase in the spatial Gini seems to be driven by the distribution of nighttime light becoming more unequal—although it has also been accompanied by a decrease in the population Gini. Summing up: lights get more concentrated, while population increases and spreads.

1.6.2 Quantile Analysis: Who Benefits?

To complement the findings on inequality, it is important to understand the impact of soy expansion on different strata of society. It is possible that all groups have become better off, with some just benefiting more than others. But it could also be the case that parts of the population have actually become worse off. To address this question, I make use of microdata on household monthly income for all Brazilian municipalities available in the last three rounds of the Brazilian demographic censuses (IBGE, 1991; 2000; 2010).⁵⁵ I then calculate the average household monthly income per resident (henceforth income per capita) for each decile of the distribution in each Brazilian municipality during each census wave. Appendix Figure A.6

⁵⁴Using census data, available roughly every ten years, Bustos et al. (2016) find that Brazilian municipalities which experienced large increases in potential soy yields after the introduction of GE soy saw higher net outflows of migrants between 2000 and 2010.

⁵⁵Throughout this subsection, what I refer to as municipalities are 1980 MCAs.

illustrates how income per capita evolves, both in monetary values (constant 2000 BRL) as well as the share of total municipality-wave income, from 1991 to 2010.

To understand how the expansion of soy affects income across all deciles, I estimate the following model:

$$y_{dmrp} = \beta_1 \overline{soy}_{mrp-1} + \sum_{d=2}^{10} \beta_d \overline{soy}_{mrp-1} \times \mathbb{1}[D_{dmrp} = d] + \eta_{rp} + \psi_{dmr} + \epsilon_{dmrp} \quad (1.6)$$

in which y_{dmrp} stands for decile d 's mean income in each municipality m in region r during each census wave p . It is measured either in constant 2000 BRL or as a share of total municipality-year income across all deciles. Similarly to Section 1.3, $\overline{soy}_{mrp-1}$ stands for the (log) production of soybeans in municipality m averaged over the ten years preceding the census year. Here, it also enters the equation interacted with a binary variable for each municipality-period income decile, D_{dmrp} (the first decile is the omitted category). On top of region-period fixed effects (η_{rp}), because this analysis is at the municipality-decile-period level, I include also municipality-decile fixed effects (ψ_{dmr}), such that I exploit income variation within each municipality-decile over time.

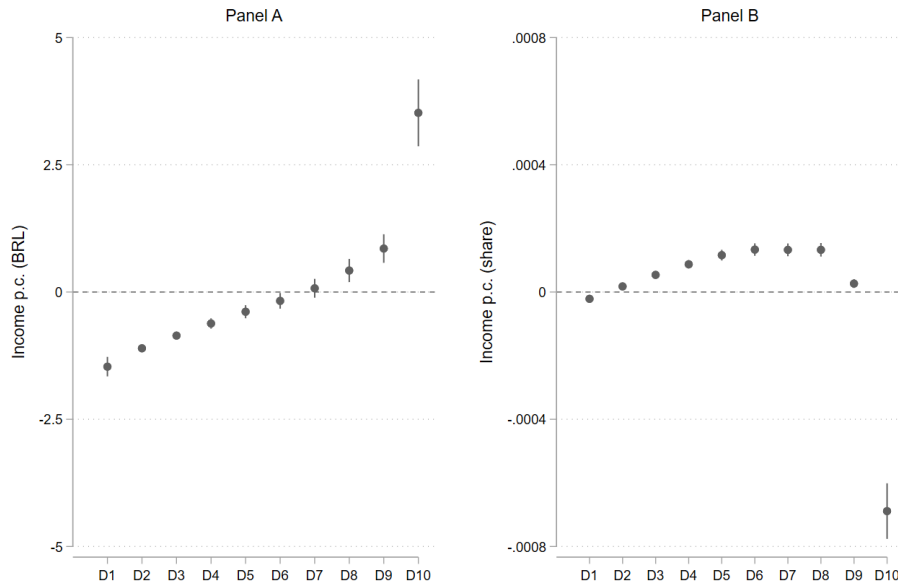
For identification purposes, in addition to instrumenting $\overline{soy}_{mrp-1}$ with $Z_{mrp} = \Delta pigstock_p \times soil_{mr}$ (similarly to equation 1.3), now the first-stage equation includes also the interaction between Z_{mrp} and each decile indicator variable, D_{dmrp} ($d \in [2, 10]$), as regressors.

Figure 1.4 plots the marginal effect associated with a 1% increase in municipal soy production (implied by the estimation in equation 1.6) for each of the two outcome variables: decile income per capita in BRL (panel A) and as a share of the total income in a municipality-period (panel B).⁵⁶ The former shows that, in absolute terms, the higher one is at the income distribution, the more one tends to benefit from increases in municipal soy production. Given the amount of capital usually required to be a competitive soy producer, it is quite likely that most of these farmers are in the top income deciles in their home municipalities.⁵⁷

But soy seems far from lifting all boats. Strikingly, for the bottom six deciles—that is, for more than half of the population—soy expansion is even detrimental to one's income, and the reduction in income increases the poorer one is. These findings are in line with the results above of rising inequality and no benefit in terms

⁵⁶Appendix Table A.10 shows the full results.

⁵⁷In fact, the level of income per capita necessary to be in the tenth decile is relatively low. Across all municipalities in 2010, the mean monthly income per capita of the top decile is BRL 2,267 (in current values), or about USD 1,288 according to the exchange rate at the time.

Figure 1.4 – Marginal Effect of Soy Expansion by Income p.c. Deciles

Notes: The figure plots the marginal effect associated with a 1% increase in municipal soy production for each per-capita income decile according to the estimation strategy described in equation 1.6. The outcome variables are income per capita in constant 2000 BRL (panel A), and the decile's share of total municipal income per capita in each census wave (panel B). Figure displays 99% confidence intervals. All regression models include decile-municipality and region-period fixed effects. Robust standard errors clustered at the level of 1980 MCAs. The first-stage Kleibergen-Paap F-statistic is 16.39.

of average municipal GDP per capita. The groups at the bottom of the distribution, to which smallholders most likely belong, tend to be the ones who often get the short end of the stick, for instance by losing access to their land.

Considering the effects on deciles' income shares offers some additional insights (panel B). Because the top decile historically owns a considerable share of the total income—approximately 44% in 1991 (Appendix Figure A.6)—even though this is the group that benefits the most from the expansion in soy, the part of the additional income that it amasses is not high enough to sustain its share. Moreover, because the bottom deciles suffer particularly large losses, those at the middle of the distribution might even see their decile's income share increase, even though their income has not increased (and may have even decreased, but less so).⁵⁸

⁵⁸This might explain why there is no clear effect on the Gini coefficient of income inequality. Appendix Table A.11 shows OLS and 2SLS results for regressing the municipal Gini of income inequality (taken from the 1991, 2000, and 2010 censuses) on average (log) municipal soy production over the ten-year period preceding the census year. In the 2SLS setup, the explanatory variable of interest is, as above, instrumented with $Z_{mnp} = \Delta \text{pigstock}_p \times \text{soil}_{mr}$. All models include municipality fixed effects and either period or region-period fixed effects. The 2SLS results indicate even a small reduction in income inequality, which is however driven only by the municipalities in the South of Brazil.

1.6.3 Deforestation and Conflict

The rise in economic inequality is likely to be closely related to other important dimensions of the soy boom: the impact on the environment (and deforestation in particular) and rural conflict. More precisely, these elements may function as escape valves when economic opportunities become scarcer. That is, as inequality increases and land becomes more consolidated, (small-holder) farmers and peasants might start looking for land elsewhere, increasing the pressure on the local native vegetation and making the occurrence of land conflicts more likely.

The link between soy, considered “the second largest agricultural driver of deforestation worldwide” (WWF, 2016), and environmental damage in Brazil is well documented in the literature. While cattle ranching is usually the ultimate cause of land-use change in Brazil, the expansion of the soy frontier—which has been moving North toward the Amazon in what is referred to as the “arc of deforestation”—is frequently an underlying force behind pasture displacement and hence deforestation (Fearnside, 2001; Barona et al., 2010; Song et al., 2021). In short, soy production has been a major driver of forest and biodiversity loss in the country (Nepstad et al., 2006; Hargrave and Kis-Katos, 2013; Carreira et al., 2024).⁵⁹

However, it remains unclear to what extent this has been driven by rising Chinese demand.⁶⁰ To address this question, I apply the empirical strategy outlined in Section 1.4, but now with (log) annual deforested area in each Brazilian municipality, between 2000 and 2020, as the outcome variable (Base dos Dados, 2023). This measure is constructed based on satellite data provided by Brazil’s National Institute for Space Research (INPE, 2023).

Consistent with Hansen and Wingender (2023), the China shock does seem to be an important part of the story (Table 1.7, panel A). The 2SLS results show that increasing municipal soy production by a (within) standard-deviation is expected to increase deforestation by approximately 3.7% (column 5). What is more, the effect, once again, is much larger than what is implied by a naive OLS estimation (column 1).

Moreover, in line with the argument that deforestation should increase more in more unequal municipalities, adding an interaction term between soy production and spatial inequality yields a positive coefficient in both setups: OLS (column 3) and

⁵⁹See Hänggli et al. (2023) for a systematic review of the literature on deforestation drivers in the Amazon biome.

⁶⁰Perhaps surprisingly, based on a long-difference model, Carreira et al. (2024) show that differential exposure to the *aggregate* China shock does not seem to be linked to deforestation rates in Brazilian municipalities. However, this only holds as long as the effect of the shock is mediated by the introduction of GE soy technology.

Table 1.7 – Deforestation and Rural Conflict

<i>Panel A – Dependent Variable: Deforestation (log)</i>								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Soy (log)	0.007*** (0.001)	0.007*** (0.001)	0.004*** (0.001)	-0.026*** (0.003)	0.035*** (0.010)	0.001 (0.005)	0.069*** (0.012)	-0.091*** (0.020)
Spatial Gini		0.051*** (0.007)	0.040*** (0.008)			0.053*** (0.007)	-0.150*** (0.028)	
Spatial Gini × Soy (log)			0.004*** (0.001)				0.070*** (0.010)	
Initial Spatial Gini × Soy (log)				0.038*** (0.004)				0.166*** (0.018)
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	No	No	No	No
Model	OLS	OLS	OLS	OLS	2SLS	2SLS	2SLS	2SLS
N	116,606	110,477	110,477	114,943	116,858	110,646	110,646	115,157
KP F-stat					118.14	132.76	32.21	71.76
<i>Panel B – Dependent Variable: Land Invasion (log)</i>								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Soy (log)	0.008*** (0.003)	0.009*** (0.003)	0.009*** (0.003)	-0.025*** (0.006)	-0.095*** (0.024)	-0.104*** (0.024)	0.277*** (0.091)	-0.245*** (0.043)
Spatial Gini		0.128*** (0.025)	0.130*** (0.029)			0.130*** (0.025)	-0.781*** (0.201)	
Spatial Gini × Soy (log)			-0.001 (0.003)				0.294*** (0.067)	
Initial Spatial Gini × Soy (log)				0.038*** (0.011)				0.205*** (0.041)
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	No	No	No	No
Model	OLS	OLS	OLS	OLS	2SLS	2SLS	2SLS	2SLS
N	61,106	61,050	61,050	60,215	61,215	61,159	61,159	60,324
KP F-stat					105.99	108.39	8.25	60.92

Notes: *Soy (log)* is the natural logarithm of municipal soybean production. *Spatial Gini* is the Gini coefficient of municipal light per capita inequality. First-stage Kleibergen-Paap F-statistic reported. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors, clustered at the municipality level, in parentheses.

2SLS (column 7)—in which case an additional instrumental variable is added to the first-stage regression, namely: $W_{mrt} = \Delta pigstock_{t-1} \times soil_{mr} \times spatialgini_{mrt-1}$. A drawback of this exercise is that inequality itself is also affected by soybean production. To at least mitigate this issue, I repeat the analysis using the initial level of inequality (that is, the spatial Gini in the year 2000) in each municipality instead of its lagged value. The results again indicate that soy production leads to higher deforestation in (initially) more unequal places (columns 4 and 8).⁶¹ However, in this case, soy production would actually slow down deforestation in a small group of observations (approximately 10% of Brazilian municipalities) that have a rather low level of initial inequality.

As suggested above, another potential implication of the distributional consequences of the soy boom is the intensification of social grievances. This is “rooted in the idea that because access to land is the cornerstone of rural life, a skewed

⁶¹Measuring initial inequality with the 1995 land Gini instead leads to similar conclusions (not reported).

distribution of landholdings can fuel rural grievances and unrest” (Albertus et al., 2018, p. 558). Rural conflict, therefore, can be seen as a typical symptom of an unequal society (Russett, 1964; Brockett, 1992). To test if this is the case in the context of the China shock, I conduct a similar analysis, but this time with rural conflict as the dependent variable. I use the (log) annual number of land invasions that occurred in each municipality, taken from Comissão Pastoral da Terra (CPT, 2020), to measure rural conflict.

Some key findings emerge from this analysis (panel B). First, on its own, there is no evidence that the soy boom has increased rural conflicts in Brazil. In fact, the 2SLS results indicate that, in aggregate terms, it may have even reduced land invasions.⁶² However, this masks important regional heterogeneity. In the Midwest in particular, where these tensions are most present, soybean production significantly increases rural conflict.⁶³

Moreover, as for deforestation, the findings on rural conflict reiterate the importance of taking inequality into consideration. Regardless of how inequality is measured (varying over time or using its initial value), it shows that also the effect on land invasions increases substantially with the level of inequality and thus emphasizes that the consequences of soy production vary significantly depending on local conditions. Therefore, bringing both deforestation as well as rural conflict into the analysis not only highlights other important aspects of the China shock, but also shed more light on the role of inequality. More broadly, it suggests that it is not possible to make sense of soy’s environmental impact without taking its socioeconomic consequences into account.

1.7 Final Remarks

While the recent rise of China as a global economic power has brought opportunities for countries around the world, it has also contributed to some worrying developments. Relying on plausibly exogenous variation in Chinese demand for causal identification, this article shows that the “other” China shock—that is, China’s growing demand for agricultural commodities (and soybeans in particular)—inadvertently worsened economic inequality in Brazilian municipalities. The process of land con-

⁶²This may be partially explained by the fact that information on rural conflict is only available from 2010 to 2020. Therefore, this part of the analysis does not include the period just after China’s accession to the WTO, such that tensions may have already dwindled once data become available. Relatedly, Falcone and Rosenberg (2022) show convincing evidence that agricultural modernization (driven by soy) increased land occupations in Brazil between 1988 and 2014.

⁶³Appendix Table A.12 shows the results for deforestation (panel A) and rural conflict (panel B) for each Brazilian region separately.

solidation that ensued benefited mostly the richest parts of the population at the expense of everyone else, leading also to an increase in deforestation and, in some cases, a surge in land invasions. The results show that the more unequal a municipality, the more it tends to suffer in terms of forest loss and violence as soy expands.

Given the characteristics of modern soy cultivation, these findings are not that surprising. In fact, they reiterate the importance of factor endowments in shaping the distribution of land and income (Sokoloff and Engerman, 2000). In a country such as Brazil, which due to its colonial history has long struggled with high levels of inequality, this recent turn of events can be especially damaging. As economic inequality tends to translate into political inequality, the entrenched interests of an increasingly powerful agrarian elite may make the enactment of more inclusive policies ever more difficult (Acemoglu and Robinson, 2008; Ziblatt, 2008; Galor et al., 2009).

This chapter thus underscores the need of harnessing the productive potential of Brazilian agriculture in a fairer and more sustainable manner. While domestic challenges such as land reform cannot be overlooked (Reydon et al., 2015), initiatives for more responsible global agricultural supply chains may be an important catalyst for change (Garrett et al., 2013a; Gardner et al., 2019). In particular, this requires putting additional emphasis on the welfare of smallholders, who often lack the scale and the resources to compete against more consolidated producers. As the evidence presented here indicates, this can help not only to tackle inequality, but also to curb deforestation and rural conflict.

Chapter 2

Global Value Chains and Firms' Environmental Performance

Joint work with Inmaculada Martínez-Zarzoso and Leila Baghdadi

Abstract

This chapter analyzes the link between joining global value chains (GVCs) and firms' environmental performance. Based on new survey data for firms in countries in Central Asia, Eastern Europe, and MENA, we use propensity score matching (PSM) to compare similar companies that differ in terms of GVC participation. Our results show that firms that enter GVCs perform better across several environmental indicators. In particular, these firms are more likely to adopt environmentally friendly production technologies, comply with more demanding standards and regulations and, in the case of manufacturing firms, monitor CO₂ emissions along their supply chains. Moreover, by combining PSM with difference-in-differences, we find that becoming active in GVCs is also linked to more efficient energy use. Our results are stronger for firms in non-EU countries, and highlight the importance of environmental regulation in ensuring that global supply chains go hand in hand with greener production.

Keywords: global value chains, sustainability, environmental regulation, firms

JEL Codes: F18, F64, O13, Q56

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2.1 Introduction

The production of goods and services has become increasingly globalized since the turn of the century. According to the OECD (2013; 2020), in the last decade alone, the share of international trade that takes place within global value chains (GVCs) almost doubled—from 40% in 2009 to over 70% in 2019. As companies spread across the globe in search of lower costs, production chains have become increasingly geographically fragmented. More than being solely exporters or importers, firms that participate in these networks share backward linkages with suppliers as well as forward linkages with customers along the entire value chain (Timmer et al., 2013).

However, as concerns with environmental degradation and sustainability gain more and more attention (Messerli et al., 2019), it is necessary to understand the consequences of globalized supply chains for the environment. On the one hand, at the aggregate level, the fragmented nature of GVCs may lead to excess waste and higher emissions due to, e.g., longer shipping routes and over-exploitation of natural resources in places with weak institutions (De Melo, 2014; World Bank, 2020). On the other hand, engaging in GVCs may facilitate the transfer of (cleaner) technologies (Antràs, 2020; Rigo, 2021) and force firms to comply with stricter environmental regulations (Ambec and Barla, 2006).

At the macro level, the rich theoretical and empirical literature on the trade-environment nexus (e.g., Antweiler et al., 2001; Cole and Elliott, 2003; Frankel and Rose, 2005; Chintrakarn and Millimet, 2006; Cole, 2006; Managi et al., 2009; Tsurumi and Managi, 2014) for the most part provides evidence of economic openness being associated with better environmental outcomes. Yet, studies at the firm level, which enable a finer identification strategy and avoid aggregation biases, are still scant, and usually focus on single-country cases and/or on firms' exporting status and environmental abatement (e.g., Cole et al., 2008 for Ghana, Albornoz et al., 2009 for Argentina, Batrakova and Davies, 2012 for Ireland, Richter and Schiersch, 2017 for Germany, Forslid et al., 2018 for Sweden).¹ As a consequence, there remains a rather limited understanding of the consequences that scattering production chains across the globe has for the environment. In particular, we know little about how this process affects—if at all—firms' environmental footprint.

To fill this gap in the firm-level literature, we investigate the link between firms' participation in global value chains and their environmental performance. Our sam-

¹This literature thus disregards the importance of two-way trading and the productivity advantage that could emerge when firms have easier access to imported inputs. This advantage could also generate a double dividend if the engagement of firms with GVCs implies that they must comply with product standards and environmental regulations.

ple covers firms in 29 countries in Central Asia, Eastern Europe, North Africa, and the Middle East, and is primarily based on data recently collected by the World Bank Enterprise Surveys (WBES). We base our empirical identification on propensity score matching (PSM), which is combined with a difference-in-differences (DID) approach when the data allow (which is the case for the analysis of energy use intensity). Furthermore, we complement our analysis with a heteroskedasticity-based instrumental variable approach (Lewbel, 2012).

Our main contributions are fourfold. First, this is the first study that examines the link between GVC participation and a wide range of environmental indicators at the firm level.² Second, as our analysis covers a broad set of countries and stretches across different survey waves, we are able to apply an identification strategy that is stricter than most studies in the related literature—which often rely solely on cross-sectional data. Third, by taking advantage of detailed information on firms' environmental records, which has been recently made available in the Green Economy Module of the WBES (World Bank, 2021), we present a comprehensive picture, across several dimensions, of how joining GVCs is linked with firms' behavior with respect to the environment. More precisely, we observe the links between GVC participation and (i) pollution monitoring, (ii) the adoption of several more climate-friendly production technologies, and (iii) the external constraints that firms face, such as environmental regulation and customer standards. Finally, we are able to identify important channels that influence the link between GVCs and the environment.

By matching similar firms, controlling for country and industry type, we find that companies that enter GVCs perform better in several ways. These firms are more likely to adopt environmentally friendly procedures—such as changing to cleaner energy sources and controlling air pollution—and, in some cases, also monitor CO₂ emissions along their supply chains. Moreover, firms that become active in GVCs have a higher probability of being subject to energy standards and catering to customers that require environmental certification. In addition to being outcomes of interest in themselves, these external constraints also work as channels that increase firms' likelihood to improve environmental performance. In fact, our results indicate that environmental regulations and standards may actually be a necessary condition to ensure that GVC participation pushes firms to operate in a more environmentally friendly manner.

Our results also show that the positive link between environmental practices

²To the best of our knowledge, this link has scarcely been investigated before in similar settings. Existing studies at the firm-level, such as Hua et al. (2020) and Zhang et al. (2021), focus on a single indicator of environmental performance, and usually on a single country.

and GVC participation is, as expected, stronger for the manufacturing sector and for firms outside of the European Union. The latter is in line with the interpretation that exposure to GVCs can be especially beneficial for firms that otherwise operate in domestic markets that do not demand high environmental standards. Importantly, we do not find evidence of systematic differences between treated and control groups after matching, and our findings are robust to changes in the specification and in the definition of GVC participation. Finally, to shed more light on a potential causal relationship, we also assess the influence of GVC participation on our indicators of environmental performance using a heteroskedasticity-based instrumental variable setup. Reassuringly, this yields similar conclusions.

The remainder of this chapter is structured as follows. Section 2.2 provides a review of the theoretical framework and the related literature. Section 2.3 introduces the data and the empirical strategy, and Section 2.4 presents the main results. We discuss the robustness of our findings and potential underlying mechanisms in Sections 2.5 and 2.6, respectively. Section 2.7 offers concluding remarks along with policy recommendations.

2.2 Literature Review

The trade-environment nexus has been extensively investigated in the economics literature, especially since the early 1990s. It gained traction after the seminal work by Grossman and Krueger (1995), which found an inverted U-shaped relationship—the so-called environmental Kuznets curve (EKC)—between a country's per-capita income and its environmental quality. This was the first study to propose the decomposition of the trade effect into three factors: scale, composition, and technique effects. Since then, an extensive body of literature has examined the impact of trade on the environment at the macro level using the proposed decomposition (Antweiler et al., 2001; Stoessel, 2001; Cole and Elliott, 2003; Frankel and Rose, 2005; Chintrakarn and Millimet, 2006; Cole, 2006; Managi et al., 2009; Farhani et al., 2014; Tsurumi and Managi, 2014; Roy, 2017).

Even though the results at the aggregate level are not always conclusive, there is some evidence of a net positive effect of trade liberalization on environmental quality. Policymakers have often used the EKC hypothesis and the subsequent literature to argue that trade is actually good for the environment and that opening up to international competition would not only increase income per capita, but also lead to higher government investment in environmental protections and to stricter environmental regulation. Applying this reasoning to our setting, we conjecture

that participation in GVCs—which reportedly increases welfare (Fagerberg et al., 2018)—could also lead to improvements in terms of firms' environmental footprint, because participating companies would likely need to comply with more demanding production standards.

Other prominent competing theories related to the effects of trade liberalization on the environment are the pollution haven hypothesis (PHH) and the environmental Porter hypothesis (EPH). The former, which is related to the composition effect of trade, suggests that countries with relatively weak environmental regulation will attract and specialize in highly polluting industries (Copeland and Taylor, 2004). The EPH, on the other hand, is linked to the technique effect and posits that compliance with stricter environmental regulation will push some firms to become more competitive and to increase exports.³ Nevertheless, not all firms may be able to bear the additional costs of regulatory compliance, such that some are likely to exit the market (Ambec et al., 2013). Accordingly, extrapolating this argument to GVCs suggests that firms that join international production chains may benefit from stronger international competition and higher standards, and could be more likely to adopt greener technologies and reduce their emissions—or at least their emission intensity.

At the micro level, with the help of the new-new trade theories that place the firm at the center of the analysis (Melitz, 2003), three hypotheses related to firm heterogeneity and environmental quality have been put forward: the pollution reduction by rationalization hypothesis (PRR), the distressed and dirty industry hypothesis (DDI), and the pollution offshoring hypothesis (POH) (e.g., Cherniwchan et al., 2017; Forslid et al., 2018; Najjar and Cherniwchan, 2021). Najjar and Cherniwchan (2021) give a detailed exposition of all three hypotheses, which they claim have a common theoretical foundation as described by Melitz (2003).

On the one hand, the PRR hypothesis predicts that trade liberalization shifts the market share toward high-productivity firms, which are able to drive down emissions to a greater degree than their low-productivity counterparts. The latter—usually more polluting firms—may eventually exit the market, while cleaner, high-productivity firms go on to become exporters. The DDI, on the other hand, suggests that trade liberalization may cause emissions to rise if the uncompetitive and distressed domestic firms forsake abatement expenditures or if pollution control is reduced. Finally, although the POH has similarities with the PHH—as both hypotheses predict trade liberalization to lead to the offshoring of emission-intensive

³Additionally, exporters might also have access to cleaner technologies due to the embodied technology transfer that is inherent to trade (Cui and Qian, 2017).

industries to countries with weaker environmental regulations—the former focuses rather on intermediate goods and the fragmentation of production processes that enable the most polluting intermediate stages of production to be offshored.

Among the existing studies that use firm-level data, most focus on the link between exporting and emission intensity. Holladay (2016) examines data for firms in the United States and finds that, in 14 out of the 20 industries covered by the study, exporters pollute less per output than their peers. Jinji and Sakamoto (2015) show similar results for Japanese firms, while Richter and Schiersch (2017) find a positive relationship between export intensity and carbon dioxide productivity for German manufacturing firms. Moreover, Forslid et al. (2018) show that investments in pollution abatement are positively correlated with productivity and exports. Their study examines the impact of trade liberalization on Swedish manufacturing firms and concludes that, as the scale of production of exporting firms increases, they are able to boost investments in abatement and, in turn, reduce emissions.

The few empirical studies that test the POH (Levinson, 2010; Michel, 2013; Li and Zhou, 2017; Cole et al., 2021)—that is, whether firms tend to offshore the most polluting activities of the production process—often show conflicting results. For instance, in the United States, although Levinson (2010) reports that the composition of imports became less energy-intensive in the final two decades of the 20th century, Li and Zhou (2017) find that the country's firms often opt to offshore pollution. Furthermore, Cole et al. (2021) examine pollution offshoring in Japan from 1988 to 2013, and find that the composition of imports has become dirtier over time. In particular, carbon embodied in Japanese imports has increased at a higher rate than in exports.

In terms of data and methodology, our work is particularly related to Del Prete et al. (2017) and Rigo (2021), as both studies also use firm-level data from the World Bank Enterprise Survey to investigate the benefits of GVC participation. The former uses data for Egypt and Morocco and the latter, for 18 low and middle-income countries. Whereas Del Prete et al. (2017) focus on the effects on firms' productivity, Rigo (2021) examines whether GVCs foster technology transfers. Both studies address potential endogeneity by using PSM combined with a DID estimation. The results indicate that firms that participate in GVCs are more productive and more likely to use foreign technology.

To the best of our knowledge, only two studies address the relationship between GVCs and environmental quality (proxied by the emissions of a single pollutant) using disaggregated data. Zhang et al. (2021) apply a multi-regional structural decomposition analysis to sectoral data over the period between 2000 and 2014,

finding that production activities linked to GVCs improved in terms of CO₂ emission intensity, while domestic production within individual countries became more emission-intensive. This deterioration caused by domestic production was more than offset by the GVC effect, resulting in a net decrease in global emission intensity of 1.7%, driven in particular by emerging economies. The second study, Hua et al. (2020), analyzes the effect of firms' participation in GVCs on SO₂ emissions using panel data from Chinese firms for the period between 2000 and 2007, and shows that emission intensity decreases as a consequence of firms' engagement in GVCs. The results are stronger for firms with domestic ownership, those producing more pollution-intensive goods, and those exposed to more stringent environmental regulations.

Finally, in addition to the more “traditional” GVC literature, which focuses on the international division of labor and in which our work is embedded, it is important to acknowledge the existence of a rich body of research that devotes special attention to the relational aspects of these chains (e.g., Humphrey and Schmitz, 2002; Gereffi et al., 2005; Brancati et al., 2017; Mazzi et al., 2021). These studies emphasize that the rise of modern GVCs should be understood not only as the mere increase in the degree of trade integration, but also as a phenomenon that involves transactions of a fundamentally different nature. That is, not as anonymous and one-shot, as usually portrayed in the traditional trade theory, but rather based on highly customized inputs and predicated upon repeated interactions between firms (Antràs, 2020). Therefore, features such as whether suppliers are under corporate control, the depth of intra-firm informal ties, and the degree of involvement in product conception—in short, the governance of a global value chain and the position of firms within it—become an essential part of the analysis. Nevertheless, “these two literatures have largely evolved in isolation,” partially because of “the significantly different sources of data” that each of them relies on (Antràs, 2020, p. 17).

2.3 Data and Methodology

2.3.1 Data

Our main data source for analyzing the relationship between participation in global value chains and the environment is the World Bank Enterprise Survey (World Bank, 2021), which in addition to several firm characteristics collects information about firms' perception of the business environment in their country and the constraints that they face. We construct a panel with up to 1,459 firms from 29 countries in Central Asia, Eastern Europe, North Africa, and the Middle East for two waves of

the sample (2012–13 and 2018–20).⁴

The survey, which is representative of the non-agricultural private economy in each country, contains information from manufacturing and service sectors worldwide, and is stratified according to the sector of activity, firm size, and geographical location. Moreover, the sampling methodology is stable over time, as it is designed to provide panel data sets. Only formally registered companies with at least five employees are interviewed, and state-owned firms are excluded. Although business owners and senior-level managers are usually the respondents, other managers are sometimes also interviewed to answer specific questions. Typically, between 1,200 and 1,800 interviews are conducted in larger economies, 360 interviews in medium-sized economies, and around 150 interviews in small economies. The questionnaire covers a broad range of business environment topics, including access to finance, corruption, infrastructure, crime, competition, and performance measures.⁵

Environmental Outcomes

Particularly relevant for our purposes is the Green Economy Module, which was included in recent versions of the survey (2018–20) for several countries and collects information about the measures that firms have been taking to improve their environmental performance. These questions capture different aspects of firms' environmental records. From the information available in the Green Economy Module, we construct nine of the ten outcome variables of interest that we use in this study to assess firms' environmental performance, which we classify according to three different categories: pollution monitoring, adoption of greener technologies, and external environmental constraints.

First, we measure pollution monitoring with a binary variable that indicates whether a firm reports monitoring CO₂ emissions along its supply chain (*CO₂ Monitor*). Second, to assess the adoption of more environmentally friendly procedures, we track whether firms adopt measures to generate climate-friendly energy on site (*Adopt CFE*), improve energy efficiency (*Adopt EE*), manage energy consumption (*Adopt EM*), control air pollution (*Adopt Air*), and minimize waste (*Adopt Waste*). Finally, we capture the external environmental constraints faced by firms using three indicators: whether firms are subject to an energy performance standard in its operations (*Subject ES*), whether they perceive environmental regulations as an obstacle

⁴For the list of countries included in our sample, see Table B.1 in the Appendix. The selection of countries is explained by the availability of the Green Economy Module.

⁵For more details, see the WBES Sampling Notes, available at https://www.enterprisesurveys.org/content/dam/enterprisesurveys/documents/methodology/Sampling_Note.pdf.

to their activities (*Obstacle ER*), and whether they cater to customers that require environmental standards (*Customers*).⁶

Taken together, these nine variables form a rather comprehensive picture of how environmentally friendly the production of these firms is.⁷ For instance, out of the more than 1,400 firms in our sample, only around 3% have monitored their emissions along their supply chains, 14% have adopted measures to limit air pollution, and close to 10% have adopted more climate-friendly energy generation on site. Table B.3, in the Appendix, reports descriptive statistics.

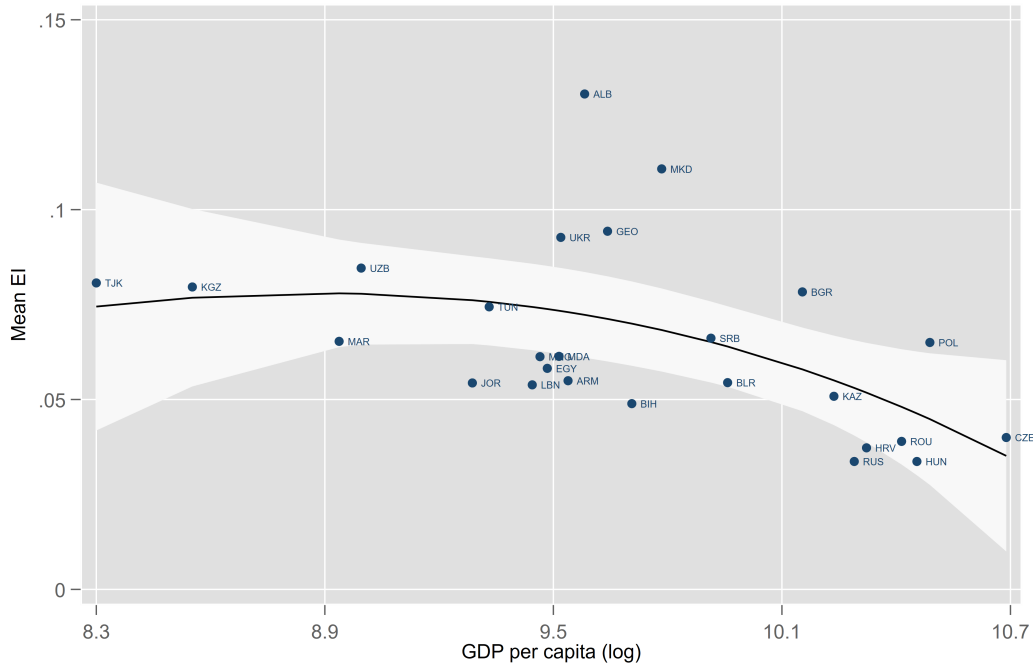
As previously mentioned, the Green Economy Module is only available for the last wave of our panel. Hence, we include, as an additional dependent variable, a measure of firms' energy use intensity (*EI*) that can be calculated for both waves in our data, and is an objective and widely used indicator of environmental performance (e.g., Eskeland and Harrison, 2003; Aguayo and Gallagher, 2005; Stern, 2007; Cole et al., 2008; Batrakova and Davies, 2012). Following the literature, we define energy use intensity as the total annual cost of energy (electricity and fuel) divided by total sales. In this case, we can make use of the temporal dimension of our data and control for firms' time-invariant characteristics by analyzing, for each firm, the difference in energy use intensity across the two available waves.⁸

Interestingly, we find an inverted U-shaped relationship between (log) GDP per capita and average *EI* across the countries in our sample (Figure 2.1), which is consistent with the EKC hypothesis. That is, energy use intensity increases with income per capita only up to a point, and then decreases for richer countries in our sample. Bulgaria, Croatia, Czech Republic, Hungary, Poland, and Romania, all members of the European Union, are located in the downward-sloping part of the curve.

⁶Table B.2, in the Appendix, provides the definition of all variables used in the empirical analysis.

⁷As is often the case with survey data, one may be worried about social desirability bias. This would be a concern particularly if GVC firms are more likely to misreport their environmental performance than domestically-oriented firms. While we cannot rule out this possibility, we deem it unlikely. First, the questions used to create our dependent variables are quite direct and leave little room for respondent discretion. Second, at least in the case of our indicators of external constraints, it is not clear that an affirmative answer (i.e., facing stricter regulation) is more desirable. Third, using *EI*—which is calculated based on different independent and objective measures of energy consumption and total sales—as an additional outcome yields results that point in the same direction.

⁸We acknowledge that energy prices in real terms can differ across countries, but this is less of an issue in our setting because we focus on the difference over time and most of our specifications include country fixed effects.

Figure 2.1 – Energy Use Intensity and Income per Capita

Notes: The figure plots the mean value of energy use intensity (*EI*) by country (in the 2018–20 wave) against current 2020 (log) GDP per capita for all countries in our sample. Data on energy use intensity taken from WBES (World Bank, 2021). Data on GDP per capita come from the World Development Indicators (World Bank, 2024a). Countries are identified by their ISO 3-digit alphabetic codes, and their full names are listed in Table B.1 in the Appendix.

GVC Participation

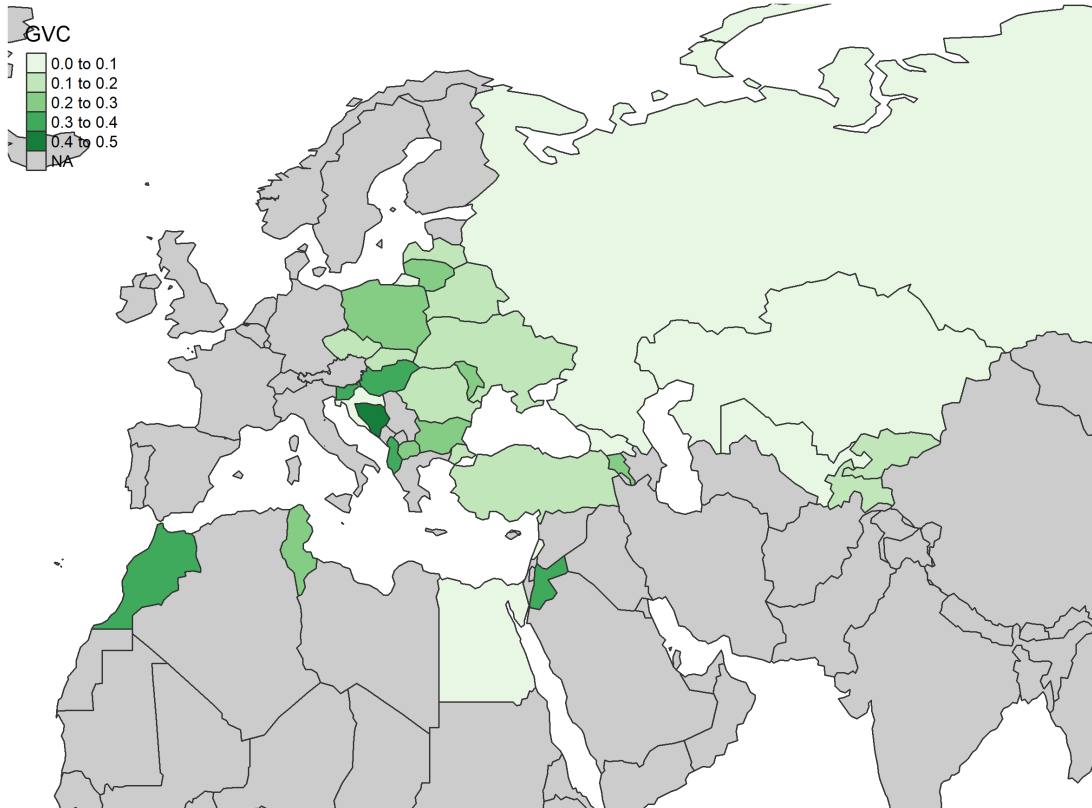
To assess firms' participation in global supply chains, we follow the recent literature (e.g., Benkovskis et al., 2020; World Bank, 2020; Rigo, 2021) and consider that a firm participates in GVCs if it is involved in two-way trading, that is, if it both exports and imports.⁹ We do so because, unlike either solely exporting or importing a certain good, being simultaneously an exporter and an importer is arguably a defining feature of a firm's insertion into a sequential production line that is scattered across the globe. As the World Bank puts it, “[w]hen a given firm in a given country both imports and exports, it is natural to conclude that this firm participates in GVCs” (World Bank, 2020, p. 30).¹⁰ Moreover, our baseline definition has the advantage of also capturing firms that only export and import indirectly, ensuring that we do not overlook those participating in GVCs through national intermediaries. That is, we

⁹We discuss alternative definitions of GVC participation in Section 2.5.

¹⁰Note that ours is a measure of the extensive margin of GVC participation. Furthermore, we acknowledge that this definition disregards some valuable dimensions of GVCs. For instance, due to data limitations, we cannot distinguish between different forms of GVC governance, such as arm-length or relational linkages—as discussed by, e.g., Gereffi et al. (2005), Brancati et al. (2017), and Mazzi et al. (2021)—that may also influence the extent to which supply chains foster the adoption of cleaner production practices. Therefore, we focus on the international trade dimension of GVCs.

consider a firm to be an exporter (importer) if it exports (imports) a positive share of its sales (inputs and supplies), regardless if it does so directly or via domestic intermediaries.¹¹

Figure 2.2 – Mean GVC Participation



Notes: The figure reports mean GVC participation rates for all countries in our sample during the 2018–20 wave. A firm is considered to participate in GVCs if it both exports a positive share of its production and imports (at least) part of its inputs. Authors' calculations based on WBES data (World Bank, 2021).

To identify the link between joining GVCs and the environmental variables introduced above, we rely on firms that switch from not being a two-way trader in the first wave to being one in the second period (*GVC Switcher*). This is our main explanatory variable of interest and the treatment variable in our matching setup described below.¹² According to our baseline definition, approximately 17% of the firms that did not participate in GVCs in 2012–13 started doing so in 2018–20. Moreover, we observe substantial heterogeneity in terms of GVC participation across the countries in our sample. As Figure 2.2 shows, according to the 2018–20

¹¹In the Enterprise Survey questionnaire, firms are asked about the share of their sales that is (i) sold domestically, (ii) indirectly exported and (iii) directly exported. They also provide information on the percentage of their purchases of material inputs or supplies that is of domestic and foreign origin, and whether they import directly or not.

¹²The control group is composed of firms that did not make the switch and therefore do not participate in GVCs in any period in our sample.

data, countries located in Eastern Europe show the highest participation rates in GVCs, although with noticeable differences. For instance, while almost 48% of firms are connected to GVCs in Bosnia and Herzegovina, only around 8% do so in Russia. Among North African countries, Morocco exhibits the highest rate of engagement, whereas Egyptian firms have the lowest participation rate in the region. In Central Asia all countries display rather low participation rates (below 20%).¹³

2.3.2 Empirical Strategy

The main challenge in estimating the effect of joining GVCs on environmental performance is that the decision to participate in such trade networks is hardly independent of firms' characteristics and particularly of their environmental footprint. Given that the group of participating firms is not randomly selected from the entire firm population, a naive comparison between GVC and non-GVC participants is likely contaminated by selection bias. To mitigate this concern, we estimate propensity scores to match firms that are similar in several observable characteristics, but differ with respect to our treatment of interest—i.e., we compare firms that started participating in GVCs in the second wave (switchers or treated firms) with those that did not switch (control firms), but are otherwise similar in terms of observable characteristics (Rosenbaum and Rubin, 1983; Heckman et al., 1997). More precisely, we estimate the treatment probability using a probit model specified as follows:

$$GVCSwitcher_{icsw} = \beta_0 + \mathbf{X}'_{icsw-1}\beta_1 + \gamma_c + \pi_s + \varepsilon_{icsw} \quad (2.1)$$

where $GVCSwitcher_{icsw}$, our treatment variable, takes a value of 1 if firm i , located in country c and active in industry s , did not participate in GVCs in wave $w - 1$, and started doing so in the following period, w . Vector $\mathbf{X}_{icsw-1}$ includes firm characteristics that affect both the participation in GVCs and the environmental outcomes in our analysis. In our baseline specification, we follow Rigo (2021) and include variables to capture whether the firm (in wave $w - 1$) exports (*Exporter*), imports (*Importer*), and/or is foreign-owned (*Foreign-Owned*), as well as the natural logarithm of the number of workers that it employs (*Workers*). Finally, we add country and industry (ISIC rev. 3.1) fixed effects (γ_c and π_s , respectively).

Moreover, we also estimate models with an extended set of covariates, which includes, in addition to those just listed, skill intensity share (*SIS*), firm age (*Firm Age*), and the natural logarithm of total factor productivity (*TFP*).¹⁴ In line with the

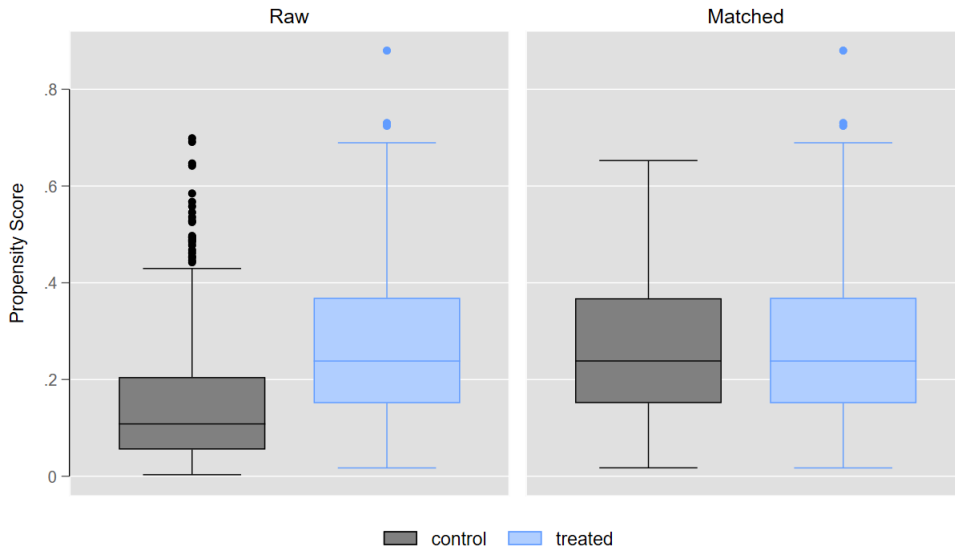
¹³Figure B.1, in the Appendix, shows similar maps for all our dependent variables.

¹⁴We estimate the TFP as in Wooldridge (2009). The results remain largely unchanged when we adopt alternative approaches, such as Petrin et al. (2004) (not shown, but available upon request).

related theoretical and empirical literature, these are the most relevant factors that should affect both the decision to participate in GVCs and subsequently determine the firm's environmental outcomes (e.g., Antràs, 2020; Rigo, 2021). All variables in monetary values are deflated and converted to constant 2015 US dollars.¹⁵ Finally, ε_{icsw} is the error term. In all specifications, we use robust standard errors clustered at the country level to account for potential heteroskedasticity.

We are thus able to compare pairs of firms that have a similar predicted probability of getting treated, although only one of the two ends up receiving treatment. This strategy requires in particular that, conditional on the propensity score, the assignment into treatment is as good as random (conditional independence assumption), and that the distributions of treatment and control groups have common support.¹⁶ Figure 2.3 below shows that treatment and control groups are balanced after matching on the baseline set of covariates. In our baseline estimation, non-GVC participants are matched to neophyte two-way traders according to their propensity scores using the five nearest neighbors.¹⁷

Figure 2.3 – Balance Plot



Notes: Balance plot of control and treatment groups before and after propensity score matching as specified in equation 2.1.

We acknowledge that the inclusion of TFP, which is a generated regressor, may lead to problems of inference, but since it is not part of our baseline set of control variables, and is extensively used in the related literature, we opted to keep it in the extended set of covariates nevertheless.

¹⁵Moreover, to make sure that outliers do not drive our results, we winsorize (at the 99th percentile) all variables that take extreme values. In any case, our conclusions are not affected by this decision.

¹⁶We discuss in more detail whether these conditions are satisfied in our case as well as alternative specifications in Sections 2.4 and 2.5.

¹⁷Table B.4, in the Appendix, reports the probit results. Different matching algorithms yield similar conclusions, as discussed in Section 2.5.

In order to estimate the environmental premium, that is, the difference in environmental performance between GVC and non-GVC firms, we specify the following non-parametric equation:

$$\beta_{ATT} = E[y_{icsw} | \rho, GVC Switcher_{icsw} = 1] - E[y_{icsw} | \rho, GVC Switcher_{icsw} = 0] \quad (2.2)$$

where y_{icsw} denotes the environmental outcome of interest for firm i in wave w , $GVC Switcher_{icsw}$ is the treatment variable, ρ denotes the propensity score estimated in equation 2.1, and β_{ATT} is the average treatment effect on the treated (that is, the difference in performance between matched GVC and non-GVC firms). Not only is the ATT arguably the most relevant policy parameter in our setting, but by focusing on the effect on the treated rather than on the whole population of firms (i.e., the ATE), we are able to weaken the requirements in terms of unconfoundedness and common support, since in this case we “only” need to care about the matching quality for firms in the treatment group (Caliendo and Kopeinig, 2008).

A main limitation of matching is that treatment and control groups can still differ substantially in terms of unobservable characteristics. To address this concern, we complement our analysis with a DID matching estimator (PSD-DID, as in Greenaway and Kneller, 2004; Greenaway et al., 2005; De Loecker, 2007; Yang and Mallick, 2010; Rigo, 2021). We do so exclusively for *EI* because only for this outcome variable the data are available for more than one time period. This enables us to get rid of any unobserved (time-invariant) firm characteristics that may bias the estimation if left unaddressed, because the DID estimator compares the within-group differences in outcomes before and after treatment.¹⁸

The crucial assumption in this case is that, without the treatment, the outcomes for both groups would have followed parallel paths—i.e., there should not be differential pre-trends. Based on (limited) data from a previous WBES wave (2008–09), we do not find evidence, at that stage, of different trends between firms that remained without treatment and those that would become GVC participants later on: the difference in mean *EI* across both groups in 2008–09 is not significant ($p = 0.69$).¹⁹

¹⁸By combining both approaches (PSD-DID), we are able to address one of the major weaknesses of matching methods—namely that they rely only on observable characteristics (Heckman et al., 1997; Blundell et al., 2005).

¹⁹Only a few companies are surveyed consecutively over these three survey waves, and therefore the number of firms for which we have data available throughout the 2008–20 period is quite limited.

2.4 Main Results

2.4.1 Monitoring, Adoption, and Constraints

We begin by presenting PSM results for the nine dependent variables derived from the Green Economy Module (columns 1 to 9 in Table 2.1). For the baseline set of covariates (panel A), all the estimated ATT are positive, and most are statistically significant at conventional levels.

As shown in columns 2 to 6, firms that join GVCs have a significantly higher probability of adopting more environmentally friendly modes of production across all indicators in this dimension. In particular, in terms of energy use, these firms are more likely to adopt climate friendly energy generation on site (8.2pp), adopt measures to improve energy efficiency (7.3pp), and manage energy consumption (11.3pp). Moreover, GVC participation is also linked to a higher probability of controlling air pollution (8.4pp) and minimizing waste (8pp). Considering the mean value of these variables, these are all rather sizeable gains. This implies for instance that, for the average firm, participating in global value chains is associated with almost a twofold increase in the probability of adopting CFE.

The results for external constraints (columns 7 to 9) indicate that firms that join GVCs are more likely to be subject to energy performance standards (7.7pp) and to deal with customers that are more concerned about sustainability (5.9pp). Again, these coefficients are substantial in magnitude. However, we do not observe significant differences in terms of CO₂ monitoring (column 1) nor with regard to attitudes toward environmental regulation (column 8).

Table 2.1 – GVC and Environmental Performance

<i>Dependent Variable:</i>	(1) CO ₂ Monitor	(2) Adopt CFE	(3) Adopt EE	(4) Adopt EM	(5) Adopt Air	(6) Adopt Waste	(7) Subject ES	(8) Obstacle ER	(9) Customers	(10) EI (fd)
Panel A: Baseline Set of Covariates										
ATT (GVC Switcher)	0.019 (0.018)	0.082*** (0.024)	0.073* (0.038)	0.113*** (0.036)	0.084*** (0.027)	0.080** (0.037)	0.077*** (0.027)	0.054 (0.035)	0.059** (0.027)	-0.171* (0.095)
N	1,428	1,342	1,386	1,383	1,337	1,356	1,395	1,346	1,435	469
Panel B: Extended Set of Covariates										
ATT (GVC Switcher)	0.029 (0.020)	0.095*** (0.016)	0.090** (0.041)	0.160** (0.071)	0.183*** (0.010)	0.131** (0.052)	0.024 (0.041)	0.077** (0.036)	0.108*** (0.024)	-0.018*** (0.004)
N	498	454	480	473	448	463	495	481	500	327
Method	PSM	PSM	PSM	PSM	PSM	PSM	PSM	PSM	PSM	PSM-DID

Notes: The table reports the average treatment effect on the treated (ATT), and the associated robust standard errors in parentheses. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. In panel A, firms are matched on the baseline set of variables: (ln) number of employees, exporter dummy, importer dummy, and foreign-owned dummy. In panel B, the extended set of explanatory variables includes, in addition, skill intensity share, firm age, and (ln) TFP. *EI* denotes energy use intensity measured in first differences (fd). All probit specifications include country and sector fixed effects. Variable definitions are listed in Table B.2.

Panel B shows results controlling for the extended set of variables. Although the number of observations is substantially reduced, matching firms on this larger

batch of variables hardly changes our findings, as most coefficients remain positive and statistically significant.²⁰ In fact, the coefficients associated with the measures of adoption of cleaner production methods increase slightly in magnitude. Furthermore, in this case we also find evidence that firms that join GVCs are more likely to consider environmental regulations as an obstacle (7.7pp), although we do not observe significant effects in terms of firms being subject to environmental standards. Finally, we again observe that treated firms tend to have significantly more environmentally demanding customers.

2.4.2 Energy Use Intensity

This subsection discusses the PSM-DID results using the first difference (between waves) of energy use intensity as the dependent variable. Despite the reduced number of observations available in this case—due to the fact that many firms do not report their energy costs in both waves—we again find evidence of GVC participation being linked to better environmental performance (column 10 in Table 2.1). For the baseline set of covariates, firms that start participating in GVCs reduce their energy use intensity by 17.1 points vis-à-vis the control group. Matching firms on the extended set of covariates also yields a negative, but substantially smaller coefficient (1.8-point reduction in energy use intensity).²¹

All in all, the results in Table 2.1 consistently indicate that switching into GVC participation is associated with better environmental practices at the firm level. There is not a single dimension in which the treatment is negatively correlated to firms' performance. In particular, given the evidence for all of the adoption indicators as well as for the reduction of energy use intensity, we interpret these findings as a sign that GVCs might facilitate the diffusion of greener practices and technologies throughout these networks. The extent to which this is driven by firms facing stricter regulation and/or more pressure to do so is a matter that we examine below in Section 2.6. Another possible explanation is that firms have more opportunities to learn about better practices once they get connected to GVCs. Given that the time span between the two waves is relatively short—between 5 and 8 years, depending on when firms were interviewed—and that (treated) firms may

²⁰Given the difference between the samples in panels A and B, we also run the baseline specifications (from panel A) on the smaller samples (of panel B). Also in this case the results are fairly similar and available upon request.

²¹Estimating the model specification that includes only the baseline set of control variables on the subsample of the data that also contains information on the extended set of covariates yields a negative and statistically significant coefficient of interest, although of smaller magnitude (3.5-point reduction, $p = 0.00$)

have switched at any point in between, we consider this possibility less likely.²²

Although we cannot completely rule out the issue of self-selection, our empirical strategy arguably alleviates this concern. First, by focusing on the switch into GVCs (and not on the participation status *per se*), we make sure that control and treatment groups have the same GVC status in the baseline period. Furthermore, by matching these firms, we compare pairs that are similar across many important characteristics, such that self-selection would need to take place via unobserved characteristics that are uncorrelated with the variables used for the estimation of the propensity score. In the case of PSM-DID, unobserved heterogeneity would then need to vary over time in order to be a threat for the identification. These and other issues regarding the robustness of our results are addressed in more detail in Section 2.5 below.

2.5 Robustness

This section tests the robustness of our results to modifications in the matching algorithm and in the definition of GVC participation, and checks whether our results are representative for a larger sample of firms. Additionally, we conduct a placebo estimation, and also construct indices of environmental performance to aggregate our different outcome variables. Finally, we shed more light on the issue of empirical identification by making use of a heteroskedasticity-based instrumental variable approach.

2.5.1 Matching Algorithm

First, we replicate the main results, obtained with nearest-neighbor matching, using alternative matching algorithms (Table 2.2). In panel A, we again match firms to their five nearest neighbors, but now restrict the matching to observations in the common support. Because there are only a few out-of-support cases, the number of observations hardly changes across most columns, such that the coefficients remain largely unaltered.

The models in Panels B and C, which also impose common support, use caliper and kernel matching, respectively. Overall, the coefficients retain the expected sign, and most results are significant at conventional levels. Therefore, our main findings do not hinge on the choice of the matching algorithm.

²²Accordingly, there is little empirical evidence that international trade improves firms' learning opportunities (e.g., Melitz, 2003; Wagner, 2007; Wagner, 2012; Mazzi et al., 2021).

Table 2.2 – Matching Algorithm

<i>Dependent Variable:</i>	(1) CO ₂ Monitor	(2) Adopt CFE	(3) Adopt EE	(4) Adopt EM	(5) Adopt Air	(6) Adopt Waste	(7) Subject ES	(8) Obstacle ER	(9) Customers	(10) EI (fd)
Panel A: Common Support (NN5)										
ATT (GVC Switcher)	0.022 (0.021)	0.083*** (0.025)	0.089** (0.036)	0.112*** (0.036)	0.094*** (0.027)	0.075** (0.037)	0.084*** (0.024)	0.051 (0.034)	0.063** (0.030)	-0.184* (0.105)
N	1,425	1,339	1,384	1,381	1,336	1,354	1,393	1,342	1,431	462
Panel B: Common Support (Caliper)										
ATT (GVC Switcher)	0.019 (0.024)	0.076** (0.039)	0.032 (0.052)	0.105* (0.054)	0.068 (0.042)	0.024 (0.052)	0.045 (0.040)	0.083 (0.053)	0.096** (0.038)	-0.217* (0.131)
N	1,428	1,342	1,386	1,383	1,337	1,356	1,395	1,346	1,435	469
Panel C: Common Support (Kernel)										
ATT (GVC Switcher)	0.025 (0.018)	0.088*** (0.029)	0.078** (0.038)	0.119*** (0.038)	0.073** (0.032)	0.081** (0.038)	0.059** (0.030)	0.060 (0.041)	0.066** (0.030)	-0.187 (0.120)
N	1,428	1,342	1,386	1,383	1,337	1,356	1,395	1,346	1,435	469
Method	PSM	PSM	PSM	PSM	PSM	PSM	PSM	PSM	PSM	PSM-DID

Notes: The table reports the average treatment effect on the treated (ATT), and the associated robust standard errors in parentheses. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Firms are matched on the (ln) number of employees, exporter dummy, importer dummy and foreign-owned dummy. Panel A shows results for nearest neighbor matching imposing common support. Specifications in panel B impose common support and restrict matches to caliper = 0.2sd. Panel C shows results for Epanechnikov kernel matching for observations in the common support. All probit specifications include country and industry fixed effects. Variable definitions are listed in Table B.2.

2.5.2 GVC Definition

Next, we test the extent to which our results are sensitive to different definitions of GVC participation (Table 2.3). Defining our treatment in a stricter way—requiring that, in addition to becoming two-way traders, firms also hold an internationally-recognized quality certification—our previous conclusions remain valid.²³ As in Section 2.4, we also find that entering GVCs is positively and significantly associated with most environmental indicators (panel A). In fact, we find positive effects not only—as before—on all adoption measures and external constraints, but also on pollution monitoring: firms that become two-way traders and are internationally certified are 6.7pp more likely to monitor CO₂ consumption along their supply chain. A plausible interpretation is that this stricter definition represents an increase in treatment intensity. In this sense, it is not surprising that most coefficients increase in magnitude and accuracy.²⁴ Notably, however, in this case we do not find a significant improvement in *EI*—even though the coefficient still has the expected negative sign.

We also present the results for more lenient treatment definitions, in which we only require firms to start exporting (panel B) or importing (panel C). As previously discussed, simultaneously exporting and importing is arguably a constitutive feature

²³Table B.5, in the Appendix, shows results using (lagged) GVC participation *per se*, instead of *GVC Switcher* as the main explanatory variable of interest. Although we find this alternative specification less appealing in terms of identification, this exercise yields similar conclusions. Note that in this case we cannot add exporter and importer dummies to the probit model, otherwise we would perfectly predict the treatment variable (by definition).

²⁴According to this stricter criterion, only around 7% of the firms in our sample receive treatment.

Table 2.3 – Alternative GVC Definitions

<i>Dependent Variable:</i>	(1) CO ₂ Monitor	(2) Adopt CFE	(3) Adopt EE	(4) Adopt EM	(5) Adopt Air	(6) Adopt Waste	(7) Subject ES	(8) Obstacle ER	(9) Customers	(10) EI (fd)
Panel A: Strong GVC										
ATT (GVC Strong Switcher)	0.067*** (0.023)	0.129*** (0.027)	0.105** (0.047)	0.240*** (0.036)	0.095*** (0.031)	0.244*** (0.041)	0.135*** (0.017)	0.079* (0.041)	0.164*** (0.040)	-0.199 (0.137)
N	1,522	1,414	1,485	1,465	1,402	1,411	1,454	1,451	1,533	424
Panel B: Exporter Only										
ATT (Export Switcher)	0.048*** (0.014)	0.072*** (0.027)	0.019 (0.037)	0.067** (0.033)	0.046 (0.028)	0.036 (0.037)	0.039* (0.021)	0.056* (0.032)	0.030 (0.030)	-0.001 (0.018)
N	1,239	1,164	1,191	1,191	1,154	1,163	1,198	1,177	1,239	427
Panel C: Importer Only										
ATT (Import Switcher)	0.010 (0.014)	0.058** (0.023)	0.114*** (0.035)	0.121*** (0.021)	0.061** (0.028)	0.062** (0.027)	0.034 (0.027)	0.018 (0.046)	0.114*** (0.023)	-0.170*** (0.060)
N	797	771	775	789	762	770	789	743	808	222
Method	PSM	PSM	PSM	PSM	PSM	PSM	PSM	PSM	PSM	PSM-DID

Notes: The table reports the average treatment effect on the treated (ATT), and the associated robust standard errors in parentheses. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Firms are matched on the (ln) number of employees, exporter dummy, importer dummy and foreign-owned dummy. All probit specifications include country and industry fixed effects. Variable definitions are listed in Table B.2.

of being inserted in GVCs. Firms that either only export or only import may indeed be part of global production chains, but may as well be, e.g., firms that have all of their production in one location and simply export the final product afterwards. As expected, the magnitude of most coefficients is substantially reduced, but we still observe significant coefficients across several dimensions: firms that begin to export (import) show significantly better performance in five (seven) of ten indicators.²⁵

2.5.3 Expanded Sample

For identification purposes, in most of our analysis we make use of lagged explanatory variables. This means that firms for which information from the previous wave is not available are excluded from our estimations. A potential concern is that the remaining firms might not be representative of the full sample, which could lead to survivorship bias, for instance. To check whether our findings are in fact hold for the universe of firms in the WBES data, Table 2.4 reports PSM results from an alternative empirical strategy that allows us to keep a much larger number of firms in our sample.²⁶ It is reassuring that our conclusions remain largely unchanged—both for the baseline as well as for the extended set of covariates (panels A and B, respectively).

²⁵The fact that exporting firms tend to perform better than their peers has been recurrently observed in the literature. For more information, see, e.g., Bernard and Jensen (1999), Melitz (2003), Wagner (2007), and Wagner (2012).

²⁶Note that current exporter and importer dummies cannot be included in the underlying probit model in this case—otherwise we would, by definition, perfectly predict the treatment variable (current GVC participation). Besides, we do not include *EI* as a dependent variable in these exercises, since it is defined in first differences, thus requiring that firms appear in both waves of the survey. We are grateful to an anonymous reviewer for pointing out this concern.

Table 2.4 – Expanded Sample

<i>Dependent Variable:</i>	(1) CO ₂ Monitor	(2) Adopt CFE	(3) Adopt EE	(4) Adopt EM	(5) Adopt Air	(6) Adopt Waste	(7) Subject ES	(8) Obstacle ER	(9) Customers
Panel A: Baseline Set of Covariates									
ATT (GVC)	0.020 (0.012)	0.041*** (0.014)	0.094*** (0.016)	0.097*** (0.017)	0.071*** (0.016)	0.095*** (0.015)	0.024 (0.016)	-0.001 (0.012)	0.070*** (0.015)
Observations	21,123	19,411	20,616	20,056	19,254	19,836	20,603	20,607	21,223
Panel B: Extended Set of Covariates									
ATT (GVC)	0.018 (0.012)	0.075*** (0.015)	0.174*** (0.022)	0.157*** (0.029)	0.109*** (0.015)	0.144*** (0.036)	0.022 (0.028)	0.059 (0.040)	0.057*** (0.019)
N	5,849	5,450	5,718	5,654	5,500	5,687	5,805	5,864	5,879

Notes: The table reports the average treatment effect on the treated (ATT) and the associated robust standard errors in parentheses. The treatment group is defined according to current GVC participation. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. In panel A, firms are matched on the baseline set of variables: (ln) number of employees and foreign-owned dummy. In panel B, the extended set of explanatory variables includes, in addition, skill intensity share, firm age, and (ln) TFP. All probit specifications include country and industry fixed effects. Variable definitions are listed in Table B.2.

2.5.4 Placebo Tests

In order to increase our confidence that we identify a meaningful link between GVC participation and environmental performance, we also test whether we find significant changes in three unrelated (“placebo”) dependent variables: top manager’s years of experience working in the sector (*Manager*), and whether the firm considers either tax rates (*Obstacle Tax*) or labor regulation (*Obstacle Labor*) an obstacle. We do not expect GVC participation to influence any of the three variables. Otherwise, our results could simply be reflecting some spurious correlations—for instance, that firms joining GVCs are becoming better overall and/or facing more demanding constraints in general. Reassuringly, as reported in Table B.6 in the Appendix, we do not find significant coefficients for any of the three alternative outcome variables.

2.5.5 Environmental Index

Given that we deal with a rather large number of outcome variables in our analysis, an issue could be the extent to which they are correlated with one another. A cross-correlation matrix of the ten dependent variables shows that for only one pair (*Adopt EE* and *Adopt EM*, unsurprisingly) the coefficient is above .50—with the remaining coefficients being considerably lower in magnitude.²⁷ Besides, a principal component analysis yields three different components with an eigenvalue above 1, which together account for only around half of the total variance. We further observe that the variances of most of our dependent variables are rather unique, indicating that it is arguably justified to treat them separately.

For completeness, we present in this subsection an additional robustness check that reduces the number of outcome variables. We aggregate the nine binary out-

²⁷See Table B.7, in the Appendix, for more details.

come variables into three different indices: the (i) Environmental Composite Index (*ECI*), which is the average across all nine of the variables, as well as the (ii) Adoption Composite Index (*ACI*) and the (iii) Constraint Composite Index (*CCI*), which are the averages within each of the respective dimensions—that is, *ACI* considers only the five adoption variables, and *CCI* only those three related to external constraints.²⁸ By construction, all indices vary between 0 and 1, with higher values indicating higher environmental compliance.

Table 2.5 – Composite Indices

<i>Dependent Variable:</i>	ECI	ACI	CCI
ATT (GVC Switcher)	0.059*** (0.019)	0.069*** (0.022)	0.051** (0.021)
N	1,091	1,217	1,284

Notes: The table reports the average treatment effect on the treated (ATT), and the associated robust standard errors in parentheses. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Firms are matched on the (ln) number of employees, exporter dummy, importer dummy and foreign-owned dummy. All probit specifications include country and industry fixed effects. Variable definitions are listed in Table B.2.

Table 2.5 shows that using each of these indices as the outcome variable of our baseline specification (equation 2.2) validates our previous findings. Firms that become GVC participants score on average 0.059 points higher on the overall index of environmental performance—about one third of the standard deviation. Moreover, these firms also perform better in terms of the adoption and the constraint composite indices (by 0.069 and 0.051, respectively, on average).

2.5.6 Instrumental Variable Approach

To complement our previous matching analysis and shed more light on the existence of a potential causal relationship between global value chains and environmental performance, we exploit the exogenous variation in the geographic location of firms to implement a heteroskedasticity-based instrumental variable approach with both external as well as generated instruments. Given that the geographic location of a firm is arguably largely unrelated to its environmental performance and is not determined by its current trading strategy, but rather by a number of other factors—such as input availability, climate, infrastructure, as well as important historical

²⁸More precisely, *ECI* takes the average across the following variables: *CO₂ Monitor*, *Adopt CFE*, *Adopt EE*, *Adopt EM*, *Adopt Air*, *Adopt Waste*, *Subject ES*, *Obstacle ER*, and *Customers*. *ACI* includes only *Adopt CFE*, *Adopt EE*, *Adopt EM*, *Adopt Air*, and *Adopt Waste*, while *CCI* takes *Subject ES*, *Obstacle ER*, and *Customers* into account. Note that it is not necessary to create an index for the monitoring dimension, as it consists of a single variable (*CO₂ Monitor*).

factors—we use the inverse of the firm's straight-line distance to the coast (*Dist*) as an (external) instrument for GVC participation.²⁹

Already in the 18th century, Adam Smith (1776) recognized the importance of access to the sea for trading.³⁰ Since then, several scholars have examined the importance not only of proximity to the coast but also of geographical distance, in general, for international commerce (e.g., Frankel and Romer, 1999; Faye et al., 2004; Feyrer, 2021). In short, being closer to the coast and other markets tends to facilitate trade. In our context, we also expect it to be easier (*ceteris paribus*) for firms closer to the sea to connect to global value chains.³¹

In this case, our set of control variables includes binary indicators to capture whether a firm exports (*Exporter*), imports (*Importer*), is foreign-owned (*Foreign-Owned*), the natural logarithm of the number of workers the firm employs (*Workers*), as well as a dummy variable (*EU*) that indicates whether the firm is located in a member country of the European Union, thus accounting for the different regulatory framework.³²

Because we acknowledge that a firm's location may still be influenced by other (unobserved) factors that we cannot control for and that it may affect environmental indicators through channels other than GVC participation, our estimation supplements the external instrument with instruments generated in a modified GMM setup to further improve identification (Lewbel, 2012).³³

By making use of Lewbel's (2012) two-stage procedure, instead of having to rely on a standard exclusion restriction, one can exploit heteroskedasticity for identification, as long as the regressors are uncorrelated with the product of heteroscedastic errors.³⁴ The empirical strategy then consists of using, in addition to the external instrument (*Dist*), generated instruments (*Z*) obtained using the residuals from the auxiliary regression in the following manner:

²⁹We thank the World Bank's Enterprise Analysis Unit for sharing with us firms' (masked) geographic coordinates.

³⁰Although the use of air transport is becoming more widespread also for trading, ocean freight shipping is still the primary mode of transport—particularly for low and middle-income countries (Feyrer, 2019).

³¹The average firm in our sample is located 353km away from the sea, but this distance increases to almost 2000km in some cases. Alternatively, one could also calculate road distances and/or distances to the nearest port as a more precise measure of access to the sea. These alternatives, however, likely suffer from more severe endogeneity concerns.

³²We further examine the role of being part of the European Union in Section 2.6.

³³See Lewbel (2012) for more details on the method, and Emran and Hou (2013) for an example of a similar application.

³⁴A potential caveat is that, since the identification is based on higher moments, it may be noisier than those based on standard exclusion restrictions.

$$Z_j = (X_j - \bar{X})\xi \quad (2.3)$$

where X is the vector of exogenous variables described above (which is mean-centered), and ξ is the vector of residuals from the first-stage estimation. Results from Lewbel's second-stage regressions for each of our ten environmental performance indicators are shown in Table 2.6.

Table 2.6 – Heteroskedasticity-based Instruments (2nd Stage)

Dependent Variable:	(1) CO ₂ Monitor	(2) Adopt CFE	(3) Adopt EE	(4) Adopt EM	(5) Adopt Air	(6) Adopt Waste	(7) Subject ES	(8) Obstacle ER	(9) Customers	(10) EI
GVC	0.041 (0.056)	0.121 (0.092)	0.056 (0.115)	0.344*** (0.106)	0.182** (0.093)	0.259** (0.101)	0.015 (0.095)	0.012 (0.108)	0.112 (0.091)	-0.022 (0.022)
Exporter	0.023 (0.016)	0.051** (0.026)	0.086** (0.040)	-0.003 (0.037)	0.002 (0.030)	-0.033 (0.037)	0.084*** (0.032)	0.103** (0.045)	0.034 (0.030)	0.014 (0.015)
Importer	0.012 (0.009)	0.025 (0.016)	0.078*** (0.026)	0.047* (0.026)	0.022 (0.020)	0.037 (0.027)	-0.011 (0.019)	-0.048 (0.030)	0.003 (0.019)	-0.011 (0.009)
Foreign-Owned	-0.003 (0.020)	-0.016 (0.034)	0.034 (0.059)	0.148** (0.061)	0.059 (0.053)	0.069 (0.062)	0.041 (0.046)	0.005 (0.062)	-0.032 (0.037)	-0.017 (0.025)
Workers (log)	0.005 (0.004)	0.028*** (0.007)	0.050*** (0.011)	0.051*** (0.011)	0.050*** (0.008)	0.029*** (0.011)	0.010 (0.008)	0.005 (0.012)	0.026*** (0.008)	-0.006* (0.003)
EU	-0.015 (0.010)	0.020 (0.026)	0.055 (0.039)	0.008 (0.039)	0.030 (0.030)	0.270*** (0.044)	-0.004 (0.029)	0.081* (0.041)	-0.005 (0.027)	-0.023*** (0.007)
N	1,428	1,359	1,386	1,391	1,337	1,360	1,395	1,346	1,435	768
KP F-stat	71.48	45.67	68.36	45.98	53.78	58.52	68.45	60.45	72.88	90.49
J test (p-value)	0.09	0.68	0.42	0.20	0.19	0.60	0.15	0.05	0.41	0.32

Notes: Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses. Model with generated as well as external instruments. External instrumental variable: Inverse distance. First-stage Kleibergen-Paap F-statistic and the p-value associated with Hansen's J test reported. Variable definitions are listed in B.2.

We again find that joining global value chains is associated with better environmental indicators. This time, however, we only find statistically significant coefficients for a narrower set of outcomes: firms connected to GVCs are more likely to adopt better energy and waste management procedures as well as measures to control air pollution. With regard to the instruments, the Kleibergen-Paap F-statistics are above conventional thresholds of instrument relevance (Stock and Yogo, 2005), and the Hansen's J statistics are mostly supportive of instrument validity.³⁵

2.6 Mechanisms

In this section, to improve our understanding of the mechanisms that drive our findings, we present separate analyses by firm location and broad sectors of economic activity, and explore the role of external constraints as a potential channel.

³⁵Table B.8, in the Appendix, shows first-stage results. As expected, inverse distance is positively and significantly correlated with GVC participation. The same is the case for the heteroskedasticity-based instruments (0000G to 0000K) that result from interacting the five centered explanatory variables with the auxiliary residuals obtained from the auxiliary regression described in the text.

2.6.1 Location

Firms in different parts of the world often operate in very different circumstances. In particular, they usually have to deal with specific environmental regulations and cater to customers with different preferences and expectations. If these forces are indeed important drivers of sustainable firm behavior—i.e., firms have more incentives to incur high costs to improve their environmental record if their regulators and customers care about it—we should expect to see the larger benefits of GVC participation for firms that operate in less demanding domestic markets.³⁶

To test this in our sample, we split firms according to whether they are based in member countries of the European Union (EU), as we expect that EU-based firms—even if they do not participate in GVCs—already face stricter regulation and more demanding customers to begin with. This is precisely the pattern reported in Table 2.7. Despite the relatively small sample, estimating our baseline model only for EU firms (panel A), we find fewer dimensions of environmental performance that benefit from GVC participation. In particular, in this case we do not observe a significant change in the likelihood of being subject to energy standards, deeming environmental regulation an obstacle, or serving more environmentally conscious customers. That is, none of the coefficients for our measures of external constraints is significant for firms based in the EU. Nevertheless, we still observe that these companies have a higher probability of monitoring CO₂ emissions and adopting measures to control air pollution.

Table 2.7 – Firm Location

<i>Dependent Variable:</i>	(1) CO ₂ Monitor	(2) Adopt CFE	(3) Adopt EE	(4) Adopt EM	(5) Adopt Air	(6) Adopt Waste	(7) Subject ES	(8) Obstacle ER	(9) Customers	(10) EI (fd)
Panel A: EU Countries										
ATT (GVC Switcher)	0.167*** (0.062)	0.084 (0.095)	-0.014 (0.138)	0.132 (0.123)	0.319*** (0.056)	-0.071 (0.098)	-0.080 (0.055)	-0.006 (0.090)	0.028 (0.051)	0.023 (0.022)
N	147	105	146	131	131	137	142	148	148	29
Panel B: Non-EU Countries										
ATT (GVC Switcher)	-0.004 (0.021)	0.083*** (0.028)	0.110*** (0.042)	0.128*** (0.042)	0.055* (0.029)	0.053 (0.040)	0.071** (0.030)	0.041 (0.044)	0.074*** (0.024)	-0.198* (0.113)
N	1,266	1,218	1,224	1,238	1,192	1,203	1,226	1,181	1,270	426
Method	PSM	PSM	PSM	PSM	PSM	PSM	PSM	PSM	PSM	PSM-DID

Notes: The table reports the average treatment effect on the treated (ATT), and the associated robust standard errors in parentheses. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Firms are matched on the (ln) number of employees, exporter dummy, importer dummy and foreign-owned dummy. All probit specifications include country and industry fixed effects. Variable definitions are listed in Table B.2.

For non-EU firms (panel B), we find rather contrasting results. In line with the argument that they face less scrutiny domestically, we see that GVC participation is significantly associated with the presence of environmental constraints—both in

³⁶Ideally, one could examine the institutional features of each country to which firms export to or import from. This information is unfortunately not available for the firms in our sample.

terms of energy regulation and customer standards. Moreover, these firms are more likely to adopt greener technologies (with the exception of waste minimization), and show a significant improvement in energy efficiency.

2.6.2 External Constraints

In our main results, we treat external constraints (*Subject ES*, *Obstacle ER* and *Customers*) as outcome variables similarly to measures of technology adoption and pollution monitoring. Nevertheless, as discussed in the comparison between EU and non-EU firms, features such as environmental regulation and customers' sustainability concerns can act as important channels to push firms that switch into GVC participation to adopt greener technologies and/or put more efforts into monitoring pollution.

Although we risk running into the issue of “bad controls” (Angrist and Pischke, 2009) and cannot test these channels in a more rigorous setting, we consider it worthwhile to explore such considerations in a basic OLS setup. Therefore, we regress our measures of monitoring and adoption on the treatment variable as well as on the three constraint indicators and their interaction with *GVC Switcher*. We include, additionally, the usual set of control variables as well as country and industry fixed effects.³⁷

Table 2.8 shows the results for three different adoption indicators: *Adopt CFE*, *Adopt EM*, and *Adopt Air*.³⁸ For these variables, our results suggest that firms are more likely to introduce these innovations only if their regulators and/or customers demand it. In fact, in some specifications, we find that GVC participation on its own may even make it less likely for firms to adopt greener technologies. It is only for companies that face stricter standards that the overall effect of GVC participation is positive. These exploratory findings point to the importance of proper regulation to ensure that participation in global value chains creates the correct incentives for firms to improve their environmental performance.

2.6.3 Sectoral Differences

Lastly, we turn our attention to the sector in which firms operate. Table 2.9 presents the results for firms in the manufacturing and service sectors. As the dependent variables of interest are arguably more relevant to firms in the manufacturing sector,

³⁷Unlike the PSM models, the OLS model does not include dummies for exporter and importer, as they are highly correlated with *GVC Switcher*.

³⁸Similar results for the other dependent variables are reported in Tables B.9 to B.11 in the Appendix.

Table 2.8 – External Constraints

	(1)	(2)	(3)	(4)	(5)
Panel A – <i>Dependent Variable: Adopt CFE</i>					
GVC Switcher	0.102** (0.050)	0.008 (0.026)	-0.074 (0.057)	0.011 (0.028)	-0.167*** (0.060)
× Subject ES		0.410*** (0.145)			0.295** (0.133)
× Obstacle ER			0.221** (0.087)		0.183*** (0.068)
× Customer				0.500*** (0.144)	0.310** (0.134)
N	1,226	1,226	1,226	1,226	1,226
Panel B – <i>Dependent Variable: Adopt EM</i>					
GVC Switcher	0.153 (0.096)	0.171 (0.116)	-0.086 (0.100)	0.098 (0.111)	-0.028 (0.091)
× Subject ES		-0.078 (0.187)			-0.211 (0.152)
× Obstacle ER			0.302** (0.148)		0.207 (0.153)
× Customer				0.305** (0.154)	0.359** (0.178)
N	1,254	1,254	1,254	1,254	1,254
Panel C – <i>Dependent Variable: Adopt Air</i>					
GVC Switcher	0.044 (0.041)	-0.010 (0.037)	-0.034 (0.047)	0.001 (0.034)	-0.095** (0.045)
× Subject ES		0.229** (0.096)			0.190* (0.100)
× Obstacle ER			0.098 (0.067)		0.092 (0.056)
× Customer				0.227* (0.123)	0.113 (0.134)
N	1,208	1,208	1,208	1,208	1,208
Country FE	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes
Control	Yes	Yes	Yes	Yes	Yes

Notes: Robust standard errors in parentheses. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Control variables not shown in the table: Subject ES, Obstacle ER, Customer, (ln) number of employees, and foreign-owned dummy. Country and industry fixed effects included in all specifications. Variable definitions are listed in Table B.2.

it is unsurprising that they drive most of our results, particularly in terms of monitoring, technology adoption, and energy use intensity. Moreover, in all cases, the coefficients go in the expected direction—that is, treated firms show better environmental performance than their non-treated counterparts.

In the service sector, treated firms are still more likely to adopt better measures of energy management, and to be subject to energy standards. However, such results

should be interpreted with caution, as the number of observations is rather limited.

Table 2.9 – Firm Sector

<i>Dependent Variable:</i>	(1) CO ₂ Monitor	(2) Adopt CFE	(3) Adopt EE	(4) Adopt EM	(5) Adopt Air	(6) Adopt Waste	(7) Subject ES	(8) Obstacle ER	(9) Customers	(10) EI (fd)
Panel A: Manufacturing										
ATT (GVC Switcher)	0.027*** (0.007)	0.093*** (0.024)	0.075* (0.043)	0.104*** (0.037)	0.058** (0.027)	0.096** (0.043)	0.028 (0.030)	0.042 (0.040)	0.056* (0.033)	-0.189*** (0.065)
N	1,254	1,188	1,217	1,221	1,182	1,211	1,225	1,183	1,260	386
Panel B: Services										
ATT (GVC Switcher)	0.016 (0.034)	-0.070 (0.130)	0.064 (0.057)	0.130*** (0.050)	0.029 (0.080)	-0.168 (0.139)	0.139* (0.082)	0.113 (0.128)	0.017 (0.025)	-0.009 (0.027)
N	92	69	89	70	66	59	90	89	91	22
Method	PSM	PSM	PSM	PSM	PSM	PSM	PSM	PSM	PSM	PSM-DID

Notes: The table reports the average treatment effect on the treated (ATT), and the associated robust standard errors in parentheses. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Firms are matched on the (ln) number of employees, exporter dummy, importer dummy and foreign-owned dummy. All probit specifications include country and industry fixed effects. Variable definitions are listed in Table B.2.

2.7 Conclusion

In this chapter, we present evidence that firms that join GVCs perform better across several environmental indicators. Using new data for firms included in the World Bank Enterprise Survey, we observe that companies that start producing within GVCs are more likely to adopt cleaner production practices, face stricter external constraints—such as environmental regulation and standards—and, in some cases, also monitor CO₂ emissions.

Based on firm-level data, we provide the first comprehensive assessment of the implications of internationalizing production chains for firms' environmental performance. As such, given the pressing need to curb emissions and make production cleaner (IPCC, 2018), our findings indicate that increasing firms' exposure to international buyers and suppliers may contribute to achieving those goals. This is in line with the broader literature, which documents a positive link between trade and environmental performance at the country level (e.g., Antweiler et al., 2001; Frankel and Rose, 2005).

We find that joining global value chains is linked to better environmental practices particularly for firms that otherwise operate in less regulated domestic markets, such as those based in non-EU countries. That is, places that often have laxer environmental regulations, and in which customers (or importers of intermediate goods) less frequently require environmental standards. In fact, we find evidence that GVC participation is only associated with improvements in environmental performance if firms face stricter regulation and/or pressure from their buyers. Therefore, to ensure that global value chains contribute to firms becoming more responsive to environ-

mental concerns, it is paramount that the internationalization of supply chains is accompanied by proper environmental regulation.³⁹

Nevertheless, there are several questions that our study cannot address. First, the firms included in our sample may not be representative of those operating in low-income countries. Consequently, further work is needed to investigate the extent to which our results can be extrapolated to different settings. Second, we do not have information on the origin and destination of import and export flows. This is potentially an important determinant of the consequences of GVC participation for the environment, as firms that come to face international buyers and suppliers based in more environmentally conscious countries are likely to benefit the most—in environmental terms—from joining global supply chains. Additionally, matching firms based on their propensity score does not account for the fact that there may still be selection based on unobservable characteristics. Even though we complement our analysis with an instrumental variable approach and our PSM-DID results for energy use intensity are robust to selection on unobservable factors that do not vary over time—and thus at least mitigate these concerns—future work must be done to ensure causal identification.

Moreover, our study does not capture important environmental externalities, at the aggregate level, associated with more geographically-scattered supply chains, such as higher emissions from longer shipping routes (e.g., Gallagher, 2009; World Bank, 2020), that should certainly be considered to determine the overall impact of GVCs on the environment. Furthermore, the beneficial changes that we document are likely driven by improvements in technique, but do not necessarily imply improvements in the overall scale of emissions (Grossman and Krueger, 1995). Besides, a substantial share of transactions within global supply chains is centered around the exchange of commodities, such as oil and grains, which have a considerable impact on the environment (Lambin et al., 2018). This, however, is not addressed in our study, which covers only firms in the manufacturing and service sectors, and therefore deserves more attention in future research.

Finally, since we focus on the international trade dimension of GVCs, more work needs to be done to understand the influence of relational components, such as firms' position within GVCs and the chain's governance structure, on firms' environmental practices. GVCs based on relational linkages may be especially conducive to the

³⁹Although we are mostly thinking of customers as other firms down the supply chain, we believe that companies may also benefit from being exposed—even if indirectly—to more demanding final consumers, as their preferences can arguably be transmitted all the way up the chain. Besides, as our sample consists mainly of middle-income countries, the average firm in our data is probably not situated at the end of the supply chain, where most of the value added comes from (Antràs, 2020). Therefore, the firms in our data set are particularly likely to benefit from GVC participation.

diffusion of better environmental practices, as they are more likely to encourage knowledge and technology transfers across firms.

Chapter 3

The Speed of Aid: Strategic Urgency in International Emergency Relief

Joint work with Andreas Fuchs

Abstract

Timely assistance is a precondition for effective emergency relief in the aftermath of natural disasters. This chapter shows that donor countries take faster aid decisions if they have stronger strategic interests at stake. We analyze a trilateral panel (donor, donor, recipient) of daily humanitarian aid decisions of 43 donor countries following 516 fast-onset natural disasters between 2000 and 2022. Identification relies on daily variation in donor responses and a series of multidimensional fixed effects. Our analysis reveals a bandwagon effect as donors follow their peers' commitments. This is largely explained by trade competition: the more donors compete over export and import markets, the faster they react to each other. The results are driven by government-to-government aid and underscore the importance of recipient-specific lead donors, who are natural first movers. These findings suggest that commercial competition can distort emergency relief and highlight that strategic interests shape even ostensibly altruistic behavior in international humanitarian aid.

Keywords: humanitarian assistance, disaster relief, aid speed, donor competition, United Nations, emergency appeals, trade competition

JEL Codes: F35, F42, F53, H12, H84, O19, Q54

3.1 Introduction

More than 7,300 natural and man-made disaster events occurred around the globe since 2000. They caused 1.23 million casualties, affected the livelihoods of more than 4 billion people, and accumulated economic costs of almost USD 3 trillion (CRED & UNDRR, 2020). To cope with these catastrophes, countries often require international humanitarian assistance. Importantly, when earthquakes strike or volcanoes erupt, a difference of a couple of days—if not hours—in the decision to provide aid can save lives and significantly reduce welfare losses (e.g., Clarke and Dercon, 2016; Pople et al., 2021). Accordingly, the international community recognizes speed as an essential element in ensuring the effectiveness of emergency relief. This is formalized in the Principles of Good Humanitarian Donorship (GHD), which emphasize rapid assistance, aiming for “flexible and timely funding” (Principle 5) and “readiness to offer support to the implementation of humanitarian action” (Principle 17).¹

Individual donors also consistently highlight rapid response as a top priority. The United States Agency for International Development (USAID), for example, declares that “[a]ll efforts must be made to ensure that timely and appropriate assistance is efficiently delivered to the neediest victims” (USAID, 2020, p. 4). When needed, USAID’s Office of US Foreign Disaster Assistance (OFDA) claims to be able to send a response team “within hours of an emergency.”² Therefore, given the importance of a fast response to disasters, it is crucial to evaluate humanitarian aid activities not only based on their financial volume and distribution (Eisensee and Strömberg, 2007; Strömberg, 2007; Raschky and Schwindt, 2012; Eichenauer et al., 2020; Pathak and Schündeln, 2022), but also on the response’s timeliness.

Despite donors’ penchant for portraying humanitarian aid as strictly need-based, strategic (especially commercial and geopolitical) donor interests play a considerable role in allocation decisions, both across and within countries (Drury et al., 2005; Fink and Redaelli, 2011; Annen and Strickland, 2017; Bommer et al., 2022). Therefore, also the speed with which donors react to emergencies may be driven not solely by disaster impact and humanitarian concerns. In fact, taking into consideration

¹The list of the 24 Principles and Good Practice of Humanitarian Donorship is available at <https://www.ghdinitiative.org/ghd/gns/principles-good-practice-of-ghd/principles-good-practice-ghd.html> (accessed October 2024). Most of the 43 donors that endorse this initiative are included in our empirical analysis.

²See the OFDA’s fact sheet, available at https://web.archive.org/web/20250201154931/https://www.usaid.gov/sites/default/files/2022-05/OFDA_Fact_Sheet.pdf (accessed October 2024). The document was removed following the near-total freeze on foreign aid under the Trump administration. To provide another example, Ireland’s Department of Foreign Affairs emphasizes its goal to “respond effectively, efficiently and in a timely manner to the humanitarian needs of crisis affected peoples” (Irish Aid, 2009, p. 13).

previous work on donor coordination—or lack thereof (Aldasoro et al., 2010; Fuchs et al., 2015; Davies and Klasen, 2019; Asmus-Bluhm et al., 2025)—, we expect commercial and geopolitical competition between donors to be a particularly important threat to a timely, and thus effective, aid provision. This aligns with Clarke and Dercon (2016, p. 4), who criticize that the global humanitarian system, with its current emphasis on post-disaster management, favors a process of decision-making that may have become “far too politicized, leading to delays, poor decisions, and bad coordination.”

Considering how little we know about the speed of aid, we first analyze, descriptively, whether (donor and recipient) political and commercial interests are associated with the speed with which countries decide to provide relief. If aid speed follows the same logic as its allocation, close trade partners and geopolitically relevant countries should receive a more favorable treatment—i.e., faster assistance. Hence, we capture the number of days elapsed from the onset of an emergency until a donor’s *first* commitment to provide relief. To do so, we link daily data on humanitarian aid flows from the United Nations (UN) Office for the Coordination of Humanitarian Affairs (OCHA, 2024) to disaster start dates provided by EM-DAT (Guha-Sapir, 2024). The focus on commitment—instead of delivery—dates is justified for both theoretical as well as practical reasons. For one, as we argue later on, the date when the aid decision is taken is the more relevant variable if the goal is to assess donors’ strategic considerations (Drury et al., 2005). Second, delivery dates are only available for a small fraction of observations.

Our sample consists of 516 emergencies following fast-onset natural disasters that took place in 128 (recipient) countries between 2000 and 2022, leading to more than 3,500 aid ties with 43 different donor countries.³ In line with our expectations, we find that donors react faster to natural disasters that occur in countries that are more important trade partners. Geopolitics, however, plays an ambiguous role. While donors appear to pamper close allies with speedy assistance, they also seem to accelerate aid to geopolitical “foes,” which aligns with the argument that humanitarian aid serves as a cheap, low-commitment instrument of foreign policy in cases where general development assistance is not politically palatable (Fink and Redaelli, 2011).

To more rigorously investigate whether donor countries’ strategic interests—and commercial competition between donors in particular—distort the speed of donors’ aid commitments in the aftermath of natural disasters, we take full advantage of

³Our donor sample includes, in addition to all G20 members, any country that has provided emergency aid at least once a year, on average, in the period between 2000 and 2022. Note that we remove Monaco, as it is a microstate.

the fine-grained nature of our data by constructing a trilateral panel data set (i.e., donor, donor, recipient) of humanitarian aid commitments at daily frequency. The resulting dataset, with more than 3 million observations, enables us to observe how donor countries move after each other, such that we can investigate whether their (strategic) interactions have consequences for the timing of their aid decisions. Contrary to the existing literature that typically studies the final allocation outcome, we track the whole decision-making process as it unfolds, day after day.

We first hypothesize that how long a donor takes to decide to provide emergency relief is influenced by bandwagon (or herding) effects, such as peer pressure, which we measure by computing the total number of (other) donors that have committed aid during the previous days. As Fink and Redaelli (2011, p. 752) put it, donors “may be exposed to international ‘peer pressure’ or may want to profit from economies of scale in the provision of aid.”⁴ Importantly, we expect these forces, which have been shown to influence the final aid allocation (Round and Odedokun, 2004; Fink and Redaelli, 2011; Frot and Santiso, 2011), to matter also for the speed of donor decision-making in the course of emergency events. In fact, we argue that our setting offers an excellent opportunity to identify the ways in which donors influence each other.

In addition, given the widespread evidence of competition between donors (Mascarenhas and Sandler, 2006; Djankov et al., 2009; Davies and Klasen, 2019), we expect that a potential crowding-in effect should vary with the salience of donors’ strategic interests—and in particular with the degree of commercial competition between them, which we measure based on the similarity in either export or import structure between a given pair of donors in the disaster-affected recipient country (Finger and Kreinin, 1979; Fuchs et al., 2015). Thus, our second hypothesis is that, when donors observe their competitors providing assistance, they respond quicker in order to secure their interests in the recipient country—be it in terms of protecting their export markets or securing the provision of import goods. Geopolitical antagonism, on the other hand, has less straightforward implications. As already suggested previously, donors may have sensible reasons to favor both “allies” as well as “foes.” We take this dimension into account by constructing a measure of geopolitical alignment, which is based on countries’ voting pattern at the United Nations General Assembly.

Identification relies on a series of multidimensional fixed effects. Importantly, emergency-day fixed effects for a period of 180 days after each disaster onset absorb

⁴It is also possible that countries could instead free ride on the aid donations of other countries, which would imply a crowding-out effect (Davies and Klasen, 2019). This is more likely to be the case when aid has a dominant public-good component.

all emergency-specific daily variation that is common to all donors on a given day.⁵ Thus, we make sure that confounders, such as (common) information flows and media pressure, do not bias the parameters of interest. Identification therefore stems from *within-day variation* in donors' aid decisions for each disaster event. Furthermore, in our strictest specification, we also include donor-specific fixed effects for each emergency. These fixed effects account for unobserved heterogeneity in a donor's response to a specific catastrophe, including the decision itself of whether to provide aid, thus getting rid of a potential selection issue. They also account for each donor's links to a particular recipient country, which include historical, cultural, commercial, and geopolitical ties, and address the fact that donors' expertise may differ according to the type of disaster. In this case, as the aid decision itself is accounted for, we focus solely on the timing of the aid decision, such that we are able to isolate the dynamics that accelerate or delay donor response. As we argue in more detail below, the inclusion of these multidimensional fixed effects yields causally-credible estimates of the bandwagon and strategic competition effects.

Our results show, first, that the number of donors that donated during the previous days indeed exerts considerable influence over other donors' speed of reaction. Every additional (first) aid commitment within the previous three days increases the likelihood that another donor decides to provide assistance on a given day by approximately 0.3 percentage points, which is sizable given that the daily average probability to start aiding is below 1%. Second, we find that the extent to which donors follow each other increases with their degree of commercial competition in a given recipient country. Although a competition effect over import markets also exists, donors seem to be particularly sensitive to threats to their export interests: the recent (first) commitment of a donor with the same export structure in the affected country increases the daily aid probability by up to 1.2 percentage points. Possibly due to the reasons discussed above, we do not come to similar conclusions with regard to geopolitical competition between donors.

These findings are robust to a series of tests. Importantly, controlling for potentially confounding factors, such as informational similarity between donors, does not alter any conclusions. Our results are also robust to the use of time windows of different length and accounting for bilateral trade volumes in our competition measures. Reassuringly, we show evidence that donor decisions themselves—and not spurious correlation—trigger the response of other donors, as we do not see significant effects in a placebo test in which we anticipate peer commitments by up to five days.

⁵The 180-day cut-off stems from the UN's definition of a flash appeal.

To shed more light on the crowding-in effect and more clearly identify the direction of influence between a pair of donors, we leverage the presence of so-called lead donors (Steinwand, 2015), who are arguably the natural first movers during humanitarian crises and are therefore well-positioned to steer the aid behavior of other countries. We find that these (recipient-year-specific) lead donors exert a considerable influence on other donors, serving as an important driver of the crowding-in effect. Notably, their influence is significantly larger than that of ordinary donors, underscoring their potential to accelerate the international aid community's response during humanitarian crises. Once again, export interests play a dominant role. A donor with an export structure similar to the lead donor's in the affected country is up to 2.8 percentage points more likely to commit aid within three days of the lead donor's aid decision. We come to similar conclusions when we analyze the response to donor countries in the G7. Importantly, however, we show that not all donors seem to use emergency relief to engage in disputes over these strategic interests. For a set of "good" donors, who predominantly rely on humanitarian motives to guide their provision of aid (Dreher and Fuchs, 2015), there is, as expected, hardly any evidence of competition.

Moreover, we examine important mechanisms underlying our main findings. We find that both the bandwagon as well as the strategic competition effects are driven by government-to-government aid, rather than aid provided through international and non-governmental organizations. In line with our argument, this type of aid provides the recipient government with more autonomy in deciding how to use the funds, making it more effective for donors seeking to curry favor with recipient countries (Raschky and Schwindt, 2012). This finding further highlights the role of strategic considerations in the speed of emergency aid. Our results show, in addition, that commercial competition between donors is mostly limited to medium-severity disasters. This is consistent with the idea that very severe disasters restrict donors' ability to pursue strategic agendas (e.g., due to public scrutiny), while minor disasters may not attract sufficient attention to warrant efforts to obtain favors from the recipient government.

Taken together, our results show that donor countries react with more urgency in situations where they have stronger commercial interests at stake. Decisions to assist are also affected by the geopolitical relationship between donor and recipient (although not between donors). These findings stand in sharp contrast to the UN Resolution 46/182 (OP2), which states that "humanitarian assistance must be provided in accordance with the principles of humanity, neutrality and impartiality" (OCHA, 2009) and thus raise important questions about the effectiveness of bilateral

humanitarian aid in general. These concerns underscore the necessity to promote mechanisms that foster cooperation and coordination among donors of humanitarian assistance in international fora. As climate change continues to increase the frequency and the intensity of extreme weather events, the urgency and relevance of timely emergency aid after natural disasters are likely to become even more salient in the future (IPCC, 2023).

The attention policymakers and aid practitioners devote to timely relief notwithstanding, this issue has not yet received significant scholarly consideration. To the best of our knowledge, our work is the first to empirically analyze daily foreign aid decisions. Previous research has investigated the speed of aid *disbursements after* the initial aid decision was made (Kilby, 2011; Kersting and Kilby, 2016). Our study is also related to McDowell (2017), who analyzes the speed of loan approval after countries have submitted a letter of intent to the International Monetary Fund.

Our study sets itself apart from previous work in three key ways: (i) we focus on emergency aid, where response times are measured in days rather than months, making speed an even more decisive factor for effectiveness; (ii) we leverage fine-grained daily data that allows for a more precise analysis of decision-making dynamics and interactions between donors—importantly, we observe donor decisions as emergency events unfold, and not only the final aid allocation; and (iii) we explore the influence of strategic donor competition on response times in the context of humanitarian aid—an area often presumed to be less politicized.

More broadly, we contribute to the economics literature on international humanitarian assistance (Strömberg, 2007; Raschky and Schwindt, 2012; Becerra et al., 2014; Nunn and Qian, 2014; Becerra et al., 2015; Raschky and Schwindt, 2016) and, in particular, to the strand that is concerned with the determinants of its allocation decisions (Eisensee and Strömberg, 2007; Annen and Strickland, 2017; Fuchs and Öhler, 2021; Mogge et al., 2023). Our work extends this literature by shifting the focus from the traditional questions of “to whom?” and “how much aid?” to the crucial question of “how fast?,” demonstrating that strategic donor interests not only influence aid volumes but also response times.

Our analysis also contributes to a rich strand of research on aid donor interactions, which includes studies of both coordination (Djankov et al., 2009; Aldasoro et al., 2010; Bourguignon and Platteau, 2015) and competition (Fuchs et al., 2015; Steinwand, 2015; Davies and Klasen, 2019) between donors. Despite broad acknowledgment of a widespread lack of coordination and frequent speculation about the harm it creates, the current empirical and theoretical understanding of the underlying causes of donor fragmentation is still rudimentary at best (Steinwand, 2015).

We contribute to this debate by analyzing on-going aid decisions after a clearly identified event. By, roughly speaking, watching the response to disaster emergencies as it unfolds, day after day, we are able to identify new patterns of interaction and competition among donors as well as important features of the political economy of aid that have been neglected so far. Because we are not limited to examining only the final picture, as most of the previous literature is, our vantage point lets us track the implications that the decision of one donor has for other aid providers “on the go.” Thus, in addition to observing who is moving after whom, we can single out strategic drivers of daily aid decisions.

Finally, we contribute to the literature on the economics of natural disasters (e.g., Kahn, 2005; Noy, 2009; Strobl, 2011; Strobl, 2012; Cavallo et al., 2013; Felbermayr and Gröschl, 2013; Felbermayr and Gröschl, 2014; Kunze, 2021). Previous work has explored the (mostly adverse) economic and political effects of natural disasters around the globe, including declines in life satisfaction, reduced life expectancy, increased migration, and heightened conflict risk (Neumayer and Plümper, 2007; Nel and Righarts, 2008; Luechinger and Raschky, 2009; Boustan et al., 2012; Berlemann, 2016). Our analysis of aid response times adds a crucial dimension to understanding how the international community reacts to these adverse effects with humanitarian assistance, highlighting how strategic interests may compromise the speed and thus arguably the effectiveness of such efforts.

While our study examines the international political economy across a comprehensive set of global disaster events, our findings parallel an important strand of literature that analyzes domestic political economy in the context of specific disasters. Most relevant to our study, there is evidence that domestic aid allocation decisions are shaped by political considerations, including co-partisanship between local and national leaders, ethnic favoritism, voter behavior, and electoral cycles (Cole et al., 2012; Pathak and Schündeln, 2022; Schneider and Kunze, 2023; Berlemann et al., 2024). By analyzing the speed of emergency aid decisions through the lens of international donor competition, our study reveals a crucial but previously unexplored mechanism through which political economy considerations shape the effectiveness of disaster relief efforts in addition to those studied at the domestic level.

We proceed as follows: Section 3.2 describes how we measure the speed of aid decisions after fast-onset natural disasters and provides several stylized facts, highlighting how response speed relates to commercial competition and geopolitical ties. In Section 3.3, based on daily (i.e., emergency-day) aid decisions, we present our main analysis, testing the bandwagon and strategic competition hypotheses. Ro-

bustness tests are discussed in Section 3.4. In Section 3.5, we investigate potential mechanisms. Finally, Section 3.6 offers concluding remarks.

3.2 Measuring the Speed of Aid

3.2.1 Humanitarian Aid and Natural Disasters

Humanitarian aid consists of “intervention[s] to help people affected by natural disasters and conflict to meet their basic needs and rights” (OCHA, 2024). Throughout our study, we rely on data on humanitarian aid flows provided by the Financial Tracking System (FTS) of the UN Office for the Coordination of Humanitarian Affairs (OCHA, 2024) and restrict our analysis to flows that have been linked to a particular natural disaster in FTS, which are events that typically lead to a UN emergency appeal. Particularly in these cases, a fast provision of aid is key to its success (Clarke and Dercon, 2016).

The FTS tracks flows worldwide and is based on self-reported information, which is provided by donor governments and/or recipient agencies, collected from donor websites, or quoted in pledging conferences.⁶ The FTS is widely used in policy analysis and academic research (e.g., Fink and Redaelli, 2011; Raschky and Schwindt, 2012; Eichenauer et al., 2020) and is arguably the best database available for analyses of humanitarian aid that, like ours, are not restricted to OECD donors, but rather encompass a wide range of donor countries.⁷

While the database covers information on aid flows provided by more than 160 bilateral donors, including infrequent donor countries such as the Democratic Republic of Congo and North Korea, we focus on donor countries with significant aid activities. As such, we limit our analysis to G20 members and all other countries that have provided emergency aid after at least one fast-onset natural disaster event per year, on average. In other words, we remove donor countries that do not satisfy the threshold of 23 records of emergency aid provision in the sample period (2000–2022). We are left with 43 donor countries with 3,500 emergency aid linkages with

⁶In cases where donation data stem from various sources, FTS invests significant efforts into cross-validation and reconciliation. For a more detailed description of the data collection and subsequent cross-checking process, see OCHA (2004). By comparing FTS records with data of the OECD’s Development Assistance Committee, Fink and Redaelli (2011) find only minor differences between both databases, which shows that FTS has relatively good data coverage. See Harmer and Cotterrell (2005) for a discussion of strengths and weaknesses of FTS data.

⁷In contrast to the commonly used OECD Creditor Reporting System and the project-level database AidData (Tierney et al., 2011), FTS has the advantage that it covers virtually every country in the world. Even countries with a low aid transparency, like China and Saudi Arabia, are covered by FTS.

128 recipient countries. While FTS reports humanitarian aid flows contributed, committed, and pledged, we exclude the latter as these represent only a “non-binding announcement of an intended contribution or allocation by the donor” (OCHA, 2024). Appendix Table C.1 reports all donor countries in our sample together with the number of (first) aid provisions, amount contributed, and average speed in days.

To be able to compute the speed of aid and establish several stylized facts, it is crucial that the database links humanitarian aid flows directly to specific natural disasters and contains information on aid decision dates. We make use of the *decision date* variable from FTS, which captures the “[d]ate on which a donor is reported to have made a funding commitment.”⁸ Inspired by Fuchs and Klann (2013), we then measure the speed of country i ’s aid decision after disaster event k in recipient country r by computing the duration, in days, from disaster onset, $StartDate_{i,k,r,t}$, until the day on which donor i first decided to provide assistance to recipient r to cope with disaster event k , $DecisionDate_{i,k,r,t}$. Formally, we define:

$$Duration_{i,k,r,t} = DecisionDate_{i,k,r,t} - StartDate_{i,k,r,t} + 1 \quad (3.1)$$

where data on $DecisionDate_{i,k,r,t}$ come from FTS, as explained above, and information on $StartDate_{i,k,r,t}$ is taken from the International Disaster Database (EM-DAT), maintained by the Centre for Research on the Epidemiology of Disasters (Guha-Sapir, 2024).⁹ This required going through a careful process of assigning each humanitarian aid flow, from FTS, to the corresponding disaster reported by EM-DAT. This task was performed, independently, by at least two different coders. We then arbitrated in the case of any discrepancies.

If a donor commits to provide aid on the start day of the disaster event, $Duration_{i,k,r,t}$ takes a value of 1. Higher values imply slower aid speed.¹⁰ As already mentioned above, since we study emergency assistance, we only include aid flows with a decision time smaller than or equal to 180 days. Aid delivered with longer delays hardly aims at urgent needs that require speedy assistance, and the selection of 180 days as cut-off level is in line with the UN’s definition of a flash appeal, which structures a coordinated humanitarian response for up to six months

⁸See FTS glossary at <https://fts.unocha.org/glossary> (accessed October 2024).

⁹The database covers information on disaster characteristics, such as the disaster type, magnitude, number of people affected, number of people killed, and—crucial for our purposes—the start and end dates. All disasters included in the data set must meet at least one of the following criteria: (i) 10 or more people have died, (ii) 100 or more people have been affected, (iii) a state of emergency has been declared, or (iv) a call for international assistance has been made in response to the wreck.

¹⁰In the case of storms, to account for donors’ efforts toward disaster preparation, all aid decisions taken in the week before the onset are assumed to have been made on day 1.

after the start of an emergency. Importantly, we focus on decision date—instead of delivery date—because this is arguably the dimension with most room for strategic considerations and therefore the purest measure to assess how donors respond to each other (Drury et al., 2005). Delivery dates, on the other hand, are subject to other contingencies that may obfuscate donors’ preferences and priorities. Additionally, data on delivery dates are less precise and only available for 7% of the sample. As the FTS acknowledges in its glossary, “[i]f this [delivery] date is not available, FTS uses the decision date or as last resort, the date the information was reported to FTS.”¹¹

Moreover, to be able to precisely measure the speed and timing of humanitarian aid, we restrict our analysis throughout the chapter to disaster types with a clearly identifiable start date. This means that we, similarly to Fink and Redaelli (2011), limit the sample to fast-onset disasters and thus exclude events such as drought, extreme temperature, and insect infestation. The remaining disaster types include earthquakes, floods, mass movements (or landslides), storms, and volcanic activity. In a second step, we keep only those events for which EM-DAT records a precise start day—i.e., we exclude entries for which only the start month or year is available.¹² This focus on fast-onset disasters comes with the advantage that it mitigates endogeneity concerns, because the exact timing of these disasters is largely unpredictable.¹³ Furthermore, the natural catastrophes themselves typically do not last more than a day—in contrast to the humanitarian catastrophe that they trigger, which can last much longer. Hence, differently from slow-onset events, such as cold winters or droughts, there is no reason to believe that the speed of aid may affect the occurrence and timing of a rapid-onset disaster nor whether and when such event is considered to be a humanitarian crisis.

Our final sample consists of 446 fast-onset disasters. Since 29 of these affect more than one country at the same time, we end up with 516 emergencies, which we define as a humanitarian crisis following a disaster event in a specific country.¹⁴ Table 3.1 reports, by disaster type, information on the frequency and severity of the events in our sample. For each group, it also shows the average number of donor countries that provides assistance as well as the fastest donor to respond (on average). Appendix Table C.2 provides descriptive statistics of all variables.

¹¹See FTS glossary at <https://fts.unocha.org/glossary> (accessed October 2024).

¹²The precise start date is missing for 18 disasters (or about 4% of all disaster in the sample).

¹³While earthquakes cannot be reliably predicted, storms are, at best, predictable two weeks in advance (Zhang et al., 2019).

¹⁴For example, we treat the 2004 Indian Ocean tsunami as nine emergencies as it affected nine countries in our sample: India, Indonesia, Malaysia, Maldives, Myanmar, Seychelles, Somalia, Sri Lanka, and Thailand.

Table 3.1 – Fast-onset Natural Disasters (2000–2022)

Disaster type	No. of events	Avg. no. people killed	Avg. no. people affected	Avg. no. donors	Fastest donor (Avg. days)
Earthquake	81	28,491	2,958,217	23	Indonesia (4.5)
Flood	255	227	3,336,679	12	India (14.5)
Mass movement (wet)	19	331	62,087	11	Italy, Singapore (4)
Storm	143	7,092	1,910,771	15	Israel (7)
Volcanic activity	18	135	75,397	12	Belgium, Finland, Turkey (5)
All	516	9,871	2,711,719	16	Indonesia (10.1)

Notes: The table shows, for each type of fast-onset disaster, the number of events, the average number of people killed and affected, the average number of donors that responded, and the fastest donor. The fastest donor is selected based on the lowest average duration, in days (shown in parentheses), between disaster start and first aid commitment. Data from EM-DAT (Guha-Sapir, 2024) and FTS (OCHA, 2024) for the period between 2000 and 2022.

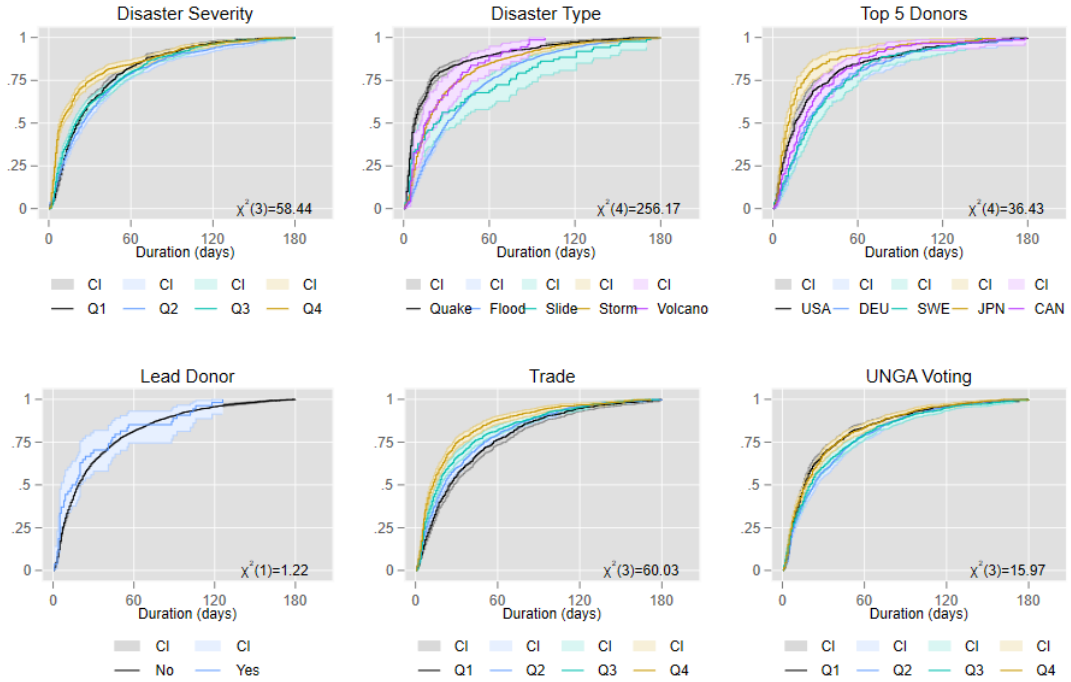
3.2.2 Stylized Facts

We start with a descriptive (survival) analysis of our data at the emergency level, which produces several stylized facts.¹⁵ For donor countries that eventually provide emergency relief in response to a fast-onset natural disaster, we analyze the timing of their first aid decisions. Specifically, we calculate their failure functions, which represent the probability that a country will make an aid decision (“fail”) at a given time point, conditional on not having donated up to that point. We observe that these functions differ significantly according to some disaster characteristics and donors’ strategic interests.

Stylized Fact 1: Donor countries respond faster to the most severe disasters. The top-left panel of Figure 3.1 displays the failure functions for each quartile of disaster severity. We measure disaster severity with the number of people killed, which is provided by EM-DAT (Guha-Sapir, 2024). We find that donors react particularly fast to emergencies in the top quartile of the fatality distribution, whereas the differences between the other three quartiles are much less pronounced. In our sample, of all the 729 aid decisions involving disasters in the top severity quartile, only around 5% (39) had not yet been made by the 50th day after the onset. For each of the other quartiles, the share of yet-to-be-decided commitments by the 50th day is 19% or higher. This suggests that aid speed is, at least to some extent, responsive to humanitarian needs.

Stylized Fact 2: Donor countries respond faster to earthquakes than to other disaster types. It is conceivable that different types of disasters trigger different responses from the aid community, thus potentially also affecting the speed of aid. For example, Eisensee and Strömberg (2007) report that the newsworthiness of emergencies depends on disaster type. The top-middle panel of Figure 3.1 displays

¹⁵We use duration models to allow for the probability of deciding to provide aid to depend on the time elapsed since the disaster onset.

Figure 3.1 – Stylized Facts: Failure Functions

Notes: The figure shows Kaplan-Meier failure functions according to the following characteristics: (i) quartiles according to disaster severity, based on the number of people killed by each disaster (top-left); (ii) disaster type, i.e., earthquakes, floods, landslides, storms, and volcanic activity (top-middle); (iii) for each of the top five donor countries in the sample in terms of the number of aid provisions, the United States (USA), Germany (DEU), Sweden (SWE), Japan (JPN), and Canada (CAN) (top-right); (iv) whether the donor is considered to be a lead donor as defined in Section 3.3 (bottom-left); (v) quartiles according to the bilateral trade volume (exports and imports) between donor and recipient (bottom-middle); and (vi) quartiles according voting alignment between donor and recipient at the UNGA (i.e., $\Delta UNGA$). The figure shows 95% confidence intervals.

the failure functions for each disaster type. We observe significant differences in the speed of aid. Donor countries' response time is the shortest after earthquakes. This finding does not seem to be driven by different disaster types being intrinsically more or less catastrophic, as we come to the same conclusion in a regression setup that accounts for disaster severity and recipient need (Appendix Figure C.1). Specifically, we control for the logged number of people killed and people affected (both from EM-DAT), the logged and lagged recipient GDP per capita and population size (both from World Bank, 2024a), and donor-recipient and year fixed effects. Notably, donors are on average almost 12 days faster to respond to earthquakes than to floods. Among other things, this is consistent with the evidence that media coverage—which varies across disaster types and devotes particular attention to earthquakes—influences donor behavior (Eisensee and Strömberg, 2007).

Stylized Fact 3: There is significant variation in response time across donor countries. Among the top 20 donor countries in terms of the number of aid provisions in the sample, Japan is, on average, the fastest to decide to provide

assistance. This is illustrated by the top-right panel in Figure 3.1, which shows the failure functions of the top five donors: the United States, Germany, Sweden, Japan, and Canada. On average, while Japan takes 15 days to make its first aid commitment, the United States—the most active donor in our sample—needs significantly longer, 21 days ($p = 0.016$). This finding is corroborated by a regression analysis that includes the top five donor countries in the sample and controls for the logged number of people killed and people affected, logged and lagged recipient GDP per capita and population size, as well as disaster-type, recipient, and year fixed effects (Appendix Figure C.1). Relative to the average donor country in the sample, Canada, Japan, and the United States are significantly faster to commit assistance, while Germany is significantly slower.

Moreover, the bottom-left panel in Figure 3.1 shows that lead donors are faster than their peers. A lead donor is a country that not only is the most important aid provider for a given recipient country, but also maintains long-term (often historical) ties with it. We will further develop—and properly define—this concept in Section 3.3. For now, it suffices to say that there seems to be a certain set of (recipient-specific) prominent donors that are not only faster, but may also be particularly influential in their spheres of influence.

Stylized Fact 4: Donors respond faster to emergencies that take place in countries that are more important trade partners. Considering the widespread evidence that the cross-country allocation of humanitarian aid is influenced by commercial interests (Fink and Redaelli, 2011), we investigate whether close trade partners receive speedier assistance. Such a preferential treatment may help, for instance, mitigate the damage to commercial ties caused by catastrophes (Gassebner et al., 2010). If a donor country exports a substantial part of its production to a country that was hit by a natural disaster, it may be inclined to provide aid faster than usual to make sure that its exports do not lose their market. Likewise, donors that rely on particular countries for crucial imports should react more quickly to avoid having their supply chains disrupted.

In both cases, emergency relief can cushion the impact of the disaster on trade in at least three ways. First, it may directly contribute to repairing crucial trade infrastructure, such as ports and roads. Second, by providing quick assistance, donor countries can help reduce, indirectly, the impact on aggregate economic activity that often follows natural disasters and may in turn affect trade ties. Finally, the faster a donor acts, arguably the more likely its assistance is going to be seen as a gesture of goodwill by the affected country, and thus more likely it is to help preserve (or

even boost) existing commercial ties.

We measure the intensity of trade ties with the sum of exports and imports flows (in constant 2010 US dollars) between donor and recipient and then divide the sample in quartiles according to the bilateral trade value (data from IMF, 2024). The survival analysis indicates that donors respond significantly faster to disasters in recipient countries that are more important commercial partners. As we show in the bottom-middle panel of Figure 3.1, the higher the trade quartile, the faster donors respond on average. Appendix Figure C.2 reveals that, in fact, both exports and imports are associated with a faster response. Moreover, we find similar evidence in a regression model that controls for the logged number of people killed and people affected, logged and lagged recipient GDP per capita and population size, and donor, recipient, and year fixed effects. Donors in higher quartiles respond more quickly: compared with the bottom quartile, the time required to commit is on average four, six, and nine days shorter for donor-emergency pairs in the second, third, and fourth quartiles, respectively (Appendix Figure C.1).

Stylized Fact 5: Donors provide faster assistance to the most and least geopolitically aligned countries. Geopolitics is another factor that may distort the delivery of humanitarian aid. To assess the role played by political alignment between donor and recipient, we use information on the voting alignment at the UN General Assembly between these countries.¹⁶ More precisely, our indicator of political alignment between donor i and recipient r is the absolute difference of their ideal points, as calculated by Bailey et al. (2017).¹⁷ That is, $\Delta UNGA_{i,r,t} = |\text{idealpoint}_{i,t} - \text{idealpoint}_{r,t}|$.

However, it is not clear, *a priori*, how political alignment affects the speed of humanitarian assistance. On the one hand, closer political ties could be associated with countries receiving faster aid from their allies as donors may speed up their emergency aid to express their support for befriended countries or to help ensure the survival of politically aligned governments in cases where a severe disaster threatens political stability (Drury and Olson, 1998; Drury et al., 2005). On the other hand, donors may give preferential treatment to persuade adversaries or recipients that are politically distant to make concessions to the donor in the future, given that emer-

¹⁶Measures of UN voting alignment are widely used in the empirical aid literature (e.g., Thacker, 1999; Kilby, 2009; Kilby, 2011; Faye and Niehaus, 2012; Kersting and Kilby, 2016; Dreher et al., 2021b). See Dreher et al. (2024) for a literature review.

¹⁷For each year, Bailey et al. (2017) estimate a country's ideal point within a one-dimensional preference space. A country's voting behavior (i.e., yes, no, or abstain) in each UNGA vote is modeled as a function of its ideal point. Therefore, the more similar the voting behavior of two countries, the closer their ideal points will be, resulting in a smaller absolute difference between the two.

gency assistance tends to be a more appropriate tool to buy policy concessions from less aligned countries than general development assistance. In contrast to the former, the provision of assistance aimed at long-term economic and structural development requires a fair amount of collaboration between donor and recipient and hence at least some goodwill to facilitate negotiations (Fink and Redaelli, 2011; Annen and Strickland, 2017). Emergency aid, on the other hand, requires hardly any negotiation and considerably less coordination with recipient countries and thus presents donors with the opportunity to engage with recipient countries that are less politically aligned by making use of more flexible and low-commitment arrangements.

The bottom-right panel of Figure 3.1 indicates that indeed both seem to be true, as donor-recipient pairs at the extremes of the distribution of $\Delta UNGA$ (first and fourth quartile) display shorter response times than observations in quartiles two and three. That is, donors commit aid more quickly if the disaster affects either a closely aligned country (Q1) or a country with a very different geopolitical agenda (Q4). Apparently, donors both reward closely aligned governments *and* try to sway countries that are out of their sphere of influence.¹⁸

The case of the 2010 Haiti earthquake provides a prime example of these two opposing mechanisms. In the aftermath of the disaster, the Republic of China (Taiwan)—which back then maintained diplomatic relations with only 14 countries, including Haiti—engaged in a large-scale humanitarian mission. Taipei’s first rescue team reached Haiti on January 16 and the first medical team arrived five days later. (The People’s Republic of) China, which considers Taiwan a renegade province and attempts to isolate the island diplomatically, showed similar generosity towards Haiti, despite refusing diplomatic relations with the government in Port-au-Prince as a consequence of Haiti’s diplomatic recognition of Taiwan. Tubilewicz (2012, p. 6) describes these activities of the two Asian donors as “aid competition.” Beijing’s first rescue team reached Haiti two days *before* the one sent by Haiti’s close ally Taiwan. In line with the behavior of China in the case of the Haiti earthquake, Fink and Redaelli (2011) find politically *less* affine countries to be more likely to receive emergency aid from a particular donor.

Summing up, we conclude that the speed of aid shows considerable variation across levels of disaster severity, disaster types, donor countries, and the strength of geopolitical and commercial ties. We focus on the latter and move to a more rigorous empirical setting to analyze strategic competition in the speed of emergency aid.

¹⁸Appendix Figure C.3 presents the same finding in a different manner, as it splits the sample only in two groups based on whether donor-recipient pairs are at the extreme (Q1 and Q4) of the distribution or not (Q2 and Q3). However, these findings are not statistically significant at conventional levels in a related regression model presented in Appendix Figure C.1.

3.3 Daily Analysis

To delve deeper into donor decision-making on the speed of emergency aid, we turn our analysis to the emergency-day level. For each disaster event, we study how donors' daily decisions to provide (or withhold) aid are affected by their peers.¹⁹ Specifically, we analyze how two key factors influence the timing of humanitarian aid commitments: (i) bandwagon effects, measured by the number of donors who have committed assistance to address a particular emergency, and (ii) the extent to which donors compete—particularly due to commercial interests—in the affected country.

Accordingly, we test two hypotheses. First, we posit that recent aid commitments by other donors significantly increase a donor's likelihood of providing assistance (*bandwagon hypothesis*), as donors frequently observe and take cues from their peers—particularly from well-connected countries with on-the-ground expertise—either through informal exchanges or formal discussions in international organizations. We expect these dynamics to influence not only whether donors provide aid, but also *when* they decide to do so.²⁰ Alternatively, it is possible that a donor's commitment discourages the aid provision of other donors. This may be especially likely in cases in which aid is seen as an international public good (Schweinberger and Lahiri, 2006). Yet, based on the cross-country literature on aid allocation, we expect the positive effects of peer influence to outweigh any crowding-out effects.²¹

Second, we hypothesize that donors' reactions to their peers are mediated by their strategic interests (*competition hypothesis*). Specifically, the more the interests of a particular donor conflict with those from an “active” peer, defined as a donor that has committed aid within the previous three days, the faster we expect the former to react to the decision of the latter. This implies that the influence that donors have on each other varies depending on their degree of competition. That is, on top of a bandwagon effect, we expect commercial competition to amplify responsiveness

¹⁹Note that this is not a duration analysis as the outcome of interest is a binary variable that captures whether a donor commits aid on a given day. In our regression setup also the explanatory variables vary on a daily basis. Moreover, data censoring is of little concern, as we observe donors' decision-making up to 180 days after disaster onset. If they have not decided to assist until then, but would still do so at some later stage, it would hardly classify as emergency relief anyway.

²⁰Our baseline analysis examines the impact of aid commitments made within the last three days as decision-making might be hampered through bureaucratic processes and weekends. This also accounts for differences in time zones. Since this decision is arbitrary to a certain extent, we document the robustness of our results to windows of different lengths in Section 3.4.

²¹There is considerable evidence suggesting that aid decisions are often influenced by bandwagon effects from other donors (e.g., Round and Odedokun, 2004; Fink and Redaelli, 2011). So far, however, this herding behavior has only been observed in the final allocation, which makes it difficult to assess whether donors happen to make, autonomously, similar decisions or rather respond to one another.

when rival donors vie for the goodwill of and influence in the recipient country. A donor is arguably more likely to engage to secure its interests if its rival has recently done so, such that the net effect that donors have on each other should vary according to their degree of competition. We focus on two particular sources of commercial competition between donors: exports and imports interests. Later, we take also geopolitical interests into consideration.

3.3.1 Empirical Strategy

To test our hypotheses, we take full advantage of the daily nature of our data. We construct a panel data set in which the unit of analysis is donor-emergency by day. That is, for every disaster event, we observe each of our 43 donor countries for a maximum of 180 days from disaster start or until the day in which the donor (first) decides to provide aid.²² We thus analyze whether and how, during a specific emergency, the decision to provide aid on a given day is influenced by the behavior of other donors that committed to assist within the last three days. According to the bandwagon hypothesis, these previous commitments should encourage other donor countries, which have not yet committed to assist, to accelerate their aid decision.

To investigate the role of commercial interests in donors' response to their peers, we examine the (sectoral) trade overlap between a pair of donors in a given aid recipient country. Arguably, the higher the (lagged) overlap, the more likely it is that donors compete on that given market. We thus follow Finger and Kreinin (1979) and calculate both export and import similarity indices for each donor pair in a particular recipient country.²³ More precisely, the Export Similarity Index, *ESI*, of donor countries i and j in recipient country r and year t is given by:

$$ESI_{i,j,r,k,t} = \sum_s \text{Min}(X_s^{i,r,k,t}; X_s^{j,r,k,t}) \in [0, 1] \quad (3.2)$$

where $X_s^{i,r,k,t}$ represents donor i 's exports in sector s to recipient r in year t as a share of i 's total exports to r in t . Sectoral international trade data (SITC, Rev. 2) come from Growth Lab (2024). Analogously, we use the sectoral import data to calculate the Import Similarity Index (*ISI*). Both *ESI* and *ISI* take values between zero and one, with one indicating perfect similarity. The more similar the export or import structure of a pair of donors in a given recipient country, the more we expect them to compete with each other. To make sure that the similarity is not affected by the

²²As already mentioned above, the selection of 180 days as cut-off level is in line with the UN's definition of a flash appeal.

²³See Fuchs et al. (2015) and Asmus-Bluhm et al. (2025) for applications of the export and import similarity indices in the empirical aid literature.

emergency, we take the value from the year before the disaster. Appendix Figure C.4 illustrates these measures by displaying the average *ESI* and *ISI* between two major donors in our sample, the United States and Japan, in each recipient country.

Our goal is to analyze how donors respond to the collective pressure of recent aid commitments, rather than to individual donor countries. Therefore, we extend these measures and construct weighted sums of *ESI* and *ISI* to take the daily collective competitive pressure into account. Specifically, we sum all donors who declared their first aid commitment within the previous three days, weighting each by either their export or import similarity index (in the previous year, $t - 1$). These aggregate measures of export and import competition (*ExpComp* and *ImpComp*, respectively) are calculated as follows:

$$ExpComp_{i,j,r,k,[d-3,d-1]} = \sum_j Aid_{j,k,r,[d-3,d-1]} \times ESI_{i,j,r,k,t-1} \quad (3.3)$$

$$ImpComp_{i,j,r,k,[d-3,d-1]} = \sum_j Aid_{j,k,r,[d-3,d-1]} \times ISI_{i,j,r,k,t-1} \quad (3.4)$$

Therefore, $ExpComp_{i,j,r,k,[d-3,d-1]}$ and $ImpComp_{i,j,r,k,[d-3,d-1]}$ capture the aggregate daily competitive pressure faced by donor i by weighting all donors ($j \in J$) that decided to provide assistance in the previous three days by their trade similarity with donor i in recipient country r . To illustrate how the aggregation works, consider the 7.2-magnitude earthquake that hit eastern Türkiye on October 23, 2011. On the next day, France and Russia were the first donors to commit assistance. One day later (October 25), Japan decided to provide aid, followed by Austria on October 26. As an example, the *ExpComp* assigned to Spain on October 27, when the country committed to assist, is 2.002, which is the sum of Spain's lagged (i.e., 2010) export similarity index (relative to Türkiye) with France (0.696), Russia (0.199), Japan (0.591), and Austria (0.516) combined, which are the donors that made their aid decision in the three days before Spain's.

With this in mind, we proceed to estimate the following linear probability model using ordinary least squares:

$$\begin{aligned} Aid_{i,k,r,d} = & \gamma_1 NDonors_{j,k,r,[d-3,d-1]} + \gamma_2 ExpComp_{i,j,k,r,[d-3,d-1]} + \\ & \gamma_3 ImpComp_{i,j,k,r,[d-3,d-1]} + H(Days_{i,k,r,d}) + \\ & \eta_d + \theta_d + \kappa_d + \lambda_i + \pi_{k,r} + v_{i,k,r,d} \end{aligned} \quad (3.5)$$

where the dependent variable, $Aid_{i,k,r,d}$, is binary and indicates whether donor coun-

try i , on day d , decides to provide humanitarian assistance (for the first time) to recipient country r in response to emergency k . On the right-hand side of the equation, we include the number of donors that decided to start providing assistance to disaster k in recipient country r within the last three days, $NDonors_{k,r,[d-3,d-1]}$, and the two measures of aggregate daily competitive pressure introduced above, $ExpComp_{i,j,k,r,[d-3,d-1]}$ for exports and $ImpComp_{i,j,k,r,[d-3,d-1]}$ for imports. In terms of the discussion above, the bandwagon hypothesis implies a positive γ_1 and the competition hypothesis requires positive γ_2 and/or γ_3 .

Moreover, in equation 3.5, we add a polynomial function, $H(\cdot)$, of the number of days passed between disaster start and the decision date of donor i to provide its first assistance to take into consideration a potential (non-linear) influence of the total time passed since the disaster onset.²⁴ To account for unobserved characteristics that may influence the timing of aid provision, we include in all specifications weekday (η_d), day-of-the-month (θ_d), month (κ_d), donor (λ_i), and emergency-recipient fixed effects ($\pi_{k,r}$). We are thus able to remove, for instance, all the time-invariant (during a particular emergency) donor and recipient characteristics that affect decision-making—such as donor aid infrastructure and particularities of each type of disaster—but are unrelated to the influence of other donors.

In our preferred specifications, we also include emergency-day and emergency-donor fixed effects. The former absorb all emergency-day-specific variation that is common to all donors on a given day. This rules out that our results are driven by confounding factors, such as disaster repercussions (like aftershocks) or new information about the severity of emergencies becoming available to the international community. Identification in this setting therefore stems from *within-day variation* in donors' aid decisions relative to a particular emergency. As such, we are able to pin down in which ways a donor's decision (not) to follow the decision of other donors depends on its strategic interests. Although this comes with the disadvantage of no longer being able to estimate γ_1 (and thus test the bandwagon hypothesis), it significantly improves the identification of γ_2 and γ_3 and hence our ability to test the competition hypothesis.

In turn, emergency-donor fixed effects—22,187 binary variables for each donor-emergency pair—account for all time-invariant (during the 180-day emergency period) and disaster-specific donor characteristics. This includes the decision of whether to provide aid at all, therefore taking care of any potential selection issue. Moreover, this approach also captures donors' different levels of expertise with

²⁴In our baseline specification, we control for a polynomial function of degree 3. Note that this polynomial becomes redundant in specifications with emergency-day fixed effects.

specific disaster types, historical or geopolitical ties with disaster-affected countries, as well as domestic pressures, influenced for instance by media coverage or diaspora networks. Consequently, in this case our analysis focuses solely on the timing of donors' first commitments, which enables us to isolate the dynamics that accelerate or delay their response.

3.3.2 Main Results

Table 3.2 presents the main results. We examine whether donors react to the aid decision of other donors and whether this reaction is a function of export and import competition. We find that aid patterns are contagious: each new donor in the past three days measurably increases the probability of new donors today, lending support to the bandwagon hypothesis. All else being equal, the likelihood that a donor commits to aid increases by 0.3 percentage points (column 1) for every additional commitment. This effect is sizable in light of the average daily probability to aid (0.1%) after a given fast-onset disaster.

Table 3.2 – Bandwagon and Competition: Main Results

	Dependent Variable: <i>Aid</i>					
	(1)	(2)	(3)	(4)	(5)	(6)
No. of donors	0.003*** (0.000)	-0.000 (0.000)		0.002*** (0.000)	0.000 (0.000)	
Export Competition		0.009*** (0.002)	0.012*** (0.002)		0.005** (0.002)	0.004** (0.002)
Import Competition		0.004*** (0.001)	0.004*** (0.001)		0.002** (0.001)	0.001 (0.001)
H(Daycount)	Yes	Yes	Yes	Yes	Yes	Yes
Weekday FE	Yes	Yes	Yes	Yes	Yes	Yes
Day FE	Yes	Yes	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Emergency FE	Yes	Yes	Yes	Yes	Yes	Yes
Donor FE	Yes	Yes	Yes	Yes	Yes	Yes
Emergency-Day FE	No	No	Yes	No	No	Yes
Emergency-Donor FE	No	No	No	Yes	Yes	Yes
N	3,542,509	3,221,432	3,221,432	3,542,449	3,221,371	3,221,371

Notes: Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Two-way clustered (at donor and recipient level) standard errors in parentheses. Baseline results using three-day window after decision day.

Turning to the competition hypothesis (column 2), we find that, as expected, donors' probability to follow their peers' aid decisions increases with their degree of trade competition in the disaster-affected country. In the case of fully overlapping export patterns, another donor's (first) aid commitment within the last three days increases the probability of a new donor contributing today by nearly one percentage

point. Import similarity shows a similar effect, though less than half as strong. Importantly, when we account for these competition variables, the general bandwagon effect disappears entirely, indicating that donor competition—not mere imitation—drives the clustering of aid decisions. The size of the strategic competition effect becomes even larger when we include emergency-day fixed effects (column 3). One additional commitment now raises the probability of a follow-up aid decision by 1.2 (0.4) percentage points when donors share the same export (import) structure in the disaster-affected country.

In columns 4 to 6, we repeat the analysis including emergency-donor fixed effects. As the allocation decision itself is absorbed, this allows us to isolate the timing of the aid decision. Our findings continue to support the bandwagon hypothesis (column 4), which again is fully mediated by the competition variables (column 5). Notably, both the export as well as import competition effect are reduced in size. The reduction observed in column 6, relative to column 3, suggests that timing accounts for one-third of the competitive export response, while the aid allocation decision itself drives the remaining two-thirds. While the export competition parameter is statistically significant across all specifications, this is not always the case for import competition—though also positive, it is estimated imprecisely in the most demanding specification.

Taken together, these results show that commercial competition plays a crucial role in donor behavior after natural disasters. When donors exhibit similar trade patterns in recipient countries, they seem to more closely monitor and respond to each other’s aid decisions. This suggests that donor countries use disaster aid strategically to maintain their commercial influence, and their export penetration in particular, in aid recipient countries.²⁵

3.3.3 Lead Donor

To further examine the influence that donor countries exert on one another, we single out particularly influential donors that are well-positioned to steer the aid behavior of other countries. Rather than imposing a fixed set of lead donors assumed to be equally relevant across all recipient countries, we adopt Steinwand’s (2015) empirical

²⁵It could be the case that only *strategic* goods, such as scarce natural resources, matter in the case of securing import markets. However, re-estimating our models with an import similarity index based only on key and strategic goods yields comparable results (not reported). That is, competition over export markets continues to be the larger force responsible for speeding up emergency aid decisions. Specifically, we consider metalliferous ores and metal scrap (SITC2 28), all fuel subcategories (32, 33, 34, 35), medicinal and pharmaceutical products (54), and iron and steel (67) as strategic goods. For more information on the definition of strategic goods, see European Commission (2017).

approach. This method identifies, for every recipient country and year, whether a lead donor exists and, if so, who it is. Specifically, Steinwand (2015, p. 445) defines lead donors as those in “a long-time stable exclusive relationship” with the recipient country, “in which the donor continuously acts as biggest provider of foreign aid.”

Broadly speaking, lead donors are those that typically maintain long-term bonds with recipient countries within their sphere of influence and play a widely recognized leadership role in the aid community. Therefore, they usually possess the greatest amount of local expertise and are thus able to set the pace for other donors. As Steinwand (2015, p. 445) notes, a lead donor’s “relevance for overall aid policy is reflected in the fact that the international aid community often expects lead donors to shoulder a larger burden than others when dealing with problems in affected states.” Since lead donors are natural first movers after natural disasters, they offer a good opportunity to observe chains of reaction by distinguishing who follows whom. Consistent with this idea, the bottom-left panel of Figure 3.1 shows that recipient-year-specific lead donors provide assistance significantly faster than their peers.

To empirically determine which country (if any) is the lead donor in each recipient country in a given year, we use data on official development assistance (ODA) provided by the OECD (2024)²⁶ and follow Steinwand (2015) by requiring that the lead donor meets all of the following five criteria: (i) the lead donor has the largest share in the recipient country’s aid receipts in a given year; (ii) the lead donor has the largest share in the recipient country’s aid receipts during at least five out of nine consecutive years;²⁷ (iii) the lead donor must not drop out of the first place (in terms of the aid share) for more than two consecutive years within this nine-year window; (iv) the share of the lead donor must be substantially larger than that of the second largest donor;²⁸ and (v) the lead donor must operate in a concentrated environment (i.e., the recipient country has a low donor fragmentation).²⁹ According to these criteria, the most frequent lead donor in our sample is the United States (33 recipient-year pairs), followed by Japan (24), Australia (15), and France (12). About 22% of the recipient-year pairs in our sample have a lead donor. Figure 3.2 indicates the most frequent lead donor (if any) for each recipient country during our

²⁶These flows include both (gross) humanitarian assistance and all other forms of ODA.

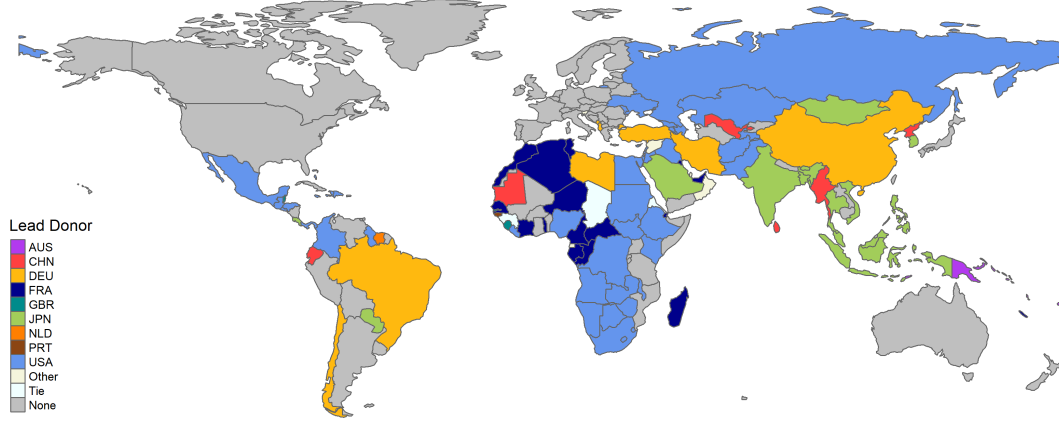
²⁷This is to avoid short-term fluctuations. Steinwand (2015) argues that aid programs often last around four to five years and thus requires the lead donor to be the major aid provider in the majority of the time during approximately two aid cycles.

²⁸To be precise, the difference between the top two shares must be larger than the conditional median of this difference for the subsample that satisfies the longitudinal criteria.

²⁹The aid environment in a recipient-year is considered concentrated if the Hirschman-Herfindahl Index (HHI) of donor concentration is above median for the subsample that satisfies the longitudinal criteria.

sample period.

Figure 3.2 – Most Frequent Lead Donor by Recipient Country (2000–2022)



Notes: The figure displays the most frequent lead donor of ODA for each recipient country between 2000 and 2022, based on the definition in Steinwand (2015). Data on total (gross) ODA disbursement from OECD (2024). Donor countries listed as “other” include those that were the most frequent lead donor for less than three recipient countries, namely: Austria, Italy, Kuwait, New Zealand, Spain, Turkey, and the United Arab Emirates.

We then proceed to estimate the effect of lead donors’ aid decisions on those of their peers. Following our previous model (equation 3.5 above), we again allow this influence to be mediated by the degree of export and import similarity between a regular donor and the lead donor. More precisely, we estimate, for donor i and lead donor j , the following equation:

$$\begin{aligned} Aid_{i,k,r,d} = & \gamma_1 AidLead_{k,r,[d-3,d-1]} + \gamma_2 AidLead_{k,r,[d-3,d-1]} \times ESI_{i,j,k,r,t-1} \\ & + \gamma_3 AidLead_{k,r,[d-3,d-1]} \times ISI_{i,j,k,r,t-1} + \gamma_4 ESI_{i,j,k,r,t-1} + \gamma_5 ISI_{i,j,k,r,t-1} \\ & + H(Days_{i,k,r,d}) + \eta_d + \theta_d + \kappa_d + \lambda_i + \pi_{k,r} + v_{i,k,r,d} \end{aligned} \quad (3.6)$$

where $AidLead_{k,r,[d-3,d-1]}$ indicates whether the current lead donor in recipient country r committed to assist within the previous three days. $ESI_{i,j,k,r,t-1}$ and $ISI_{i,j,k,r,t-1}$, as defined above, capture the similarity in export and import structure with respect to recipient country r in year $t - 1$, but now between donor i and lead donor j .

The results in panel A of Table 3.3 show that lead donors exert considerable influence over their peers. Even before taking competition into account (column 1), we find that the decision of a lead donor to provide assistance makes it more likely that other donors will follow suit in the following days. This provides additional support for the bandwagon hypothesis—although, as before, the coefficient loses significance once the interaction terms are included. In line with the previous results,

the impact on aid decision is driven by export competition (column 2) and is robust to the inclusion of both emergency-day fixed effects (column 3) as well emergency-donor fixed effects (columns 4 and 5). A lead donor’s aid commitment within the last three days makes it up to 2.8 percentage points more likely that a donor country with an identical export structure (in a given affected country) also decides to provide aid. However, in contrast to the results for all peers reported above, we do not find import competition to be a significant driver of the bandwagon effect in this case.

Summing up, we find that lead donors accelerate their peers’ aid commitments and that this effect is more pronounced if donors compete over export markets in the disaster-affected country. Remarkably, the export competition with the lead donor prevails even over the competition with all other donors combined, as columns 6 and 7 indicate.

Panel A includes only emergency-recipient pairs where the recipient country has a lead donor at the time of the disaster—which is the case for about a quarter of the emergencies. While this subset is of particular interest, it is not necessarily representative of the universe of fast-onset natural disasters that occurred between 2000 and 2022. Therefore, panel B repeats the specifications from panel A using the full sample. The conclusions from columns 1 to 5 are fairly similar: lead donors continue to influence the aid decision of other donors, primarily through export competition. Columns 6 and 7, however, indicate that, unlike in panel A, the lead donor effect no longer dominates. This suggests that while lead donors exert substantial influence within their respective spheres—sometimes even more than all other donors combined—, this effect gets blurred when considering the broader set of disaster events. Altogether, these results show that the decisions of lead donors considerably affect the aid strategy of their peers. As such, lead donors can play an important role to mobilize disaster relief, which is particularly important to support underfunded emergency appeals.

3.3.4 G7 and “Good” Donors

Expanding on our assessment of certain important individual donors, we now proceed to analyze two specific sets of countries. First, we replicate the analysis introduced above for G7 countries. Like lead donors, G7 nations are among the most influential aid providers and, given their economic weight, are arguably more likely to use aid as a tool to advance their own agendas and may thus be more likely to prompt other donors to react.³⁰ However, unlike lead donors, which are recipient-

³⁰We use the specification with emergency-donor but without emergency-day fixed effects to ensure that we can still estimate the bandwagon parameter.

Table 3.3 – Lead Donor

	Dependent Variable: <i>Aid</i>						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A – Lead Sample							
Aid Lead	0.004** (0.002)	-0.004 (0.003)		-0.002 (0.001)			
ESI * Aid Lead		0.025** (0.011)	0.028*** (0.010)	0.014** (0.006)	0.013** (0.005)	0.016* (0.009)	0.012* (0.006)
ISI * Aid Lead		0.008 (0.008)	0.006 (0.010)	0.005 (0.005)	0.002 (0.007)	0.002 (0.010)	0.002 (0.008)
Export Competition						0.007 (0.004)	0.001 (0.003)
Import Competition						0.003 (0.002)	-0.000 (0.002)
N	752,156	651,072	651,072	651,063	651,063	651,072	651,063
Panel B – Full Sample							
Aid Lead	0.004* (0.002)	-0.003 (0.002)		-0.001 (0.001)			
ESI * Aid Lead		0.023** (0.010)	0.025** (0.010)	0.012* (0.007)	0.011* (0.006)	0.009 (0.009)	0.007 (0.006)
ISI * Aid Lead		0.007 (0.007)	0.005 (0.009)	0.005 (0.006)	0.001 (0.007)	0.001 (0.010)	0.000 (0.007)
Export Competition						0.012*** (0.002)	0.004** (0.002)
Import Competition						0.004*** (0.001)	0.001 (0.001)
N	3,531,137	3,531,137	3,531,137	3,531,079	3,531,079	3,210,100	3,210,041
H(Daycount)	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Weekday FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Day FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Emergency FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Donor FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Emergency-Day FE	No	No	Yes	No	Yes	Yes	Yes
Emergency-Donor FE	No	No	No	Yes	Yes	No	Yes

Notes: Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Two-way clustered (at donor and recipient level) standard errors in parentheses. Baseline results using three-day window after decision day. When some interaction term with a similarity index is included, then the specification also controls for the similarity index separately.

specific, the influence that G7 countries exert should be more widespread. In other words, we expect these countries to be relevant in more settings, although the degree to which they matter may be smaller than that of lead donors in their areas of influence. As Appendix Table C.3 shows, we find evidence in support of the competition hypothesis for five of the seven countries: Canada, France, Germany, Japan, and the United States. What is more, the magnitude of this effect is comparable to that observed in the analogous specification for lead donors.³¹

Second, we present results for a group of so-called “good” donors—Denmark, Netherlands, Norway, and Sweden—, who “are widely perceived as providing de-

³¹There is of course a considerable overlap between the two groups as G7 countries are often prominent lead donors.

velopment aid predominantly based on humanitarian motives” (Dreher and Fuchs, 2015, p. 1013).³² Hence, unlike lead donors and G7 countries, in this case we expect significantly lower levels of competition. In fact, our results provide hardly any evidence of competition vis-à-vis these “good” donors (Appendix Table C.4). The coefficients on the interaction between *Aid* and *ESI* are consistently positive but very small and only turn weakly significant for the Netherlands—the only country among the “good” donors which is classified as a lead donor. However, and perhaps surprisingly given the positive reputation that these donors enjoy among their peers, we also do not see any sign that the aid decision of these donors, by itself, attracts additional donors, as indicated by the insignificant coefficient on *Aid*.

Finally, we estimate how donor countries react to the aid decision of each individual donor in our sample. Most donors (28 out of 43) seem to be able to generate a bandwagon effect on their peers—and in no case is there any evidence of a negative, that is, crowding-out effect (Appendix Figure C.5). The strategic competition effect, however, is limited to a much smaller set of countries (Appendix Figure C.6), suggesting that only a few donor countries are influential—and perhaps economically important—enough to trigger competitive responses from their peers. In terms of export competition, these include major trading countries like China, Germany, and Japan.

3.4 Robustness

In Table 3.4, we run additional regressions to test the robustness of the evidence supporting the bandwagon and commercial competition hypotheses. In what follows, we use our preferred specification with emergency-donor fixed effects as baseline. In the case of specifications related to the competition hypothesis, we show, in addition, the stricter regressions that include also emergency-day fixed effects for comparison.

First, the choice of a three-day response window used above might appear arbitrary. Replacing it by a one-day (columns 1 and 2) or a one-week window (columns 3 and 4) does not change our baseline findings, both with respect to the bandwagon effect as well as export competition. Both effects are larger when we apply the shorter one-day window, indicating a stronger crowding-in effect in the immediate aftermath of another donor’s aid decision. The coefficient size of *ImpComp* is stable across windows but differs in precision. In the case of a one-week window, the import competition also becomes statistically significant at the 1% level.³³

³²We do not include Canada as a “good” donor because it is also part of the G7.

³³Using different time windows (not shown), ranging from one up to ten days, yields comparable results.

Second, the probability of a donor country to assist others may vary throughout the fiscal year (Eichenauer, 2020). For instance, at the end of budget years, countries may be particularly inclined to spend whatever is left and, therefore, provide emergency relief. To make sure that patterns such as year-end spending spikes do not get in the way of our analysis, we repeat our preferred specification (column 6 in Table 3.2), but now including also donor-month fixed effects—given that the fiscal year may vary from country to country. As column 5 indicates, the coefficients of interest barely change.

Third, a critical reader might argue that our measures of commercial similarity could actually be picking up dimensions of similarity other than competition and therefore just proxying for the underlying (unobserved) affinity between donors. An important concern is that not all donors may receive information about disaster impact at the same time, and thus one could expect that more similar donors receive these news at more similar frequencies. If commercial similarity happens to be correlated with information transmission between countries, our results could be spurious.

Table 3.4 – Bandwagon and Competition: Robustness

	Dependent Variable: <i>Aid</i>						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
No. of donors (1d)	0.003*** (0.001)						
Export Competition (1d)		0.007** (0.004)					
Import Competition (1d)		0.001 (0.002)					
No. of donors (7d)			0.001*** (0.000)				
Export Competition (7d)				0.004*** (0.001)			
Import Competition (7d)				0.001*** (0.000)			
Export Competition					0.004** (0.002)	0.004** (0.002)	0.005** (0.002)
Import Competition					0.001 (0.001)	0.001* (0.001)	0.002* (0.001)
Information						-0.001 (0.000)	
Geopolitical Alignment							-0.001 (0.001)
Weekday FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Day FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Emergency-Donor FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Emergency-Day FE	No	Yes	No	Yes	Yes	Yes	Yes
Donor-Month FE	No	No	No	No	Yes	No	No
N	3,542,449	3,221,371	3,542,449	3,221,371	3,221,371	3,221,371	3,204,847

Notes: Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Two-way clustered (at donor and recipient level) standard errors in parentheses. Baseline results using three-day window after decision day. Columns 1 to 4 show results for two alternative time windows, one (1d) and seven days (7d).

To account for this potential source of bias, we construct an information similarity index (*InfoSI*) based on the difference in informational globalization levels between donors i and j : $\Delta KOFI_{i,j,t} = |KOFI_{i,t} - KOFI_{j,t}|$, where *KOFI* is the informational component of the KOF Index of Globalization (Dreher, 2006; Gygli et al., 2019). It combines measures of *de facto* and *de jure* informational globalization, such as internet bandwidth and press freedom, which should affect how fast disaster news arrive in a country. We then normalize $\Delta KOFI_{i,j,t}$ to obtain an index, $InfoSI_{i,j,t}$, that varies between zero and one.³⁴

We expect a pair of countries with a high *InfoSI* to obtain new information about a given disaster at more similar times. Finally, we aggregate it over the donors that committed to assist within the previous three days to construct *Info*, which we include as a control variable in column 6.³⁵ If the informational channel is spuriously driving our results, it should dominate and render *ExpComp* and/or *ImpComp* insignificant.

Reassuringly, the effect of commercial competition remains very similar in terms of size, although less precisely estimated. This provides additional support to our interpretation that donors are not moving in sync because they simply receive disaster information at similar times, but are rather speeding up in response to other donors they see as potential export—and, to a lesser extent, import—competitors in a given recipient country.

Fourth, as discussed previously, another important dimension is geopolitical alignment. On top of being important on its own—as we show in Section 3.2—geopolitical interest may also be correlated with commercial disputes.³⁶ As such, one might argue, for instance, that if donor countries with a similar export structure are, for whichever reason, more geopolitically aligned, we could falsely attribute our previous results to commercial competition rather than geopolitical motivations. To rule out this possibility, we employ a commonly used indicator of geopolitical alignment, based on voting at the United Nations General Assembly, to create a geopolitical similarity index, which we call *GSI* (e.g., Thacker, 1999; Kilby, 2009; Dreher et al., 2008). Similarly to the commercial measures introduced previously, we then aggregate *GSI* into *GeoAlign*. Note that, differently from the approach used to assess geopolitical alignment in Section 3.2 (between donor and recipient), we now use the ideal-point distance between pairs of donors,

³⁴That is, $InfoSI_{i,j,t} = \frac{-\Delta KOFI_{i,j,t} - \text{Min}(-\Delta KOFI_{i,j,t})}{\text{Max}(-\Delta KOFI_{i,j,t}) - \text{Min}(-\Delta KOFI_{i,j,t})}$.

³⁵In line with *ESI* and *ISI*, we use one-year lagged *InfoSI* values.

³⁶Because geopolitical similarity *between donors* does not seem matter on its own—that is, it is insignificant even when it is included in a specification without the commercial similarity variables (not reported)—, in this part of the analysis we focus on the threat posed by it being a potential confounder.

i.e., $\Delta UNGA_{i,j,t} = |idealpoint_{i,t} - idealpoint_{j,t}|$ (data from Bailey et al. 2017). Finally, we normalize this variable such that the index ranges from zero to one. The more similar the votes cast by donor countries i and j in year t are, the higher the index.³⁷

As previously discussed, and differently from our measures of export and import similarity, the implications of donor countries having more similar foreign-policy preferences are not straightforward. Whereas a positive *GeoAlign* coefficient would indicate that donors are more likely to follow the leadership of important countries if they are politically aligned—and thus signal cooperation—, a negative coefficient could be evidence of donor competition fueled by geopolitical rivalries. The net effect is thus an empirical question.

As can be seen in column 7 in Table 3.4, the coefficient for *GeoAlign* is a rather precisely estimated zero, which could indicate that the positive and negative forces described above cancel each other out. Most importantly, our main finding of export and import competition speeding up emergency aid is largely unaffected by the inclusion of *GeoAlign*.

Taken together, our finding that commercial competition—predominantly in terms of exports—accelerates aid giving is robust to various changes in our regression model and is not consistent with alternative interpretations, such as informational similarity and geopolitical alignment, as the coefficients of interest remain remarkably stable across all specifications in Table 3.4.

Furthermore, Appendix Figures C.7 and C.8 show that our results are not driven by a single year or donor, respectively. Rerunning our baseline specifications for the bandwagon as well as the competition hypotheses, but each time excluding one sample year or donor, continues to yield statistically significant coefficients of similar magnitude.

With regard to the measures of commercial competition, *ExpComp* and *ImpComp*, although long established in the literature (Finger and Kreinin, 1979; Fuchs et al., 2015; Asmus-Bluhm et al., 2025), one concern is that they do not take the trade *volume* into account. That is, donors may have similar sectoral shares of exported or imported goods to or from certain recipient country, while substantially differing in terms of the absolute value traded. To account for this issue, we adjust the two similarity indices (before the aggregation) by multiplying each of them with the logged traded amount—exports ($X_{i,r,t-1}$) or imports ($M_{i,r,t-1}$)—between donor i and recipient r in year $t - 1$. We then use the same aggregation method, described in equations 3.3 and 3.4, to calculate these adjusted measures, $ExpComp^{Adj}$ and

³⁷More precisely, $GSI_{i,j,t} = \frac{-\Delta UNGA_{i,j,t} - \text{Min}(-\Delta UNGA_{i,j,t})}{\text{Max}(-\Delta UNGA_{i,j,t}) - \text{Min}(-\Delta UNGA_{i,j,t})}$.

$ImpComp^{Adj}$:

$$ExpComp_{i,j,r,k,[d-3,d-1]}^{Adj} = \sum_j Aid_{j,k,r,[d-3,d-1]} \times \ln(X_{i,r,t-1}) \times ESI_{i,j,r,k,t-1} \quad (3.7)$$

$$ImpComp_{i,j,r,k,[d-3,d-1]}^{Adj} = \sum_j Aid_{j,k,r,[d-3,d-1]} \times \ln(M_{i,r,t-1}) \times ISI_{i,j,r,k,t-1} \quad (3.8)$$

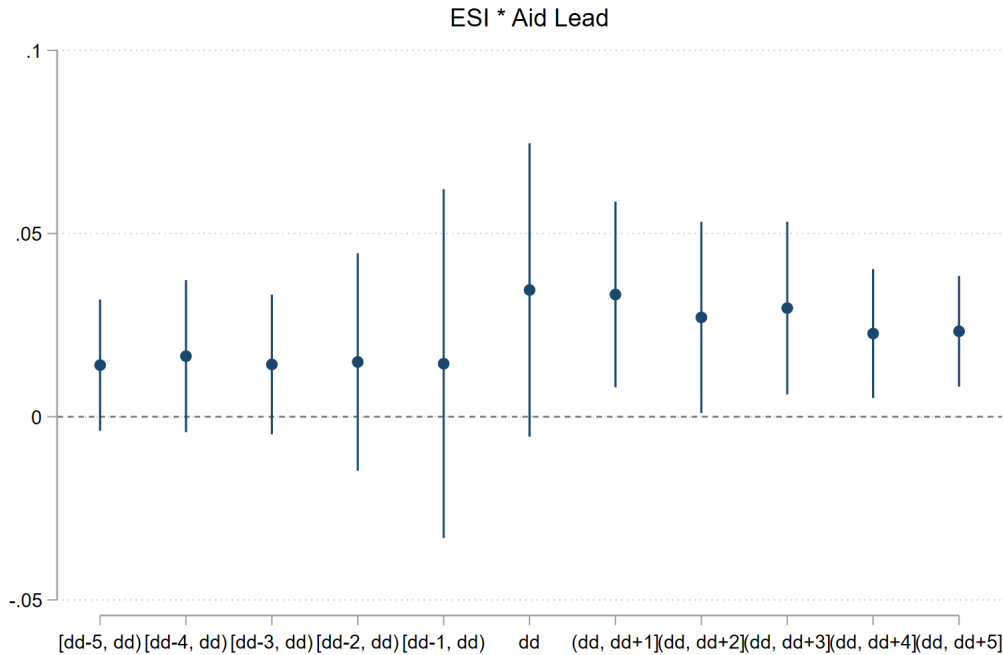
As Appendix Table C.5 shows, our conclusions remain unchanged. The more donors' exports and imports overlap, also when the traded amount is taken into account, the faster donors respond to each other, which we interpret as evidence of commercial competition.

Finally, we use the lead donor setting to conduct a “placebo test.” To increase our confidence that we are indeed identifying the influence that these donors have over their peers and address concerns about spurious correlation, we code placebo aid decision variables that pretend that the lead donor decided to provide assistance *before* it has actually done so—in a window that stretches back in time from one day prior to the actual decision date ($dd - 1$) up to five days in the past ($dd - 5$). In line with expectations, we observe no significant effects when we look at the placebo coefficients, which thus strengthens the argument that the lead donor is indeed leading other donors—and not being led. Accordingly, it is only *after* the lead donor's commitment (from $dd + 1$ onward) that the effect becomes significant—although, as expected, it wanes the further the window stretches in time.³⁸ Figure 3.3 plots the main coefficient of interest, $ESI \times AidLead$, for each of these specifications.

3.5 Mechanisms

In what follows, we examine the forces driving the bandwagon effect as well as commercial competition between donors. First, we investigate the role of different types of humanitarian aid and the way in which it is channeled. Second, we devote more attention to the severity of each disaster to shed more light on donors' motivations.

³⁸Note that, except for the specification that measures the response to the lead donor's decision day (dd) itself, the other models are based on time windows that do not include the decision day. This is consistent with our baseline strategy, which makes use of a window ranging from one to three days after the aid decision and thus does not include the day on which the decision was taken.

Figure 3.3 – Lead Donors: Various Reaction Windows (ESI $\times$ Aid Lead)

Notes: The figure shows the coefficient for ESI $\times$ Aid Lead from different regressions. These are based on the specification in column 3 in Table 3.3 (panel A), but each with a different time window. As indicated on the horizontal axis, these include a window containing only the decision date (*dd*) itself as well as intervals ranging from the decision date (open interval) up to five days prior or after the actual decision date (closed interval). Regressions include donor, and emergency-day fixed effects. The figure shows 95% confidence intervals.

3.5.1 Aid Channel and Type

Donor countries must choose the type of assistance they provide—financial or in kind—and through which channel it is funnelled. Channels include direct transfers to recipient governments or indirect deliveries through multilateral and international organizations (IOs), such as UN agencies, non-governmental organizations (NGOs), and member organizations of the International Federation of Red Cross and Red Crescent Societies (IFRC). As acknowledged by the aid allocation literature (e.g., Cassen, 1986; Dollar and Levin, 2006; Raschky and Schwindt, 2012), these choices too are likely shaped by donors’ strategic interests and reflect, among other considerations, the extent of discretion and flexibility entrusted to recipient countries. In particular, there is evidence that “[d]onors are more likely to give bilateral transfers/cash transfers to countries where they want to promote strategic or political interests” (Raschky and Schwindt, 2012, p. 112). This is because these are typically the modalities that come with fewer restrictions on use and implementation. Therefore, if donors are competing with one another with the goal of maximizing goodwill with the recipient government and hence help secure their commercial interests, we should expect them to choose the type and the channel that are more likely to please its beneficiary. That is, if a donor country wishes to stave off some

commercial competitor who recently committed to assist, it is more likely to be successful if it engages directly with the recipient country.³⁹ The choice of the type of aid, however, seems to be less straightforward, as we will discuss below.

Starting with aid channel, specifications in columns 1 to 4 in Table 3.5 are a variant of our baseline model with emergency-day and emergency-donor fixed effects, in which we replace our dependent variable by a binary variable that indicates whether donor country i on day d decides to provide humanitarian assistance to recipient country r in response to disaster k through the following implementing organizations: recipient government (column 1), IFRC (column 2), IOs (column 3), or NGOs (column 4).

Consistent with the argument that donor countries are competing against each other to please recipient countries that are important trade partners, we observe that our result of commercial competition is indeed driven by government-to-government aid, i.e., the channel usually preferred by recipient governments as this gives them direct control of the funds and goods received (column 1). Furthermore, also in line with our expectations, when donor countries opt to provide emergency aid via non-state actors, commercial competition does not seem to influence their decision (columns 2 to 4). In fact, donor countries may choose these options precisely because they wish to bypass certain recipient governments (Acht et al., 2015).

While these results for aid channels are in line with our expectations, the results for aid types in columns 5 and 6 are, at first glance, puzzling. We find that the strategic competition effect is only statistically significant, at conventional levels, for in-kind aid.⁴⁰ A potential explanation is that, although one could expect attempts to curry favors with local governments to be made predominantly via financial aid (as in Raschky and Schwindt, 2012), in-kind aid may also be used in cases in which it is the most effective—and thus arguably also the one preferred by recipient countries in distress. This is consistent with the fact that the share of in-kind aid is higher for more severe natural disasters.⁴¹ Therefore, in some cases what pleases recipient governments the most may simply be what is most effective in terms of addressing the pressing needs that arise in the aftermath of natural disasters. Importantly, the observed effect on *financial* assistance, though marginally insignificant, helps rule

³⁹Differently from the competition hypothesis, the bandwagon effect does not have clear implications for the choice of aid type and channel. Therefore, in this part, we focus on the strictest specification (with emergency-day fixed effects), in which case we are not able to identify the bandwagon parameter.

⁴⁰Testing the statistical significance of the *ExpComp* coefficient for financial aid (column 5) yields a p-value of 0.118.

⁴¹Dividing the sample by the median number of people killed in each disaster, the share of in-kind flows is 22% for the bottom half of the distribution, but 26% for the upper half—and rises to 32% for the upper quartile.

out an alternative explanation: that donors with similar export structures simply follow each other because they share similar logistical networks in recipient countries.

Table 3.5 – Aid Channel and Type: Competition

	Dependent Variable: <i>Aid</i>					
	(1)	(2)	(3)	(4)	(5)	(6)
Export Competition	0.002** (0.001)	0.001 (0.001)	-0.000 (0.001)	0.000 (0.001)	0.002 (0.001)	0.002** (0.001)
Import Competition	0.001* (0.000)	-0.000 (0.001)	-0.000 (0.000)	0.000 (0.000)	0.000 (0.001)	0.001*** (0.000)
Emergency-Donor FE	Yes	Yes	Yes	Yes	Yes	Yes
Emergency-Day FE	Yes	Yes	Yes	Yes	Yes	Yes
Channel/Type	Govt-Govt	Govt-IFRC	Govt-IO	Govt-NGO	Financial	In Kind
N	3,221,371	3,221,371	3,221,371	3,221,371	3,221,371	3,221,371

Notes: Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Two-way clustered (at donor and recipient level) standard errors in parentheses. Baseline results using three-day window after decision day. The table shows results for bilateral humanitarian aid provided to different implementing agents—recipient government (Govt), the International Federation of Red Cross and Red Crescent Societies (IFRC), multilateral and international organizations (IO), and non-governmental organizations (NGO)—as well as for different aid types, financial and in kind.

3.5.2 Disaster Severity

Donors may react differently depending on how severe natural disasters are. In particular, disaster severity may tilt the balance between humanitarian and strategic objectives one way or another.

On the one hand, more severe emergencies may reduce donors’ ability to act strategically with their aid timing. When faced with large disasters, they are more likely to respond fast regardless of which other donors are moving before them. In other words, donors might have less room for “strategic urgency.” This is not to say that this urgency is driven purely by altruistic motives—media attention, for instance, may propel aid providers to act (Eisensee and Strömberg, 2007)—but it is then not primarily driven by competition with other donors. Minor emergencies, on the other hand, are generally less relevant and salient also for the affected countries, such that the behavior of donors is arguably less scrutinized in these cases. Therefore, we expect medium-scale disasters to offer the most room for strategic considerations: they are significant enough to warrant attention but not so devastating such that most donors have basically no alternative but to act fast.

To examine whether this is the case, we rerun our preferred specification across different levels of disaster severity, dividing our sample into quintiles based on the death toll of each emergency. In line with our argument above, Table 3.6 shows that export competition occurs primarily in settings of medium severity (second and third quintiles). The number of deaths in these emergencies ranges from 18 to

Table 3.6 – Disaster Severity: Competition

	Dependent Variable: <i>Aid</i>				
	(1)	(2)	(3)	(4)	(5)
Export Competition	0.001 (0.002)	0.005* (0.002)	0.006*** (0.002)	0.000 (0.002)	0.006 (0.004)
Import Competition	-0.001 (0.002)	0.002 (0.002)	-0.000 (0.001)	0.002 (0.002)	0.002 (0.001)
Emergency-Day FE	Yes	Yes	Yes	Yes	Yes
Emergency-Donor FE	Yes	Yes	Yes	Yes	Yes
Quintile	Q1	Q2	Q3	Q4	Q5
N	609,632	597,052	589,310	581,162	477,026

Notes: Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Two-way clustered (at donor and recipient level) standard errors in parentheses. Baseline results using three-day window after decision day. The quintiles of disaster severity are calculated based on the death toll of each emergency.

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We also test the bandwagon hypothesis across quintiles of disaster severity (again excluding emergency-day fixed effects for obvious reasons). We find the bandwagon effect to be robust across the severity distribution and to become more pronounced as disaster severity increases (Appendix Table C.6). This indicates that, while the the strategic competition effect is limited to mid-sized catastrophes, donors seem to consistently take cues from their peers and even more so when the stakes are highest.

3.6 Conclusion

Although the allocation of emergency relief has been extensively studied (e.g., Drury et al., 2005; Fink and Redaelli, 2011; Raschky and Schwindt, 2012), far less attention has been given to one of the most critical determinants of its effectiveness: the speed of aid. Our analysis reveals substantial variation in how quickly donor countries respond to fast-onset natural disasters, providing new evidence that these differences are influenced by strategic motives alongside humanitarian considerations. Specifically, we find that close trade partners receive faster assistance.

Moreover, the results from our empirical analysis at the daily level highlight the powerful influence that donors have on each other. On the one hand, we observe a bandwagon effect: whenever donors decide to provide assistance, they encourage their peers to quickly follow suit. In the context of humanitarian relief, this shows that certain donors can play an important role to improve the speed—and thus the effectiveness—of aid provision. On the other hand, our findings indicate that the extent to which donors respond to other aid providers is mediated by their own

agendas, and in particular by an effort to secure their export markets and, to a lesser extent, the source of their imports. The more donors compete, commercially, over a given recipient country, the faster they respond to each other. Consistent with the expectation that the choice of aid channel is endogenous to donors' motivations, our findings are driven by government-to-government aid.

While one could naively conclude that commercial competition could end up being beneficial as it induces donors to act faster, there are at least three important caveats. First, assistance motivated by donors' own commercial goals is arguably less effective than a pure need-based approach.⁴² Second, based on the ethical principles of humanitarian law, emergency aid has been devised as a tool to provide quick relief to countries in distress and, to a much greater extent than development aid, is often advertised as being altruistic (Drury et al., 2005). As our results indicate, however, this is not what happens in practice. This is likely to thus (further) erode donor credibility and create even more obstacles for a coordinated response. Third, to the extent that donor competition fuels aid fragmentation, it further compromises the effectiveness of emergency relief, in particular—in the case of humanitarian aid—by overburdening recipient bureaucracies at times of great vulnerability.⁴³

We acknowledge several limitations and avenues for future research. First, we do not examine actual delivery date and focus rather on commitment dates. Although aid commitments are legally binding and arguably the most relevant variable from a strategic point of view, if the data were available, expanding the analysis to include the day on which aid flows arrive at the disaster-affected area would further improve our understanding about the implications for aid effectiveness. Second, future research on emergency aid should put more emphasis on how aid requests from disaster-affected countries affect donor response, as recipient behavior is an important part of the donor decision to provide aid (Carnegie and Dolan, 2021). Third, future studies on emergency aid could compare the speed with which donor countries respond with that of international organizations and private donors. This is particularly relevant given the evidence that the United Nations adheres to its core principles of neutrality, impartiality, and independence in disaster aid allocation (Dellmuth et al., 2021). Finally, although a speedy decision-making process

⁴²One strand of the aid literature argues that donor motives matter for aid effectiveness (Kilby and Dreher, 2010; Dreher et al., 2013; Dreher et al., 2018).

⁴³There is plenty of evidence of the detrimental consequences of donor fragmentation. See, for example, Easterly (2007), Knack and Rahman (2007), Djankov et al. (2009), and Gutting and Steinwand (2017)—though Gehring et al. (2017) disagree. However, on the positive side, fragmentation may also reduce the likelihood of negative aid shocks and hence of violent conflicts in recipient countries (Nielsen et al., 2011; Strange et al., 2017). While this potentially beneficial aspect should not be neglected, it is much less relevant for emergency assistance than for general development assistance.

is an important prerequisite for (most types of) disaster aid to be successful, a fast response following a disaster is not the only objective of emergency assistance. Disaster preparedness, for example, should be an important part of humanitarian aid activities (Clarke and Dercon, 2016). To the extent to which a long decision time stems from aid coordination efforts among donors, donors should not be solely judged on their aid promptness. Beyond the timeliness of the aid decision, future research should evaluate the effectiveness of disaster aid efforts more broadly, the study of which is currently largely confined to country case studies.

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Appendix A

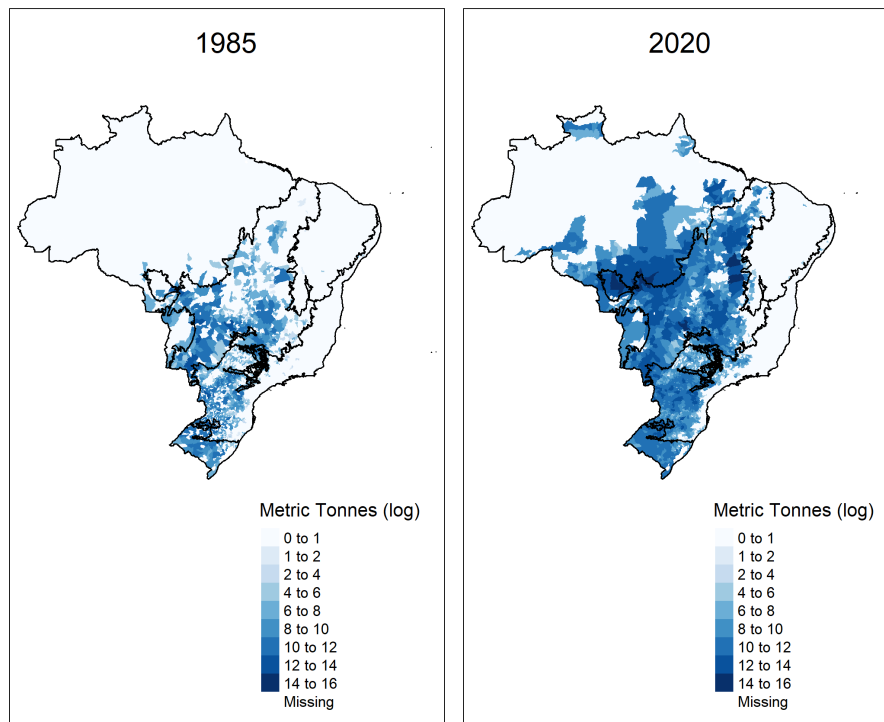
The (Other) China Shock and the Brazilian Soy Boom: Cui Bono?

A.1 Acknowledgements

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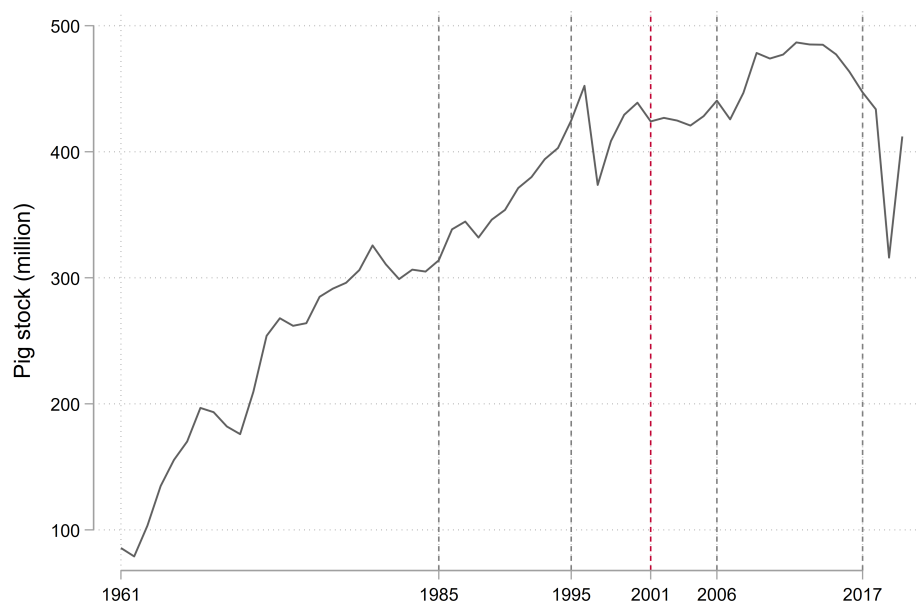
A.2 Additional Figures and Tables

Figure A.1 – Municipal Soybean Production



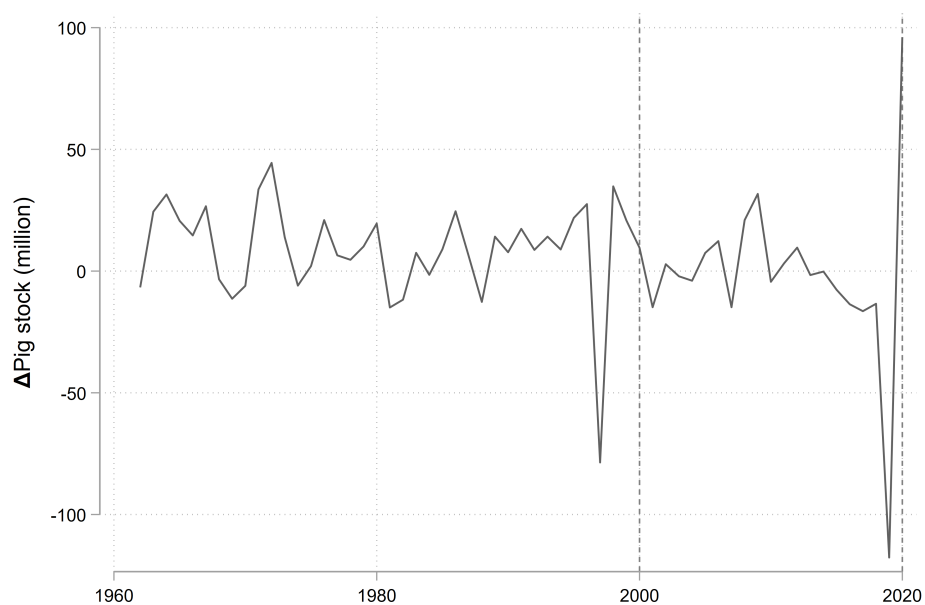
Notes: Figure shows soybean production (log) in each Brazilian municipality in 1985 (left) and 2020 (right). A unit (metric tonne) is added to each municipality before the log transformation to avoid losing observations. Data source: IBGE (2023a).

Figure A.2 – Chinese Pig Stock



Notes: Figure shows the size of the Chinese pig stock (in million) over time. Vertical gray lines indicate the four census waves used in Section 1.3. The vertical red line marks the year of China's accession to the WTO. Data source: FAO (2023).

Figure A.3 – Chinese Pig Stock: Annual Variation



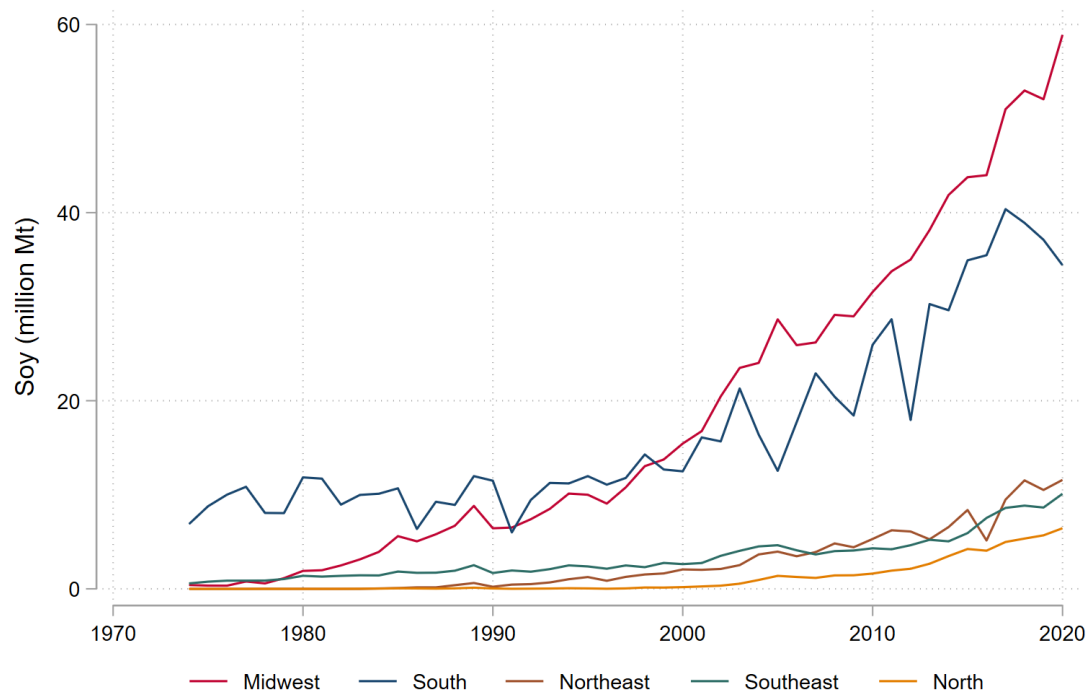
Notes: Figure shows the annual variation in size of the Chinese pig stock (in million) over time. Vertical gray lines indicate the period of analysis in Section 1.4. Data source: FAO (2023).

Figure A.4 – Fazenda Angela Tereza



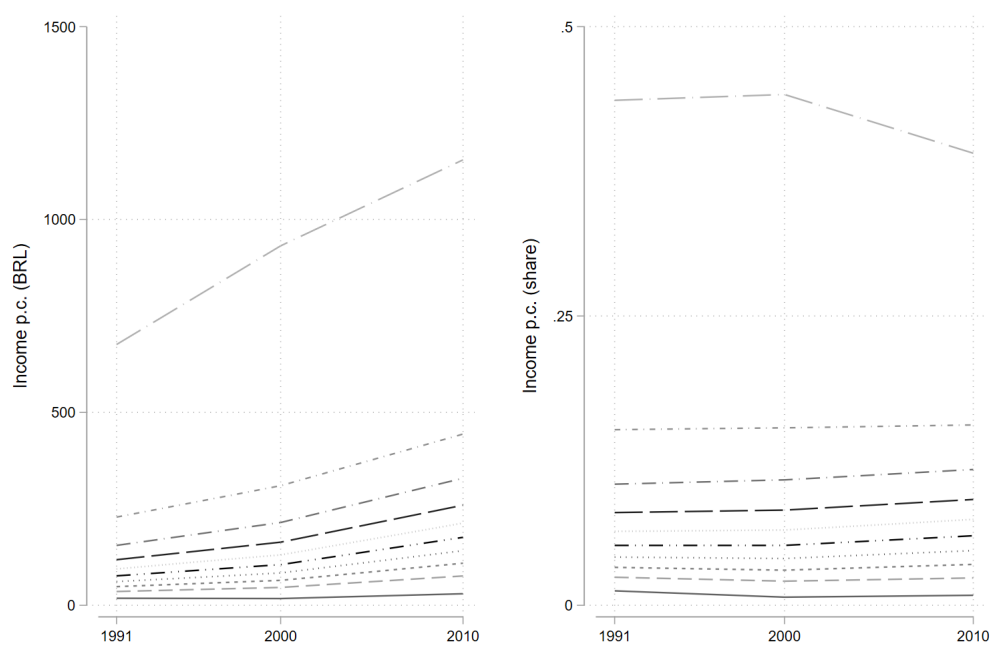
Notes: Satellite images, from Google, of Sorriso (MT). Fazenda Angela Tereza is located at 13° 16' 54" S, 55° 22' 27" W.

Figure A.5 – Total Soybean Production by Region



Notes: Figure shows total annual production of soybeans across all five Brazilian major regions over time. Data source: IBGE (2023a).

Figure A.6 – Income p.c.: Trends by Decile



Notes: The figure plots, for each income decile, the evolution of average per-capita income by decile in constant 2000 BRL (left-hand side) and each decile's share of the total municipal income (right-hand side) during the last three demographic census waves (IBGE, 1991; 2000; 2010).

Table A.1 – Descriptive Statistics

Variable	Mean	SD	Min	Max	Count
Panel A: Data used in Section 1.3					
Land Gini	0.72	0.11	0.16	0.99	15,149
WTO	0.50	0.50	0.00	1.00	15,196
Soy (log)	2.57	3.86	0.00	16.41	15,176
Soil	4.83	1.93	0.00	9.89	15,196
Population (log)	9.70	1.18	6.57	16.30	15,196
Imports (log)	15.73	1.50	14.09	17.87	15,196
Δ Pigstock	45.79	40.92	6.59	110.93	15,196
Property Area (log)	3.84	1.04	0.05	8.44	15,149
No. Properties (log)	1.70	2.44	0.00	11.60	15,190
No. Coop (log)	2.41	3.31	0.00	12.51	15,190
Panel B: Data used in Section 1.4					
Spatial Gini	0.70	0.27	0.02	1.00	116,210
Soy (log)	3.01	4.34	0.00	14.64	116,970
Soy CHN (log)	1.58	3.38	0.00	14.30	94,690
Soy EU (log)	1.19	2.88	0.00	13.45	94,690
Soil Suitability	4849.59	1934.24	0.00	9995.00	116,865
Δ Pigstock	-0.82	35.26	-117.68	96.11	116,970
NTL	1.03	0.97	0.00	4.16	116,865
Δ Cattle (CHN)	-1.94	3.08	-12.43	2.64	116,970
Δ Steel (CHN)	4478.60	2834.81	-1848.13	8925.67	116,970
Δ Pigstock (EU)	-0.63	2.32	-5.33	2.77	116,970
GDP pc (log)	7.81	1.05	-0.17	12.34	116,821
Spatial Gini (VIIRS)	0.34	0.17	0.02	1.00	50,084
Corn (log)	6.59	2.90	0.00	15.16	116,970
Sugarcane (log)	5.53	4.74	0.00	16.14	115,059
Soy Ngb. (ln)	8.91	20.02	0.00	150.83	116,970
Deforestation (log)	5.53	1.13	0.00	9.55	116,970
Land Invasion (log)	0.04	0.47	0.00	9.50	61,270

Table A.2 – Land Inequality (1st Stage)

<i>Instrumented Variable:</i>	Soy (log) (1)	Soy (log) (2)	Soy (log) (3)	WTO $\times$ Soy (log) (4)	Soy (log) (5)	WTO $\times$ Soy (log) (6)
Δ Pigstock $\times$ Soil	0.000** (0.000)	-0.007*** (0.001)	-0.003*** (0.000)	-0.011*** (0.000)	-0.001*** (0.000)	-0.007*** (0.000)
WTO $\times$ Δ Pigstock $\times$ Soil			-0.007*** (0.001)	0.002*** (0.001)	0.006*** (0.001)	0.041*** (0.001)
WTO			1.130*** (0.058)	2.729*** (0.072)	-1.145*** (0.127)	-4.194*** (0.159)
Δ Pigstock			0.014*** (0.001)	0.051*** (0.001)	0.004*** (0.001)	0.021*** (0.001)
Imports (log)					0.485*** (0.028)	1.476*** (0.037)
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Period FE	Yes	Yes	No	No	No	No
N	7,556	7,556	15,133	15,133	15,133	15,133

Notes: Δ *Pigstock* is the annual change in the size of the total pig stock in China. *Soil* is the average soil suitability for soybeans in the municipality. *WTO* is a binary variable that takes the value 1 if period *p* is after 2001. *Imports (log)* stands for the natural logarithm of Chinese annual soybean imports. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors, clustered at the level of 1970 MCAs, in parentheses.

Table A.3 – Spatial Inequality (1st Stage)

<i>Instrumented Variable:</i>		Soy (log)			Soy CHN (log)	
	(1)	(2)	(3)	(4)	(5)	(6)
Δ Pigstock $\times$ Soil	-0.004*** (0.000)	-0.002*** (0.000)	-0.002*** (0.000)	-0.003*** (0.000)	-0.002*** (0.000)	-0.002*** (0.000)
GDP (log)			0.006 (0.005)			-0.001 (0.008)
Tax (log)			0.008 (0.006)			0.018** (0.008)
VA Industry (log)			0.007 (0.006)			-0.006 (0.008)
VA Services (log)			0.004 (0.006)			0.020** (0.008)
VA Agro (log)			0.055*** (0.006)			0.010 (0.008)
Population (log)			-0.255** (0.120)			0.290* (0.152)
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	-	-	Yes	-	-
Region-Year FE	No	Yes	Yes	No	Yes	Yes
N	116,203	116,203	115,994	88,783	88,783	88,662

Notes: Δ *Pigstock* is the annual change in the size of the total pig stock in China. *Soil* is the average soil suitability for soybeans in the municipality. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors, clustered at the municipality level, in parentheses. Note that the point estimates and standard errors relative to the instrumental variable (Δ *Pigstock* $\times$ *Soil*) are multiplied by 1,000 for presentation purposes.

Table A.4 – Spatial Gini: Quintiles and Regions

<i>Dependent Variable: Spatial Gini</i>					
<i>Panel A – Regions</i>					
	(1)	(2)	(3)	(4)	(5)
Soy (log)	0.030*** (0.006)	0.085 (0.191)	0.820*** (0.298)	0.019*** (0.004)	-0.270 (0.344)
Region	Midwest	South	Northeast	Southeast	North
Municipality FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
Model	2SLS	2SLS	2SLS	2SLS	2SLS
N	9,717	24,563	37,549	34,994	9,380
KP F-stat	35.09	0.50	8.09	353.47	0.59
<i>Panel B – GDP per capita (Quintiles)</i>					
	(1)	(2)	(3)	(4)	(5)
Soy (log)	0.264* (0.154)	0.585** (0.260)	0.041*** (0.014)	-0.015 (0.018)	-0.151** (0.066)
Quantile	Q1	Q2	Q3	Q4	Q5
Municipality FE	Yes	Yes	Yes	Yes	Yes
Region-Year FE	Yes	Yes	Yes	Yes	Yes
Controls	No	No	No	No	No
N	23,037	23,054	22,976	22,975	23,059
KP F-stat	4.21	5.43	38.64	20.74	6.23

Notes: Soy (log) is the natural logarithm of municipal soybean production. First-stage Kleibergen-Paap F-statistic reported. Significance levels: * p < 0.1, ** p < 0.05, *** p < 0.01. Robust standard errors, clustered at the municipality level, in parentheses.

Table A.5 – Spatial Autocorrelation

<i>Dependent Variable: Spatial Gini</i>					
	(1)	(2)	(3)	(4)	(5)
Soy (log)	0.067*** (0.016)	0.067*** (0.021)	0.067*** (0.025)	0.067** (0.027)	0.067** (0.029)
Municipality FE	Yes	Yes	Yes	Yes	Yes
Region-Year FE	Yes	Yes	Yes	Yes	Yes
Controls	No	No	No	No	No
Model	2SLS	2SLS	2SLS	2SLS	2SLS
SE	Conley (50km)	Conley (100km)	Conley (150km)	Conley (200km)	Conley (250km)
N	116,203	116,203	116,203	116,203	116,203
KP F-stat	73.30	40.15	25.67	18.72	15.51

Notes: Soy (log) is the natural logarithm of municipal soybean production. First-stage Kleibergen-Paap F-statistic reported. Significance levels: * p < 0.1, ** p < 0.05, *** p < 0.01. Conley standard errors for different distance cut-offs in parentheses.

Table A.6 – Alternative Instruments

	<i>Dependent Variable: Spatial Gini</i>			
	(1)	(2)	(3)	(4)
Soy (log)	0.798*** (0.205)	1.359** (0.593)	-6.661 (8.958)	-1.410 (1.030)
Instrument	Δ Pig Stock (China)	Δ Cattle Stock (China)	Δ Steel (China)	Δ Pig Stock (EU)
Municipality FE	Yes	Yes	Yes	Yes
Region-Period FE	Yes	Yes	Yes	Yes
Controls	No	No	No	No
Trend	Yes	Yes	Yes	Yes
N	116,203	116,203	116,203	116,203
KP F-stat	15.19	5.23	0.55	1.88

Notes: Soy (log) is the natural logarithm of municipal soybean production. Instrumental variable: Δ *Pig Stock (China)* is the annual change in the size of the total pig stock in China (column 1); Δ *Cattle Stock (China)* is the annual change in the size of the total cattle stock in China (column 2); Δ *Steel (China)* is the annual change in the total Chinese production of crude steel (column 3); and Δ *Pig Stock (EU)* is the annual change in the size of the total pig stock in European Union (column 4). First-stage Kleibergen-Paap F-statistic reported. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors, clustered at the municipality level, in parentheses.

Table A.7 – European Demand

	<i>Dependent Variable: Spatial Gini</i>			
	(1)	(2)	(3)	(4)
Soy (log)	0.002 (0.010)	-0.006 (0.011)		
Soy EU (log)			0.029*** (0.010)	0.027*** (0.010)
Municipality FE	Yes	Yes	Yes	Yes
Region-Year FE	Yes	Yes	Yes	Yes
Controls	Yes	No	Yes	No
Model	2SLS	2SLS	2SLS	2SLS
N	115,994	116,203	88,662	88,783
KP F-stat	41.28	30.96	29.69	29.39

Notes: Soy (log) is the natural logarithm of municipal soybean production. Soy EU (log) is the natural logarithm of municipal soybean exports to the European Union. First-stage Kleibergen-Paap F-statistic reported. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors, clustered at the municipality level, in parentheses.

Table A.8 – Sectoral Value Added

<i>Dependent Variable:</i>	VA Agriculture		VA Industry		VA Service	
	(1)	(2)	(3)	(4)	(5)	(6)
Soy (log)	0.037 (0.031)	2.544*** (0.487)	-0.538* (0.289)	3.927 (7.335)	-0.437 (0.394)	-3.841 (12.525)
GDP	0.000 (0.000)	0.000 (0.000)	0.021 (0.016)	0.021 (0.016)	-0.068*** (0.025)	-0.068*** (0.025)
Population	0.000** (0.000)	0.000** (0.000)	-0.002* (0.001)	-0.002* (0.001)	0.003* (0.002)	0.003* (0.002)
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Region-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Model	OLS	2SLS	OLS	2SLS	OLS	2SLS
N	116,775	116,775	116,775	116,775	116,775	116,775
KP F-stat		114.95		114.95		114.95

Notes: Soy (log) is the natural logarithm of municipal soybean production. First-stage Kleibergen-Paap F-statistic reported. Significance levels: * p < 0.1, ** p < 0.05, *** p < 0.01. Robust standard errors, clustered at the municipality level, in parentheses.

Table A.9 – Inequality: Light and Population

<i>Dependent Variable:</i>	NTL Gini		Pop. Gini	
	(1)	(2)	(3)	(4)
Soy (log)	-0.000 (0.001)	0.069*** (0.013)	-0.000*** (0.000)	-0.008*** (0.002)
Municipality FE	Yes	Yes	Yes	Yes
Region-Year FE	Yes	Yes	Yes	Yes
Controls	No	No	No	No
Model	OLS	2SLS	OLS	2SLS
N	116,204	116,204	116,858	116,858
KP F-stat		114.43		118.14

Notes: Soy (log) is the natural logarithm of municipal soybean production. First-stage Kleibergen-Paap F-statistic reported. Significance levels: * p < 0.1, ** p < 0.05, *** p < 0.01. Robust standard errors, clustered at the municipality level, in parentheses.

Table A.10 – Quantile Analysis

	Income p.c. (BRL) (1)	Income p.c. (share) (2)	Income p.c. (BRL) (3)	Income p.c. (share) (4)
Soy (log)	-13.618*** (0.666)	-0.000*** (0.000)	-147.306*** (7.464)	-0.002*** (0.000)
Soy × D2	3.425*** (0.196)	0.000*** (0.000)	36.236*** (1.686)	0.004*** (0.000)
Soy × D3	5.605*** (0.306)	0.001*** (0.000)	61.311*** (2.846)	0.008*** (0.000)
Soy × D4	7.887*** (0.406)	0.001*** (0.000)	85.230*** (3.952)	0.011*** (0.001)
Soy × D5	10.331*** (0.502)	0.001*** (0.000)	108.456*** (5.015)	0.014*** (0.001)
Soy × D6	12.261*** (0.580)	0.002*** (0.000)	129.919*** (6.000)	0.016*** (0.001)
Soy × D7	13.842*** (0.676)	0.001*** (0.000)	154.684*** (7.211)	0.015*** (0.001)
Soy × D8	16.509*** (0.823)	0.001*** (0.000)	189.799*** (8.881)	0.015*** (0.001)
Soy × D9	20.500*** (1.085)	0.000*** (0.000)	233.178*** (10.939)	0.005*** (0.001)
Soy × D10	40.504*** (4.140)	-0.007*** (0.001)	501.144*** (25.622)	-0.067*** (0.003)
Decile-Municipality FE	Yes	Yes	Yes	Yes
Region-Period FE	Yes	Yes	Yes	Yes
Model	OLS	OLS	2SLS	2SLS
N	114,373	114,373	114,364	114,364
KP F-stat			16.39	16.39

Notes: Soy (log) is the natural logarithm of municipal soy production. D2 to D10 stand for each municipality-period income decile. Monetary values in constant 2000 BRL. Income per capita share represents each decile's share of the total income per capita in a given municipality-period pair. First-stage Kleibergen-Paap F-statistic reported. Significance levels: * p < 0.1, ** p < 0.05, *** p < 0.01. Robust standard errors, clustered at the level of 1980 MCAs, in parentheses.

Table A.11 – Income Gini

<i>Dependent Variable: Income Gini</i>						
<i>Panel A – OLS</i>						
	(1)	(2)	(3)	(4)	(5)	(6)
Soy (log)	-0.000 (0.000)	0.001 (0.001)	0.001 (0.001)	-0.001* (0.001)	0.003* (0.001)	-0.003** (0.002)
Region	Brazil	Midwest	South	Southeast	Northeast	North
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Period FE	No	Yes	Yes	Yes	Yes	Yes
Region-Period FE	Yes	No	No	No	No	No
N	11,472	768	2,016	4,200	3,972	516
<i>Panel B – 2SLS</i>						
	(1)	(2)	(3)	(4)	(5)	(6)
Soy (log)	-0.007** (0.003)	-0.047 (0.163)	-0.009** (0.004)	-0.003 (0.003)	0.677 (8.366)	0.011 (0.022)
Region	Brazil	Midwest	South	Southeast	Northeast	North
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Period FE	No	Yes	Yes	Yes	Yes	Yes
Region-Period FE	Yes	No	No	No	No	No
N	11,472	768	2,016	4,200	3,972	516
KP F-stat	164.12	0.09	56.71	170.88	0.01	1.80

Notes: Soy (log) is the natural logarithm of municipal soy production. First-stage Kleibergen-Paap F-statistic reported. Significance levels: * p < 0.1, ** p < 0.05, *** p < 0.01. Robust standard errors, clustered at the level of 1980 MCAs, in parentheses.

Table A.12 – Deforestation and Rural Conflict: Regions

<i>Panel A – Dependent Variable: Deforestation (log)</i>					
	(1)	(2)	(3)	(4)	(5)
Soy (log)	0.115*** (0.023)	1.289 (17.538)	1.010*** (0.356)	-0.012*** (0.003)	-0.026 (0.257)
Region	Midwest	South	Northeast	Southeast	North
Municipality FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Controls	No	No	No	No	No
Model	2SLS	2SLS	2SLS	2SLS	2SLS
N	9,786	24,948	37,674	35,022	9,428
KP F-stat	35.18	0.01	8.13	353.02	0.51
<i>Panel B – Dependent Variable: Land Invasion (log)</i>					
	(1)	(2)	(3)	(4)	(5)
Soy (log)	0.167*** (0.058)	-0.012 (0.044)	-0.337 (0.257)	-0.001 (0.005)	-1.705 (1.376)
Region	Midwest	South	Northeast	Southeast	North
Municipality FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Region-Year FE	No	No	No	No	No
Controls	No	No	No	No	No
Model	2SLS	2SLS	2SLS	2SLS	2SLS
N	5,126	13,068	19,734	18,348	4,939
KP F-stat	26.12	1.65	6.83	266.56	1.73

Notes: Soy (log) is the natural logarithm of municipal soy production. First-stage Kleibergen-Paap F-statistic reported. Significance levels: * p < 0.1, ** p < 0.05, *** p < 0.01. Robust standard errors, clustered at the municipality level, in parentheses.

Appendix B

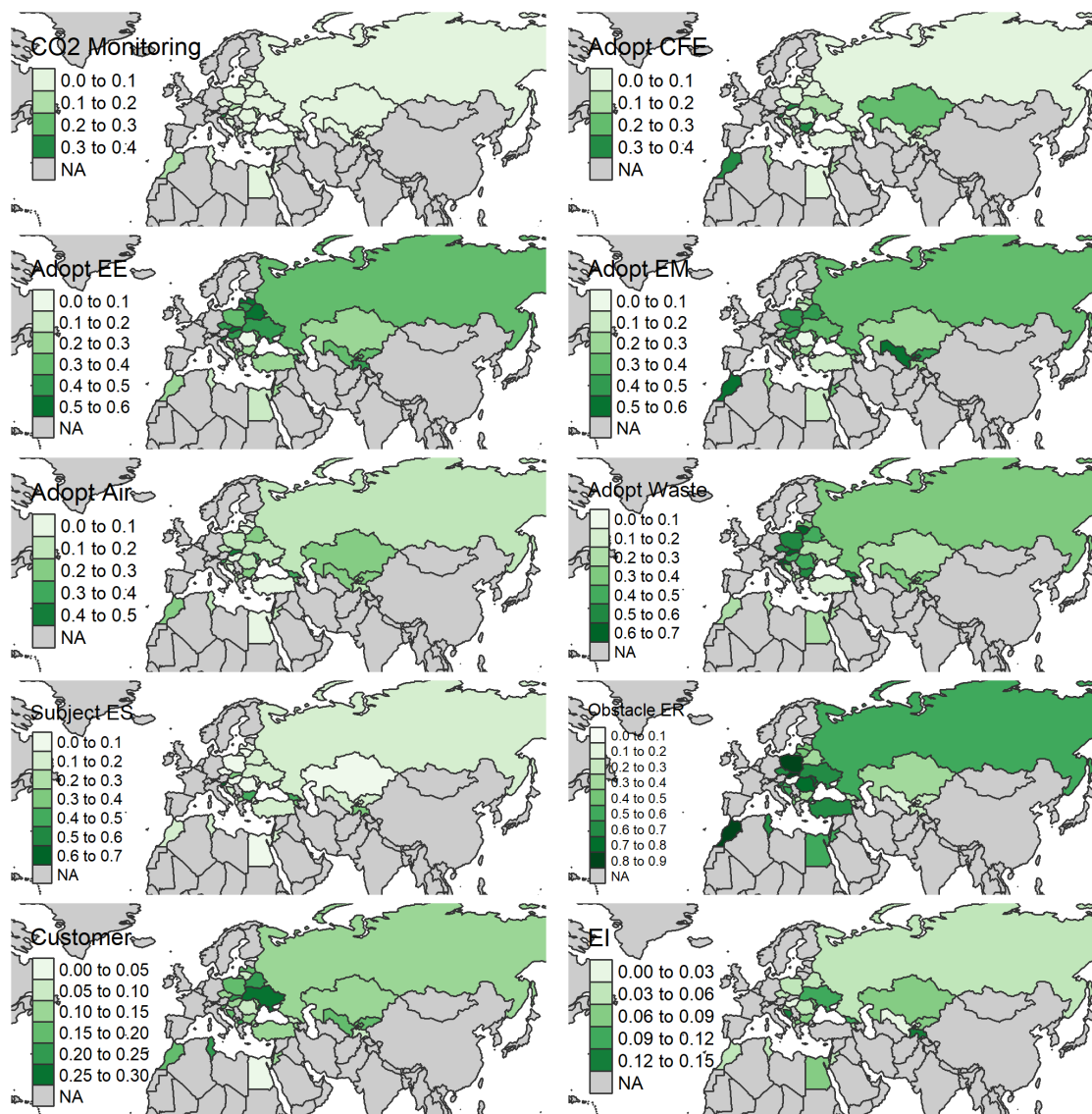
Global Value Chains and Firms’ Environmental Performance

B.1 Acknowledgements

Chapter 2 benefited from the comments and suggestions made by two anonymous reviewers, as well as Laura Barros, Davide Del Prete, Andreas Fuchs, and the participants at the GIGA Workshop Economics and Governance of Sustainability in Global Value Chains (October 2021), the Göttingen Development Economics Seminar (May 2022), the XVII Arnoldshain Seminar (June 2022), and the International Conference on Development Economics (June 2022). We also thank Atanas Spasov for proofreading this article. Funding from the German Federal Ministry for Economic Cooperation and Development through the Research Network Sustainable Global Supply Chains (project no. 2020.9591.7) is gratefully acknowledged. Inmaculada Martínez-Zarzoso acknowledges support from the projects: PID2020-114646RB-C42 funded by MCIN-AEI/10.13039/501100011033 (Ministerio de Ciencia e Innovación), CIPROM/2022/50 PROMETEO (Generalitat Valenciana) and UJI-B2020-57 (Universitat Jaume I).

B.2 Additional Figures and Tables

Figure B.1 – Mean Environmental Outcomes



Notes: Figure reports mean rates for each variable (as indicated) for all countries in our sample during the second wave (2018–20). Authors' calculations based on WBES data (World Bank, 2021).

Table B.1 – Number of Observations by Dependent Variable and Country

	CO ₂ Monitor	Adopt CFE	Adopt EE	Adopt EM	Adopt Air	Adopt Waste	Subject ES	Obstacle ER	Customer	EI (fd)
Albania	32	32	32	32	31	32	32	31	32	17
Armenia	19	15	20	20	16	16	20	19	20	6
Belarus	25	25	25	25	24	24	24	24	25	12
Bosnia and Herzegovina	21	22	23	22	20	20	23	20	23	9
Bulgaria	24	20	22	23	22	20	20	23	24	9
Croatia	9	9	9	9	9	9	9	12	9	11
Czech Republic	10	8	10	8	9	10	10	11	10	9
Egypt	155	143	146	151	148	144	153	154	155	117
Georgia	22	21	22	19	19	19	20	21	22	12
Hungary	15	13	15	15	12	15	15	15	15	2
Jordan	54	54	53	53	50	49	50	58	53	27
Kazakhstan	38	36	37	35	35	34	36	35	38	9
Kyrgyz Republic	46	45	45	46	45	46	44	46	45	14
Latvia	11	9	11	11	9	10	11	10	11	0
Lebanon	45	45	45	45	44	45	45	46	45	36
Lithuania	17	16	17	16	14	16	16	17	17	0
Macedonia, FYR	25	25	25	25	25	25	25	24	25	20
Moldova	28	26	28	27	26	26	28	28	28	17
Morocco	37	25	35	27	25	24	37	34	37	6
Poland	37	32	37	33	32	34	35	38	37	2
Romania	24	19	25	22	23	23	25	24	26	12
Russian Federation	174	169	166	171	168	171	167	171	175	38
Slovak Republic	10	9	10	10	9	10	10	9	10	0
Slovenia	5	6	6	6	6	6	6	6	6	0
Tajikistan	42	40	38	40	39	40	36	39	41	18
Tunisia	65	57	58	61	44	54	64	64	65	46
Turkey	299	299	292	298	298	299	299	232	301	0
Ukraine	89	90	86	92	88	90	88	88	91	31
Uzbekistan	50	49	48	49	47	49	47	47	49	29

Table B.2 – Variable Definitions

Variable	Definition
GVC Switcher	Firm becomes a two-way trader
GVC Strong Switcher	Firm becomes a two-way trader and internationally certified
CO ₂ Monitor	Firm monitors CO ₂ emissions along its supply chain (last 3FY)
Adopt CFE	Firm adopts more climate-friendly energy generation on site (last 3FY)
Adopt EM	Firm adopts energy management (last 3FY)
Adopt Waste	Firm adopts waste minimization, recycling and waste management (last 3FY)
Adopt Air	Firm adopts air pollution control measures (last 3FY)
Adopt EE	Firm adopts any measures to enhance energy efficiency (last 3FY)
Subject ES	Firm is subject to an energy performance standard in its operations (last 1FY)
Obstacle ER	Environmental regulations are an obstacle
Customers	Customers require certifications or adherence to some environmental standards
EI	Total annual cost of electricity and fuel divided by total sales
Exporter	Firm exports
Importer	Firm imports
Foreign Owned	Firm is owned by foreigners (10% or more)
Workers	Total number of workers
SIS	Number of non-production workers divided by total number of workers
Firm Age	Firm age in years
TFP	Total factor productivity (Wooldridge 2009)
Manager	Top manager's years of experience working in the sector
Obstacle Tax	Tax rates are an obstacle
Obstacle Labor	Labor regulations are an obstacle
ECI	Environmental Composite Index
ACI	Adoption Composite Index
CCI	Constraint Composite Index
Dist	Straight-line shortest distance from firm's geographic coordinates to the coast (in km)
EU	Firm is located in the European Union

Table B.3 – Descriptive Statistics

	Mean	SD	Min	Max	Count
GVC Switcher	0.17	0.37	0.00	1.00	1,459
GVC Strong Switcher	0.07	0.26	0.00	1.00	1,393
CO ₂ Monitor	0.03	0.18	0.00	1.00	1,428
Adopt CFE	0.09	0.29	0.00	1.00	1,359
Adopt EE	0.28	0.45	0.00	1.00	1,386
Adopt EM	0.28	0.45	0.00	1.00	1,391
Adopt Air	0.14	0.34	0.00	1.00	1,337
Adopt Waste	0.29	0.45	0.00	1.00	1,360
Subject ES	0.13	0.34	0.00	1.00	1,395
Obstacle ER	0.53	0.50	0.00	1.00	1,346
Customers	0.13	0.34	0.00	1.00	1,435
EI	0.06	0.11	0.00	1.95	768
Exporter	0.17	0.37	0.00	1.00	1,459
Importer	0.44	0.50	0.00	1.00	1,459
Foreign-Owned	0.05	0.22	0.00	1.00	1,459
Workers (log)	3.19	1.24	0.01	7.00	1,459
SIS	0.27	0.19	0.00	1.00	1,416
Firm Age	22.70	16.18	1.00	162.00	1,438
TFP (log)	8.22	1.55	3.63	15.34	538
Manager	22.36	11.69	1.00	65.00	1,426
Obstacle Tax	0.78	0.41	0.00	1.00	1,425
Obstacle Labor	0.55	0.50	0.00	1.00	1,434
ECI	0.21	0.20	0.00	1.00	1,110
ACI	0.21	0.27	0.00	1.00	1,237
CCI	0.27	0.25	0.00	1.00	1,284
Dist	352.94	458.86	0.01	1966.51	1,459
EU	0.12	0.32	0.00	1.00	1,459

Notes: CFE, EE, EM, ES, ER and EI denote climate-friendly energy generation, energy efficiency, energy management, energy performance standards, environmental regulations and energy use intensity, respectively. See Table B.2 for the complete definition of each variable.

Table B.4 – Probit

	GVC Switcher	
	(1)	(2)
Exporter	0.479*** (0.108)	0.349 (0.252)
Importer	0.237** (0.102)	0.335* (0.188)
Foreign-Owned	0.035 (0.169)	-0.044 (0.367)
Workers (log)	0.193*** (0.036)	0.060 (0.080)
SIS		-0.590 (0.382)
Firm Age		0.011** (0.005)
TFP (log)		0.125** (0.061)
Country FE	Yes	Yes
Industry FE	Yes	Yes
N	1,459	510
No. of Countries	29	22

Notes: The table reports the propensity score estimation (probit). Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses. In column 1, firms are matched on the baseline set of covariates: (ln) number of employees, exporter dummy, importer dummy, and foreign-owned dummy. The specification in column 2 controls, in addition, for skill intensity share, firm age, and (ln) TFP. All probit specifications include country and sector fixed effects. Variable definitions are listed in Table B.2.

Table B.5 – Lagged GVC Participation Status

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
<i>Dependent Variable:</i>	CO ₂ Monitor	Adopt CFE	Adopt EE	Adopt EM	Adopt Air	Adopt Waste	Subject ES	Obstacle ER	Customers	EI (fd)
ATT (GVC, lagged)	0.035*** (0.011)	0.051*** (0.017)	0.118*** (0.024)	0.029 (0.036)	0.064*** (0.018)	0.108*** (0.025)	0.017 (0.019)	-0.054 (0.035)	0.054* (0.028)	-0.002 (0.014)
N	2,144	2,036	2,074	2,090	2,007	2,047	2,085	2,033	2,156	762

Notes: The table reports the average treatment effect on the treated (ATT), and the associated robust standard errors in parentheses. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Firms are matched on the baseline set of variables: (ln) number of employees, and foreign-owned dummy. All probit specifications include country and sector fixed effects. Variable definitions are listed in Table B.2.

Table B.6 – Placebo

	(1)	(2)	(3)
<i>Dependent Variable:</i>	Manager	Obstacle Tax	Obstacle Labor
ATT (GVC Switcher)	0.972 (0.837)	0.040 (0.030)	0.005 (0.037)
N	1,421	1,421	1,428

Notes: The table reports the average treatment effect on the treated (ATT), and the associated robust standard errors in parentheses, for different dependent variables (as indicated). Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Firms are matched on the (ln) number of employees, exporter dummy, importer dummy and foreign-owned dummy. All probit specifications include country and sector fixed effects. Variable definitions are listed in Table B.2.

Table B.7 – Correlation Matrix

(1)										
	CO ₂ Monitor	Adopt CFE	Adopt EE	Adopt EM	Adopt Air	Adopt Waste	Subject ES	Obstacle ER	Customers	EI (fd)
CO ₂ Monitor	1									
Adopt CFE	0.215***	1								
Adopt EE	0.150**	0.382***	1							
Adopt EM	0.132**	0.384***	0.541***	1						
Adopt Air	0.120*	0.274***	0.366***	0.325***	1					
Adopt Waste	0.121*	0.190***	0.253***	0.375***	0.381***	1				
Subject ES	0.0799	0.127*	0.266***	0.247***	0.183***	0.157**	1			
Obstacle ER	0.0775	0.0724	-0.0342	-0.00481	-0.0186	0.0226	0.0189	1		
Customers	0.224***	0.212***	0.201***	0.185***	0.281***	0.214***	0.225***	0.104*	1	
EI (fd)	-0.382***	-0.211***	-0.0322	-0.115*	-0.0445	-0.00800	-0.0364	-0.0356	-0.204***	1

Table B.8 – Heteroskedasticity-based Instruments (1st Stage)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Inverse Distance	0.004*** (0.001)	0.004*** (0.001)	0.004*** (0.001)	0.004*** (0.001)	0.004*** (0.001)	0.004*** (0.001)	0.004*** (0.001)	0.003*** (0.001)	0.004*** (0.001)	0.003*** (0.001)
..00000G	0.528*** (0.134)	0.565*** (0.135)	0.527*** (0.135)	0.572*** (0.137)	0.558*** (0.137)	0.572*** (0.136)	0.542*** (0.137)	0.599*** (0.129)	0.558*** (0.133)	0.713*** (0.189)
..00000H	0.381** (0.149)	0.399** (0.157)	0.371** (0.151)	0.426*** (0.155)	0.396** (0.157)	0.399** (0.157)	0.409*** (0.150)	0.376** (0.155)	0.408*** (0.150)	0.525** (0.211)
..00000I	0.083 (0.178)	0.126 (0.178)	0.046 (0.201)	0.088 (0.192)	0.149 (0.165)	0.097 (0.177)	0.074 (0.185)	0.101 (0.183)	0.072 (0.187)	-0.138 (0.282)
..00000J	0.146*** (0.040)	0.148*** (0.041)	0.139*** (0.041)	0.143*** (0.042)	0.156*** (0.040)	0.149*** (0.041)	0.135*** (0.041)	0.143*** (0.041)	0.144*** (0.041)	0.104* (0.058)
..00000K	0.329*** (0.124)	0.378*** (0.136)	0.305** (0.129)	0.339** (0.135)	0.393*** (0.128)	0.365*** (0.126)	0.314** (0.126)	0.271** (0.134)	0.323** (0.126)	0.264 (0.220)
Exporter	0.131*** (0.027)	0.143*** (0.026)	0.138*** (0.027)	0.141*** (0.027)	0.140*** (0.027)	0.145*** (0.026)	0.134*** (0.027)	0.182*** (0.027)	0.141*** (0.026)	0.209*** (0.040)
Importer	0.055*** (0.021)	0.054** (0.022)	0.053** (0.022)	0.059*** (0.021)	0.053** (0.022)	0.054** (0.022)	0.060*** (0.022)	0.054** (0.022)	0.057*** (0.021)	0.075*** (0.028)
Foreign-Owned	0.024 (0.039)	0.033 (0.040)	0.011 (0.041)	0.020 (0.040)	0.046 (0.039)	0.029 (0.040)	0.020 (0.040)	0.020 (0.040)	0.017 (0.039)	-0.042 (0.049)
Workers (log)	0.032*** (0.007)	0.032*** (0.007)	0.031*** (0.008)	0.031*** (0.008)	0.032*** (0.007)	0.032*** (0.007)	0.030*** (0.008)	0.033*** (0.008)	0.032*** (0.007)	0.023** (0.010)
EU	0.082*** (0.027)	0.087*** (0.029)	0.075*** (0.027)	0.078*** (0.028)	0.093*** (0.028)	0.089*** (0.027)	0.080*** (0.028)	0.064** (0.028)	0.079*** (0.027)	0.052 (0.041)
N	1,428	1,359	1,386	1,391	1,337	1,360	1,395	1,346	1,435	768

Notes: Dependent variable: GVC. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses. Model with generated as well as external instruments. External instrumental variable: Inverse distance. Generated instruments: ..00000G to ..00000K. Variable definitions are listed in Table B.2.

Table B.9 – Constraints: CO₂ Monitor

	(1)	(2)	(3)	(4)	(5)
GVC Switcher	-0.014 (0.015)	-0.003 (0.013)	-0.002 (0.026)	0.001 (0.009)	0.005 (0.025)
× Subject ES		-0.048 (0.066)			-0.019 (0.063)
× Obstacle ER			-0.015 (0.039)		-0.001 (0.034)
× Customer				-0.081 (0.068)	-0.071 (0.063)
Subject ES	0.030 (0.030)	0.042 (0.040)	0.029 (0.032)	0.031 (0.030)	0.036 (0.041)
Obstacle ER	0.006 (0.014)	0.005 (0.015)	0.007 (0.016)	0.005 (0.014)	0.005 (0.017)
Customers	0.058 (0.043)	0.064 (0.043)	0.060 (0.043)	0.090 (0.057)	0.089 (0.057)
Foreign-Owned	-0.016 (0.020)	-0.015 (0.021)	-0.015 (0.020)	-0.017 (0.020)	-0.016 (0.020)
Workers (log)	0.008 (0.008)	0.008 (0.008)	0.007 (0.008)	0.009 (0.008)	0.009 (0.008)
Country FE	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes
N	1,286	1,286	1,286	1,286	1,286

Notes: Dependent variable: CO₂ Monitor. Country and industry fixed effects included in all specifications. Significance levels: * p < 0.1, ** p < 0.05, *** p < 0.01. Robust standard errors in parentheses. Variable definitions are listed in Table B.2.

Table B.10 – Constraints: Adopt EE

	(1)	(2)	(3)	(4)	(5)
GVC Switcher	0.192 (0.121)	0.271* (0.146)	-0.103 (0.115)	0.246* (0.143)	-0.043 (0.110)
× Subject ES		-0.351* (0.190)			-0.187 (0.178)
× Obstacle ER			0.370** (0.167)		0.415** (0.173)
× Customer				-0.296 (0.184)	-0.301 (0.214)
Subject ES	0.575*** (0.076)	0.658*** (0.072)	0.600*** (0.067)	0.579*** (0.078)	0.652*** (0.071)
Obstacle ER	0.044 (0.060)	0.033 (0.060)	0.014 (0.062)	0.040 (0.060)	0.001 (0.062)
Customers	0.014 (0.090)	0.061 (0.090)	-0.030 (0.091)	0.133 (0.084)	0.110 (0.079)
Foreign-Owned	0.047 (0.115)	0.050 (0.109)	0.037 (0.113)	0.043 (0.115)	0.033 (0.109)
Workers (log)	0.006 (0.035)	0.007 (0.036)	0.015 (0.035)	0.010 (0.034)	0.020 (0.033)
Country FE	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes
N	1,256	1,256	1,256	1,256	1,256

Notes: Dependent variable: Adopt EE. Country and industry fixed effects included in all specifications. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses. Variable definitions are listed in Table B.2.

Table B.11 – Constraints: Adopt Waste

	(1)	(2)	(3)	(4)	(5)
GVC Switcher	-0.035 (0.099)	-0.046 (0.118)	-0.110 (0.097)	-0.008 (0.115)	-0.160 (0.098)
× Subject ES		0.047 (0.163)			0.177 (0.149)
× Obstacle ER			0.094 (0.141)		0.170 (0.145)
× Customer				-0.148 (0.157)	-0.274* (0.163)
Subject ES	0.222** (0.095)	0.211* (0.114)	0.228** (0.096)	0.223** (0.094)	0.195* (0.118)
Obstacle ER	0.009 (0.090)	0.011 (0.089)	0.001 (0.089)	0.008 (0.090)	-0.002 (0.089)
Customers	0.233** (0.095)	0.227*** (0.087)	0.222** (0.100)	0.291*** (0.102)	0.298*** (0.105)
Foreign-Owned	-0.056 (0.130)	-0.056 (0.130)	-0.060 (0.131)	-0.057 (0.130)	-0.064 (0.130)
Workers (log)	0.025 (0.033)	0.025 (0.033)	0.027 (0.032)	0.027 (0.032)	0.033 (0.032)
Country FE	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes
N	1,224	1,224	1,224	1,224	1,224

Notes: Dependent variable: Adopt Waste. Country and industry fixed effects included in all specifications. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses. Variable definitions are listed in Table B.2.

Appendix C

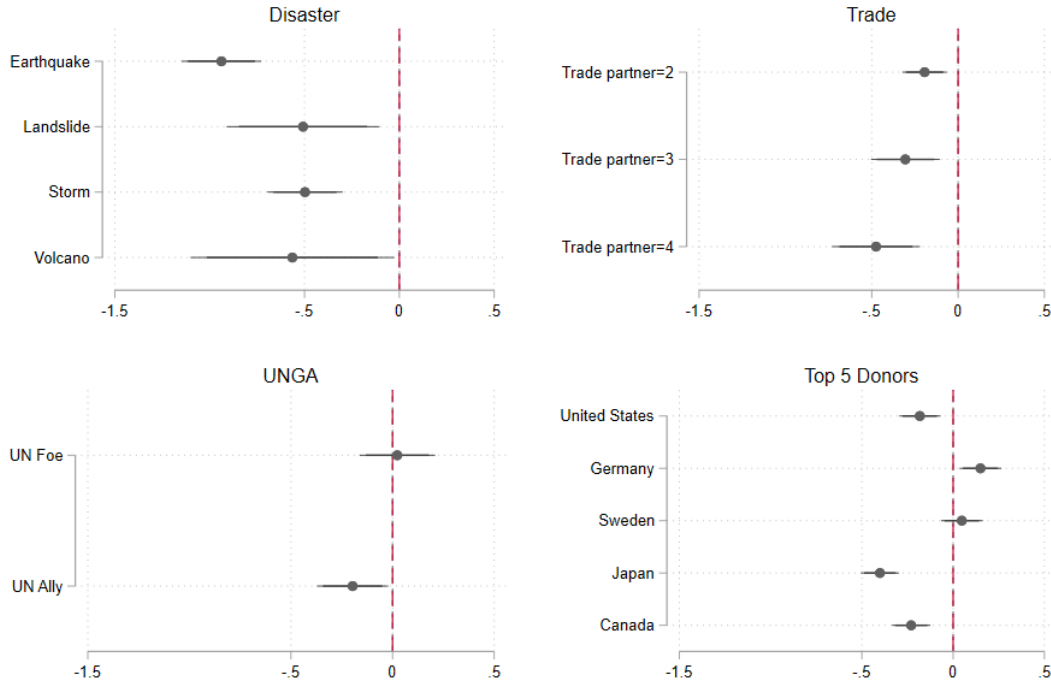
The Speed of Aid: Strategic Urgency in International Emergency Relief

C.1 Acknowledgements

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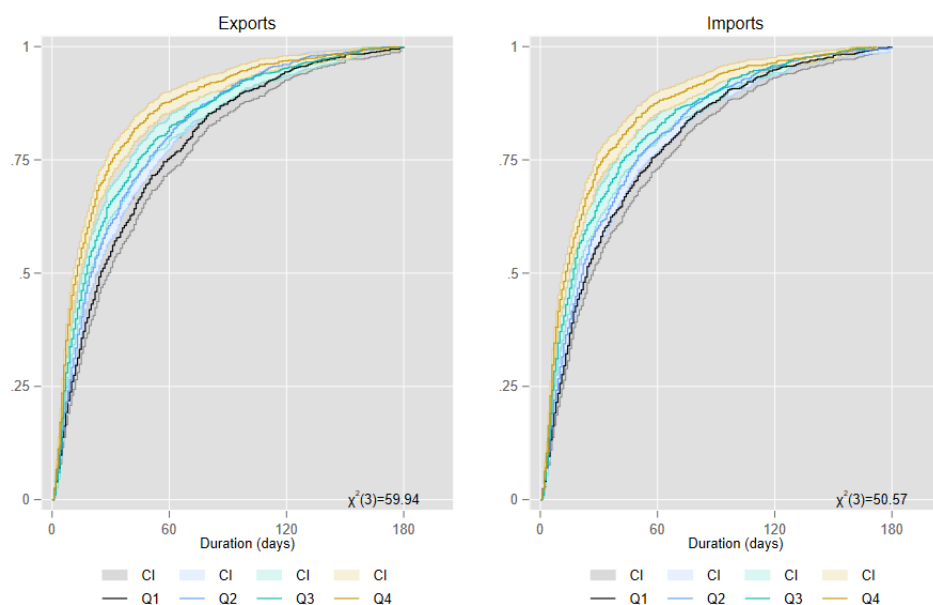
C.2 Additional Figures and Tables

Figure C.1 – Coefficient Plots: Disaster Type, Bilateral Determinants, and Top 5 Donors



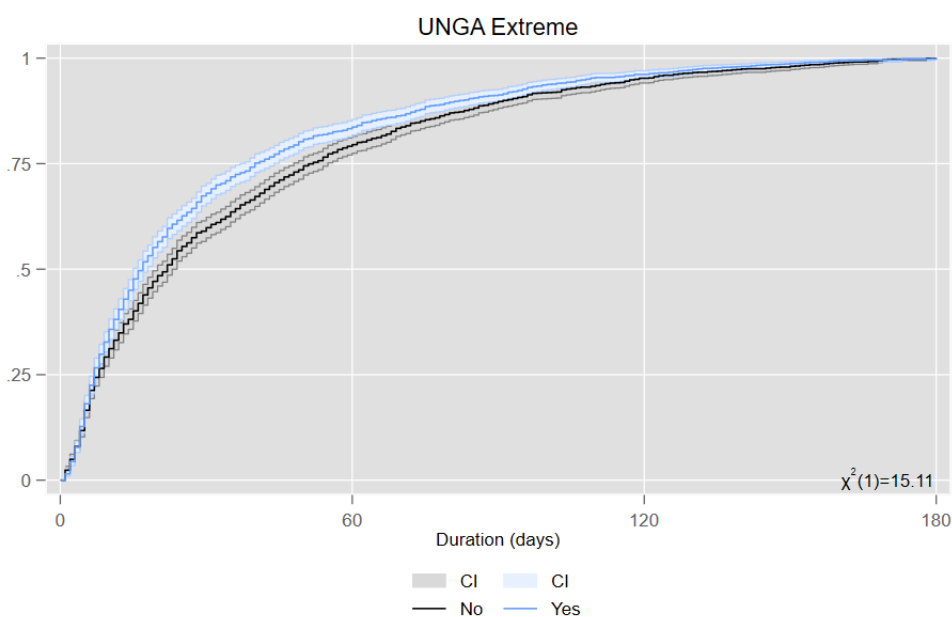
Notes: *Top-Left:* Binary variables for each disaster type (earthquake, landslide, storm, volcano and flood). Flood is the omitted category. Model controls for the logarithm of number of people killed, the logarithm of number of people affected, recipient lagged logarithm of GDP per capita, and recipient lagged logarithm of population, and includes donor-recipient and year fixed effects. *Top-Right:* Binary variables for each quartile of the distribution according to the total value of trade (exports and imports) between donor and recipient. First quartile is the omitted category. Model controls for the logarithm of number of people killed, the logarithm of number of people affected, recipient lagged logarithm of GDP per capita, and recipient lagged logarithm of population, and include donor, recipient and year fixed effects. *Bottom-Left:* UN Foe (UN Ally) is a binary indicator of whether a donor-recipient pair is in the fourth (first) quartile of the distribution according to the logarithm of number of people killed, the logarithm of number of people affected, recipient lagged logarithm of GDP per capita, and recipient lagged logarithm of population, and include donor, recipient and year fixed effects. *Bottom-Right:* The model includes dummy variables for each of the top five donor countries in terms of the number of aid provisions in the sample (United States, Germany, Japan, Sweden, and Canada), and controls for the logarithm of number of people killed, the logarithm of number of people affected, recipient lagged logarithm of GDP per capita, recipient lagged logarithm of population, as well as recipient, year, and disaster-type fixed effects. In all panels, the dependent variable is *Duration*. Two-way clustered (at donor and recipient level) standard error. Figure shows 90% and 95% confidence intervals.

Figure C.2 – Failure Function: Exports and Imports



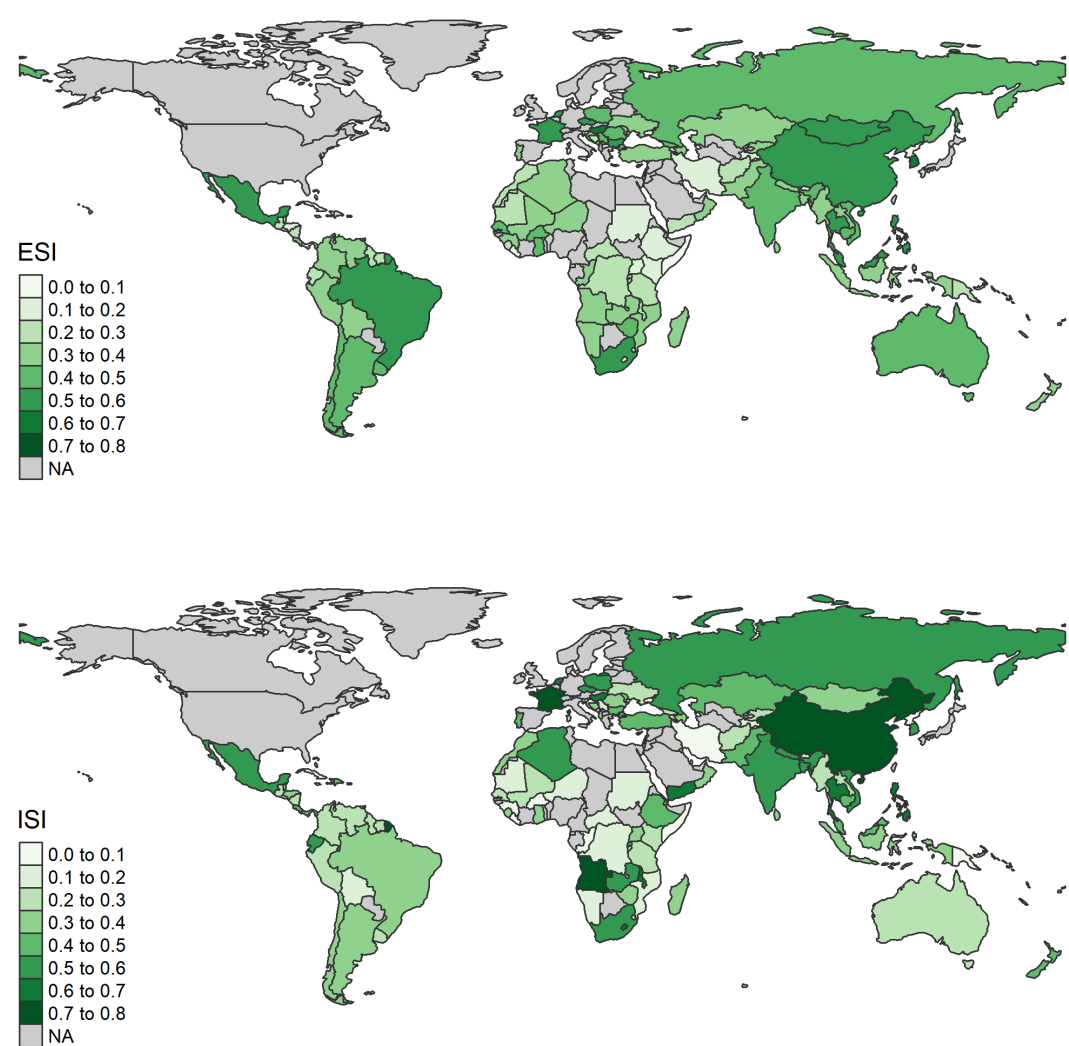
Notes: Kaplan-Meier failure functions according to donor exports (left) and imports (right) to/from the recipient country. Donor-recipient pairs are assigned to quartiles based on total value of exports and imports, respectively. Figure shows 95% confidence intervals.

Figure C.3 – Failure Function: UNGA Extremes



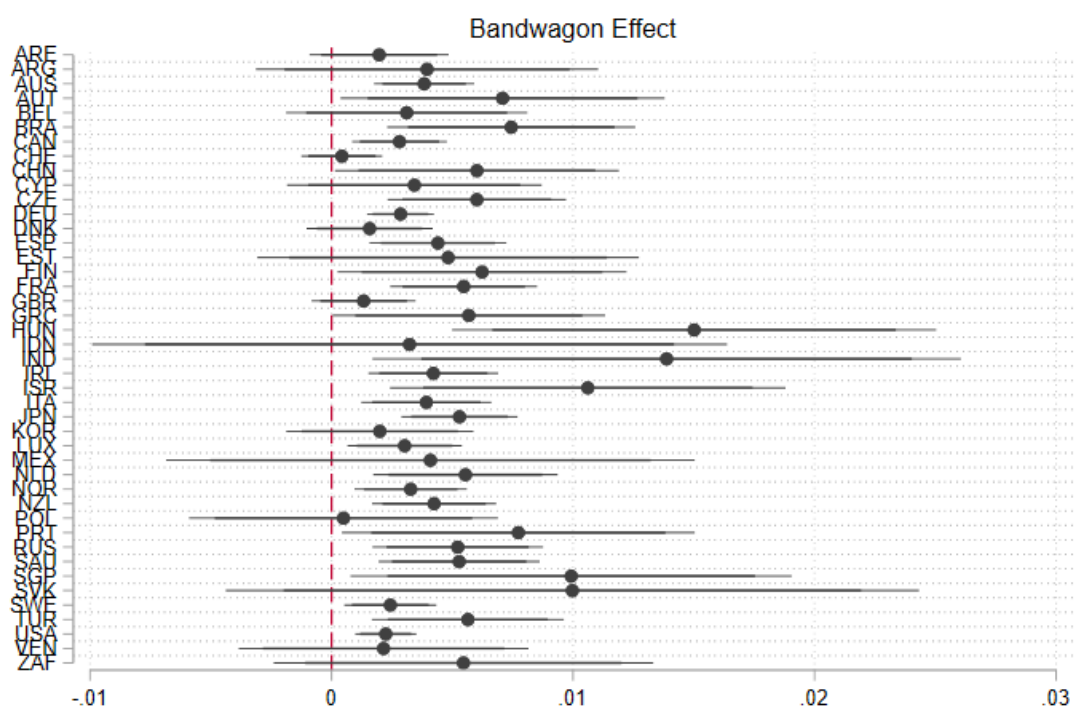
Notes: Kaplan-Meier failure functions according to UNGA voting. Donor-recipient pairs are assigned to trade and UNGA voting quartiles based on $\Delta UNGA$. The first and fourth quartiles are coded as extremes, while quartiles two and three are not. Figure shows 95% confidence intervals.

Figure C.4 – Export and Import Similarity Indices of Japan and the United States (2000–2021 Average)



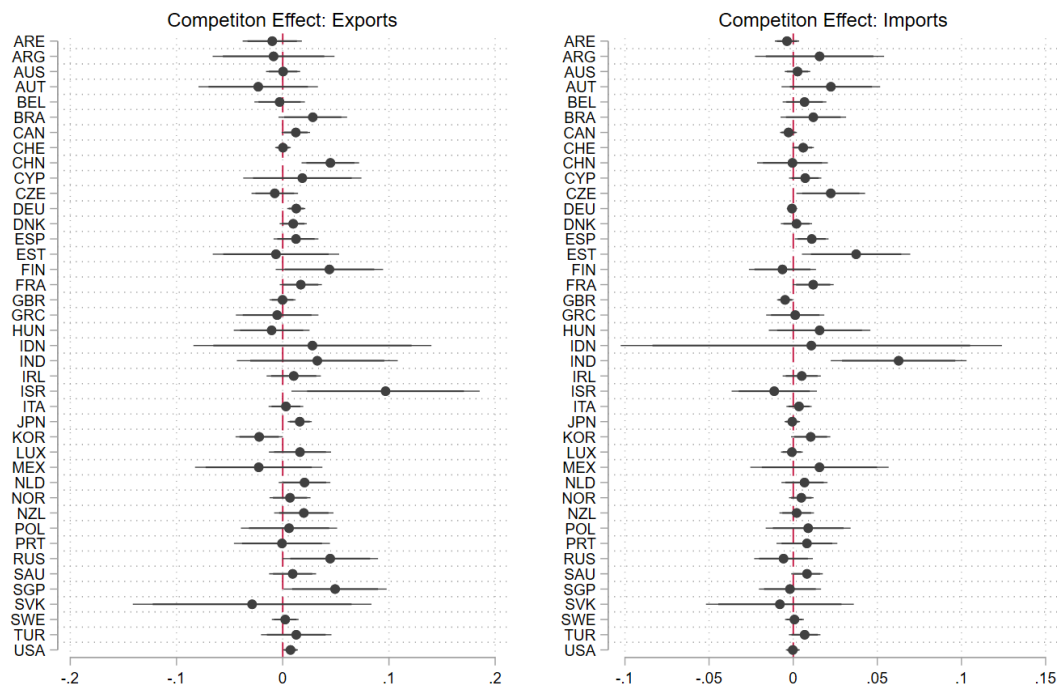
Notes: The figure shows the average export (ESI, top) and import (ISI, bottom) similarity indices between Japan and the United States between 2000 and 2021 for each recipient country. See Section 3.3 for details on how these indices are calculated. Authors' calculations based on data from Growth Lab (2024).

Figure C.5 – Reaction to Individual Donors: Bandwagon



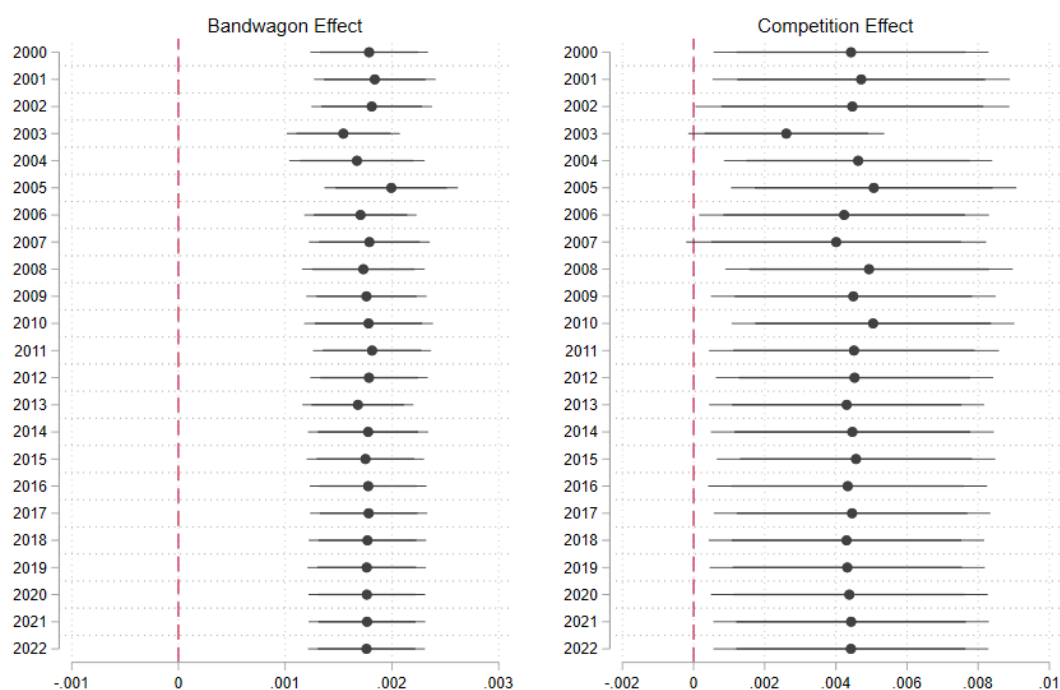
Notes: The bandwagon effect refers to the coefficient on the number of donors who decided to donate in the following three days after the donor country indicated. The specification includes emergency-donor fixed effects. The figure shows 90% and 95% confidence intervals.

Figure C.6 – Reaction to Individual Donors: Competition



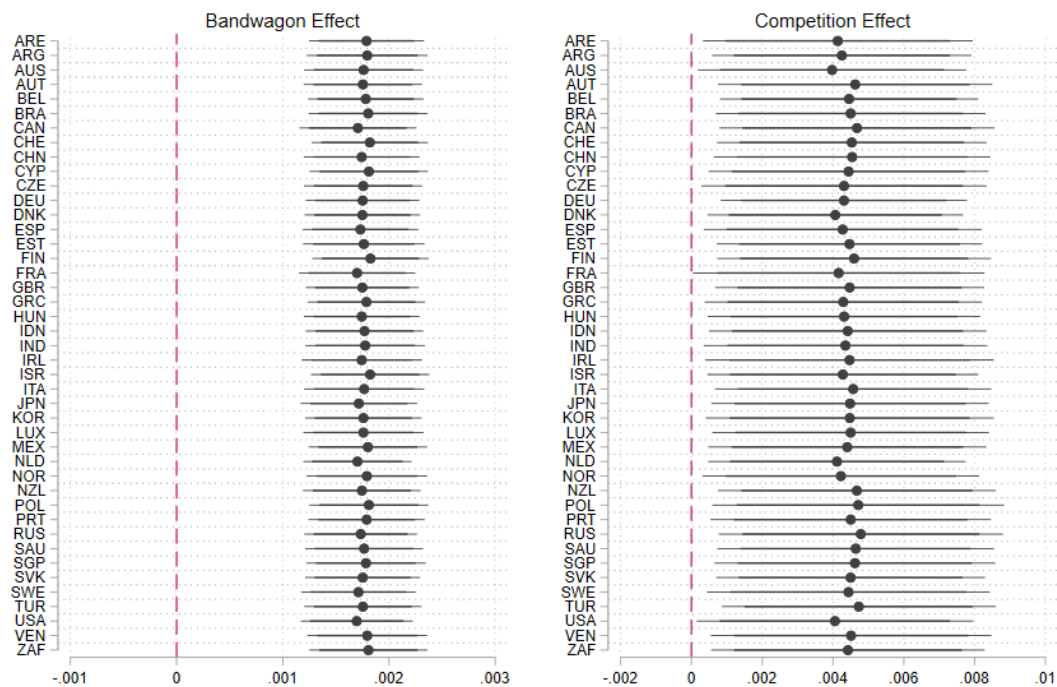
Notes: The strategic competition effect refers either to the coefficient associated with *ExpComp* (exports) or *ImpComp* (imports) in response to the aid decision of the donor country indicated. The window of response follows the baseline specification of three days. The specification includes emergency-donor fixed effects. The figure shows 90% and 95% confidence intervals.

Figure C.7 – Robustness: Excluding Years



Notes: The bandwagon effect refers to the coefficient on the number of donors who decided to donate in the previous three days. The specification includes emergency-donor fixed effects. The strategic competition effect refers to the coefficient associated to the *ExpComp* coefficient, also relative to the previous three days, in a specification with emergency-donor and emergency-day fixed effects. The figure shows 90% and 95% confidence intervals.

Figure C.8 – Robustness: Excluding Donors



Notes: The bandwagon effect refers to the coefficient on the number of donors who decided to donate in the previous three days. The specification includes emergency-donor fixed effects. The strategic competition effect refers to the coefficient associated to the *ExpComp* coefficient, also relative to the previous three days, in a specification with emergency-donor and emergency-day fixed effects. The figure shows 90% and 95% confidence intervals.

Table C.1 – List of Donors

Donor	No. of first commitments	Avg. duration (days)	Total contributed (USD million)
Argentina	22	24.4	.1
Australia	126	30.83	59.77
Austria	61	39.35	16.98
Belgium	74	41.67	37.43
Brazil	39	30.03	7.59
Canada	186	31.84	35.56
China	49	24.53	49.53
Cyprus	27	58.65	1.1
Czech Republic	67	30.72	6.95
Denmark	119	30.98	14.71
Estonia	33	19.76	1.96
Finland	60	39.24	21.42
France	144	28.34	51.32
Germany	266	38.7	53.96
Greece	50	25.03	6.94
Hungary	32	25.67	1.22
India	22	15.83	15.52
Indonesia	9	10.11	7.23
Ireland	113	36.64	23.74
Israel	26	16.68	2.83
Italy	152	31.37	39.35
Japan	201	21.7	79.93
Luxembourg	124	47.48	16.47
Mexico	18	40.5	5.44
Netherlands	100	31.06	50.42
New Zealand	84	36.58	31.8
Norway	173	41.62	47.37
Poland	34	29.04	9.03
Portugal	25	25.86	1.72
Russia	50	30.68	38.5
Saudi Arabia	67	39.28	408.69
Singapore	46	26.16	1.1
Slovak Republic	23	18.06	6.45
South Africa	15	33.5	1.81
South Korea	88	22.01	11.2
Spain	129	30.6	68.56
Sweden	208	37.71	40.02
Switzerland	138	49.67	32.01
Türkiye	74	37.71	24.13
United Arab Emirates	84	50.17	29.25
United Kingdom	133	33.85	75.01
United States	385	31	102.28
Venezuela	23	21.48	.12

Notes: The table lists all donor countries included in the analysis. For each donor, it reports the total number of (first) aid commitments made, the average duration, in days, between disaster start and the the first commitment, as well as the total amount, in million US dollars, contributed in these commitments. Data from EM-DAT (Guha-Sapir, 2024) and FTS (OCHA, 2024) for the period between 2000 and 2022.

Table C.2 – Descriptive Statistics

	Mean	SD	Min	Max	Count
<i>Disaster-Level Analysis</i>					
Duration (days)	33.51	36.39	1.00	180.00	3,081
Total deaths	9871.49	35432.37	1.00	222570.00	3,311
(R) UNSC	0.10	0.30	0.00	1.00	3,522
(R) UNGA Ideal Point	-0.46	0.61	-2.15	2.34	3,504
(R) GDP p.c. (log)	7.64	0.95	5.65	10.89	3,464
(R) Corruption Index	-0.56	0.62	-1.80	2.33	2,946
(R) Population (log)	16.94	2.03	9.30	21.01	3,513
(D) UNGA Ideal Point	1.05	0.82	-1.41	2.92	3,497
(D) GDP p.c. (log)	10.44	0.70	6.68	11.63	3,500
(D) Population (log)	17.08	1.64	12.97	21.05	3,522
Exports, D to R (log)	13.39	3.16	0.00	21.07	3,352
Imports, from R to D (log)	13.03	3.68	0.12	22.01	3,295
Δ UNGA	1.58	0.88	0.00	4.40	3,479
<i>Daily Analysis</i>					
Aid	0.00	0.03	0.00	1.00	3,542,509
No. of donors (1d)	0.03	0.25	0.00	12.00	3,993,840
No. of donors	0.10	0.52	0.00	23.00	3,993,840
No. of donors (7d)	0.23	0.95	0.00	25.00	3,993,840
Export Competition	0.02	0.15	0.00	10.96	3,661,725
Export Competition (7d)	0.06	0.27	0.00	11.65	3,661,725
Import Competition	0.03	0.20	0.00	9.51	3,661,725
Import Competition (7d)	0.07	0.37	0.00	13.20	3,661,725
Geopolitical Alignment	0.07	0.39	0.00	19.55	3,975,080
Information	0.07	0.39	0.00	17.76	3,984,380
Aid Lead	0.01	0.09	0.00	1.00	851,400
ESI Lead	0.29	0.19	0.00	0.77	726,608
ISI Lead	0.30	0.25	0.00	0.99	726,608

Table C.3 – G7 Donors

	Dependent Variable: <i>Aid</i>						
	CAN	FRA	DEU	ITA	JPN	GBR	USA
Aid	0.001 (0.001)	-0.003 (0.002)	-0.001 (0.001)	0.003 (0.002)	0.002 (0.001)	0.003** (0.001)	0.000 (0.001)
ESI * Aid	0.012* (0.007)	0.017* (0.010)	0.013*** (0.004)	0.003 (0.008)	0.016*** (0.006)	-0.000 (0.006)	0.007** (0.003)
ISI * Aid	-0.003 (0.002)	0.012* (0.006)	-0.001 (0.002)	0.003 (0.004)	-0.001 (0.002)	-0.005** (0.002)	-0.000 (0.002)
H(Daycount)	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Weekday FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Day FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Emergency-Donor FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Emergency-Day FE	No	No	No	No	No	No	No
N	3,092,243	3,111,236	3,137,368	3,110,705	3,098,166	3,119,227	3,147,169

Notes: Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Two-way clustered (at donor and recipient level) standard errors in parentheses. Baseline results using three-day window after decision day. G7 donors include Canada (CAN), France (FRA), Germany (DEU), Italy (ITA), Japan (JPN), the United Kingdom (GBR), and the United States (USA).

Table C.4 – “Good” Donors

	Dependent Variable: <i>Aid</i>			
	DNK	NLD	NOR	SWE
Aid	-0.002 (0.003)	-0.002 (0.002)	0.001 (0.002)	0.002 (0.001)
ESI * Aid	0.010 (0.006)	0.021* (0.012)	0.007 (0.010)	0.002 (0.006)
ISI * Aid	0.002 (0.005)	0.007 (0.007)	0.005 (0.004)	0.001 (0.003)
H(Daycount)	Yes	Yes	Yes	Yes
Weekday FE	Yes	Yes	Yes	Yes
Day FE	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes
Emergency-Donor FE	Yes	Yes	Yes	Yes
Emergency-Day FE	No	No	No	No
N	3,053,653	3,096,628	2,922,518	3,052,840

Notes: Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Two-way clustered (at donor and recipient level) standard errors in parentheses. Baseline results using three-day window after decision day. “Good” donors include Denmark (DNK), the Netherlands (NLD), Norway (NOR), and Sweden (SWE).

Table C.5 – Robustness: Adjusted Similarity Indices

	Dependent Variable: <i>Aid</i>			
	(1)	(2)	(3)	(4)
No. of donors	0.000 (0.000)		0.000 (0.000)	
Export Competition (Adj.)	0.490*** (0.140)	0.647*** (0.124)	0.260** (0.116)	0.246** (0.101)
Import Competition (Adj.)	0.233*** (0.062)	0.268*** (0.070)	0.089* (0.053)	0.113* (0.060)
H(Daycount)	Yes	Yes	Yes	Yes
Weekday FE	Yes	Yes	Yes	Yes
Day FE	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes
Emergency FE	Yes	Yes	Yes	Yes
Donor FE	Yes	Yes	Yes	Yes
Emergency-Donor FE	No	No	Yes	Yes
Emergency-Day FE	No	Yes	No	Yes
N	2,956,568	2,956,568	2,956,505	2,956,505

Notes: Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Two-way clustered (at donor and recipient level) standard errors in parentheses. Baseline results using three-day window after decision day. Coefficients and standard errors associated with $ExpComp^{Adj}$ and $ImpComp^{Adj}$ divided by thousand for presentation purposes.

Table C.6 – Disaster Severity: Bandwagon

	Dependent Variable: <i>Aid</i>				
	(1)	(2)	(3)	(4)	(5)
No. of donors	0.000* (0.000)	0.001* (0.000)	0.001*** (0.000)	0.001*** (0.000)	0.003*** (0.000)
Weekday FE	Yes	Yes	Yes	Yes	Yes
Day FE	Yes	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes	Yes
Emergency-Donor FE	Yes	Yes	Yes	Yes	Yes
Emergency-Day FE	No	No	No	No	No
Quintile	Q1	Q2	Q3	Q4	Q5
N	683,887	648,198	634,942	620,488	517,409

Notes: Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Two-way clustered (at donor and recipient level) standard errors in parentheses. Baseline results using three-day window after decision day. The quintiles of disaster severity are calculated based on the death toll of each emergency.

Declaration for Admission to the Doctoral Examination

I, Samuel Siewers, confirm

- i that the dissertation that I submitted (“On the Consequences of Trade: Inequality, the Environment, and Aid”) was produced independently without assistance from external parties, and not contrary to high scientific standards and integrity,
- ii that I have adhered to the examination regulations, including upholding a high degree of scientific integrity, which includes the strict and proper use of citations so that the inclusion of other ideas in the dissertation are clearly distinguished,
- iii that in the process of completing this doctoral thesis, no intermediaries were compensated to assist me neither with the admissions or preparation processes, and in this process,
 - No remuneration or equivalent compensation were provided
 - No services were engaged that may contradict the purpose of producing a doctoral thesis
- iv that I have not submitted this dissertation or parts of this dissertation elsewhere.

I am aware that false claims (and the discovery of those false claims now, and in the future) with regards to the declaration for admission to the doctoral examination can lead to the invalidation or revoking of the doctoral degree.

Göttingen, March 12, 2025,

Author Contributions

The main part of the dissertation consists of three independent chapters. The contributions of each co-author to the respective chapter are summarized as follows:

The (Other) China Shock and the Brazilian Soy Boom: Cui Bono?: This chapter is single-authored. I was responsible for the conceptualization, data preparation and analysis, as well as the writing of the manuscript.

Global Value Chains and Firms' Environmental Performance: The project is co-authored with Inmaculada Martínez-Zarzoso and Leila Baghdadi. All of us contributed to the conceptualization. I carried out most of the data preparation and analysis. The writing was mostly done by Inmaculada Martínez-Zarzoso and me.

The Speed of Aid: Strategic Urgency in International Emergency Relief: The project is co-authored with Andreas Fuchs. We equally contributed to the conceptualization and the writing of the manuscript. I carried out most of the data preparation and analysis.

Göttingen, March 12, 2025,
