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China shock: environmental impacts in Brazil

We study whether the "China shock", defined as China's rapid emergence in global markets, caused environmental impacts in Brazilian municipalities, since previous evidence points to effects on real wages and formal sector employment over the period of 2000 to 2010. Building on recent theoretical developments, we implement a shift-share strategy to explore variation in economic specialization between municipalities and find that China's direct influence on the deforestation of the Amazon and Cerrado was on average insignificant, which is supported by the literature. On the other hand, China's demand for commodities seemed to increase pollution-related mortality of children in mining municipalities, a result obtained by comparing it to mortality caused by other factors. However, we show that this is most likely explained by a municipality's degree of specialization in mining activities rather than its exposure to trade with China. We conclude that the environmental effects of the China shock on Brazilian municipalities were small, if not negligible.

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1 Introduction

Over the course of the 2000's, China quickly rose to a central position in global markets, effectively rearranging international trade. Particularly for Brazil, strengthening commercial bonds with China led to further specialization in services and export-oriented activities, such as the production of primary commodities, while manufacturing sectors faded in face of increased competition from imports. As activities tend to cluster spatially, this "China shock" translated into heterogeneous effects across different regions of Brazil: according to Costa et al. (2016), labor markets in exporting locations had better outcomes on average than those in regions with other specializations. Since this implies that Brazilian regions went through different changes in activity levels based on their specialization, we ask whether the "China shock" also entailed heterogeneous effects on environmental quality as an externality result following the change in local economic activity.

Our identification strategy relies on Autor et al. (2016)'s claim that China's engagement in international trade can be modeled as a natural experiment. Since China went from being a minor to the most important of Brazil's trade partners over the period of 2000 to 2010, we would expect regions previously specialized in goods increasingly exported to and imported from China to face a disproportionate exposure to trade in comparison to those with other specializations. We explore this difference in "treatment status" to investigate whether trade with China influenced environmental quality in Brazil.

Using a "shift-share" strategy to obtain measures of local exposure to trade, we find that municipalities in the Brazilian Amazon originally specialized in the production of goods imported by China experienced, on average, a smaller increase in deforested area than those with other economic specializations: an increase of \$1,000 in exports per worker is associated with an average reduction of 0.063 percentage points in the share of a municipality's area that is deforested between 2000 and 2010. Considering that, according to our metric, deforestation increased 4.15 percentage points on average[1], our estimate suggests that exporting locations have deforested relatively less than non-exporting ones. This difference between exposed and unexposed municipalities turns out to be mostly driven by the mining sector, but vanishes once we control for the share of a municipality's area protected by conservation units and indigenous territories. Our results seem to align with previous evidence presented by López and Galinato (2005) and Faria and Almeida (2016) for the drivers of deforestation of the Amazon rainforest: the impact of trade tends to be small and outclassed by other factors, such as the existence of conservation units and disputes over property rights. Regarding deforestation of the Cerrado biome, our estimates suggest that the "China shock" did not entail any differences between exposed and unexposed municipalities. This is most likely explained by the fact that the Cerrado

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[1]An average weighted by the municipality's area in km^2.

savanna was already considerably devastated as of 2000, the baseline year of our study, leaving little room for variation to be produced by the shocks.

We also explore changes in mortality rates as an attempt to study environmental impacts other than deforestation. Ideally, we would investigate possible connections between the "China shock" and concentration of pollutants, but adequate measures of environmental quality are unavailable at the level of Brazilian municipalities. Hence, we study mortality rates as a "second best" approach: we run separate regressions for different groups of causes of death, some closely associated with pollution. Under the assumption that there exists an environmental channel linking the "China shock" to mortality rates, we would expect its effects to manifest on groups of illnesses associated with poor environmental quality, while unrelated causes should return statistically insignificant estimates.

At first glance, our results indicate that China's "demand for exports" shock had a positive influence on mortality at earlier ages due to pollution-related causes. For children less than one year old, an increase of $1,000 in exports per worker made the change in mortality due to pollution-related illnesses higher by 0.074 children per 100,000 inhabitants, on average, when compared to municipalities relatively unaffected by the export shock. This is a relevant impact when we take into account that, between 2000 and 2010, the nationwide average reduction in mortality of children of this age due to pollution-related illnesses was of 0.166 per 100,000 people[2]. The estimated effect becomes smaller and loses significance when we increase the age to which we restrict the population, becoming negative for ages beyond 10 years. Importantly, no such pattern appears when we run regressions for mortality due to sanitation-related illnesses, external factors (such as violence) or all causes. These results would be compelling indirect evidence for the effects of trade with China on the Brazilian environment were the trade shocks not mainly driven by the mining sector. Because of this, the most likely explanation is that the estimated impact has more to do with how specialized a municipality was in mining activities rather than its exposure to China. Our results do not provide solid basis for arguing that the "China shock" entailed environmental effects strong enough to translate into impacts on mortality rates, although we are not able to completely rule them out either.

Among the contributions of this study is a first approach to the until now unexplored environmental consequences of the increased commercial interaction between Brazil and China. Currently, the "China shock" is mostly studied by economists interested in evaluating its impacts on employment and the allocation of productive resources. Concern over how these changes may affect the emission of pollutants or the removal of natural vegetation is less often manifest. We hope, then, that our results motivate further research on this environmental dimension of the "China shock", which may prove useful to the development of more general insights for the trade and environment literature.

[2]An average weighted by the municipal population at that age group.

In addition, our work is one of the first empirical studies to implement a "shift-share" strategy to estimate causal effects under a formal theoretical framework. Until very recently, no standard procedure or recommended practices concerning the implementation of shift-share variables existed. Identification was in large part a "black box", to quote Goldsmith-Pinkham et al. (2018), as researchers often diverged on what elements were crucial to ensure consistency in estimation. We are benefited, however, by a new set of studies bent on establishing a formal basis for the shift-share research design, which enabled us to clearly state the identification hypotheses and shortcomings of this approach in our application.

The text goes as follows: section 2 discusses previous literature on trade and the environment, describes the "China shock" and establishes its relevance to Brazil. Section 3 presents our empirical strategy, section 4 describes the data we use and section 5 discusses our results. Section 6 concludes.

2   The China shock

Throughout this study, the term "China shock" refers to China's emergence as a major actor in trade. Over the course of a decade, China drastically expanded both its exports and imports, as depicted by the growth rates in figure 1. The volume of goods it supplied to and absorbed from the rest of the world is impressive: as of 2000, China's exports accounted for 4% of the total value exported by all countries; in 2010, this share had reached 10.5%, growing from 250 billion to 1.27 trillion 2000's US dollars, of which more than half came from the electronics and textiles sectors. A similar picture holds for imports, these being relatively concentrated in electronics and ores.

Figure 1: China's growth in trade (1996 = 100)
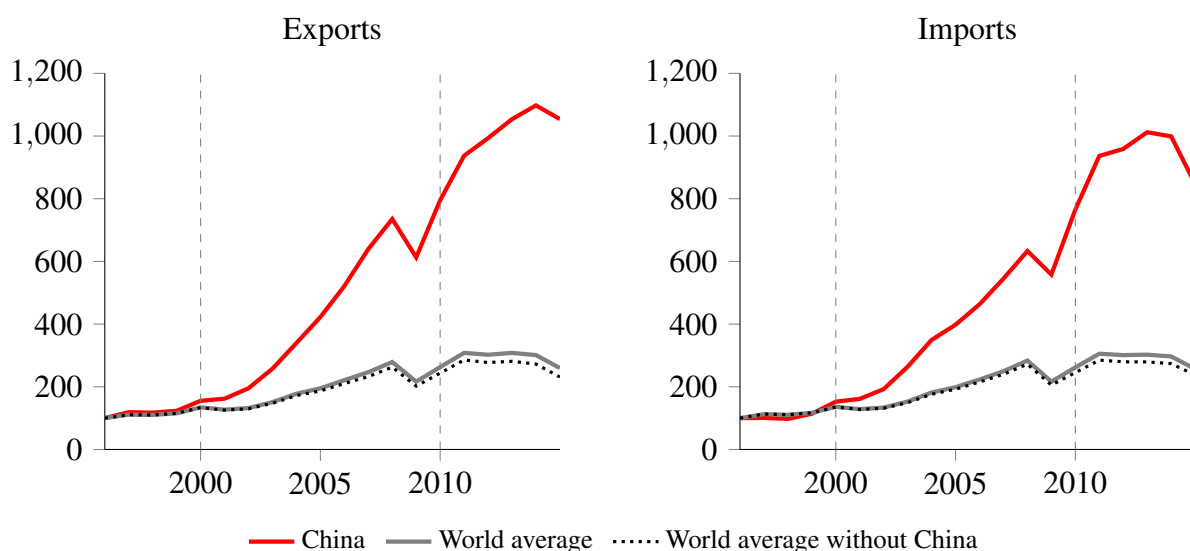
Source: CEPII.

Autor et al. (2016) reckon that this event provides a "rare opportunity" for empirical studies on international trade[3]. Their argument hinges on the claim that China's advance over global markets could be modeled as a "natural experiment" to which the rest of the world was subjected. Three characteristics of China's economic growth and opening to trade are brought forward to justify treating it as an exogenous shock:

a) it was unexpected, despite general awareness of the liberalization reforms being implemented after the 1980's;

b) it was largely driven by the reallocation of domestic production factors, as China accumulated idle capacity and room for productivity gains during its period as a closed economy;

c) its growth in trade displayed an accentuated pattern: exports of manufactures and imports of raw materials, reflecting China's own comparative advantages.

Essentially, Autor et al. (2016) defend that external factors had little to do with China's emergence in trade. Rather, it was mostly its own characteristics and political decisions that shaped its opportunities for economic growth, being fully explored once restrictions to trade were removed. Hence, no other countries could be accounted for influencing China's advance over global markets, allowing us to treat it as exogenous from the perspective of the rest of the world. Put figuratively, China rather "broke into" foreign markets than was "brought in" by other countries.

Under this premise, Autor et al. (2013) attempted to measure the influence of China's import competition over the decline of manufacturing employment in the US over the period of 1990 to 2007. They explore differences in regional economic specialization to create spatial variation in exposure to trade with China, which they achieve using a "shift-share" strategy[4]. To address the concern that characteristics specific to American labor markets might have been affecting China's competitiveness in the US (which would violate the assumption of exogeneity of the China shock), they instrument their exposure to trade measure with China's imports to eight other high-income countries. They estimate that an increase of $1,000 in imports per worker led to an average decline in manufacturing employment of nearly 0.6 percentage points relative to unexposed locations. They calculate that import competition from China could be held accountable for about 25% of the total reduction in manufacturing employment in the period.

Turning to environmental impacts, Bombardini and Li (2018) investigate whether China's export expansion had any effects on its infant mortality rates, as they suspect that the deterioration of air quality observed between 1990 and 2010 simultaneous to the increase in economic activity was a relevant factor to explain changes in health conditions. Though they study the

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[3]Although they argue specifically for the effects of trade on labor markets in developed countries, we believe this claim can be generalized to other contexts.

[4]We should note that Autor et al. (2013) themselves never do actually refer to their empirical strategy as such.

same event that motivates Autor et al. (2016)'s concept of "China shock", it is obvious that the exogeneity argument does not follow in their context: the increase in exports to the rest of the world is very likely correlated to unobserved Chinese characteristics that can influence its environmental quality. Bombardini and Li (2018) then use exogenous variation from tariff changes and, similarly to papers mentioned above as well as our own study, proceed using "shift-share" variables to scatter shock variation throughout the country. A novelty in their application is the construction of "pollution shocks", which they achieve by interacting the change in exports of each sector with industry-specific pollutants emission coefficients. This enables them to account for heterogeneity in pollution intensity among the various activities Chinese regions can specialize in; for example, while two locations may be assigned the same exposure to tariff shocks in monetary terms, they may differ in pollution intensity, which is then captured by a separate slope parameter in the model. Their estimates suggest that regions originally specialized in highly polluting exported goods experienced a higher persistence of infant mortality, a result that is robust to a set of controls and alternative measures of local exposure to changes in tariffs. Through a 2SLS regression, they also show that the predicted change in SO_2 concentration and particulate matter was positively correlated with change in mortality: worse air quality due to increased production of "dirty" goods seems to have led to higher persistence of child mortality.

2.1 Brazil's exposure to China

The China shock had world-wide reach, but Brazil could be seen as a particularly affected country. Figure 2 illustrates how growth rates of exports and imports differed between Brazil's trade with China and with the average country along the 2000's. Whereas in 2000 China accounted for 2% of both Brazil's imported and exported value, these shares in 2010 had increased to 14% and 15% respectively, establishing it as the major purchaser of Brazilian products and second-greatest foreign supplier to Brazilian markets, after the US.

Brazil's trade pattern is also concentrated on few products, as shown in tables A1 and A2 of Appendix A: in 2010, 44% of all imports were of fuels, electrical and mechanical machinery; among imports from China specifically, the latter two responded for 53%. As for exports, 67% of the total value shipped to China came solely from the mining industry and grain farming, mainly iron ores and soybeans. Hence, we would expect Brazilian regions specialized in exported and imported goods to have experienced relatively high exposure to the China shock throughout the 2000's, which could have translated into environmental impacts intense enough to be detected by our empirical strategy. We must be aware, however, that in 2000 Brazil's exports were already considerably concentrated on the set of commodities mentioned above, implying that Brazil could have been particularly prone to trade with China. This is a potential problem because if Brazil-specific characteristics did contribute to intensifying trade relations

Figure 2: Brazil's growth in trade (1996 = 100)
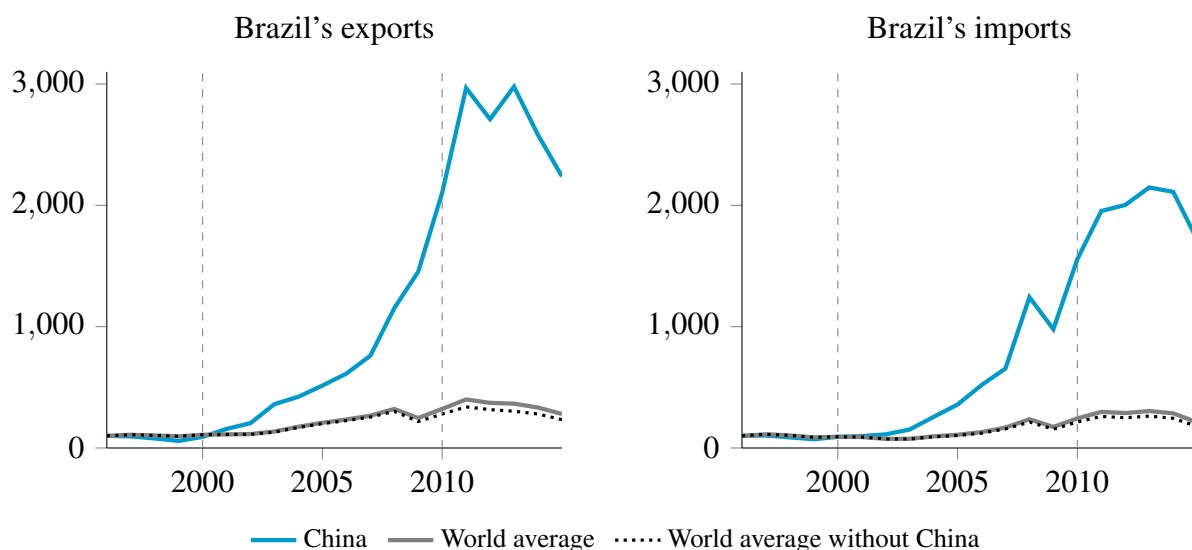
Source: CEPII.

with China, then the exogeneity assumption of the China shock is violated. We employ an instrumental variables strategy to address this issue, which we discuss thoroughly in section 3.

Since we are interested in whether trade with China led to heterogeneous changes in the environment at the level of Brazilian municipalities, it is important to understand how this shock scattered throughout the country, as our quasi-experimental approach relies on regional differences in industry specialization. Costa et al. (2016) study whether trade with China affected local labor markets in Brazil. Similarly to Autor et al. (2013), they model local measures of exposure to trade using "shift-share" variables and fit a first-differenced specification for the years of 2000 and 2010, so that their coefficients are interpreted as the shock's effect on the rate of change of the outcome variable, rather than on the variable itself. They estimate that regions originally specialized in goods imported by China experienced on average higher increases in real wages and formal sector employment when compared to regions with other specializations. These were donned the "winning" locations; the "losers", by comparison, were those originally specialized in goods exported by China, where growth rates of real wages were on average smaller. Additionally, they provide evidence of "losing" regions experiencing smaller rates of immigration, which is consistent with Haddad and Maggi (2017), whose model predicted further specialization in export-oriented activities[5]. Somewhat surprisingly, though, Costa et al. (2016) also find that these "losing" regions experienced smaller rates of emigration. They argue that, since activities tend to cluster spatially, the possibilities of gains from migration are slimmer for individuals in locations facing China's competition because neighboring regions,

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[5]Haddad and Maggi (2017) use a computable general equilibrium (CGE) model to simulate the impacts of the China shock on Brazil. Their results illustrate the importance of inter-regional trade and general equilibrium effects, which are suggested to have acted as an "insurance" against the shock.

which are the lowest-cost alternatives for moving, are likely facing similar difficulties.

We borrow considerably from Costa et al. (2016)'s empirical strategy, as they implement several changes when adapting Autor et al. (2013)'s methodology to the context of Brazil. In addition to addressing potential issues related to particular characteristics of Brazil's regional labor markets, they split the China shock in two: one of "demand for exports" and one of "supply of imports". This is a necessary change over the original application, since Brazil's increased imports from China were accompanied by growth in exports as well. Costa et al. (2016) also introduce a different approach to instrumenting the China shocks[6], which we find to be an improvement over the original and thus replicate in our study.

2.2 This study

Costa et al. (2016)'s results indicate that the China shock led to relative increases in the average wage of regions that in 2000 were specialized in producing goods that Brazil would export to China in the following years. Simultaneously, Haddad and Maggi (2017)'s model shows these regions receiving a higher influx of workers. Since an expansion of labor supply would *coeteris paribus* drive wages down, the estimated increase in wages must have resulted from a larger upward shift of the local demand curve for labor than the downwards one of its supply. Considering Brazil's main exports to China are products of capital-intensive sectors, such as mining and commodity-oriented agriculture, the shift in labor demand was likely due to an increase in output by firms, rather than substitution of capital for labor. We then ask: could these changes in activity levels, which differ between regions depending on local specialization, have led to heterogeneous impacts on environmental quality?

We first look at deforestation. An increase in activity levels may have led to higher demand for inputs and services that compete with forest cover, such as land for farming or the extraction of timber. We investigate possible effects of the China shock on changes in deforested area of municipalities in the Cerrado and Amazon biomes. Particularly for the latter, previous studies attempted to investigate the impacts of trade on deforestation. López and Galinato (2005) found that trade openness could either increase or decrease deforestation rates depending on whether the country has comparative advantages in producing "forest-competing" crops, those that expand by replacing natural vegetation cover. They compared Brazil, Indonesia, Malaysia and the Philippines, all of which have tropical forests, from 1980 through 1999 and find a relatively small net impact of trade. For Brazil specifically, which started adopting a more liberal trade agenda in the beginning of the 1990's, they found a negative effect of trade openness on deforestation of the Amazon. They argue that trade reduced incentives supporting the expansion of "forest-competing" crops, since, aside from the cultivation of soybeans, agricultural production

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[6]A concern similar to Autor et al. (2013)'s arises in their study, as they worry that trade flows between China and Brazil might have been partially influenced by factors specific to Brazil.

in the Amazon consisted mainly of goods that were not traded or were substitutes to imports. Hence, opening to foreign markets led productive factors out of the forest area towards regions where export-oriented sectors were located; in other words, trade reduced deforestation through a composition effect. However, they consider this to be a small impact, arguing that domestic policies such as tax and credit incentives were much more relevant to explain changes in forest cover. Faria and Almeida (2016)'s results go along with this conclusion: although they estimate a positive influence of openness to trade on deforestation, their coefficients are quite sensible to changes in model specification, with point estimates varying greatly in size and significance. The effects on deforestation of conservation units and existence of disputes over land property rights are shown to be significant and exceed those of trade in all of their specifications, suggesting that trade may indeed be of secondary importance to explain forest loss in the Amazon.

Using the full sample of municipalities, we proceed to study changes in the emissions of pollutants, which economic theory predicts to be linked to activity levels through production externalities. The ideal inquiry would look at local levels of pollution to investigate whether an environmental impact exists, but it is unfortunately unachievable due to the poor quality of Brazil's data on air and water pollution at the level of municipalities[7]. We are also unable to include in our model different slope parameters for shocks according to their pollution intensity, as in Bombardini and Li (2018), since no reliable source produces this information for all the activities we consider in our study. Hence, we settle for a "second best" approach exploring changes in mortality: on the assumption that there exists an environmental channel linking the China shock to mortality rates, we expect effects to manifest on illnesses most associated with poor environmental quality, while unrelated causes should return statistically insignificant estimates.

3 Empirical strategy

This study deals mainly with the estimation of reduced form equations, which generally follow:

$$\Delta y_i = \beta_1 \cdot XD_i + \beta_2 \cdot IS_i + \boldsymbol{W}_i' \boldsymbol{\gamma} + \Delta \varepsilon_i, \tag{1}$$

where $\Delta y_i = y_{i,2010} - y_{i,2000}$ is the change in an outcome of municipality i, XD_i and IS_i are measures of i's exposure to China's "demand for exports" and "supply of imports" shocks, respectively, $\Delta \varepsilon_i$ is the change in unobserved time-varying determinants of Δy_i and $\boldsymbol{W}_i$ is a vector of controls. In addition to an intercept, $\boldsymbol{W}_i$ may include lags of Δy_i and relevant socioeconomic

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[7]The National Water Agency of Brazil (ANA) states that the currently available data on water quality is spatially and temporally inconsistent, as no standard procedures for collection of measurements were ever implemented. In 2018, ANA has launched a new agenda (details in http://pnqa.ana.gov.br/pnqa.aspx) for addressing these issues and start producing coherent information to be used in research. For a similar diagnosis on the collection of air quality data, see IEMA (2014).

characteristics, most of which we opt to hold fixed at baseline levels to avoid potential endogeneity issues: since trade shocks might influence regional characteristics other than environmental quality, the actual change of any covariate could correlate to unobserved time-varying determinants of outcome Δy_i.

Equation (1) is estimated in first-differences due to the "shift-share" structure of the trade exposure variables XD and IS, which we detail below. Consequently, municipal fixed effects are accounted for and any covariates we hold constant are interacted with year dummies to keep them from vanishing in the first-differences transformation. These are to be interpreted, then, as capturing trends that regions with similar values of each characteristic would be expected to follow, just as the intercept captures a general trend effect associated with the passing of time.

3.1 Measuring exposure to trade

Following Autor et al. (2013) and Costa et al. (2016), we employ a weighting strategy to build local measures of exposure to trade with China from country-level data. This is necessary because actual local exports and imports information is not provided by municipalities. Hence, we define change in exposure of municipality i to trade with China as:

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\begin{aligned}
XD_i &= \sum_j s_{i,j} \cdot g^X_{BC,j} \\
IS_i &= \sum_j s_{i,j} \cdot g^M_{BC,j},
\end{aligned}
\tag{2}
$$

where $s_{i,j}$ is the share of region i's workers employed in industry j and $g^X_{BC,j}$ and $g^M_{BC,j}$ are the change in exported (from Brazil to China) and imported value (by Brazil from China) of goods produced in industry j, respectively. With XD and IS defined as such, the empirical model in (1) becomes a "shift-share" research design, because our key variables are interactions of a set of shocks ("shifters") with a set of weights ("shares") that introduce cross-sectional variation in exposure[8]. Hanna and Oliva (2015) and Christian and Barrett (2017) interpret shift-share designs as continuous differences-in-differences frameworks where treatment status is not a binary assignment, but rather a gradient across the cross-section units. In this setting, the "identifying variation" comes from comparing regions with high and low exposure to trade with China before and after the shocks. Thus, failure to control for relevant factors determining local environment-related outcomes could be understood as a violation of the well-known "parallel trends" assumption. In our study, "parallel trends" would require regions differing only in the degree of exposure to trade to have gone through similar changes in environmental outcomes had the China shocks not occurred.

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[8]In the canonical setting presented by Adão et al. (2018), the set of weights $s_{i,j}$ adds up to unity for each i. This is not the case in our study, since j indexes traded sectors only. The implications of this issue are minor: it slightly alters the manner we specify our identification hypotheses, as we discuss in Appendix B.

How do *XD* and *IS* capture a municipality's sensitivity to China's demand and supply shocks? For an industry j, consider the fraction of trade flow F assigned to location i, over which we aggregate across sectors to build (2):

$$s_{i,j} \cdot g^F_{BC,j} = \underbrace{\frac{L_{i,j}}{L_i} \cdot \frac{\Delta F_{BC,j}}{L_{B,j}}}_{\text{Shift-share}} = \underbrace{\left(\frac{L_{i,j}}{L_{B,j}}\right) \cdot \frac{\Delta F_{BC,j}}{L_i}}_{\text{Autor et al. (2013)}}, \quad (3)$$

where $\Delta F_{BC,j} = F_{BC,j,2010} - F_{BC,j,2000}$ is the change in value traded of j between Brazil and China (where F can denote exports (X) or imports (M)), $L_{i,j}$ is the number of region i's workers employed in industry j, $L_{B,j}$ is the number of Brazilian workers employed in j and L_i is the total number of workers located in i. Hence, if $\Delta X_{BC,j}$ and $\Delta M_{BC,j}$ are given in dollars, XD_i and IS_i measure exposure in dollars per worker.

The first equality follows the canonical shift-share structure as presented in Adão et al. (2018) and Borusyak et al. (2018): $L_{i,j}/L_i$ are the "shares" $s_{i,j}$ and $\Delta F_{BC,j}$ are the "shifters" $g^F_{BC,j}/L_{B,j}$, with $L_{B,j}$ serving as a scaling factor. However, Autor et al. (2013)'s interpretation of the variables in (2) implies a different arrangement of the terms in (3), as given by the second equality. Essentially, it exchanges $L_{B,j}$ for L_i, meaning weights would be given by the term in parentheses, interpreted as i's importance for Brazil in the domestic production of goods from industry j. The scale factor would then be i's total number of workers, which is included so that regions with different levels of activity can be compared. However, scaling can cause undesirable distortions: since we are interested in the environmental impact of economic activities, dividing by L_i may understate this effect in regions that have a large number of workers across many industries. This might happen because, for a given $L_{i,j}$, higher L_i reduces $s_{i,j} \cdot g^F_{BC,j}$ even though pollution emissions by j would be no smaller than if we observed the same amount of employment $L_{i,j}$ in a region h that has less workers in total (that is, $L_{i,j} = L_{h,j}$ and $L_i > L_h \implies s_{i,j} \cdot g^F_{BC,j} < s_{h,j} \cdot g^F_{BC,j}$). We try to account for this problem by choosing appropriate regression weights.

Throughout this text, we stick to the definition given by the first equality in (3), which is supported by a recent set of studies that seek to formalize the theory behind the shift-share structure. Note, however, that the comment we made above based on Autor et al. (2013)'s interpretation holds regardless since, even though shares actually refer to labor in regions (L_i) instead of labor in industries ($L_{B,j}$), both expressions are mathematically the same.

For most of the following discussion, we ignore the existence of the scale factor $L_{B,j}$, mainly to save in notation. This is a harmless simplification, because whatever issues it may cause come through the same channels as the labor shares $s_{i,j}$, since $L_{B,j} = \sum_i L_{i,j}$. Also, the theoretical shift-share framework, as laid out by Borusyak et al. (2018), does not consider a scale factor, so by choosing to omit it for now we keep our exposition closer to the canon[9].

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[9]Refer to Appendix B for more details on this simplification. Alternatively, one could define $\tilde{s}_{i,j} \equiv s_{i,j}/L_i$ and the

The weights in (3) are constructed from employment levels observed in the baseline year, 2000. This choice is actually important, because we rely on the exogeneity of China shocks, $Cov(\Delta F_{BC,j}, \Delta \varepsilon_i | \boldsymbol{W}_i) = 0$, to identify β_1 and β_2. To see why, notice that Costa et al. (2016)'s results suggest that employment levels themselves were partially determined by trade with China throughout the 2000's. Also, recall that we are working under the hypothesis that the environmental quality of a municipality reflects to some degree the types and scale of economic activities it performs, for which we proxy with how labor is allocated in it. Therefore, if a municipality's economic specialization is correlated with unobserved determinants of environmental quality, using labor shares from 2010 (or any other year in between) would bias our estimates, since then $Cov(s_{i,j,\tau}, \Delta \varepsilon_i | \boldsymbol{W}_i) \neq 0$ and $Cov(s_{i,j,\tau}, \Delta F_{BC,j} | \boldsymbol{W}_i) \neq 0$, which would imply $Cov(\Delta F_{BC,j}, \Delta \varepsilon_i | \boldsymbol{W}_i) \neq 0$, where τ is any year after 2000.

To illustrate this argument, suppose that a region started specializing in the production of some good that China imports from Brazil *after* intensification of trade between the two countries had begun. This would likely imply that $Cov(s_{i,j,\tau}, \Delta F_{BC,j} | \boldsymbol{W}_i) \neq 0$, since it is reasonable to expect agents would want to benefit from improved possibilities of gains from trade, hence shifting their production resources to the exporting activity. If we assign to this region a level of exposure based on $s_{i,j,\tau}$, can we be sure that the environmental effect we capture is entirely due to China's influence? Probably not, since the change in specialization could have been facilitated by laxness in local environmental regulation, meaning we would be overstating China's effect by mixing it with the region's own relative disregard for its environmental quality. Therefore, we would be better off using shares $s_{i,j}$ that are not influenced by trade shocks, as they would more likely be free from endogenous responses of region-specific factors that distort the true impact.

One might worry, though, that not even using baseline shares we would be able to support the assumption that $Cov(s_{i,j}, \Delta F_{BC,j} | \boldsymbol{W}_i) = 0$. For example, regions with large initial concentration of workers in industry j might benefit from a productivity shock concomitant with growth of trade between Brazil and China. In this case, we could have a fraction of the increase in exports of j actually being due to these products being "pushed", rather than "pulled", from Brazil to China. Alternatively, some regulation implemented in between 2000 and 2010 might render domestic production of a good impracticable, meaning that imports of said good would rise due to these being "pulled" by Brazil instead of "pushed" by China, regardless of the year labor shares are evaluated in. In such cases, the exposure to trade measures in (2) would also be violating $Cov(\Delta F_{BC,j}, \Delta \varepsilon_i | \boldsymbol{W}_i) = 0$, leaving us incapable of recovering our parameters of interest. Therefore, we need to ensure that the variation coming from changes in value traded between Brazil and China throughout the 2000's does not carry any influence from characteris-

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following arguments would still apply, since both $s_{i,j}$ and L_i are variables specific to Brazil. We do not do this because it would then be incorrect to refer to $\tilde{s}_{i,j}$ as "shares".

tics specific to Brazilian municipalities.

3.1.1 A proper China shock

Though we argued for interpreting China's increasing relevance in global trade as a "natural experiment" from the perspective of the rest of the world, it is unlikely that growth in trade between Brazil and China went completely unaffected by events and characteristics specific to Brazil. Consider the following decomposition of an industry j's growth in exports from China to Brazil[10], based on Goldsmith-Pinkham et al. (2018):

$$\Delta M_{BC,j} = \Delta M_{B,j} + \Delta X_{C,j} + \Delta \tilde{M}_{BC,j}. \tag{4}$$

We can write it as the sum of changes in Brazilian imports due to factors specific to industry j in Brazil ($\Delta M_{B,j}$), changes in Chinese exports due to factors specific to industry j in China ($\Delta X_{C,j}$), and a term accounting for eventual idiosyncrasies that may exist in the relations between China and Brazil when trading products of j ($\Delta \tilde{M}_{BC,j}$). If we wish to study China's impact on Brazil's environmental quality, it is necessary we remove the variation that is not driven solely by China-specific factors from our exposure to trade measures. Ideally, then, we would have our variables in (2) constructed with $\Delta X_{C,j}$ and $\Delta M_{C,j}$ instead of $\Delta M_{BC,j}$ and $\Delta X_{BC,j}$.

To approximate these unobserved China-specific components of growth in trade, we follow Costa et al. (2016) and construct instrumental variables that also use a shift-share structure, a strategy sometimes referred to in the literature as a "Bartik" IV approach. For every industry j, we run regressions of the form:

$$\begin{aligned}
\frac{\Delta \bar{M}_{r,j}}{\bar{M}_{r,j,2000}} &= \alpha_j + \delta_j^M \cdot \mathbb{1}_{\{r=China\}} + v_{r,j} \\
\frac{\Delta \bar{X}_{r,j}}{\bar{X}_{r,j,2000}} &= \gamma_j + \delta_j^X \cdot \mathbb{1}_{\{r=China\}} + \mu_{r,j},
\end{aligned} \tag{5}$$

where $\mathbb{1}_{\{r=China\}}$ takes value 1 when country r is China and zero otherwise, $\Delta \bar{M}_{r,j} = \bar{M}_{r,j,2010} - \bar{M}_{r,j,2000}$ is the change in value of j imported by r from the rest of the world excluding Brazil and, analogously, $\Delta \bar{X}_{r,j}$ is the change in value of j exported by country r to the rest of the world except Brazil. Regressions for imports and exports are weighted, respectively, by country r's imported and exported volume of industry j's goods in 2000 to mitigate the influence of growth rates from countries that initially had small relevance in these markets. Estimated $\hat{\delta}_j^M$ and $\hat{\delta}_j^X$ inform China's deviations from the global average in import and export growth of j. Since we are considering a "world without Brazil", these coefficients do not carry influence from Brazil-specific factors. Additionally, as Costa et al. (2016) point out, they are not affected by world-encompassing events such as commodity price shocks or technological innovations

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[10]An analogous argument follows for growth in exports from Brazil to China.

because these are captured by industry fixed effects $\hat{\alpha}_j$ and $\hat{\gamma}_j$. Hence, $\hat{\delta}_j^M$ and $\hat{\delta}_j^X$ plausibly inform how much of China's trade performance can be accounted to its own intrinsic factors and are thus appropriate to model a China shock[11]. In fact, we do so by interacting these coefficients with the initial trade levels between Brazil and China:

$$\Delta\hat{M}_{BC,j} = \hat{\delta}_j^X \cdot M_{BC,j,2000}$$
$$\Delta\hat{X}_{BC,j} = \hat{\delta}_j^M \cdot X_{BC,j,2000}. \tag{6}$$

The predicted imports and exports above are the changes in traded value of goods from industry j we would expect to observe were these trade flows to be completely driven by Chinese characteristics, meaning they are proxies for $\Delta X_{C,j}$ and $\Delta M_{C,j}$ in (4), respectively. The instruments for our exposure variables are then simply obtained by replacing the actual trade flows in (2), donning them the shift-share structure:

$$ivXD_i = \sum_j s_{i,j} \cdot \Delta\hat{X}_{BC,j}$$
$$ivIS_i = \sum_j s_{i,j} \cdot \Delta\hat{M}_{BC,j}. \tag{7}$$

Figure 3 highlights the municipalities in the tenth decile of exposure to the China shocks. The two maps at the top row show the exposure to trade measures calculated with the actual changes in imports and exports between Brazil and China, while the bottom two maps present the instruments given by (7). The pattern of most affected municipalities is quite similar between "actual" and instrument shocks, which could suggest that the observed changed in trade flows was already largely due to China's pressure, with little influence of Brazil-specific characteristics. Indeed, this seems to be true for exports, as the observed shock XD and its instrument $ivXD$ are highly correlated (97.2%). The same cannot be said about imports, however, since the correlation coefficient for IS and $ivIS$ is 33.4%, implying that domestic factors explain a large part of Brazil's change in imported value.

3.1.2 Identification with shift-share instruments

The IV strategy above lends credibility to the assumption that $Cov(s_{i,j}, \Delta\hat{F}_{BC,j}|\boldsymbol{W}_i) = 0$ and, thus, $Cov(\Delta\hat{F}_{BC,j}, \Delta\varepsilon_i|\boldsymbol{W}_i) = 0$, where $\Delta\hat{F}_{BC,j}$ is the predicted change in trade flow F obtained through (6). Nevertheless, if local specialization in 2000 and environmental quality of a region i are correlated with some unobserved time-varying confounder, one might worry that our exposure to trade will be endogenous in spite of whatever claims we make about the exogeneity of the China shocks. That is, there might be concern that identification of β_1 and β_2 also depends on the exogeneity of labor shares since, after all, they are the weights used to generate the shift-share variables, as shown in equation (2).

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[11]One could interpret Autor et al. (2016)'s argument for using the China shocks in quasi-experimental designs as a claim that $\hat{\delta}_j^M$ and $\hat{\delta}_j^X$ should be statistically different from zero for a significant number of industries.

Figure 3: Municipalities in the 10th decile of exposure to export and import shocks
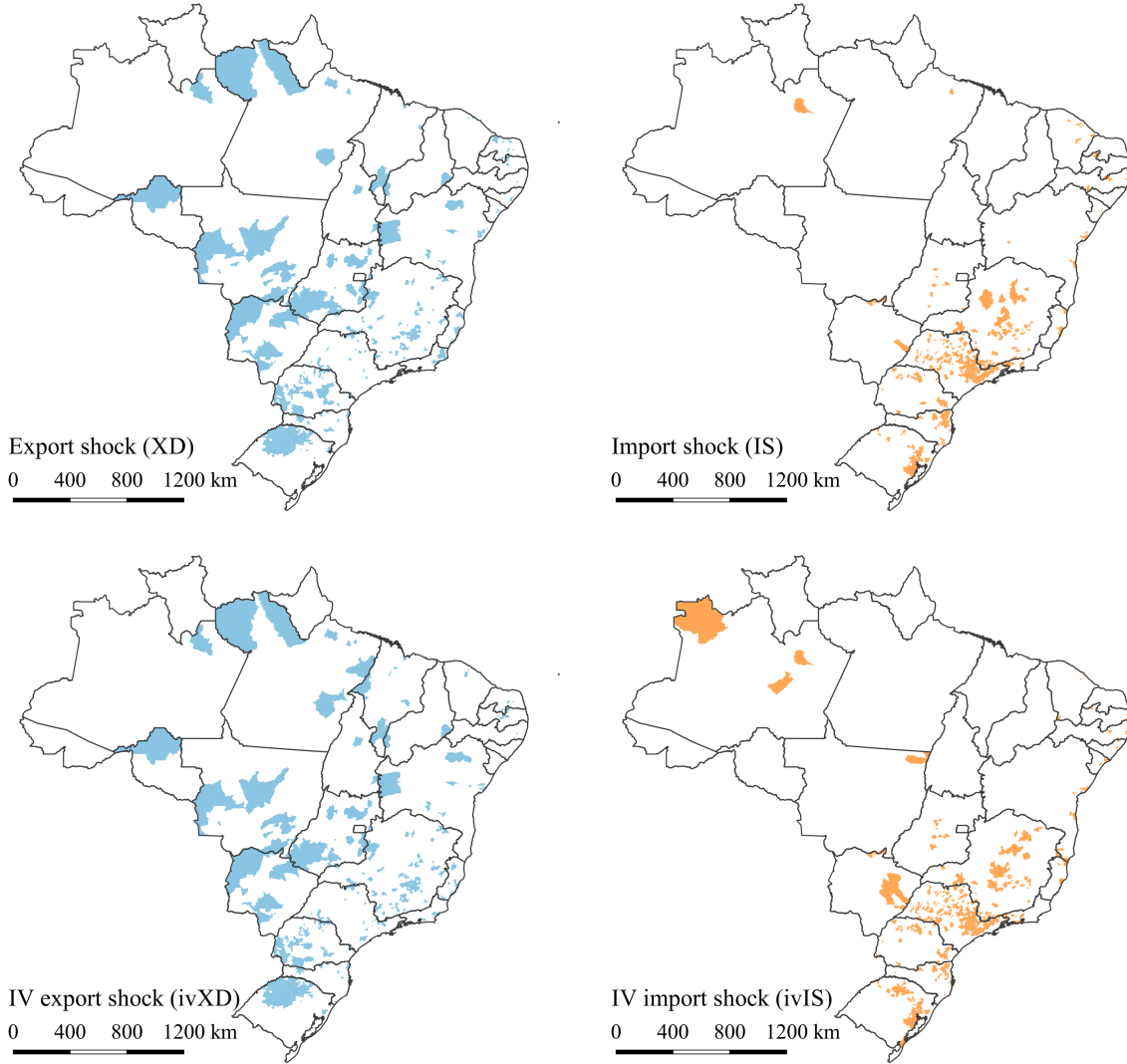
Baum-Snow and Ferreira (2015) and Bombardini and Li (2018) explicitly state that consistent shift-share estimates require labor shares used as weights to be exogenous. However, the requirement that $Cov(s_{i,j}, \Delta \varepsilon_i | \boldsymbol{W}_i) = 0$ should hold is a difficult assumption to support when $s_{i,j}$ are taken from the same year the quasi-experiment starts. In our application, we might have $Cov(s_{i,j,2000}, \varepsilon_{i,2000} | \boldsymbol{W}_i) \neq 0$ due to, for example, differences in local legislation that favor some activities over others, which consequently also affects environmental quality. To address this problem, Autor et al. (2013) opted for using labor shares lagged by a decade, relying on the assumption that $\Delta \varepsilon_i$ are not autocorrelated. Though easier to argue for the exogeneity of $s_{i,j}$ in this case and, thus, imbuing estimates with a greater degree of credibility, it does so on the cost of precision, since the shift-share variable would be more prone to incorrectly assign fractions of the shock variation to regions whose profile of economic activities changed significantly since the year lagged shares refer to.

Fortunately, recent developments in the shift-share literature show that we can use baseline-

year shares in the exposure to trade variables and still be able to estimate our model consistently. Borusyak et al. (2018) argue that identification in the shift-share framework relies on exogeneity of the "shift" element (that is, the shocks) rather than the weights used to allocate it throughout regions. They establish a numerical equivalence to the shift-share IV estimator and arrive at two identification hypotheses, neither of which impose restrictions on shares $s_{i,j}$. This is convenient because our predicted Brazil-China trade flows in (6) are much more likely to satisfy the orthogonality condition than baseline-year labor shares $s_{i,j}$. We present a detailed discussion on this topic, as well as the identification hypotheses, in Appendix B.

3.2   Interpreting the estimates

Supposing that the necessary conditions for consistent estimation of our model with shift-share variables and instruments are satisfied, we now discuss how to interpret our estimates. Notice in figure 3 how specially the export shocks is considerably dispersed throughout Brazil's territory, which is vastly diverse in physical and socioeconomic characteristics. A natural concern then arises over the adequacy of modeling the environmental impacts of the China shocks as homogeneous across municipalities, which is how they are treated in equation (1).

Based on the canonical model presented in equation (B1) of Appendix B, Adão et al. (2018) explore this issue in a setting where the effects of shocks can differ across locations and industries; that is, the "true" impacts, given by $\beta_{i,j}$, are allowed to vary along i and j. In this case, they show that shift-share estimates correspond to a weighted average of the $\beta_{i,j}$, with weights increasing not only in local labor shares $s_{i,j}$, but also in the variance of industry-specific shocks g_j[12]. Therefore, our estimates $\hat{\beta}_1$ and $\hat{\beta}_2$ will more accurately represent the impacts in municipalities that had relatively high shares of workers employed in traded activities in 2000.

Besides this issue, we must also address whether our estimates provide convincing evidence for environmental effects of the China shocks. Evidently, nothing about the shift-share variables in (2) pertains to environmental quality per se; they are simply measures of a regions' exposure to trade, given in dollars per worker. In our regressions regarding mortality rates, for example, the estimates may reflect changes in environmental quality, but are also likely to capture effects related to changes in income and employment. Shocks to available income may lead individuals to increase or decrease private expenditures in health care and prevention. Likewise, a change in local labor market attractiveness may induce migration, leading to a change in the composition of workers. In both cases, health quality could change in response to the China shocks even if

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[12]Adão et al. (2018) study the statistical properties of the shift-share estimator by repeatedly drawing shocks g_j. Therefore, we cannot decompose $\hat{\beta}$ so as to find every industry-region specific parameter $\beta_{i,j}$ because we observe only the actual values of each shock; i. e., we cannot know the variance of g_j. Under the share exogeneity assumption, though, one could decompose $\hat{\beta}$ in an average of industry-specific parameters β_j associated with "Rotemberg weights", as demonstrated by Goldsmith-Pinkham et al. (2018).

local environmental quality went unaltered.

Bombardini and Li (2018) face the same problem in their study, which they try to account for by introducing a shift-share variable, in addition to one similar to (2), that accounts for industry-specific pollution intensity (so that larger weights are assigned to "dirtier" sectors). The idea is to separate income channels from environmental ones by comparing two regions with similar values in the monetary dimension of the shock, as we mentioned in section 2. However, this will be the case only if the more pollution-intensive industries are not also the ones who better remunerate labor, as otherwise weighting industries by pollution intensity would be similar to weighting them by wage rates. Despite this drawback, it is nonetheless a promising attempt at addressing confounding income effects. Unfortunately, we cannot replicate their approach since the emission coefficients they use to quantify pollution intensity are obtained from the US Environmental Protection Agency (EPA), which reports them only for manufacturing activities. No equivalent measures exist for non-manufacturing sectors, notably agriculture, which is of major importance to Brazil.

It is also problematic that we define China's trade shocks as changes over a period of 10 years. One could argue that an environmental impact would be more easily perceived without any influence of confounders as soon as local activity reacted to the shocks. This is because, for example, the response of the rest of the economy to the shocks could be delayed by market imperfections and, also, inhabitants might not adopt defensive behavior immediately, thus not "clouding" effects on mortality outcomes. It is very unlikely, though, that this brief *coeteris paribus* window for evaluating environmental changes would last up to a decade. This goes along with Haddad and Maggi (2017)'s main criticism of quasi-experimental designs based on the China shocks: because the economy has complex linkages through sectors, space and time, general equilibrium effects are bound to follow a disturbance to any specific industry, with various adjustments occurring simultaneously, going in different directions at different paces. Without a CGE approach or a specific estimable equation derived from a structural model, it is hard to disentangle these effects through regression analysis.

In our application, the interference of general equilibrium effects translates into an issue of "measurement error" in the exposure to trade measures, which leads to attenuation bias. This is because such effects incite responses to China shocks from regions not expected to do so. For example, locations not predicted to be exposed to shocks might experience changes in activity levels due to being specialized in inputs used in activities that are directly affected. Also, the expansion of a sector may advance into regions that initially had few or no workers employed in it. On the other hand, general equilibrium effects can lead to shifts in activity levels in regions predicted to be but not directly affected by China's demand and supply of goods. For instance, a region that does not purchase in foreign markets a good which it also produces locally, might be affected nonetheless through market share loss in other regions that do import that good.

We try to minimize the noise introduced by general equilibrium effects with the controls we include in $\boldsymbol{W}_i$. Recall that our estimation strategy can be seen as a continuous differences-in-differences design: if regions with common pre-trends are expected to experience similar general equilibrium effects, then we might reduce attenuation bias by choosing a relevant set of observable factors to control for.

3.3 Inference

Economic activity is very often spatially correlated, so it seems reasonable to calculate standard errors robust to heteroskedasticity across geographical clusters instead of the standard Huber-White estimates. In our application, we cluster municipalities in "microregions", state subdivisions conceived to group contiguous municipalities that not only share similarities in economic specialization but are also complementary to each other in terms of production chains and distribution of goods (BRASIL, 1989), therefore prone to be similarly influenced by the China shocks.

Adão et al. (2018), however, are concerned that the shift-share structure of the exposure variables can lead a location i to be correlated with another location h through similarity in industry specialization. They show that the regression residuals of a shift-share model can themselves have the shift-share structure, so that similar allocation of labor across activities between two regions cause them to have similar residuals. Hence, we might have $Cov(\Delta \varepsilon_i, \Delta \varepsilon_h) \neq 0$ regardless of geographical proximity, which can lead to deceptively small standard error estimates and, thus, overrejection of statistical insignificance of the estimated coefficients. This issue is particularly perverse because, given the spatial nature of problems that shift-share modeling is usually adopted for, researchers are inclined to trust the common practice of computing standard errors in geographic clusters to deal with correlated residuals. However, if two regions similar in industry specialization are geographically far apart, there can be residual correlation unaccounted for.

To address this issue, Adão et al. (2018) propose a new estimator for standard errors in shift-share models, which we present in Appendix C. When replicating Autor et al. (2013)'s study, they find that their confidence intervals were considerably broader than those constructed with traditional Huber-White or geographical cluster-robust standard errors. Since this suggests that residual correlation due to similarities in industry specialization can present non-negligible problems to inference, we calculate their standard errors based on our main results and report them alongside the previously mentioned microregion-robust standard errors in Appendix C.

4 Data

All monetary variables used in this study were converted to 2000's American dollars using the US's implicit GDP deflator and the year-average dollar to Brazil's reais exchange rate. The geographical level of our analysis is municipalities, therefore departing from Costa et al. (2016), who use microregions. This is because we are not interested in regional labor markets outcomes per se, which would indeed be more fittingly measured in microregions rather than in municipalities. For our purposes, the use of labor data is merely instrumental: we use it to approximate the group of activities taking place in each municipality and the scale at which they are performed. Since we are interested in the impact of economic activities on the environment around it, a finer unit of observation might be desirable as we expect such effects to be relatively concentrated on the surroundings of where the activity is effectively performed[13]. Because new municipalities were created over the course of the period we are studying, we had to group together those that in 2000 originated new ones in 2010, leaving us with 5500 municipalities. We further aggregate them when we include a lag of the outcome variable among the controls, as municipalities also increased in number along the 1990's. In these specifications, we have 4264 municipalities.

In our regressions, we generally include mesoregion-year dummies as controls, therefore restricting the comparison of municipalities to those belonging to the same mesoregion. Given that "microregions" are contained in "mesoregions"[14], we could instead include microregion-year dummies, which should be more effective in conveying the argument that we are comparing municipalities that satisfy the condition of parallel pre-trends. We do not do this because a significant amount of microregions contain too few municipalities, an issue partially due to the groupings mentioned above. For example, in the Legal Amazon, 46 of the 100 microregions have less than 6 municipalities, while this is the case for only 1 of the 29 mesoregions. A similar picture holds for the full sample: 40% of the microregions contain less than 6 municipalities, whereas this is a problem for less than 7% of the mesoregions. Since we are working with only two years of data, we opt for not using microregion-year dummies to avoid excessively restricting sample variation. As we mentioned earlier, however, we do use microregions when calculating cluster-robust standard errors, whose estimates benefit from a higher number of clusters.

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[13] As one may expect, this is not necessarily the case. For example, Lipscomb and Mobarak (2017) show evidence that municipalities in Brazil tend to let pollution in rivers accumulate as it moves along their jurisdiction, leaving the burden of cleaning it to its downstream neighbor.

[14] "Mesoregions" group contiguous microregions within a state using analogous criteria to those followed when grouping municipalities in microregions (BRASIL, 1989). Note, though, that neither of these groupings is associated with an administrative level, whereas municipalities and states are.

4.1 Employment and socioeconomic characteristics

Our main source for information at the level of municipalities is the 2000's Population Census, conducted by the Brazilian Institute of Geography and Statistics (IBGE). Specifically, we use microdata from the households sampled to answer a detailed questionnaire, which contemplates several individual and household aspects. We observe whether a person works, in which municipality, formally or informally and in what sector, according to the National Classification of Economic Activities for Household Surveys (CNAEdom), which contains over 200 activities spanning from services to agricultural and manufacturing sectors. Besides employment information, we also use the 2000's Census microdata for per capita income and the share of each municipality's households that are in urban areas, have access to a water supply system and are contemplated by sewerage services.

We use the 2010's Population Census by IBGE to compute the share of each municipality's population aged 10 years or more that has not immigrated from another municipality in the last decade. This is used in an attempt to account for possible effects of internal migration: we restrict the sample to municipalities where the majority of residents fit this criterion, expecting it to help assess the influence of compositional changes in population on municipal health outcomes.

4.2 International trade

We use the BACI database, maintained by the Center of Prospective Studies and International Information (CEPII), for data on global trade. It is based on the United Nations Commodity Trade Statistics Database (COMTRADE), which as of now compiles data from over 170 countries[15]. BACI presents traded value and weight of commodities classified in accordance to the Harmonized System codes (detailed up to 6 digits). We use this information not only to describe trade between Brazil and China, but also for all countries that reported trade flows in 2000 and 2010, since our IV strategy requires us to run regressions for every country except Brazil[16]. We crosswalk from trade codes to CNAEdom using the correspondence table Costa

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[15]For some tables, we report data from ComexStat, which is Brazil's public query system for trade statistics. Both COMTRADE and ComexStat present the same information, but the latter provides it with greater detail, such as trade by state.

[16]The great advantage of using CEPII's database over COMTRADE itself is that, despite efforts for standardization, COMTRADE's information can be problematic because countries themselves report trade records. For instance, since there is no unified policy for transportation and taxing of foreign goods, it is not clear how much of a reported traded value actually refers to the commodity itself and not to freight costs or import taxes. As a result, one transaction can present a large discrepancy between the value a country allegedly exports and the other allegedly imports. CEPII BACI addresses these issues so as to provide one consistent data set in which tracking down a trade flow from the perspective of the importer and the exporter yields the same results. Details

et al. (2016) provide along with the online version of their paper.

4.3 Deforestation of the Amazon rainforest and the Cerrado savanna

The National Institute of Spatial Research (INPE) conducts the "PRODES" projects, which monitor the state of deforestation in the Amazon rainforest and the Cerrado savanna using satellite images. Figure 4 displays the original vegetation cover of both biomes. We use data made publicly available through tables and shapefiles to build our deforestation outcomes at the municipality level. PRODES monitors deforestation of the "shallow cut" kind; that is, complete removal of forest cover in a short period of time, spotted by comparing satellite images of the forest from consecutive years. This means that regions with slowly vanishing vegetation are not accounted for when calculating yearly increases in deforested area. Also, areas with compromised visibility in satellite images due to clouds, as well as those mixed with vegetation from other biomes and hydrography, are reported separately and likewise not considered when computing deforestation.

"PRODES Amazônia", designed to keep track of deforestation in the Brazilian portion of the rainforest (referred to as the "Legal Amazon" area), was implemented in 1988 and has been running uninterruptedly ever since. The deforestation rate calculated from its data is an important indicator of effectivity in detaining anthropic devastation of the forest, and was used as the basis for Brazil's commitments made in 2015's UN Climate Change Conference. However, methodological changes adopted throughout the years make it difficult to conceal recent with historic data, reason why INPE only publicizes deforestation data from 2000 onwards. Conveniently, though, it is already mapped across municipalities.

"PRODES Cerrado" is a more recent initiative that shares the same purpose of its counterpart for the Amazon. It uses data from previous monitoring programs established through international agreements and is currently funded by the World Bank. INPE recently made available a shapefile for the evolution of Cerrado devastation starting in 2000. These are not mapped across municipalities, however, meaning we had to fit it to an administrative borders' grid provided by IBGE to calculate the change in deforested area by municipality.

Figure 5 displays the state of deforestation of the Amazon rainforest and Cerrado savanna in 2000. Also depicted are municipalities which had rural establishments cultivating soybeans as of 2006, according to the Census of Agriculture, conducted by IBGE. Notice how many of these municipalities are in the Cerrado area, especially in regions already largely devastated in Mato Grosso (MT), Goiás (GO) and Mato Grosso do Sul (MS). Since the most significant part of the production of soybeans takes place in these states, as shown in table A3 in Appendix A, the maps suggest that soybean farming expanded where there was already little natural vege-

at http://www.cepii.fr/PDF_PUB/wp/2010/wp2010-23.pdf.

Figure 4: Natural occurrence of Amazon and Cerrado biomes in Brazil
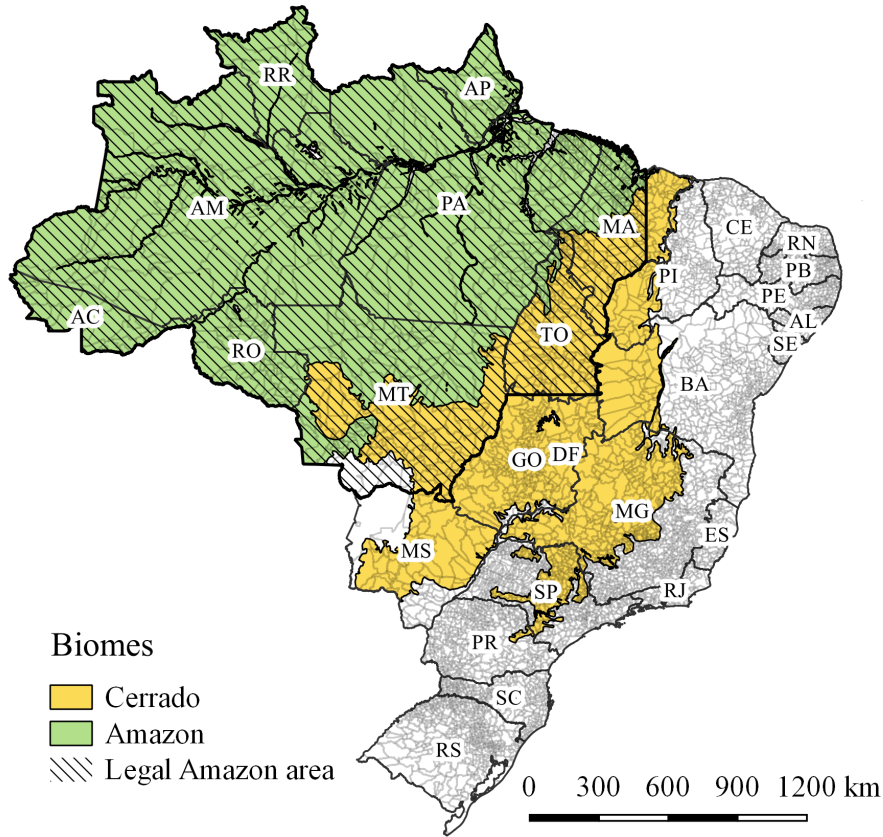
Source: IBGE.

tation remaining. Without much left to deforest, these municipalities might have experienced relatively small changes in deforested area over the period we are studying. However, there are also municipalities with soybean farming in Brazil's North and North-East regions, in the states of Bahia (BA), Piauí (PI), Maranhão (MA) and Tocantins (TO), which appear to be relatively less devastated in 2000. These regions may then present enough variation in vegetation cover to allow us identify potential effects of trade with China on deforestation.

4.4   Mortality

We build mortality outcomes using data from DataSUS, an online databank maintained by Brazil's Unified Healthcare System (SUS). Every entry informs the municipality of occurrence, municipality of residence, age, gender, skin color and cause of death according to the 10th International Classification of Diseases (ICD-10), among other characteristics. We are then able to aggregate these data to the municipality level by age group and causes, allowing us to study whether effects of the China shocks on mortality differ for varying levels of vulnerability to pollution-related illnesses. We have data spanning from 1996 to 2010, meaning we are able to include a "short lag" (from 1996 to 2000) of the outcome variable in our model to help control for he possible influence of pre-existing trends in mortality.

Figure 5: "Shallow cut" deforestation in the legal Amazon and Cerrado area

Sources: INPE and IBGE.
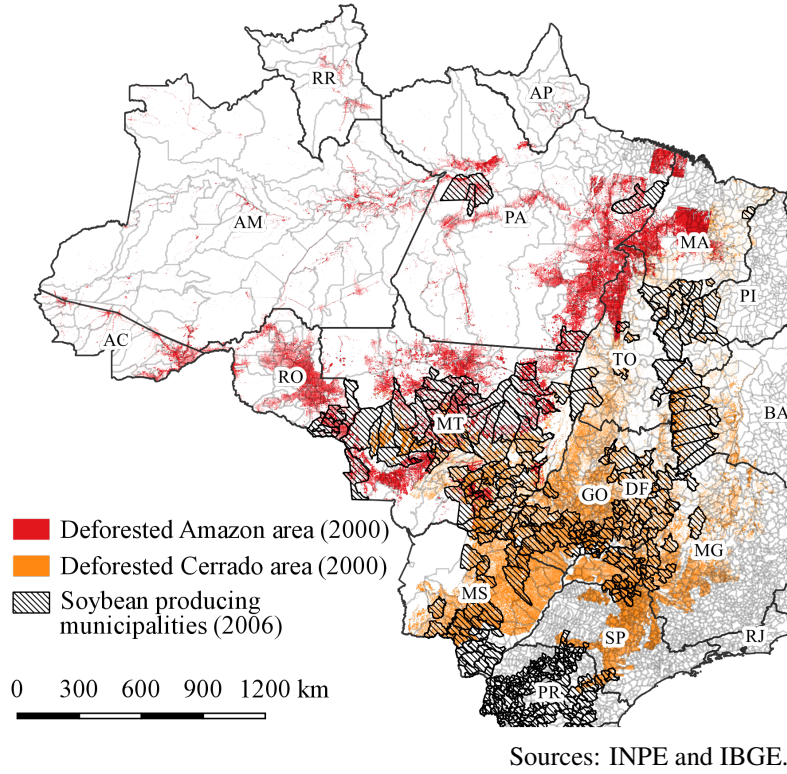
5 Results

5.1 Effects on deforestation

Table 1 describes the behavior of deforestation in the Amazon and Cerrado regions, measured as the change in the share of a municipality's area that was deforested between 2000 and 2010. While the average municipality in the Cerrado deforested more than its counterpart in the Amazon, the empirical distribution of deforestation in the latter has heavier tails and some clear outliers. As a robustness check, we dropped the 70 municipalities with change in deforested area higher than 20 percentage points and the results were nearly unaltered.

We also report statistics for municipalities with high exposure to the shocks, which we portrayed earlier in figure 3. Notice how average increases of deforestation in municipalities most affected by China were smaller than the general rates for both the Cerrado and Amazon regions, suggesting that trade-oriented municipalities were less aggressive towards the environment. On the other hand, this could be reflecting the fact that those municipalities were, on average, already more deforested in 2000, especially in the Cerrado.

Our estimates for the impacts of the China shocks on deforestation of the Amazon are presented in table 2. Coefficients are to be read as the expected change in percentage points following an increase of US$1,000 per worker in a municipality's exposure to trade. Column 1 presents slope coefficients for an OLS fit with no controls, while the others report 2SLS

Table 1: Descriptive statistics - difference in deforested area as share of municipality

	Mean	Std. dev.	Baseline	1st q.	Median	3rd q.	Max
Amazon	4.15	7.41	10.5	.163	1.16	5.29	97.9
High IS	4.06	6.16	11.5	.135	.475	6.13	41
High XD	3.53	5.5	10.6	.289	.744	6.13	46.9
Cerrado	7.16	5.97	25.7	2.27	6.02	11	37.3
High IS	6.85	6.68	48.8	1.28	4.32	12.7	25.2
High XD	6.87	5.26	34.9	2.15	5.88	10.9	21.6

Statistics weighted by the municipality's area in km^2. "High XD" and "high IS" refer to municipalities in the 10th decile of exposure to the China shocks.

estimates using (7) as instruments for our exposure to trade measures. All regressions are weighted by the municipality's area in km^2. The Kleibergen-Paap (KP) statistics refer to a heteroskedasticity-robust test of weak correlation between the instruments and actual measures of exposure to trade.

While the "supply of imports" shock *IS* did not cause any differences between exposed and unexposed municipalities, the "demand for exports" shock *XD* is associated with a significant negative effect in the first four specifications. Notice how the coefficient barely changes from column 1 to 2, a consequence of the very high correlation between *XD* and its instrument mentioned in section 3. In column 3, controlling for mesoregional trends and municipal characteristics, we estimate that exporting municipalities experienced an average reduction of 0.063 percentage points in deforested area relative to unaffected ones for every US$1,000 in exports per worker. Column 4 tests the coefficients' sensitivity to the inclusion of the share of a municipality's area that was already deforested in 2000. Because the model is estimated in first-differences, this term actually expresses the accumulated change in a municipality's deforested area between 2000 and the last year when 100% of its original forest cover was still intact. Since this likely refers to remote times, including 2000's deforestation does little towards controlling for immediate pre-trends, which would be our intention. Still, it shows us that *XD*'s coefficient does not depend on the stock of deforested area in the baseline year, as the estimates change only slightly from column 3 to 4.

In column 5, we control separately for the share of workers employed in soybean farming and mining activities, which account for the majority of what is exported from the Legal Amazon to China, according to table A4 in Appendix A. The share of mining workers causes *XD*'s coefficient to lose significance, turn positive and, together with the share of workers in soybeans, actually grow in magnitude. Consider table A5, where the first and second columns reproduce the third and fifth of table 2: the exclusion of the share of soybean workers, in column 3, reduces the magnitude of *XD*'s coefficient, but does not make it statistically significant.

Table 2: Difference in deforested area as share of municipality - Amazon rainforest

	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	2SLS	2SLS	2SLS	2SLS	2SLS
Import shock (IS)	-4.312	-1.743	0.218	0.212	-0.690	2.614
	(6.262)	(6.821)	(4.644)	(4.974)	(4.743)	(3.819)
Export shock (XD)	-0.134***	-0.134***	-0.063**	-0.072**	1.763	-0.018
	(0.029)	(0.028)	(0.031)	(0.034)	(1.525)	(0.032)
KP F-stat		62.30	59.99	60.39	9.494	63.23
No. of munic.	738	738	738	738	738	738
No. of clusters	100	100	100	100	100	100
Mesoregion trends			Y	Y	Y	Y
Munic. controls			Y	Y	Y	Y
% deforested (2000)				Y		
% labor in main exports					Y	
% protected lands						Y

Standard errors (in parentheses) clustered by microregions. Regressions are weighted by the municipality's area in km^2. Municipal-level controls are fixed in 2000 and interacted with year dummies, including a quadratic polynomial of per capita income, share of workers in the primary sector and in non-traded sectors, share of households in urban areas and share of territory with other vegetation or hydrography. Also, we account for visibility issues when evaluating the change in a municipality's deforested area, labeled by PRODES as obstructed by clouds or "unobserved". "KP F-stat" refers to the heteroskedasticity-robust Kleibergen-Paap test for weak instruments. The municipality of Manaus is not included in the sample, as it contains a tax-free economic zone.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

On the other hand, column 4 shows that the exclusion of the share of mining workers alone makes *XD*'s effect revert to what it was before controlling for any labor shares. Because the inclusion of these shares has the same effect as controlling for "specific" mining and soybean shocks (given by $s_{i,\text{mining}} \cdot g_{\text{mining}}$ and $s_{i,\text{soybeans}} \cdot g_{\text{soybeans}}$, as in equation (3)), we can interpret column 5 of table 2 as meaning that the China shock variation related to all other activities does not have any impact on deforestation[17]. In fact, *XD*'s coefficient increases drastically due to how little variation is left in it: the correlation between the "demand for exports" shock and the share of mining workers in each municipality is of 94% in the Amazon region, reaching 98% if we weight municipalities by their area, as we do in our regressions. By restricting the comparison of municipalities to those that are assigned similar levels of the "mining shock", we leave *XD* with little residual variation, which suggests that if there is any effect of the China shocks

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[17]Because g_j does not vary along i, it becomes a scaling factor in $s_{i,j} \cdot g_j$ when we include it as a separate regressor.

on deforestation, it is restricted to mining activities[18]. Column 5 of table A5 reinforces this argument, as it shows that a modified exports shock, where we ignored mining and soybeans when summing over activities to get the exposure to trade variables, is not relevant to explain deforestation of the Amazon.

Related to this discussion, we considered "winsorized" versions of the exposure to trade variables and their instruments to investigate the influence of possible outliers. We limited both extremes of the shocks' distributions to the 5$^{\text{th}}$ and 95$^{\text{th}}$ percentiles, with all observations outside this interval assuming the value of the closer bound. However, this procedure virtually led to the censoring of municipalities with any specialization in mining, as the correlation between the share of mining workers and XD subtracted of its winsorized counterpart is 97.4%, reaching 99.3% when weighted by municipal area. Hence, standard procedures that remove outliers actually restrict the sample in a non-random fashion, discarding all variation related to a specific economic activity. This illustrates how much the shock variation in mining products deviates from that of other sectors.

Nonetheless, we do not take from the results in table 2 that mining activities are "environment friendly". First off, change in deforested area measures only one dimension of environmental quality. Regardless of the actual amount of deforestation a typical mining operation promotes, it also involves other types of environmental hazards, such as the disposal of toxic residues. Putting that aside, notice how XD also became statistically insignificant in column 6 of table 2, where we included the share of a municipality's area that belongs to indigenous territories and two different types of conservation units, which vary in the strictness of forest protection and in what economic activities can be performed within their limits. This suggests that the China shocks are not relevant to explain deforestation once we restrict variation to municipalities with similar levels of environmental protection, implying that what our estimates capture as the impact of the "demand for exports" shock on deforestation is actually the effect of a time-varying factor associated with protected lands.

This confounding problem between the occurrence of mining activities in 2000 and the areas of protected lands is most likely caused by a shortcoming of using local labor shares as weights in the shift-share variables to distribute variation from the China shocks throughout municipalities. The labor data we take from the Census does not discriminate mining workers by the metals they extract, so that, considering that China's main imports from the mining sector were of iron ores, the shift-share variable might have assigned fractions of China's demand for iron to mining municipalities who do not actually produce it. For example, it could have assigned it to those that do not contain *large* mining operations, since the extraction of iron ores in the Legal Amazon is concentrated in the region of Carajás, Pará (PA), where Brazil's biggest iron mine is located (BRASIL, 2009). In this case, the "size" aspect would help explain why XD's

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[18]It also explains the significant drop in the associated F-stat for testing joint relevance of the instruments.

effect vanishes once we control for the existence of protected lands: the negative coefficient in column 3 of table 2 would result from comparing municipalities with large protected areas and small-scale mining, which is feasible near or inside conservation units, to those with low forest protection but no mining activities[19]. Figure 6 shows how protected lands in 2000 actually overlap several spots of mining interest in Pará and surrounding states. Mining activities are only allowed to take place in some types of "sustainable use" units, being strictly forbidden in those of "integral protection" and indigenous territories, thus making it more difficult to develop large-scale operations in these locations.

Figure 6: Protected lands and mineral occurrences in the Amazon
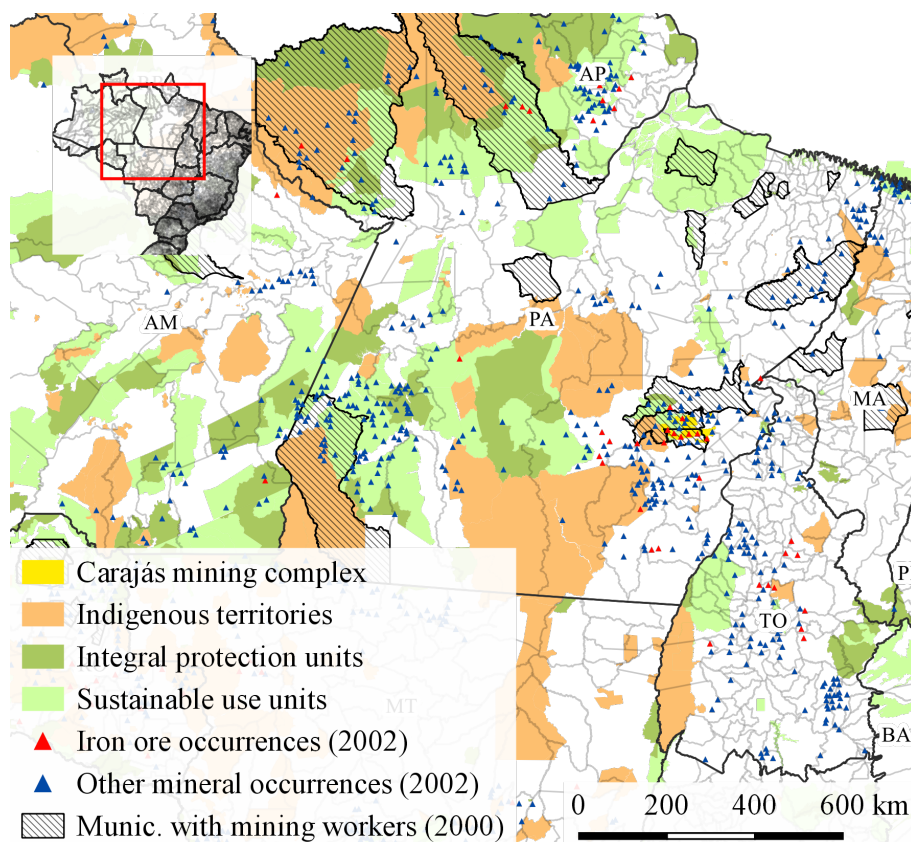
Sources: FUNAI, IBGE and the Ministry of the Environment (MMA).

However, small-scale mining activities most likely are not the main cause of the confounding problem. Recall that, according to Adão et al. (2018), the shift-share estimator gives more weight to locations with higher shares of their workers in activities affected by the China shocks, meaning that the effects we estimate mostly reflect those of municipalities with higher exposure

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[19]A recent study identified 453 illegal mining sites in the Brazilian Amazon, describing the situation as "epidemic" (MORAES, 2018). Their maps show that these sites concentrate in the state of Pará (PA) and are indeed close to or inside protected lands. While their data refers to recent years, it is not unlikely that a similar situation held throughout the 2000's, especially if we consider that monitoring technologies have improved over the years, so that running mining sites close to or inside protected areas might have been easier in the period we are studying.

to them, such as those depicted in figure 3. Comparing those maps to the one in figure 6, we see not only that mining in Carajás takes place inside a "sustainable use" conservation unit, but that the two municipalities in the north of Pará that are highly exposed to China's "demand for exports" shock also contain large areas of conservation units and indigenous territories. Mining in these locations, however, is of aluminum ores, which China did not start importing from Brazil until 2012, as figure A1 in Appendix A shows[20]. This illustrates how the shift-share misallocation issue and the confounding presence of protected lands combined to produce the deceiving result in column 3 of table 2: the shock variation generated in Carajás from iron ores exports was assigned to municipalities that not only were large producers of other minerals, but also had significant shares of their territory protected by conservation units and indigenous territories. This is corroborated by the results in table A6, where we reestimate the models in table 2 without the municipalities where the Carajás mines are located: despite removing the "true" major source of variation in XD, the coefficients barely change.

In sum, mining activities did not reduce deforestation of the Amazon rainforest: the share of workers in mining activities was endogenous in column 5 of table 2, as it correlates with the existence of protected lands, much more likely to be effective in preserving forest cover. This issue, combined with insufficiently detailed labor data, led the shift-share variable to allocate important shock variation to unaffected mining municipalities which incidentally also experienced effective forest protection. As a result, the estimated impact of China's "demand for exports" shock appears to actually have been the influence of a trend that was common to municipalities with similar shares of their territory under protected lands. Indeed, during the 2000's, while actions seeking to increase monitoring and strengthen law enforcement were implemented to reduce deforestation in the whole Legal Amazon[21], there is evidence that the

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[20]Throughout the text, we have mentioned the importance of iron in Brazil's commercial relation with China. Figure A1 provides some visual support to those statements, as it shows that iron ores were virtually the only minerals China imported from Brazil between 2000 and 2010. In addition, it helps address concerns over possible impacts of the China shocks in other mining products. Manganese is used in the production of steel, and is thus a complementary good to iron, which explains why China's relevance as an importer of ores of both these metals correlate throughout the 2000's. Coincidentally, Carajás is also the main producer of manganese ores in the Legal Amazon, with Pará accounting for 88.7% of the total volume exported to China between 2000 and 2010, according to ComexStat. Since the municipalities that export manganese to China are generally also the ones who export iron, we choose to not address this "manganese China shock" explicitly in our analysis. As for aluminum ores, it is clear that China did not directly influence its production, which lends credibility to our argument that there is a shift-share misassignment problem. The situation for other minerals is generally similar to that of aluminum, with the exception of chromium ores (not depicted), to which China's relevance as an importer rose abruptly between 2005 and 2010. These exports were entirely from the the state of Amapá (AP), where no mining workers were identified in 2000, meaning that the shift-share variable necessarily misassigns this specific shock.

[21]These actions were adopted within the scope of the PPCDAm program, which is credited to have reduced de-

specific management of conservation units also improved in this period. For instance, a larger share of the federal units reported having performed vegetation recovery, detection of threats, law enforcement and educational actions between 2008 and 2010 than in 2003 to 2005 (ICM-BIO/WWF, 2012).

Table 3 presents results for the change in deforested area of the Cerrado biome. Once the instruments are introduced, from column 2 onwards, the China shocks do not seem to cause any differences between exposed and unexposed municipalities in terms of deforestation. The KP statistic also reveals a very weak first stage, which is mainly due to the problem of low correlation between *ivIS* and *IS*. However, even when we fit our model only on *XD* (instrumented by *ivXD*) and the controls, our results do not show any significant effects, notwithstanding a much stronger first stage. Hence, deforestation in Cerrado municipalities does not appear to have been affected by the China shocks.

Table 3: Difference in deforested area as share of municipality - Cerrado savanna

	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	2SLS	2SLS	2SLS	2SLS	2SLS
Import shock (IS)	-2.562**	-9.846	0.258	0.969	0.299	-0.360
	(1.180)	(10.545)	(3.675)	(4.091)	(4.654)	(3.634)
Export shock (XD)	-0.022	-0.024	-0.081	-0.045	-0.092	-0.107
	(0.148)	(0.168)	(0.081)	(0.066)	(3.548)	(0.083)
KP F-stat		0.271	0.136	0.137	0.0951	0.136
No. of munic.	1364	1364	1364	1364	1364	1364
No. of clusters	169	169	169	169	169	169
Mesoregion trends			Y	Y	Y	Y
Munic. controls			Y	Y	Y	Y
% deforested (2000)				Y		
% labor in main exports					Y	
% protected lands						Y

Standard errors (in parentheses) clustered by microregions. Regressions are weighted by the municipality's area in km^2. Municipal-level controls are fixed in 2000 and interacted with year dummies, including a quadratic polynomial of per capita income, share of workers in the primary sector and in non-traded sectors, share of households in urban areas and share of territory with other vegetation or hydrography. "KP F-stat" refers to the heteroskedasticity-robust Kleibergen-Paap test for weak instruments.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

We mentioned earlier that absence of impacts on the Cerrado could be caused by lack of

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forestation rates in the Amazon. Assunção et al. (2015) show that it was effective in containing deforestation in spite of the influence of changes in prices of agricultural commodities in international markets.

variation in Δy_i, as municipalities in the Center-West region of Brazil were already at an advanced stage of deforestation in 2000. While reasonable, this argument actually relates to the dynamics of soybean farming in Brazil, which can explain why our estimates indicate that its expansion induced by China's demand is not a driver of deforestation in either the Amazon or Cerrado, despite commonly being presented as such[22]. Domingues and Bermann (2012) argue that soybean harvesting does not directly cause deforestation, but creates the economic incentives for other activities to do so. Historically, it has moved north through the Center-West by replacing smaller family-owned agricultural establishments, thus creating a profitable market for land. Squatters then advance over the Amazon removing forest cover, which is turned into grazing lands where low-productivity livestock farms serve as a placeholder activity to assert land possession. This practice is facilitated by the problematic regulation regarding land property rights in the Amazon, where the physical occupation of an area tends to suffice for a squatter to claim ownership, which is eventually sold to soybean farmers. Therefore, soybean harvesting does not seem to be a direct driver of deforestation; rather, it expands over portions of the forest that were cleared by other activities. Since our exposure to trade variables assign China's demand for soybeans to municipalities that already were producing it in 2000, they are unable to capture this indirect impact on deforestation.

5.2   Effects on mortality

We are interested in possible effects of the China shocks on the environment, despite the fact that reliable pollution data is unavailable at the level of municipalities. Thus, we instead focus on changes in mortality rates, which Chay and Greenstone (2003) and Bombardini and Li (2018) show to be plausibly related to changes in environmental quality. Table A7 describes empirical distributions of the number of deaths per 100,000 inhabitants, where we present statistics for each of the six age groups considered in our regressions. Considering that these are weighted by municipal population at each group[23], the fact that the average Brazilian municipality experienced an increase in mortality of all ages is a result likely driven by municipalities with larger and older population, since mortality of the five younger age groups decreased between 2000 and 2010, with the greatest reductions being of deaths of children with less than 1 and less than 5 years of age.

Table 4 presents results for mortality in general. Coefficients are to be read as the expected change in deaths per 100,000 inhabitants following an increase of US\$1,000 per worker in a municipality's exposure to trade. Columns vary in the age group Δy_i is evaluated in, but they all feature similar specifications. Panel A contains 2SLS estimates of models that include the

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[22] According to Gibbs et al. (2015), this perception went as far as to lead soybean traders to sign the Soy Moratorium in 2006, in which it was agreed to boycott producers in the Amazon.

[23] Which we also use as weights in our regressions.

exposure to trade variables, baseline municipal characteristics and a set of mesoregion dummies. Also, because we have mortality data starting in 1996, we can control for the change of mortality between 1996 and 2000, even though this term does not capture a decennial trend as a "proper" lag of Δy_i would. Panel B further adds the share of workers employed in mining activities and soybean farming[24].

Table 4: Difference in number of deaths per 100,000 inhabitants - All causes

	(1)	(2)	(3)	(4)	(5)	(6)
	Aged < 1	Aged < 5	Aged 5-9	Aged 10-19	Aged 20-39	All ages
Panel A						
Import shock (IS)	-2.531*	-2.526	-0.386	-1.654*	-3.020	-18.253**
	(1.482)	(1.569)	(0.327)	(1.003)	(2.379)	(8.968)
Export shock (XD)	-0.086	-0.186	-0.012	0.061	-0.232*	-1.011
	(0.144)	(0.171)	(0.026)	(0.049)	(0.135)	(0.704)
KP F-stat	6.951	7.066	6.509	6.936	8.376	7.470
Panel B: controlling for % of workers in mining and soybeans						
Import shock (IS)	-2.236*	-2.398*	-0.328	-2.113*	-3.215	-13.210*
	(1.296)	(1.309)	(0.306)	(1.127)	(2.140)	(7.132)
Export shock (XD)	-0.243	0.081	0.145	0.728**	-1.231***	-6.204*
	(0.733)	(0.830)	(0.162)	(0.310)	(0.359)	(3.757)
KP F-stat	70.18	71.75	69.36	73.11	69.53	70.55
No. of munic.	4264	4264	4264	4264	4264	4264
No. of clusters	552	552	552	552	552	552

Coefficients from 2SLS models, with standard errors (in parentheses) clustered by microregions. Regressions are weighted by the municipality's population of each age range in 2000. All specifications include the following municipal-level controls, fixed in 2000 and interacted with year dummies: a quadratic polynomial of per capita income, share of workers in the primary sector and in non-traded sectors, share of households in urban areas, with access to a water supply system and sewerage services. In addition, mesoregion-year dummies and a lag of the outcome variable referring to the change between 1996 and 2000, instrumented by its level in 1996 to avoid endogeneity through autocorrelation of residuals. "KP F-stat" refers to the heteroskedasticity-robust Kleibergen-Paap test for weak instruments.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Nearly all estimates in panel A are negative for both shocks, yet most cannot be distin-

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[24]Unlike our regressions for deforestation, the inclusion of the share of a municipality's area that is protected by conservation units and indigenous territories is not relevant to explain the mortality outcomes.

guished from zero. Introducing the share of workers in mining and soybean farming (panel B) changes magnitudes but, being reduced form estimates, they are not really informative on how the China shocks could be causing these changes in mortality. For instance, what would explain the increase of US$1,000 in imports per worker leading to a reduction of 18 deaths per 100,000 people relative to unexposed municipalities? Several factors are likely to be acting at once and it is not clear whether any of them is dominant or if they tend to offset each other, as suggested by the results for the five first age groups.

Our strategy to disentangle these effects is to select a subset of mortality causes that are particularly sensitive to the channels we are interested in; that is, we look for environmental impacts of the China shocks in outcomes that should be more responsive to them. Hence, we investigate changes in mortality by "pollution-related" causes, those that are supposed to be closely associated with pollution from agricultural and industrial activities. We gathered ICD codes of illnesses that studies in the public health and medical literatures report to have occurred in the surroundings of polluting production facilities. These are listed in table A8.

Additionally, we consider three other groups of causes of death: "sanitation-related", associated with sewage problems and vector-borne diseases, as presented in BRASIL (2010, p. 65); "external causes", referring to what DataSUS classifies as deaths resulting from violent actions and accidents; and an expanded group of pollution-related illnesses, which adds a set of respiratory and cardiac diseases, as well as specific cancers, to those in table A8[25]. This broader group serves as a check of the sensitivity of our estimates to changes in our selection of pollution-related illnesses. As we did for total mortality, we present the empirical distributions for these four sets of causes of death in tables A9, A10, A11 and A12.

Table 5 features our results for the change in mortality due to the first set of pollution-related causes. The "demand for exports" shock displays a distinct pattern in panel A: a positive effect on mortality of younger children that gets smaller and loses significance as the evaluated age increases, becoming negative for ages beyond 10. The positive effect on all ages is entirely driven by young children's mortality[26]. Then, for children less than one year old, an increase of US$1,000 in exports per worker would cause the change in mortality between 2000 and 2010 to increase by 0.074 children per 100,000 inhabitants, on average, when compared to municipalities relatively unaffected by the export shock. While seemingly small, this is a relevant impact if we take into account that, according to table A9, the nationwide average reduction in mortality of children in this age group was of 0.166 per 100,000 people. The small KP statistics, which indicate a weak first stage, are once again caused by the low correlation between *ivIS* and *IS*. Fitting our model only on *XD* and the controls barely changes the point estimates and returns a much higher F-statistic, as shown in table A13, which easily rejects the hypothesis that *ivXD*

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[25]These illnesses are taken from Bhopal et al. (1994) and Gianicolo et al. (2013).

[26]Although we do not show it here, estimates for ages higher than 40 were statistically zero.

is a poor instrument[27]. Panel B of table 5, however, shows that this pattern vanishes once we restrict variation to municipalities with similar specialization in mining and soybean farming. As in our results for deforestation in the Amazon, it is specifically the inclusion of the share of workers in mining that removes all explaining power from XD, suggesting an effect restricted to municipalities specialized in mining activities.

Table 5: Difference in number of deaths per 100,000 inhabitants - Pollution-related illnesses

	(1)	(2)	(3)	(4)	(5)	(6)
	Aged < 1	Aged < 5	Aged 5-9	Aged 10-19	Aged 20-39	All ages
Panel A						
Import shock (IS)	-0.193	-0.228	0.022	-0.036	-0.067	-0.088
	(0.369)	(0.391)	(0.049)	(0.077)	(0.078)	(0.702)
Export shock (XD)	0.074***	0.069**	0.008*	-0.008**	-0.001	0.077**
	(0.027)	(0.028)	(0.005)	(0.004)	(0.004)	(0.035)
KP F-stat	6.990	6.996	6.545	6.825	8.407	7.529
Panel B: controlling for % of workers in mining and soybeans						
Import shock (IS)	-0.202	-0.235	0.022	-0.035	-0.066	-0.087
	(0.375)	(0.397)	(0.049)	(0.078)	(0.078)	(0.713)
Export shock (XD)	0.216	0.180	0.005	-0.016	-0.019	0.049
	(0.162)	(0.179)	(0.024)	(0.039)	(0.041)	(0.287)
KP F-stat	91.31	79.03	4179.7	68.84	70.89	76.78
No. of munic.	4264	4264	4264	4264	4264	4264
No. of clusters	552	552	552	552	552	552

Coefficients from 2SLS models, with standard errors (in parentheses) clustered by microregions. Regressions are weighted by the municipality's population of each age range in 2000. All specifications include the following municipal-level controls, fixed in 2000 and interacted with year dummies: a quadratic polynomial of per capita income, share of workers in the primary sector and in non-traded sectors, share of households in urban areas, with access to a water supply system and sewerage services. In addition, mesoregion-year dummies and a lag of the outcome variable referring to the change between 1996 and 2000, instrumented by its level in 1996 to avoid endogeneity through autocorrelation of residuals. "KP F-stat" refers to the heteroskedasticity-robust Kleibergen-Paap test for weak instruments.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

We discussed earlier that labor shares could be endogenous, but this does not appear to be the

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[27] As we mentioned earlier, XD seems to have little influence from Brazil-specific characteristics, implying that we could use standard OLS instead of 2SLS in these regressions without the "supply of imports" shock.

case of the inclusion of the share of mining workers in the mortality regressions. Considering the characteristics we control for, it is unlikely that municipalities specialized in mining would be less successful in reducing pollution-related infant mortality because of factors other than environmental hazards caused by the mining activity itself. That is, we do not believe that these municipalities share some unobserved factor that could also explain the increase in infant deaths. Supporting this interpretation is that the pattern observed in XD's coefficient in panel A of table 5 does not appear in our regression for mortality in general nor in deaths caused by external factors, as shown in tables 4 and A14, respectively. That is, the effect seems to be specific to causes of death associated to pollution, which is corroborated by our results in table A15, where we consider the broader set of pollution-related illnesses: although the pattern is not as distinct, the signs of the coefficients are the same and statistical significance is preserved at the earlier ages. Furthermore, our estimates change only slightly when we restrict the sample to municipalities where at least 80% of the population in 2010 did not immigrate in the past decade, as shown in table A16. This suggests that the influence of modifications in the composition of municipal populations is small, partially addressing the concern that mortality rates in exporting locations, which received an influx of workers according to Haddad and Maggi (2017), could change simply due to immigrants systematically having different health conditions than the original inhabitants.

The fact that the coefficient is positive at earlier ages and fades as we evaluate older groups strengthens our argument as well: it suggests that the influence on mortality of some channel connected to mining is either growing or diminishing as age increases. Taking into account that, according to WHO (2018), children are affected by air pollution in different and more severe ways that other age groups, since organs such as the brain and lungs are still in development[28], we believe that the influence on mortality by pollution-related illnesses we see changing over age is that of poor environmental quality, which fades as the evaluated outcome becomes less vulnerable to illnesses associated to pollution. Furthermore, the results in table A17 indicate that this impact is indeed of environmental hazards caused by mining activities, since the shocks have no effects on mortality by sanitation-related causes, which usually result from untreated domestic sewage. This is important because, according to WHO/UNICEF (2000), young children are the most vulnerable of all age groups to water pollution, so that if the change in environmental quality had been caused by channels other than hazards from mining activities, such as poor infra-structure associated to unplanned and haphazard urbanization, the estimates for the effect of the shocks on infant deaths by sanitation-related illnesses would have been statistically significant.

Table 5 then suggests that mining municipalities experienced higher infant mortality by pollution-related causes due to the environmental hazards associated with their specialization.

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[28]In fact, environmental hazards are responsible for more than 25% of deaths of children under five years of age.

However, to say that the estimated coefficients in panel A can be interpreted as indirect evidence of China's "demand for exports" influence on environmental quality in Brazil is debatable. In fact, panel B suggests that the relative increase in infant mortality could have occurred regardless of the China shock, since XD's coefficient is statistically null once we hold fixed the variation in mining specialization. That is, any shock variation that is not associated with mining is not relevant to explain changes in mortality, thus raising the possibility that the effect we estimate in panel A is simply that of developing mining activities. The shift-share misallocation problem, which is also present here, supports this argument to an extent: shock variation generated in municipalities that produce iron ores was also assigned to locations specialized in other minerals, and yet the pattern in the results of table 5 can be replicated, although with smaller magnitudes and larger standard errors, even when we exclude the main iron extracting municipalities from our sample, as shown in table A18[29]. Hence, despite discarding the mining locations that were actually exposed to China's "demand for exports" shock, the results for infant mortality caused by pollution-related illnesses still echo those obtained with the full sample. If the distinctions between mining activities and any others that municipalities can specialize in were irrelevant to explain mortality, these estimates would have been insignificant and the effects in panel A of table 5 could be credibly attributed to the China shocks alone.

Although simply specializing in mining activities explains our results, the reduced magnitudes and statistical significance in table A18 may be indicative of a scale effect[30] caused by China's demand for iron ores, since the impact on infant mortality was stronger when municipalities that actually extract iron were included in the sample. Therefore, our estimates support the claim that mining activities led to environmental impacts severe enough to deteriorate local health significantly more on average than in municipalities with other specializations. On the other hand, we cannot determine whether the China shocks exacerbated this effect.

6   Conclusions

In this study, we investigated possible effects of the "China shock", China's advance over international markets between 2000 and 2010, on the environmental quality of Brazilian munic-

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[29]We removed the two Pará municipalities that contain the Carajás mining complex as well as the 25 municipalities in the Minas Gerais (MG) region known as the "Iron Quadrangle", where the state's production of iron ores is concentrated. Pará and Minas Gerais combined accounted for 85.7% of the total value and 90.2% of the total volume exported of iron ores to China between 2000 and 2010.

[30]According to Copeland and Taylor (2004), an usual approach to model the effects of trade and economic growth on the environment is to decompose them in three channels: scale, composition and technique. The "scale effect" would be the change in pollution emissions following a shift of the production possibilities frontier. With regards to the estimated impact on mortality, we could have additionally, or perhaps alternatively, a "composition effect" if the productive factors of these municipalities were redirected to mining from other economic activities.

ipalities, as these local economies turned out to be more or less exposed to China's "demand for exports" and "supply of imports" based on their economic specialization in 2000. We worked under the premise that, if the China shocks did indeed lead to environmental changes in Brazil, we should be able to estimate them by exploring this difference in exposure across municipalities.

We first checked whether China caused any changes on deforestation rates of municipalities in the Amazon rainforest and the Cerrado savanna. Our results did not identify any average differences between locations that were predicted to have been exposed to trade with China in different intensities; on the contrary, they actually highlighted how other factors, such as the presence of forest conservation units or poorly defined property rights, are much more relevant to explain the dynamics of deforestation than the economic incentives given by China's demand for commodities. Our evidence is then aligned with that provided by López and Galinato (2005): the effects of trade on deforestation are small and can go in different directions, suggesting that domestic idiosyncrasies play a larger role. In the case of Brazil specifically, they argue that government incentives for human settlement in the Amazon, which included investments in infrastructure, tax benefits and subsidized credit, were more important to determine deforestation. Faria and Almeida (2016)'s estimates of the impact of openness to trade on the Amazon support this interpretation, as the effects of conservation units and existence of disputes over property rights are significant and exceed those of trade in all of their specifications[31].

We also set out to study other measures of environmental quality since deforestation, in addition to not encompassing the whole country, is not necessarily informative of hazards to water, air and soil which may ensue from toxic residues produced by industrial and agricultural activities. Due to the lack of reliable information on pollution at the level of municipalities, we explored variation in mortality rates to investigate environmental impacts. Our estimates show that municipalities affected by China's "demand for exports" shock experienced, on average, more deaths of children under one and five years of age per 100,000 inhabitants. The increase in mortality was restricted to deaths caused by illnesses related to pollution, and, importantly, occurred in municipalities where mining was a particularly relevant activity to the local economy. This characteristic suggests that our estimates of the environmental effect of the China shocks actually capture the impact of specializing in mining activities rather than that of being exposed to trade with China. However, we did find some evidence that the "demand for exports" shock might have exacerbated this impact in municipalities that extract iron ores.

In summary, the evidence produced in our study does not support that the heterogeneous effects of the China shocks on local labor markets in Brazil were strong enough to translate into changes of environmental quality. Building on the results of Costa et al. (2016), who were

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[31]The disputes over property rights could be seen as an indirect channel through which soybean farming influences deforestation, as argued by Domingues and Bermann (2012).

interested in the "winners" and "losers" of this trade boom, it seems from our estimates that the economic gains of "winning" locations were not enhanced nor offset by changes in local environmental quality, neither were "losers" being compensated or made worse off. Due to limitations of our data and shortcomings of the shift-share strategy, general equilibrium effects may have led to attenuation bias, which could partially explain the lack of significant effects. Still, according to the literature, the effect of trade on the environment can go in conflicting directions, which may amount to a small net impact[32]. Therefore, while a different empirical strategy might be able to produce more robust evidence on environmental impacts of the China shocks, we do not find it surprising that our estimates for these effects should be small, if not insignificant. It is important to note, however, that the results say little about aggregate impacts; that is, they are not informative of environmental effects of the China shocks on Brazil as a whole. Because we focused on the comparison of municipalities, our empirical strategy could only provide insights on relative changes, thus leaving the issue of a countrywide environmental impact unaddressed.

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[32]See Copeland and Taylor (2004).

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A   Additional figures and tables

Table A1: Brazilian imports

	2000		2010	
	World	China	World	China
Mineral fuels, mineral oils, bituminous substances; mineral waxes	8.29	0.07	24.23	0.18
Nuclear reactors, boilers, machinery and mechanical appliances; others	9.03	0.17	23.09	4.55
Electrical machinery and equipment and parts thereof; others	9.13	0.36	18.00	6.47
Vehicles other than railway or tramway rolling-stock, and parts and accessories thereof	3.73	0.01	13.98	0.55
Organic chemicals	3.28	0.14	6.83	1.04
Plastics and articles thereof	1.98	0.02	5.28	0.42
All imports	55.85	1.22	147.05	20.71

Source: ComexStat. Values are in billions of 2000's US dollars free of boarding (FOB).

Table A2: Brazilian exports

	2000		2010	
	World	China	World	China
Ores, slag and ash	3.26	0.27	24.95	11.02
Mineral fuels, mineral oils, bituminous substances; mineral waxes	0.91	0.04	18.94	3.28
Sugars and sugar confectionery	1.29	0.00	10.48	0.42
Vehicles other than railway or tramway rolling-stock, and parts and accessories thereof	4.44	0.01	9.82	0.02
Meat and edible meat offal	1.61	0.01	9.61	0.18
Oil seeds and oleaginous fruits; grains, seeds, others	2.21	0.34	9.04	5.77
All exports	55.12	1.09	163.35	24.91

Source: ComexStat. Values are in billions of 2000's US dollars free of boarding (FOB).

Table A3: Main soybeans exporters among states from the Amazon and Cerrado area

	2000		2010	
	World	China	World	China
Mato Grosso (MT)	0.55	0.04	2.67	1.65
Goiás (GO)	0.18	0.01	0.67	0.48
Bahia (BA)	0.02	0	0.50	0.17
Mato Grosso do Sul (MS)	0.02	0	0.41	0.33
Maranhão (MA)	0.09	0.02	0.33	0.05
Brazil	2.19	0.34	8.93	5.77

Source: ComexStat. Values are in billions of 2000's US dollars free of boarding (FOB).

Table A4: Exports from states belonging to the Legal Amazon region

	2000		2010	
	World	China	World	China
Aluminum and articles thereof	1.01	0	0.95	0
Ores, slag and ash	0.84	0.04	7.99	3.43
Oil seeds and oleaginous fruits; grains, seeds, others	0.65	0.06	3.37	1.75
Wood and articles of wood; wood charcoal	0.49	0.02	0.51	0.04
All exports	5.10	0.14	21.41	5.56

Source: ComexStat. Values are in billions of 2000's US dollars free of boarding (FOB). States included: Acre (AC), Amapá (AP), Amazonas (AM), Maranhão (MA), Mato Grosso (MT), Pará (PA), Rondônia (RO), Roraima (RR) and Tocantins (TO).

Table A5: Difference in deforested area as share of municipality - Amazon rainforest

	(1)	(2)	(3)	(4)	(5)
	2SLS	2SLS	2SLS	2SLS	2SLS
Import shock (IS)	0.218	-0.690	0.134	0.201	1.197
	(4.644)	(4.743)	(4.646)	(4.651)	(4.724)
Export shock (XD)	-0.063**	1.763	0.055	-0.064**	
	(0.031)	(1.525)	(0.209)	(0.032)	
Others exports					1.738
					(1.540)
Observations	738	738	738	738	738
No. of clusters	100	100	100	100	100
Mesoregion trends	Y	Y	Y	Y	Y
Munic. controls	Y	Y	Y	Y	Y
% labor in mining		Y	Y		
% labor in soybeans		Y			
KP F-stat	59.99	9.494	59.51	59.45	12.33

Standard errors (in parentheses) clustered by microregions. Regressions are weighted by the municipality's area in km^2. Municipal-level controls are fixed in 2000 and interacted with year dummies, including a quadratic polynomial of per capita income, share of workers in the primary sector and in non-traded sectors, share of households in urban areas and share of territory with other vegetation or hydrography. Also, we account for visibility issues when evaluating the change in a municipality's deforested area, labeled by PRODES as obstructed by clouds or "unobserved". "KP F-stat" refers to the heteroskedasticity-robust Kleibergen-Paap test for weak instruments. The municipality of Manaus is not included in the sample, as it contains a tax-free economic zone. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A6: Difference in deforested area as share of municipality - Amazon rainforest (without Carajás)

	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	2SLS	2SLS	2SLS	2SLS	2SLS
Import shock (IS)	-4.349	-1.750	0.628	0.678	5.032	4.137
	(6.329)	(6.896)	(4.720)	(5.047)	(4.423)	(4.425)
Export shock (XD)	-0.134***	-0.134***	-0.053**	-0.061**	-1.652	-0.010
	(0.029)	(0.028)	(0.027)	(0.030)	(1.464)	(0.029)
KP F-stat		61.51	60.10	60.57	9.483	63.42
No. of munic	736	736	736	736	736	736
No. of clusters	100	100	100	100	100	100
Mesoregion trends			Y	Y	Y	Y
Munic. controls			Y	Y	Y	Y
% deforested (2000)				Y		
% labor in main exports					Y	
% protected lands						Y

Standard errors (in parentheses) clustered by microregions. Regressions are weighted by the municipality's area in km^2. Municipal-level controls are fixed in 2000 and interacted with year dummies, including a quadratic polynomial of per capita income, share of workers in the primary sector and in non-traded sectors, share of households in urban areas and share of territory with other vegetation or hydrography. Also, we account for visibility issues when evaluating the change in a municipality's deforested area, labeled by PRODES as obstructed by clouds or "unobserved". "KP F-stat" refers to the heteroskedasticity-robust Kleibergen-Paap test for weak instruments. The municipality of Manaus is not included in the sample, as it contains a tax-free economic zone, neither are Parauapebas and Canaã dos Carajás, which contain the Carajás mining complex.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figure A1: Evolution of selected mining exports

— Share of iron ores in total mining exports to China
— Share of iron ores in total value exported to China
-·-·- China's share as destination of the total exported value of iron ores
-·-·- China's share as destination of the total exported value of manganese ores
-·-·- China's share as destination of the total exported value of aluminum ores
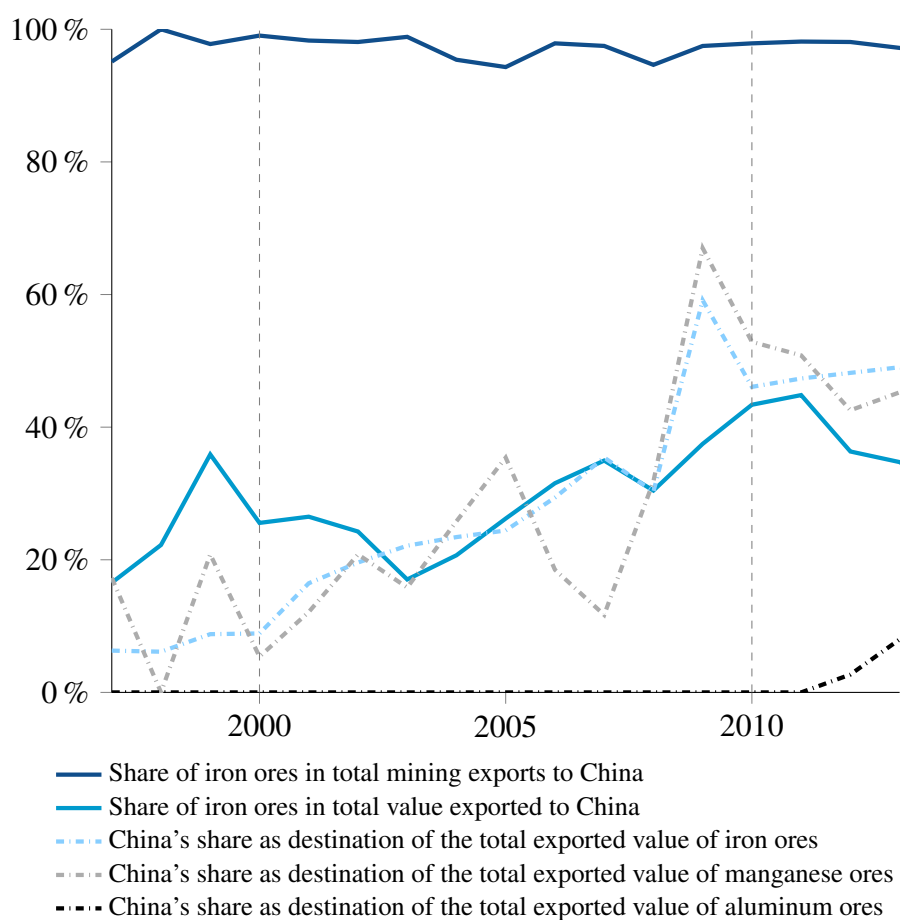
Source: ComexStat.

Table A7: Descriptive statistics - difference in number of deaths per 100.000 inhabitants

	Mean	Std. dev.	Min	1st q.	Median	3rd q.	Max
Aged < 1	-15.1	18.3	-229	-21	-11.8	-7.36	119
Aged < 5	-18.2	21.3	-234	-24.1	-14.2	-8.34	150
Aged 5-9	-0.973	4.12	-67.8	-2.02	-0.832	0	49.7
Aged 10-19	-1.78	9.72	-122	-6.97	-1.74	3.15	128
Aged 20-39	-5.35	27.6	-180	-22.8	-6.61	12.5	214
All ages	45.6	97.9	-1,071.6	-14.3	36.8	88	836

Statistics weighted by the municipality's population in each age group.

Table A8: Pollution-related illnesses

ICD-10 code	Description
B15	Acute hepatitis A
B172	Acute hepatitis E
D74	Methemoglobinemia
J00-J06	Acute upper respiratory infections
J12-J18	Pneumonia
J20-J22	Acute bronchitis, bronchiolitis; others
J45-J46	Asthma
K710, K712	Acute toxic hepatitis
K850	Acute pancreatitis
Q	Congenial malformation
T573, T650-T651	Intoxication by inorganic substances
T60	Toxic effects of exposure to pesticides
Z581	Exposure to polluted water

ICD-10 codes compiled based on Prüss-Ustün et al. (2011) and Alwahaibi and Zeka (2016).

Table A9: Descriptive statistics - difference in number of deaths per 100.000 inhabitants - Pollution-related illnesses

	Mean	Std. dev.	Min	1st q.	Median	3rd q.	Max
Aged < 1	-0.166	3.85	-81	-1.33	-0.292	.6	56.6
Aged < 5	-0.213	4.14	-81	-1.65	-0.463	.801	56.6
Aged 5-9	.005	.803	-34.5	0	0	0	44.5
Aged 10-19	.017	.866	-34.9	0	0	0	34.2
Aged 20-39	.045	.89	-52.1	-0.019	0	.029	28.4
All ages	-0.168	6.4	-91.2	-2.29	-0.28	1.51	113

Statistics weighted by the municipality's population in each age group.

Table A10: Descriptive statistics - difference in number of deaths per 100.000 inhabitants - Pollution-related illnesses (broader group)

	Mean	Std. dev.	Min	1st q.	Median	3rd q.	Max
Aged < 1	-0.171	3.87	-81	-1.34	-0.244	.709	56.6
Aged < 5	-0.171	3.92	-81	-1.34	-0.244	.709	56.6
Aged 5-9	0.002	.843	-34.5	0	0	0	44.5
Aged 10-19	0.005	1.03	-34.9	-0.003	0	.011	34.2
Aged 20-39	.024	1.91	-65.7	-0.287	0	.305	43
All ages	10.2	15.9	-169	.419	7.07	16.4	187

Statistics weighted by the municipality's population in each age group.

Table A11: Descriptive statistics - difference in number of deaths per 100.000 inhabitants - Sanitation-related illnesses

	Mean	Std. dev.	Min	1st q.	Median	3rd q.	Max
Aged < 1	-1.53	3.67	-66.4	-1.62	-0.557	0	46.9
Aged < 5	-1.81	4.26	-75.9	-2.1	-0.709	0	46.9
Aged 5-9	.016	.599	-15.5	0	0	0	22.2
Aged 10-19	-0.013	.773	-36.7	0	0	0	38.7
Aged 20-39	-0.122	1.52	-44.5	-0.258	0	0	42.7
All ages	-2.68	9.83	-161	-4.44	-1.65	.572	105

Statistics weighted by the municipality's population in each age group.

Table A12: Descriptive statistics - difference in number of deaths per 100.000 inhabitants - External causes

	Mean	Std. dev.	Min	1st q.	Median	3rd q.	Max
Aged < 1	-0.184	1.6	-51.4	-0.241	0	0	45.1
Aged < 5	-0.648	3.09	-58.9	-1.45	-0.415	0	49.5
Aged 5-9	-0.485	2.51	-56.8	-0.859	-0.245	0	49.7
Aged 10-19	-0.861	8.37	-87	-5.54	-0.409	3.67	128
Aged 20-39	.572	21.7	-137	-13.8	1.09	13.7	154
All ages	5.89	34.5	-273	-17.4	6.78	28.1	203

Statistics weighted by the municipality's population in each age group.

Table A13: Difference in number of deaths per 100,000 inhabitants - Pollution-related illnesses

	(1)	(2)	(3)	(4)	(5)	(6)
	Aged < 1	Aged < 5	Aged 5-9	Aged 10-19	Aged 20-39	All ages
Panel A						
Export shock (XD)	0.073***	0.068**	0.008*	-0.008**	-0.001	0.077**
	(0.027)	(0.028)	(0.005)	(0.003)	(0.004)	(0.035)
KP F-stat	1430.2	1333.8	6550.3	3932.8	978.5	1352.0
Panel B: controlling for % of workers in mining and soybeans						
Export shock (XD)	0.151	0.105	0.012	-0.027	-0.040	0.022
	(0.131)	(0.161)	(0.023)	(0.044)	(0.038)	(0.237)
KP F-stat	1424.7	1289.4	6164.8	2592.9	978.1	1319.4
No. of munic.	4264	4264	4264	4264	4264	4264
No. of clusters	552	552	552	552	552	552

Coefficients from 2SLS models, with standard errors (in parentheses) clustered by microregions. Regressions are weighted by the municipality's population of each age range in 2000. All specifications include the following municipal-level controls, fixed in 2000 and interacted with year dummies: a quadratic polynomial of per capita income, share of workers in the primary sector and in non-traded sectors, share of households in urban areas, with access to a water supply system and sewerage services. In addition, mesoregion-year dummies and a lag of the outcome variable referring to the change between 1996 and 2000, instrumented by its level in 1996 to avoid endogeneity through autocorrelation of residuals. "KP F-stat" refers to the heteroskedasticity-robust Kleibergen-Paap test for weak instruments.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A14: Difference in number of deaths per 100,000 inhabitants - External causes

	(1)	(2)	(3)	(4)	(5)	(6)
	Aged < 1	Aged < 5	Aged 5-9	Aged 10-19	Aged 20-39	All ages
Panel A						
Import shock (IS)	-0.185	-0.352	-0.079	-1.809**	-3.146	-6.079**
	(0.193)	(0.311)	(0.156)	(0.906)	(1.934)	(2.750)
Export shock (XD)	-0.003	-0.046	-0.007	0.016	-0.294***	-0.411**
	(0.013)	(0.029)	(0.023)	(0.045)	(0.113)	(0.194)
KP F-stat	6.823	6.906	6.509	7.253	9.196	7.567
Panel B: controlling for % of workers in mining and soybeans						
Import shock (IS)	0.011	-0.238	-0.093	-2.053**	-3.841*	-6.611**
	(0.197)	(0.275)	(0.166)	(1.001)	(2.162)	(3.265)
Export shock (XD)	-0.087	-0.009	0.027	0.507*	-1.372***	-0.767
	(0.084)	(0.121)	(0.069)	(0.265)	(0.312)	(0.618)
KP F-stat	66.85	69.96	69.41	95.51	106.3	73.62
No. of munic.	4264	4264	4264	4264	4264	4264
No. of clusters	552	552	552	552	552	552

Coefficients from 2SLS models, with standard errors (in parentheses) clustered by microregions. Regressions are weighted by the municipality's population of each age range in 2000. All specifications include the following municipal-level controls, fixed in 2000 and interacted with year dummies: a quadratic polynomial of per capita income, share of workers in the primary sector and in non-traded sectors, share of households in urban areas, with access to a water supply system and sewerage services. In addition, mesoregion-year dummies and a lag of the outcome variable referring to the change between 1996 and 2000, instrumented by its level in 1996 to avoid endogeneity through autocorrelation of residuals. "KP F-stat" refers to the heteroskedasticity-robust Kleibergen-Paap test for weak instruments.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A15: Difference in number of deaths per 100,000 inhabitants - Pollution-related illnesses (broader group)

	(1)	(2)	(3)	(4)	(5)	(6)
	Aged < 1	Aged < 5	Aged 5-9	Aged 10-19	Aged 20-39	All ages
Panel A						
Import shock (IS)	-0.213	-0.219	0.019	-0.038	-0.313	-0.186
	(0.375)	(0.380)	(0.053)	(0.084)	(0.212)	(1.372)
Export shock (XD)	0.076***	0.073***	0.006	-0.004	-0.022	0.060
	(0.027)	(0.025)	(0.006)	(0.009)	(0.018)	(0.067)
KP F-stat	6.983	6.998	6.520	6.819	8.464	7.481
Panel B: controlling for % of workers in mining and soybeans						
Import shock (IS)	-0.167	-0.174	0.022	-0.044	-0.136	0.606
	(0.308)	(0.308)	(0.051)	(0.079)	(0.211)	(1.235)
Export shock (XD)	0.124	0.125	-0.012	-0.020	-0.168***	-0.735
	(0.080)	(0.081)	(0.034)	(0.025)	(0.040)	(0.460)
KP F-stat	87.45	76.87	4627.2	69.08	114.7	70.84
No. of munic.	4264	4264	4264	4264	4264	4264
No. of clusters	552	552	552	552	552	552

Coefficients from 2SLS models, with standard errors (in parentheses) clustered by microregions. Regressions are weighted by the municipality's population of each age range in 2000. All specifications include the following municipal-level controls, fixed in 2000 and interacted with year dummies: a quadratic polynomial of per capita income, share of workers in the primary sector and in non-traded sectors, share of households in urban areas, with access to a water supply system and sewerage services. In addition, mesoregion-year dummies and a lag of the outcome variable referring to the change between 1996 and 2000, instrumented by its level in 1996 to avoid endogeneity through autocorrelation of residuals. "KP F-stat" refers to the heteroskedasticity-robust Kleibergen-Paap test for weak instruments.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A16: Difference in number of deaths per 100,000 inhabitants - pollution-related illnesses (restricted migration)

	(1)	(2)	(3)	(4)	(5)	(6)
	Aged < 1	Aged < 5	Aged 5-9	Aged 10-19	Aged 20-39	All ages
Panel A						
Import shock (IS)	-0.386	-0.484	-0.035	-0.022	-0.062	-0.396
	(0.483)	(0.502)	(0.065)	(0.114)	(0.106)	(0.937)
Export shock (XD)	0.079**	0.080**	0.005*	-0.010**	-0.003	0.087**
	(0.032)	(0.033)	(0.003)	(0.004)	(0.004)	(0.041)
KP F-stat	3.959	3.952	3.869	3.943	4.602	4.233
Panel B: controlling for % of workers in mining and soybeans						
Import shock (IS)	-0.425	-0.521	-0.038	-0.017	-0.068	-0.423
	(0.509)	(0.532)	(0.068)	(0.117)	(0.112)	(1.010)
Export shock (XD)	0.546	0.527	0.041	-0.052	0.069	0.359
	(0.558)	(0.595)	(0.070)	(0.129)	(0.127)	(1.034)
KP F-stat	3.734	3.784	3.500	3.497	3.874	3.745
No. of munic.	3387	3387	3387	3387	3387	3387
No. of clusters	523	523	523	523	523	523

Coefficients from 2SLS models, with standard errors (in parentheses) clustered by microregions. Regressions are weighted by the municipality's population of each age range in 2000. We restrict the sample to municipalities where at least 80% of the population did not immigrate in the previous decade. All specifications include the following municipal-level controls, fixed in 2000 and interacted with year dummies: a quadratic polynomial of per capita income, share of workers in the primary sector and in non-traded sectors, share of households in urban areas, with access to a water supply system and sewerage services. In addition, mesoregion-year dummies and a lag of the outcome variable referring to the change between 1996 and 2000, instrumented by its level in 1996 to avoid endogeneity through autocorrelation of residuals. "KP F-stat" refers to the heteroskedasticity-robust Kleibergen-Paap test for weak instruments.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A17: Difference in number of deaths per 100,000 inhabitants - Sanitation-related ill-nesses

	(1)	(2)	(3)	(4)	(5)	(6)
	Aged < 1	Aged < 5	Aged 5-9	Aged 10-19	Aged 20-39	All ages
Panel A						
Import shock (IS)	-0.313	-0.229	0.010	-0.038	-0.122	-0.427
	(0.412)	(0.462)	(0.039)	(0.026)	(0.146)	(0.933)
Export shock (XD)	-0.011	-0.014	-0.001	-0.007	-0.013	-0.084
	(0.017)	(0.024)	(0.002)	(0.005)	(0.011)	(0.081)
KP F-stat	6.833	6.940	6.508	7.086	8.439	7.497
Panel B: controlling for % of workers in mining and soybeans						
Import shock (IS)	-0.239	-0.193	0.021	-0.046	-0.130	-0.043
	(0.357)	(0.387)	(0.030)	(0.028)	(0.122)	(0.762)
Export shock (XD)	-0.093	-0.052	-0.019	-0.056*	0.043	-0.060
	(0.116)	(0.106)	(0.020)	(0.029)	(0.041)	(0.399)
KP F-stat	67.14	68.73	69.62	93.07	69.22	70.34
No. of munic.	4264	4264	4264	4264	4264	4264
No. of clusters	552	552	552	552	552	552

Coefficients from 2SLS models, with standard errors (in parentheses) clustered by microregions. Regressions are weighted by the municipality's population of each age range in 2000. All specifications include the following municipal-level controls, fixed in 2000 and interacted with year dummies: a quadratic polynomial of per capita income, share of workers in the primary sector and in non-traded sectors, share of households in urban areas, with access to a water supply system and sewerage services. In addition, mesoregion-year dummies and a lag of the outcome variable referring to the change between 1996 and 2000, instrumented by its level in 1996 to avoid endogeneity through autocorrelation of residuals. "KP F-stat" refers to the heteroskedasticity-robust Kleibergen-Paap test for weak instruments.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A18: Difference in number of deaths per 100,000 inhabitants - Pollution-related illnesses (without main iron ores producers)

	(1)	(2)	(3)	(4)	(5)	(6)
	Aged < 1	Aged < 5	Aged 5-9	Aged 10-19	Aged 20-39	All ages
Panel A						
Import shock (IS)	-0.206	-0.239	0.023	-0.037	-0.063	-0.084
	(0.372)	(0.394)	(0.049)	(0.078)	(0.079)	(0.708)
Export shock (XD)	0.056*	0.048	0.009	-0.007**	0.002	0.056
	(0.032)	(0.030)	(0.006)	(0.003)	(0.003)	(0.037)
KP F-stat	6.933	6.930	6.471	6.741	8.301	7.445
Panel B: controlling for % of workers in mining and soybeans						
Import shock (IS)	-0.217	-0.248	0.023	-0.036	-0.062	-0.086
	(0.378)	(0.401)	(0.050)	(0.078)	(0.079)	(0.720)
Export shock (XD)	0.220	0.187	0.005	-0.013	-0.016	0.064
	(0.164)	(0.180)	(0.024)	(0.039)	(0.042)	(0.288)
KP F-stat	6.015	6.049	5.734	5.935	6.801	6.276
No. of munic.	4239	4239	4239	4239	4239	4239
No. of clusters	552	552	552	552	552	552

Coefficients from 2SLS models, with standard errors (in parentheses) clustered by microregions. Regressions are weighted by the municipality's population of each age range in 2000. All specifications include the following municipal-level controls, fixed in 2000 and interacted with year dummies: a quadratic polynomial of per capita income, share of workers in the primary sector and in non-traded sectors, share of households in urban areas, with access to a water supply system and sewerage services. In addition, mesoregion-year dummies and a lag of the outcome variable referring to the change between 1996 and 2000, instrumented by its level in 1996 to avoid endogeneity through autocorrelation of residuals. "KP F-stat" refers to the heteroskedasticity-robust Kleibergen-Paap test for weak instruments. We exclude from our sample the two municipalities that contain the Carajás mining complex and the 25 that belong to the "Iron Quadrangle" (some are grouped together for intertemporal consistency between 1996 and 2000).

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

B   Identification hypotheses of the shift-share IV

As mentioned in section 3, Baum-Snow and Ferreira (2015) and Bombardini and Li (2018) argue that consistent estimation of a linear model using shift-share variables requires the regional shares used as weights to be exogenous. Accordingly, Goldsmith-Pinkham et al. (2018) show that the shift-share IV estimate is equivalent to an over-identified GMM estimator, where the set of shares serve as the actual instruments and the shocks are used only to build the weighting matrix. Therefore, parameter identification would depend solely on the exclusion restriction $Cov(s_{i,j}, \Delta\varepsilon_i | \boldsymbol{W}_i) = 0$, with the shocks pertaining only to the relevance of the instruments. Moreover, using this GMM equivalence, they demonstrate that the shift-share IV estimator is a weighted average of industry-specific just-identified estimators. The weights associated with each estimator are given the interpretation of "sensitivity to misspecification" elasticities: the expected percentage change in bias of the GMM estimator resulting from a percentage increase in bias of an industry's just-identified estimator caused by a misspecification in its moment condition. These weights are suggested to be an useful tool for applied research, as they draw attention to the sectors where endogeneity might be more harmful to the estimate of the overall effect.

Under Goldsmith-Pinkham et al. (2018)'s framework, we clearly could not proceed using baseline-year labor shares in (7) because the exposure to trade measures *XD* and *IS* are already built from them; that is, we would be "instrumenting" baseline shares with themselves, hence not instrumenting at all. Besides, it becomes unclear why one would bother finding a set of shifters such as China's trade shocks when feasible versions of efficient GMM estimates can be obtained; in fact, some might argue that proceeding by the latter would be a better option as it does not impose arbitrary weights on the empirical moments.

Fortunately, instead of treating labor shares $s_{i,j}$ themselves as the set of instruments and, thus, having to abide to their exclusion restriction using lagged labor shares, Borusyak et al. (2018) show that we can allow for endogenous weighting in (7) and still estimate our model consistently. They demonstrate that it is the industry-level shocks who serve as instruments, meaning that we have to ensure their exogeneity, not of the industry-location labor shares. This is convenient, since our predicted Brazil-China trade flows in (6) are much more likely to satisfy the orthogonality condition than baseline-year labor shares $s_{i,j}$. Considering that these are relatively new advances in the literature and not yet common practice, we present Borusyak et al. (2018)'s argument below to lend credibility to our own use of this framework.

Suppose that we wish to estimate the following equation, similar to (1):

$$\Delta y_i = \beta \Delta x_i + \boldsymbol{W}_i' \boldsymbol{\gamma} + \Delta\varepsilon_i, \tag{B1}$$

where i indexes "locations", $\boldsymbol{W}_i$ is a vector of covariates such that $Cov(\boldsymbol{W}_i, \Delta\varepsilon_i) = 0$ and x_i is a treatment variable that could be continuous, like our exposure to trade measures, or discrete.

We have no reason to believe that $Cov(x_i, \Delta\varepsilon_i|\boldsymbol{W}_i) = 0$ holds, so we make use of an instrumental variable. Consider a set of shocks g_j specific to "industries" (indexed by j) and a set of weights $s_{i,j} \geq 0$ denoting the expected share of each shock j a location i would receive, with $\sum_j s_{i,j} = 1$. The shift-share IV would then be:

$$z_i = \sum_j s_{i,j} \cdot g_j. \tag{B2}$$

Using $\boldsymbol{W}_i$'s annihilator matrix, we compute residualized $\Delta y_i^{\perp}$ and $\Delta x_i^{\perp}$ and obtain the indirect least squares estimator for β:

$$\hat{\beta} = \frac{\sum_i z_i \cdot \Delta y_i^{\perp}}{\sum_i z_i \cdot \Delta x_i^{\perp}}. \tag{B3}$$

Notice that the shift-share IV estimator is calculated with instrument z_i defined at the location level. $\hat{\beta}$'s consistency and causal interpretation, then, rely on a relevance condition establishing $Cov(z_i, \Delta x_i^{\perp}) \neq 0$ and on a exclusion restriction requiring $Cov(z_i, \Delta\varepsilon_i^{\perp}) = 0$. We can however express the latter as an industry-level rather than location-level condition to highlight how identification of β depends on the shocks g_j:

$$
\begin{aligned}
Cov(z_i, \Delta\varepsilon_i^{\perp}) &= \mathbf{E}_i[z_i \cdot \Delta\varepsilon_i^{\perp}] \\
&= \mathbf{E}_i[(\sum_j s_{i,j} \cdot g_j) \cdot \Delta\varepsilon_i^{\perp}] \cdot (\mathbf{E}_i(s_{i,j})/\mathbf{E}_i(s_{i,j})) \\
&= \sum_j s_j \cdot g_j \cdot \phi_j,
\end{aligned} \tag{B4}
$$

where $s_j \equiv \mathbf{E}_i(s_{i,j})$ is the average share of industry j among locations and $\phi_j \equiv \mathbf{E}_i(s_{i,j} \cdot \Delta\varepsilon_i^{\perp})/\mathbf{E}_i(s_{i,j})$ is the average value of outcome $\Delta y_i^{\perp}$ in the absence of treatment $\Delta x_i^{\perp}$[1], with larger weights being assigned to locations most exposed to j. Therefore, the exclusion restriction for consistency of $\hat{\beta}$ in (B1) can be equivalently stated in industry terms as requiring that:

$$\sum_j s_j \cdot g_j \cdot \phi_j \to 0 \tag{B5}$$

as the total number of locations $I \to \infty$. This condition is trivially satisfied by the exogeneity of labor shares, since then $\phi_j = \mathbf{E}_i(s_{i,j} \cdot \Delta\varepsilon_i^{\perp})/s_j = 0$ for every j. However, as we discuss below, (B5) can be achieved through other assumptions, meaning that Goldsmith-Pinkham et al. (2018)'s argument for exogenous shares actually provides a sufficient but not necessary condition to establish orthogonality of the shift-share IV.

According to Borusyak et al. (2018), two assumptions need to be valid for the exclusion restriction in (B5) to hold:

H1) "quasi-random shock assignment": $\mathbf{E}[g_j|\phi_j] = \mu$, for all j and some $\mu \in \mathbb{R}$;

H2) "many independent shocks": $\mathbf{E}[(g_j - \mu) \cdot (g_k - \mu)|\phi_j, \phi_k] = 0$, for all j and $k \neq j$, and $\sum_j s_j^2 \to 0$ as $I \to \infty$.

---

[1] For an "untreated" observation, we would have $\Delta y_i^{\perp} = \Delta\varepsilon_i^{\perp}$; i. e., the residualized untreated outcome is determined by the unobserved idiosyncratic factors.

Hypothesis H1 states that no industry can be expected to experience a different shock than the others given how it is distributed across locations. Because ϕ_j is a weighted average of location-specific unobserved factors, this condition essentially requires locations particularly specialized in industry j to be unable to influence the expected magnitude of shock g_j for the whole country. In our study, this is likely addressed by the predicted trade flows in equation (6), since they are proxies for the China-specific components of Brazilian exports and imports, as illustrated in (4). That is, if the trade shocks we predict are truly uncorrelated with any Brazil-specific characteristics, then it is reasonable to assume that $\mathbf{E}[g_j|\phi_j] = \mathbf{E}[g_j] = \mu$, for every industry j[2].

Recall that we assumed in equation (B2) that the labor shares add up to 1 for every location. This is not the case in our study because we consider only traded activities, which are the ones directly exposed to the China shocks. If we were to also work with labor shares for non-traded sectors, we would have $g_{\text{non-traded}} = 0$, implying that hypothesis H1 would only be satisfied if $\mathbf{E}[g_j|\phi_j] = 0$ for all j. We do not have to impose this, however, because Borusyak et al. (2018) show that this issue is accounted for as long as we control for the share of location i's workers that were initially employed in non-traded activities.

Hypothesis H2 requires shocks to be uncorrelated conditional on their respective industry-weighted averages of local unobserved factors. This would be violated in cases where one sector provided some input to another or if they produced complementary goods. Then, a shock affecting either of them would likely trigger a disturbance on the other as well. Conveniently, Borusyak et al. (2018) argue that H2 can be made less restrictive by allowing correlation within clusters of industries, but not between them. This weaker version is the natural choice for our study, since in converting from international trade codes to the activities listed in the Brazilian Census, our main source for labor data, we had to considerably aggregate sectors.

Additionally, hypothesis H2 states that the Herfindahl index of average exposure to industries must fall to zero asymptotically, meaning that we cannot have many locations specializing in a small set of industries. Note that this condition does not require every location to have a uniform distribution of workers across all sectors. In fact, locations are allowed to have workers specialized in few sectors as long as these are not always the same few; in other words, there needs to be sufficient variation in specialization across regions, but not within them. In our sample with 5500 municipalities and 82 traded activities, the Herfindahl index is 0.0243, suggesting that municipal workforce in 2000 was, on average, sufficiently dispersed across industries.

We must draw attention to a important caveat, however. Since identification hypotheses H1 and H2 deal with stochastic elements and unobserved factors, they cannot be verified em-

---

[2]It is for this reason that we argued earlier that abstracting from the scale factor $L_{B,j}$ in (3) was a harmless simplification. If the predicted trade flows still carried influence from Brazil-specific factors, then scaling the shock by $L_{B,j}$ could cause $\mathbf{E}[\Delta F_{BC,j}/L_{B,j}|\phi_j] \neq \mu/L_{B,j}$, violating H1.

pirically. Even though Borusyak et al. (2018) do suggest procedures to aid in assessing their validity, our data limitations do not allow us to reliably implement them. To test H2, for instance, they use a hierarchical model to decompose the variance of shocks g_j in random effects generated by different clusters of sectors. This is a data-demanding procedure that might not be informative given our small number of industries: in their application, they reproduce Autor et al. (2013)'s study in which 397 manufacturing sectors are tracked for two decades, while ours has 82 traded sectors for one decade only. Hence, we must assume that our claims about the exogeneity of the instrumented shocks, along with the industry aggregation implemented when treating our data, suffice for the orthogonality of the shift-share IV estimator to hold.

C   Alternative standard errors

Adão et al. (2018) propose the following estimator for standard errors in shift-share models:

$$AKM(\hat{\beta}) = \frac{\sqrt{\sum_j \hat{h}_j^2 \cdot \hat{R}_j^2}}{|\sum_i \hat{r}_i \cdot \Delta y_i|}, \quad \text{with} \quad \hat{R}_j = \sum_i s_{i,j} \cdot \hat{\Delta \varepsilon}_i, \tag{C1}$$

where $\hat{\Delta \varepsilon}_i$ are the second stage residuals from model (1), $\hat{r}_i$ are the residuals of regressing an instrument such as those in equation (7) on $\boldsymbol{W}_i$, and $\hat{h}_j$ are the coefficients of regressing $\hat{r}_i$ on the set of shares $s_{i,j}$. They also present a modified version of this estimator, where $\hat{\Delta \varepsilon}_i$ is obtained from a model estimated without the variables of interest; i. e., restricted to the hypothesis that they have no effect on Δy_i. The reasoning behind this is that, similarly to inference with geographic clusters, the standard errors estimated with (C1) might still be too small when there are few clusters. So, if the variables of interest are relevant to explain Δy_i, the restricted standard errors will be higher due to larger residuals.

In our study, estimating these standard errors requires some thought on what values to input in the formula above, because we are interested in two shift-share variables (*XD* and *IS*) instead of one, as in Adão et al. (2018). We compute $\hat{r}_i$ for each of China's "demand for exports" and "supply of imports" shocks by estimating:

$$\begin{aligned} ivIS_i &= \boldsymbol{W}_i' \boldsymbol{\delta}^{IS} + \psi^{IS} \cdot ivXD_i + r_i^{IS} \\ ivXD_i &= \boldsymbol{W}_i' \boldsymbol{\delta}^{XD} + \psi^{XD} \cdot ivIS_i + r_i^{XD}. \end{aligned} \tag{C2}$$

That is, instead of regressing an instrument solely on $\boldsymbol{W}_i$, we control for the other instrument as well. As a check, we also fitted the instruments on $\boldsymbol{W}_i$ alone, thus following Adão et al. (2018) to the letter, which caused the estimated standard errors to be slightly smaller than the values we present below.

We applied both of their standard error estimators to our main results and now discuss how they differ from the cluster-robust standard errors we used throughout our study. For deforestation of the Amazon, in table C1, the new standard errors are indeed higher than most

of those in table 2. In fact, "AKM-0", the standard error estimate obtained under the hypothesis that the shocks are irrelevant, removes all statistical significance of XD's coefficient in the model without the inclusion of protected areas or the share of workers in commodity sectors as controls. For deforestation of the Cerrado, however, "AKM" is actually much smaller than the cluster-robust standard error, making XD's effect negative and highly significant in both models 3 and 6, as shown in table C2. In an analogy to what can occur inside geographical clusters, this result possibly reveals a negative correlation between municipalities through the activities they specialize in.

Oddly, the same happens in table C3, where we consider mortality by the first group of pollution-related illnesses. The "AKM" standard errors in panel A prove to be smaller and reinforce the pattern we discussed above, with positive effects on earlier ages which disappear in older groups. Even in panel B, where we control for the share of workers in mining and soybean farming, they make point estimates significant for children under one and five years of age. "AKM-0", on the other hand, seems to provide results closer to Adão et al. (2018)'s premise, despite also being smaller than the cluster-robust estimates in some cases. In face of these unexpected outcomes, we take a conservative stance and opt to keep our results with the standard errors robust only to correlation within geographic clusters.

Table C1: Standard error comparison - deforestation of the Amazon rainforest

	(3)	(5)	(6)
	2SLS	2SLS	2SLS
Import shock (IS)	0.218	-0.690	2.614
Microregional s. e.	(4.644)	(4.743)	(3.819)
AKM s. e.	(4.947)	(5.338)	(4.664)
AKM-0 s. e.	(4.853)	(5.071)	(5.221)
Export shock (XD)	-0.063	1.763	-0.018
Microregional s. e.	(0.031)**	(1.525)	(0.032)
AKM s. e.	(0.033)*	(1.167)	(0.034)
AKM-0 s. e.	(0.057)	(1.53)	(0.036)
No. of munic.	738	738	738
No. of clusters	100	100	100
Mesoregion trends	Y	Y	Y
County controls	Y	Y	Y
% labor in main exports		Y	
% protected lands			Y

Point estimates and microregional s. e. are those of columns 3, 5 and 6 of table 2. "AKM" and "AKM-0" are obtained with Adão et al. (2018)'s estimator, given by equation (C1).

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table C2: Standard error comparison - deforestation of the Cerrado savanna

	(3)	(5)	(6)
	2SLS	2SLS	2SLS
Import shock (IS)	0.258	0.299	-0.360
Microregional s. e.	(3.675)	(4.654)	(3.634)
AKM s. e.	(2.735)	(3.624)	(2.717)
AKM-0 s. e.	(3.128)	(3.702)	(3.209)
Export shock (XD)	-0.081	-0.092	-0.107
Microregional s. e.	(0.081)	(3.548)	(0.083)
AKM s. e.	(0.019)***	(0.689)	(0.013)***
AKM-0 s. e.	(0.082)	(0.647)	(0.099)
No. of counties	1364	1364	1364
No. of clusters	169	169	169
Mesoregion trends	Y	Y	Y
Munic. controls	Y	Y	Y
% labor in main exports		Y	
% protected lands			Y

Point estimates and microregional s. e. are those of columns 3, 5 and 6 of table 3. "AKM" and "AKM-0" are obtained with Adão et al. (2018)'s estimator, given by equation (C1).

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table C3: Standard error comparison - Mortality due to pollution-related illnesses

	(1)	(2)	(3)	(4)	(5)	(6)
	Aged < 1	Aged < 5	Aged 5-9	Aged 10-19	Aged 20-39	All ages
Panel A						
Import shock (IS)	-0.193	-0.228	0.022	-0.036	-0.067	-0.088
Microregional s. e.	(0.369)	(0.391)	(0.049)	(0.077)	(0.078)	(0.702)
AKM s. e.	(0.241)	(0.249)	(0.046)	(0.053)	(0.073)	(0.454)
AKM-0 s. e.	(0.24)	(0.249)	(0.04)	(0.044)	(0.059)	(0.436)
Export shock (XD)	0.074	0.069	0.008	-0.008	-0.001	0.077
Microregional s. e.	(0.027)***	(0.028)**	(0.005)*	(0.004)**	(0.004)	(0.035)**
AKM s. e.	(0.004)***	(0.003)***	(0.001)***	(0.001)***	(0.001)	(0.005)***
AKM-0 s. e.	(0.065)	(0.061)	(0.007)	(0.006)	(0.001)	(0.069)
Panel B: controlling for % of workers in mining and soybeans						
Import shock (IS)	-0.202	-0.235	0.022	-0.035	-0.066	-0.087
Microregional s. e.	(0.375)	(0.397)	(0.049)	(0.078)	(0.078)	(0.713)
AKM s. e.	(0.275)	(0.283)	(0.052)	(0.061)	(0.085)	(0.515)
AKM-0 s. e.	(0.275)	(0.285)	(0.045)	(0.05)	(0.068)	(0.496)
Export shock (XD)	0.216	0.180	0.005	-0.016	-0.019	0.049
Microregional s. e.	(0.162)	(0.179)	(0.024)	(0.039)	(0.041)	(0.287)
AKM s. e.	(0.096)**	(0.080)**	(0.019)	(0.024)	(0.021)	(0.133)
AKM-0 s. e.	(0.123)*	(0.113)	(0.018)	(0.026)	(0.024)	(0.145)

Point estimates and microregional s. e. are those of table 5. "AKM" and "AKM-0" are obtained with Adão et al. (2018)'s estimator, given by equation (C1).

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$