Impact of US-China Trade War on Deforestation in Brazil

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Abstract

This paper examines the impact of the US-China trade war on tropical deforestation in Brazil. I present a conceptual framework that suggests that if the trade war did have an effect on deforestation, then deforestation rates should have increased more in municipalities with greater prior exposure to Chinese soy trade. Motivated by this, I use data on deforestation rates and the soybean supply chains and a difference-in-differences approach to measure the effect of the trade war on deforestation. I find that, at least in the short run, the trade war did not increase deforestation. I suggest that this finding is due to the short-lived nature of the trade war.

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1 Introduction

Starting in early 2018, the US and China engaged in a series of retaliatory tariff increases that led to a reconfiguration of global trade patterns. US soybean exports to China, which precipitously dropped following the increase in Chinese tariffs, were among the biggest casualties of this “trade war”. This slack was picked by an increase in soybean exports from Brazil to China. Over the last two decades, Brazil’s soybean boom has come at the cost of displacing tropical rainforests and grasslands by soy farms. This has led some observers to worry that the trade war-led jump in soybean exports might cause additional deforestation in the Amazon and Cerrado (Andreoni, 2018; Sullivan, 2018). In this paper, I ask whether this was indeed the case and find that, at least in short-run, the trade war did not increase tropical deforestation in Brazil.

The main hurdle in answering this question is to disentangle the effect of the trade war from other coincidental aggregate shocks. In fact, Brazil witnessed a decline in aggregate deforestation in the Amazon[1] in first few months following this shock. Therefore, I use cross-sectional variation in exposure to Chinese trade to identify the effect of the trade war.

I begin with a general conceptual framework that outlines the mechanisms through which the trade war can affect deforestation. I define a municipality’s exposure to Chinese trade as the weighted sum of market shares of soy exporters operating in that municipality. The weight for an exporter is given by that exporter’s share in Chinese soy imports. Under my framework, if certain conditions hold, I argue that if the trade war did have an effect on deforestation, then municipalities with greater exposure to Chinese soy trade must have experienced a greater increase in deforestation rates relative to muncipalities with lower exposure. The condition needed for this claim is that supply chains are not able to instantaneously adjust to external shocks such as the trade war. I argue that this is indeed a valid assumption in the context of Brazilian soy trade.

Next, I assume that no other concurrent aggregate shock operates through exposure. An implication of this assumption is that in the absence of the trade war (and municipality-specific time trends) municipalities across the exposure spectrum would have followed the same trend in deforestation.

This assumption along with the conceptual framework and sticky supply chains make my setup similar to a difference-in-differences research design with exposure to Chinese soy trade representing treatment intensity. The parallel trends assumption needed to identify causal parameters in a difference-in-differences setup is equivalent to the economic assumption that no aggregate shock concurrent with the trade war operates through exposure.

To investigate shorter-term changes around the incidence of the trade war, I assemble higher-than-annual frequency data deforestation rates from GLAD, which is a system de-

[1] In this paper, depending on context, I often use “Amazon” as a shorthand to mean Amazon and Cerrado.

signed to detect potential deforestation events at a weekly frequency. I combine these with data on the soybean supply chain between Brazilian municipalities and international export destinations. Using these data, I estimate a difference-in-differences style model to understand the effect of the trade war.

Did the trade war lead to an increase in deforestation in the Amazon and the Cerrado? I fail to find evidence that the incidence of the trade war increased deforestation rates in more exposed municipalities relative to the less exposed. In fact, I find a precise zero effect. This effect does not vary substantially across exposure quartiles. It also does not vary by municipality characteristics such as ex-ante prevelence of alternative land uses or the biome in which the municipality is located.

Why do I not find evidence of trade war induced deforestation? I consider and reject the leakage hypothesis that the trade war displaced deforestation from high exposure municipalities to low exposure municipalities, which my approach would have failed to detect. In particular, using annual data on transitions between alternative land uses, I show that the rate of transition into soy from from other land uses did not change across exposure levels at the incidence of the trade war. Instead, I suggest, based on anecdotal evidence and the short-lived nature of the trade war, that it is more plausible that exporters chose to draw down existing stocks of soy to make up for short-term increases in exports as the trade war did not substantially alter the path of Brazil's exports to China.

My paper directly contributes the recent literature on environmental consequences of the US-China trade war (Fuchs et al., 2019; Yao et al., 2021; Lu et al., 2020; Du et al., 2020)[2]. All of these studies use computable general equilibrium (CGE) models to understand the environmental effects of the trade war. In contrast, I look at actual data from before and after the trade war to obtain a data-based perspective on the question. To the best of my knowledge, my paper is the first paper to empirically examine the environmental consequences of the trade war. Similar to Fuchs et al. (2019), I focus on land use changes. In particular, based on extrapolating from historical data, they suggest that a protracted trade war can lead to high levels of deforestation in the Amazon. I present a complementary perspective that looks at the short-run impact of the trade war on deforestation based on realized outcomes.

My paper is also related to the literature on environmental impacts of international trade[3], specifically impacts via land use change. In particular, my conceptual framework is closely related to the quantitative model in Domínguez-Iino (2021). In addition, Domínguez-Iino (2021) employs a shift-share instrumental variables strategy that uses the cross-sectional variation in access to beef supply chains. I construct a similar variable. However, I use it in a difference-in-differences design, so the structural assumptions required for identification

[2]There is a larger literature that looks at other economic consequences of the trade war. Specifically, Adjemian et al. (2021) use a CGE model to estimate the market effects of the change in soybean tariffs.

[3]See Cherniwchan et al. (2017) for a recent summary.

are a bit different.

Finally, my approach is similar to the "exposure research designs" used to understand the effect of aggregate shocks in local labor markets. A prominent example of this is Autor et al. (2013). Waugh (2019) uses a similar strategy to estimate the impact of the trade war on US consumption and employment. This literature makes the assumption of imperfect labor mobility. In contrast, I make a similar assumption about slow adjustment in supply chains.

The remainder of the paper is organized as follows. Section 2 describes the policy context of the trade war and provides a background on soy trade between China and Brazil. Section 3 lays out the conceptual framework that motivates my empirical approach. Section 4 describes the data sources and the sample. Section 5 presents the main analysis and Section 6 discusses potential explanations for the findings. Section 7 concludes.

2 Background and Policy Context

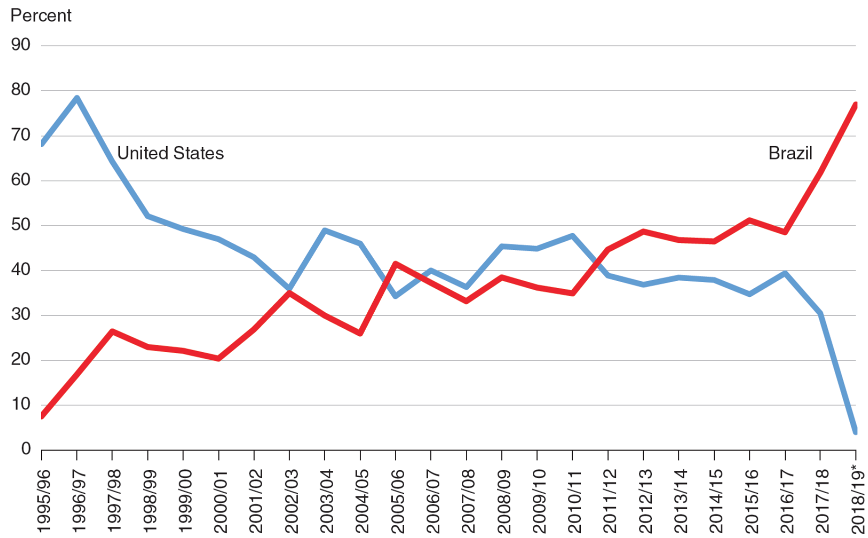
This section presents the background of the US-China trade war and provides an overview of the soy supply chain in Brazil.

US-China-Brazil soy trade

Chinese demand for soy has steadily risen over the last thirty years. Most of this soy is used to produce feed for livestock and edible oils. The overall demand for soy is relatively inelastic - animal feed and edible oils are not easily substitutable, and the quantity demanded is largely driven by the quantum of livestock and consumer demand, respectively. The secular increase in soy consumption is primarily due to the growth in consumer incomes, which has in turn changed food consumption patterns. Even in the years leading upto its accession to the WTO in 2001, China had been formulating policies that bolstered its imports of soybean to accompany the increasing demand.

Most of these imports came from US and Brazil. From 2000 to 2016, China's imports of soybeans from the US more than doubled - from 27 million metric tons (mmt) to 59 mmt - and from Brazil more than quadrupled - from 15 mmt to 63 mmt (Gale et al., 2019). In 2011, Brazil overtook US as the dominant supplier of soybeans to China. Figure 1 shows the evolution of shares of US and Brazil in Chinese soy imports.

Figure 1 China soybean imports: Shares supplied by United States and Brazil, 1995–2019



Note: This figure is taken from Gale et al. (2019). Chart shows share of soybean imports arriving in China during China's October–September market year. 2018/19 data for October 2018–February 2019. The data is from the ERS analysis of China Customs data accessed from IHS-Global Insight, Global Trade Atlas.

Brazil competitiveness in soy trade stems from relatively low production costs, especially cheap land, which is available in frontier areas of Cerrado and the Amazon. Figure 1A in the appendix shows that much of the recent soybean production growth has come from "frontier" regions of Cerrado and Amazonia.

Trade war

In March 2018, the United States imposed tariffs on imports of steel and aluminium from China. In retaliation, China announced tariffs on a list of US commodities in April 2018. Soybeans, alongwith some other agricultural commodities, were added to this list towards the end of June 2018. These tariffs were implemented in July 2018. As a result, tariff on US soybeans went up from 3% to 25%. For all other countries, including Brazil, the rate remained at 3%. The tariffs were further hiked by 5% in September 2018.

This led to a sharp decrease in Chinese imports of soybean from the U.S. and an increase in imports from Brazil. Figure 1 shows the sharp change in the shares of US and Brazilian soybeans in the Chinese market. China's soybean imports have a seasonal pattern - while U.S. soybeans arrive primarily from November to March, Brazilian soybeans peak during May to September. Following the tariffs, high volumes of Brazilian soybeans were being imported by China even in December (Gale et al., 2019).

The tariff war started winding down towards the end of 2019. Tariff rates were continually increased until September 2019, when China announced that it would not impose any additional tariffs on soy and pork. There were also reports in early 2020, later borne out by customs data, that some U.S. exporters might have their tariffs reimbursed upto a import quota. U.S. exports to China had recovered to pre-trade war levels in 2020. Still, the trade gains made by Brazil were not eroded.

The continued gains for Brazil despite U.S. recovery are explained by the increase in soybean demand in China during this period. The beginning of the trade war had co-incided with a decrease in soybean demand in China due to several factors such as environmental regulations on livestock farming and worries about African swine fever. This might be one of the reasons why imports from Brazil did not fully compensate for the reduction in those from the US. Nonetheless, there has been a marked increase in the soy exports from Brazil to China - moreover, these exports sharply picked up in the aftermath of the trade war.

Soy supply chains

International trade in Brazilian soybean is facilitated by agricultural trading firms that act as intermediaries between soy-producing farms and soy-importing firms at import destinations. These traders include well-known agri-businesses such as Cargill and Bunge, which occupy large market shares in multiple soy-producing municipalities, as well as smaller players that operate more locally. Traders own, or contract with their owners of, physical assets such as warehouses and processing facilities. Output travels to these logistical hubs from farm gate, and continues to ports, from where it is exported. At import destinations, these shipments are received by intermediary importers that connect them to wholesalers or final consumers.

This logistical supply chain is sustained by a series of contracts with local producers, asset owners as well as foreign importers. There is evidence for long-term relational contracts between traders and soybean farmers (DePaula, 2017). Anecdotal and qualitative evidence also suggests that social norms as well economic pressures help sustain these relationships between farmers and traders (Reuters, 2021; Bicudo Da Silva et al., 2020). Moreover, some traders also own their own farms. At the other end, regulations and fixed costs necessitate some degree of stability in contracts between exporting and importing intermediaries. For large commodity traders, such as Cargill and Louis-Dreyfus, that hold large market shares, the foreign importers are often subsidiaries of the same parent firm.

Therefore, trade pivots around intermediaries that have made enabling contractual and physical investments. Importantly, these investments are sticky and difficult to adjust, especially in the short-run.

3 Conceptual framework

The conceptual problem in attributing the effect of an aggregate shock, such as the trade war, is the presence of other confounding concurrent aggregate shocks. The change in aggregate deforestation around the time of the trade war is the net effect of all of these shocks. As a result, I cannot directly measure the effect of only the trade war without more assumptions. My approach is to find an auxillary variable that satisfies two conditions. First, any effect of the trade war must systematically vary with the auxillary variable. Second, the effect of other shocks must not have any systematic variation with this variable *and* there must be no pre-existing differential trends in deforestation rates along this variable. Therefore, if we detect a differential effect of the auxillary variable on deforestation at the incidence of the trade war, this effect must be due to the trade war. I use *pre-trade shock exposure to Chinese soy trade* as the auxillary variable.

This approach is similar to difference-in-differences style research design with ex-ante exposure to Chinese trade as treatment intensity. Like difference-in-differences, my approach requires that no other aggregate shocks with the trade war differentially impact areas with higher treatment intensity. This is implied by the statistical "parallel trends" assumption, which states that in the absence of the trade war, areas with different treatment intensities would have followed the same trend. This addresses the problem of differential trends due to aggregate shocks as well as those due to local factors. [4].

The remainder of this section provides a conceptual framework to understand why the effect of the trade war should be higher in municipalities with greater exposure to Chinese trade. Together with parallel trends, this justifies the use of exposure to Chinese soy trade as the auxillary variable. In particular, I claim that if deforestation did indeed increase as a result of the trade war, then it must have increased more in municipalities with greater prior exposure to Chinese soy trade. The framework highlights the assumptions required to make this claim. Moreover, it also provides potential explanations for why a trade shock that

[4] As pointed out by Callaway et al. (2021), with continuous treatment difference-in-differences might need stronger parallel trends assumptions depending on the causal estimand. While I do not explicitly translate my problem to a causal inference framework, the causal estimand relevant to my investigation is the difference between the *treatment effect* for dose d for any two different treatment intensity levels. In their notation, this is $ATT(d|d) - ATT(d'|d')$ where d, d' are different treatment intensities, or trade exposures. To identify the treatment effect, we just need to extend the standard binary treatment parallel trends assumption to the continuous treatment case. This assumption states that the path of any treated (d) potential outcomes should be the same as that of untreated potential outcomes i.e. $E[\triangle Y_t^0|D = d] = E[\triangle Y_t^0|D = 0]\ \forall d$. For comparison, they point out that the treatment effect defined above can be decomposed into two effects: a *causal effect* for increasing intensity d to d', $ATT(d|d) - ATT(d'|d)$, and a selection effect, $ATT(d'|d) - ATT(d'|d')$. Identifying the causal effect requires a stronger parallel trends assumption, which restricts paths of untreated as well as treated potential outcomes i.e. $E[\triangle Y_t^0|D = d] = \text{constant}\ \forall d$.

Moreover, my approach does not imply or require parallel *trends.* In principle, I can extend to the continuous treatment case alternative models of counterfactual paths such as parallel growth or interactive fixed effects and (Bilinski and Hatfield, 2018; Mora and Reggio, 2019). However, this is not straightforward and I leave this for future work.

increases soy exports might not necessarily cause more deforestation.

The main idea is that when a demand shock originating at an international destination is propogated through the supply chain to soy-growing municipalities, the municipalities which have more pre-existing supply chain links with that destination will face a higher incidence of the shock. The relative magnitude of this incidence depends on how quickly the supply chain can adjust to the trade shock and reconfigure supply to newer areas. To the extent the supply chain is sticky and continues to source from areas with pre-existing links, such areas should see increases in farmgate soy prices and, therefore, more incentives for farmers to deforest and bring additional land under cultivation. Therefore, under the assumption that the supply chain does not adjust instantaneously to demand shocks, an increase in deforestation implies more deforestation in municipalities with higher trade "exposure". This assumption is true, for example, if changing links in the supply chain is costly. As discussed above, this is a reasonable assumption in my context and is supported by anecdotal and empirical evidence.

More formally, this mechanism can be represented by an equation that connects total exports to a destination with municipal soybean production, and two equations that relate local soybean production to deforestation.

First, consider the link between soybean production to external demand via the supply chain. Let F denote the set of exporting all firms in Brazil and C denote the set of all final export destinations. Then, the total soy exported from municipality m, X_m, is given by

$$X_m = S_m + Y_m = \Sigma_{c \in C} \Sigma_{f \in F} \beta_{mf} \alpha_{fc} Z_c \tag{1}$$

where Y_m and S_m denote new soy production and withdrawl from accumulated storage, respectively, in municipality m, β_{mf} denotes the market share of exporter f in municipality m, α_{fc} is the share of exporter f in destination country c and Z_c are the total exports of Brazilian soybean to destination c. The term $\Sigma_{f \in F} \beta_{mf} \alpha_{fc}$ is the "exposure" of municipality m to demand conditions at destination c. It integrates over the composition of market shares in source m *and* in destination c over all the intermediary exporters that supply soybean from m to c. For example, consider two monopsonistic municipalities m and m' that only supply to exporters f and f', respectively. If f has a large market share in China than f', then m has a greater exposure to Chinese trade than m'.

(1) encapsulates information about the supply chain from a municipality m to an export destination c and links external demand to local soy production. Without further details, this is just an accounting identity. The economic content of (1) derives from the fact that (1) each term in it is endogenously determined and (2) it is a market clearing condition for soy output in every municipality. In particular, β_{mf}s are determined by choices made by exporters and soy producers and the competition with soy producers and amongst themselves within a municipality; similarly, α_{fc}s are determined by choices of exporters and importers in

destination countries. The exact details of the equilibrium are determined by the choice environment, the competitive environment and exogenous factors that shape local comparative advantages. In particular, the choice problem for exporters also includes the decision to buy new soy or deplete existing storage (the state variables in the choice problem include accumulated stocks). These are forward looking decisions that take into account the persistence of the shock and must account for the production environment, which can be constrained by productivity, availability of inputs and time-to-grow in the short run[5].

To complete the link from trade shock to deforestation, we need an equation that connects the changes in soy production, which is described by (1), to land use. This is provided by the aggregate (for a municipality) input choice function:

$$L_m^{\text{soy}} = f(Y_m, w_m, EY'_m, \phi_m) \tag{2}$$

$$L_m = L_m^{\text{soy}} + L_m^{\text{forest}} + L_m^{\text{others}} \tag{3}$$

where Y_m is the soybean output for municipality m in the current year, Y'_m is the soybean output in the following year, L_m^{soy} is the land allocations to soybean production, w_m are prices of other inputs relative to those used for deforestation, and ϕ_m is a municipality specific productivity parameter. (2) an (aggregated) function that relates land allocated to soy to soy output, given other input choices and productivity. I take a broad view of land allocated to soybean to include areas that are planned to be put under soy production in the future. This is important because the land use decisions are also dynamic - current land use choices depend on future expectations of soy production - as they require time-consuming investments to prepare the land for cultivation. Therefore, expected increases in future demand, such as a protracted trade war leading to high soybean demand, might lead cultivators to plan for future supply increases today. Changes in land use due to increase in both contemporaneous and future demand are reflected in equation (2).

(3) states that total land area in a municipality can be allocated to either soy production, forest or other uses (such as pasture). As L_m is fixed, any increase in land allocated to soy must come from decreasing either land under other uses, L_m^{others}, or from deforesting existing

[5]While they are not of direct relevance to my empirical approach, which relies on the equilibrium satisfying the high level condition that the supply chain does not immediately adjust to demand shocks, there can be many ways of specifiying the exact details of the outcome. For example, Each exporter chooses how much soy to procure from each municipality, given its own costs, choices of other exporters and soy farmers, and external demand conditions $\{Z_c\}$, which can be thought of as exogenous. Own costs are driven by municipality-specific procurement costs such as transportation, as well as the exporter's access to pre-existing physical and contractual investments as discussed above. Market shares for each exporter in each municipality, $\{\beta_{mf}\}$, are the outcomes of this game. Similarly, exporters also compete in export destinations and $\{\alpha_{fc}\}$ can be conceived of as an outcome of that game. In general, solving for such an equilibrium is a difficult computational problem (Arkolakis et al., 2021). While a structural model will need to take a stance of these details, my more modest goal of detecting an impact of the trade war requires that the equilibrium satisfies a high-level requirement that equilibrium market shares of exporters (either in municipalities or in export destinations) are sufficiently sticky.

forests, L_m^{forest}.

Thinking through the very general setup described by equations (1) and (2) explains how an external demand shock propogates through the supply chain to cause deforestation. Consider the extreme case where β_{mf} and α_{fc} do not adjust at all to a change in Z_c. An increase in export demand, $\triangle Z_c$, leads municipality m to increase its exports to c by $\Sigma_{f\in F}\beta_{mf}\alpha_{fc}\triangle Z_c$. These additional exports can come from either a change S_m or Y_m. The part of increased exports that comes from $\triangle Y_m$ leads to increased land allocation to soy $\triangle L_m^{\text{soy}}$ as dictated by (2). In addition, if $\triangle Z_c$ changes expectations about future soy production EY_m', then there might be additional land allocated to soy in the present. Finally, the magnitude of deforestation is given by how $\triangle L_m^{\text{soy}}$ is allocated between $\triangle L_m^{\text{forest}}$ and $\triangle L_m^{\text{others}}$.

In the above explanation, municipalities with higher exposure to c, as defined by $\Sigma_{f\in F}\beta_{mf}\alpha_{fc}$, will see higher deforestation, all else equal. However, β_{mf} and α_{fc} are equilibrium objects and would generally change as a response to $\triangle Z_c$. However, under the assumption of sluggish response, it would still be true that municipalities with higher exposure to c at the time of the shock experience more deforestation, if it the shock did in fact cause deforestation. This is analogous to the assumption of slow response of labor mobility employed by Autor et al. (2013) and Waugh (2019) to study the impact of trade shocks on local labor markets. Another concern could be that the trade shock shifts expectations of future production by more in currently less exposed municipalities, and these municipalities deforest more in anticipation of future production. However, these expectations are driven by perceived persistence of the shock and there is no reason why this perception should vary systematically by exposure. The only case where $\triangle Z_c$ does not induce more deforestation in areas with higher exposure is if there is no additional deforestation to begin with. Therefore, testing for a differential response to $\triangle Z_c$, also serves as a test for whether $\triangle Z_c$ increased aggregate deforestation.

Why might in increase in Z_c *not* lead to deforestation? (1)-(3) suggest several explanations. First, in (1), the increase in exports can come from depleting the existing stock, S_m. The choice between deplete the existing stock and producing more depends on several factors, including storage costs, expectation of future demand and cost faced by exporters for procuring soy, which in turn would reflect production conditions and competition between exporters and farmers. Second, even if production need to be increased, it can potentially be done by increasing other inputs I_m, instead of land allocated to soy. This substitution depends on the shape of the production function and input prices, which underlie the aggregate input demand $f(.)$ in (2). Third, as noted in (3), even if more land needs to be allocated to soy, land can be diverted from other land uses, such as pasture, instead of forests. This is governed by the the current configuration of land uses in a municipality and relative returns to other land uses.

4 Data and Sample construction

I need two sets of variables for my analysis - areas under alternative land uses and deforestation rates, and supply chain data to measure municipal exposure to Chinese soy trade. I first describe the sources for these data. Next, I explain how I choose my sample and present descriptive statistics.

4.1 Data

4.1.1 Land use and deforestation rates

I get data on land use and deforestation from two sources. First, I use the landcover maps from the MapBiomas project (Souza et al. 2020). These maps are constructed using 30m$\times$30m pixels from LANDSAT satellite imagery. Each pixel, which in its raw form is a composite of different wavelengths, is classified into different land cover types using supervised learning algorithms trained using groundtruth data. This gives me areas under and transitions between different landcover types for an annual panel of municipalities.

In addition to the annual data, I also compute municipal deforestation rates at a monthly frequency using University of Maryland's Global Analysis and Discovery (GLAD) alerts data (Hansen et al., 2016). GLAD is an automated system that uses Landsat imagery to flag disturbances in the forest canopy. Landsat satellites cover every 30m pixel on Earth roughly every eight days, so this is also the average frequency of GLAD alerts. This was devised as an early warning system for potential deforestation events. However, some of the alerts might be delayed due to cloud cover. Instead of using a weekly frequency, I aggregate data to a two-monthly frequency.

The higher-than-annual frequency measurements are useful, even though both deforestation and soy follow an annual cycle. Deforestation predominantly takes place in the dry season which ranges from June to December, depending on the region. Soybean plantation takes place towards the end of the dry season. The trade war picked up in the middle of the year - China imposed retaliatory tariffs in July 2018 - when the dry season was already underway. Monthly data allow me to observe any changes that happened after the trade war began, in the middle of the deforestation season.

While GLAD alerts allow for more frequent observations, there are two main limitations associated with this data. First, GLAD alerts provide a binary land cover classification. Unlike annual products such as MAPBIOMAS, which classify land cover into various land use types such as farming and ranches, GLAD only flags when a pixel is estimated to change from a forest to a non-forest state. Therefore, I cannot explicitly distinguish the proximate cause of deforestation. Second, the alerts might pick up natural disturbances as well as planned disturbances on forest plantations. Both these features make the GLAD alerts unsuitable for area estimation. However, if this measurement error is uncorrelated with exposure, it will not bias my estimates.

4.1.2 Municipal soybean exports

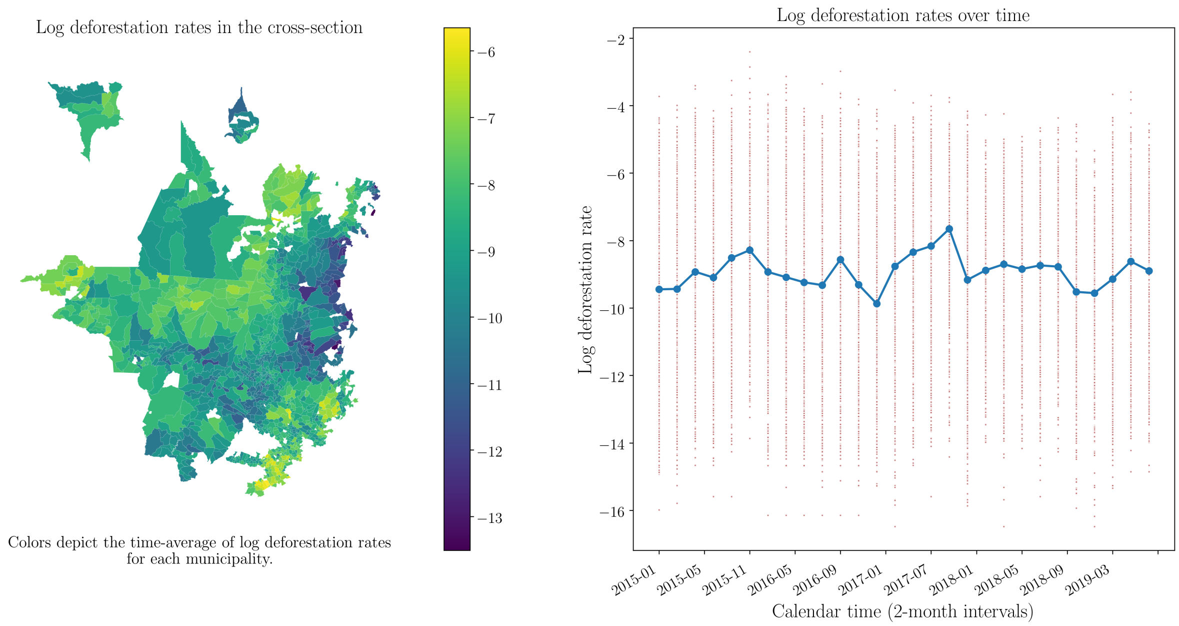
Data on municipality soy exports to foreign markets comes from the TRASE project (Trase, 2020). In the absence of direct measurement of the flow of goods along every link in the supply chain, TRASE adopts a supply chain accounting approach (called "Spatially Explicit Information on Production to Consumption Systems" or SEI-PCS) based on Godar et al. (2015). For Brazilian soy, TRASE uses shipment-level export data (such as customs records and cargo manifests) and links it to production facilities (such as farms) or processing facilities (such as warehouses and crushing plants) from taxation data, which identify the location of these facilities. Some soy exports cannot be linked back to *soy-growing* municipalities this way - because either the shipment-level records are not complete or the soy-grown in one municipality might be processed in another (not all municipalities have processing facilities). These unlinked exports (typically identified by an exporter and a processing facility) are allocated to municipalities using a linear program that maximizes the allocation to a municipality where the exporter has tax records while minimizing the transportation distance between municipalities and processing facilities, with the constraint that match each municipality's production (based on administrative data) to its total (model-implied) exports.

4.2 Sample selection and variable construction

I restrict my sample to municipalities in the Amazon and Cerrado biomes. Although soy is grown across all of Brazil, I want to focus on tropical deforestation. Further, I remove municipalities that do not grow soy. This leaves me with 1262 municipalities, of which 603 do not have exposure to China.

My deforestation sample from GLAD alerts ranges from the beginning of 2015 to the penultimate quarter of 2019. I aggregate this sample, which consists of deforestation at a 30 meter pixel level at a weekly frequency, in two ways. First, I aggregate pixels into municipalities because these are my spatial units of observation. This allows me to calculate area deforested each week in each municipality. Second, I aggregate the weekly data to a two-month frequency. This balances two objectives. It mitigates measurement error that might arise if cloud cover does not permit the timely recording of a deforestation event. At the same time, it provides me with sufficiently many and frequent time series observations to observe historical deforestation patterns and higher frequency changes around the time of the trade war. Finally, I divide the bi-monthly deforested area for each municipality by total forest standing at the end of 2014 as per MapBiomas data. Therefore, the measure for deforestation can be interpreted as a deforestation rate. Figure 2 provides a visualization of the distribution of log deforestation rates.

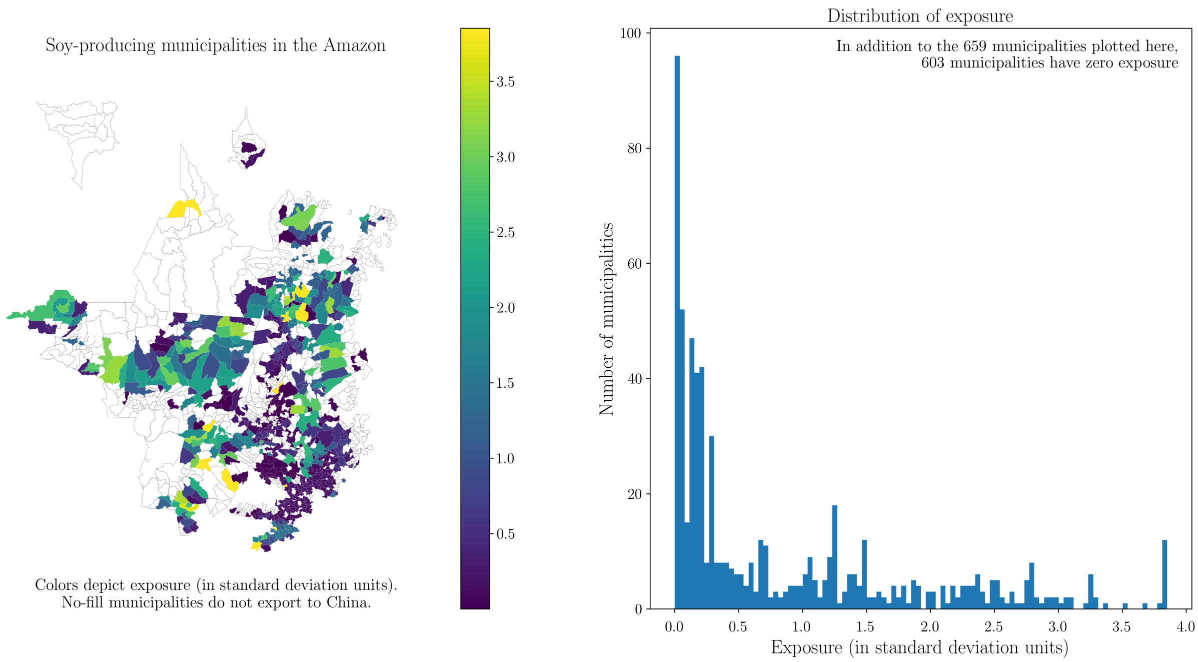
Figure 2 Log deforestation rates in the cross-section and over time



Notes: The figure shows the municipalities that are included in the sample. The left panel plots the time average of log deforestation for each municipality. The blue line in the right panel plots the time-series of the cross-sectional average of log deforestation rate. Each light brown dot represent a municipality. Therefore, the scatterplot of dots shows the period-wise distribution of log deforestation rates around each period's average.

I define a municipality's exposure to Chinese trade as the weighted sum of market shares of soy exporters operating in that municipality. The weight for an exporter is given by that exporter's share in Chinese soy imports. In other words, I define exposure as $\Sigma_{f \in F} \beta_{mf} \alpha_{fc}$ from (1), where c = China. β_{mf} and α_{fc} are computed for every municipality m and exporting firm f using TRASE data. The data on market shares in municipalities is modeled and based on noisy inputs. Therefore, there is a possibility of measurement error. Indeed, some municipalities in the data switch from being exporters in one year to non-exporters in another. To mitigate this, I measure exposure as the *average* of exposures in 2015, 2016 and 2017. Finally, I normalize my measure by dividing it by its cross-sectional standard deviation (calculated for municipalities with positive exposure). Figure 3 displays the distribution of my preferred exposure measure.

Figure 3 Distribution of exposure to Chinese trade



Notes: The figure shows the municipalities that are included in the sample. The left panel plots the exposure for each municipality. The unfilled municipalities are ones that do not export to China. This histogram in the right panel shows the distribution of exposures for exporting municipalities.

Table 1 provides some descriptive statistics of the sample.

Table 1 Descriptive statistics

	Municipalities that do not export to China				Municipalities that export to China			
	N	Mean	Median	SD	N	Mean	Median	SD
Log deforestation rate	104,776	-9.643	-9.643	2.225	46,116	-8.671	-8.671	1.799
Exposure (in SD units)	104,776	0	0	0	46,116	0.951	0.951	0.987

5 Analysis

This section explores the differential impact of the trade war by exposure to Chinese trade. I start by visualizing the data and then proceed to more formal econometric analysis.

5.1 Visual representations of the data

To visually illustrate the impact of the trade war across different levels of expsoure, I group municipalities that export to China into quartiles based on exposure and plot average log deforestation for three of these groups - the top and bottom quartiles by exposure for municipalities that export to China and group of municipalities that do not export to China ("non-exporters"). These time series, which are plotted in Figure 4, allow me to make a few observations.

First, prior to the trade war, the most exposed quartile has a higher average deforestation rate than non-exporters. The deforestation rate in the bottom quartile is a bit erratic - it starts out even lower than the non-exporters but jumps even higher than the most exposed quartile in the first few months of 2017.

Second, notwithstanding this reversal in ranking, deforestation rate trajectories between these groups are similar over time. This is consistent with deforestation being subject to similar time-varying macroeconomic shocks. It also provides a visual test of pre-trends in deforestation rate, which look parallel, with the exception of the events of early 2017. Third, the higher level of deforestation in non-exporters relative to the bottom quartile of exporters is suggestive of different drivers of deforestation between exporters and non-exporters, which might lead to different dynamics between the two. This raises a concern that non-exporters might not be the appropriate "control" group for the high exposure "treatment" group. Therefore, going forward, I present two sets of results - one for all municipalities and the other restricted to only exporters - and return to this discussion when evaluating the "parallel trends" assumption.

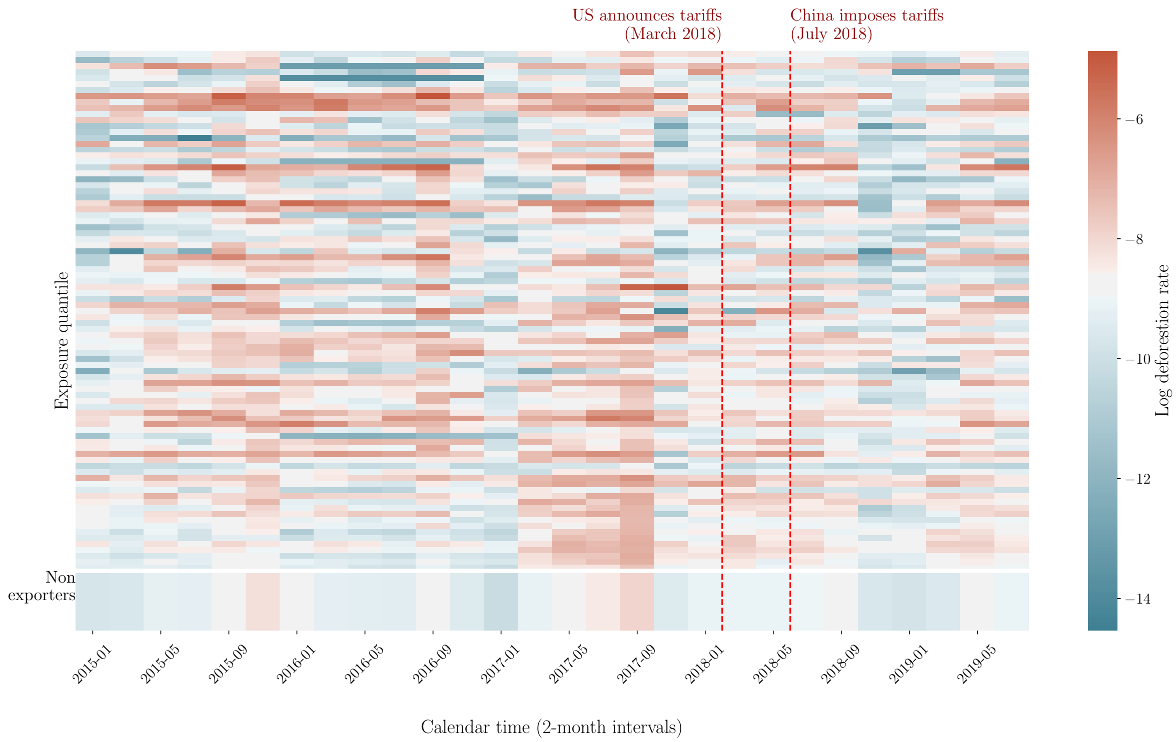
Finally, relative to the bottom quartile, deforestation in the top quartile does not show a consistent pattern after the announcement of tariffs or after China's reaction. In addition, the paths of top quartile of exporters and non-exporters appear similar. Together, this suggests that the effect of the trade war, if any, was quite muted.

Figure 4 Log deforestation rates for top and bottom quartiles and non-exporters

Notes: This figure plots the time series of average log deforestation rate for the top and bottom quartile municipalities by exposure and for the group of municipalities that do not export to China. Each period corresponds to two-months.

These features are also apparent in a more disaggregated view of the data as seen in Figure 5. Municipalities are divided into 100 quantiles and sorted based on exposure. Analogous to Figure 4, Figure 5 presents the time series of log deforestation rates for each of these quantiles in the form of a calendar-time heatmap. Columns correspond to 2-month intervals, and rows to groups of municipalities sorted by exposure. Each cell's shading corresponds to the magnitude of log deforestation rate. As before, deforestation rates are higher in more exposed municipalities, with the exception of 2017 when the deforestation disproportionately went up in the lower quartiles. Moreover, after the tariff announcement by the US in March 2018 and subsequent Chinese retaliation, there is no discernible differential change in deforestation rates across different exposure quartiles. If we extrapolate counterfactual deforestation for more exposed quartiles based on trends prior to 2017, there is *visual* evidence that association between exposure and deforestation rates did not become more pronouced in the aftermath of the trade announcement.

Figure 5 Calendar-time heatmap of log deforestation rates for each exposure percentile



Note: Municipalities are divided into 100 quantiles and sorted based on exposure, which is plotted on the y-axis. Calendar time in 2-month intervals is on x-axis. Shading in cell (x,y) corresponds to the average log deforestation rate in quantile y for period x.

5.2 Formal analysis

Next, I present formal econometric results. First, I estimate a difference-in-differences style model using the following econometric specification:

$$\log(\text{deforestation rate})_{mt} = \gamma_m + \gamma_t + \beta \text{exposure}_m \times 1(\text{Post-Chinese retaliation}) + \epsilon_{mt}, \quad (4)$$

where γ_m and γ_t are municipality and period fixed effects, respectively. The time fixed effects control for time-specific aggregate shocks, which are evident from Figure 2 as well as from the co-movement of deforestation across various exposure quantiles. Municipality fixed effects control for some municipalities having an overall higher deforestation rate than other municipalities. The frequency of the data is 2-months, so each t corresponds to a 2-month interval. 1(Post-Chinese retaliation) is an indicator for time period July 2018, when the China increased tariffs towards US soy. The variable exposure$_m$ is the exposure to Chinese soy trade, as defined in (1), normalized by its cross-sectional standard deviation.

The parameter of interest is β, which measures the after-before trade war change in the correlation between exposure and log deforestation rate, controlling for time-invariant and municipality-invariant unobservables. As deforestation is measured in logs, the interpretation of β is the percentage change in deforestation rate associated with 1 standard deviation increase in exposure.

To reiterate the earlier discussion, I am trying to disentangle the effect of the aggregate trade war shock from other concurrent aggregate shocks. From a statistical viewpoint, the assumption required to idenitfy β is the parallel trends assumption. For (4), this is equivalent to municipality-specific time-varying unobservables ϵ_{mt} being orthogonal to exposure$_m$ $\times$ 1(Post-Chinese retaliation). Note that since municipality fixed effects control for the baseline differences in deforestation due to exposure, exposure$_m$ $\times$ 1(Post-Chinese retaliation) can be interpreted as the change is exposure experienced by municipality m following a trade shock of magnitude 1.

From an economic viewpoint, this assumption implies two things. First, no other aggregate shocks operate or are correlated with the *soy* exposure channel described above. This could be a concern if, for example, the municipalities with a higher soy exposure to China also have a (similarly defined) beef exposure. Second, going back to (2), time-varying shocks to local productivity, ϕ_m, and input prices, w_m, are the key components of ϵ_{mt}. As noted earlier, the average deforestation rate, which might be correlated with *trends* in these variables, varies across exposure quantiles. Parallel trends imposes the economic assumption that trends in local productivity and input prices are uncorrelated with exposure.

Table 2 presents two-way fixed effects estimates of (4). Estimates in Column 1 and Column 3 correspond to those for the entire sample and for the sample excluding municipalities that do not export to China. Both estimates of β are negative and statistically significant. Notably, however, these are very small negative numbers and are economically not very different from zero. For example, the higher estimate of 0.119 implies that a 1 standard deviation change in exposure decreased deforestation rates on average by 0.119%. Another way to put this into perspective is to note that the median two-monthly deforestation rate for this sample is 0.015 percentage points. A decrease of 0.119 log-points implies that this median deforestation rate decreases by 0.0016 percentage points. For the entire sample, this decrease is even smaller, leading to the preliminary conclusion that the trade war did not have a meaningful impact on deforestation rates in the Brazilian Amazon and Cerrado. It could be that the true incidence of the trade war March 2018, when the US first announced tariffs on China, instead of July 2018. Columns 2 and 4 add to Columns 1 and 3, respectively, an interaction of the exposure with an indicator for the periods between March 2018 and July 2018. This does not change the estimated coefficient in a significant way suggesting the absence of such announcement effects.

Table 2 Effect of Exposure to Chinese Trade on Log Deforestation rate

	(1) All	(2) All	(3) Exporters only	(4) Exporters only
Exposure $\times$ 1(Post-retaliation)	-0.068**	-0.069**	-0.120***	-0.129***
	(0.029)	(0.031)	(0.034)	(0.037)
Exposure $\times$ 1(Post-US announcement)		-0.008		-0.095**
		(0.033)		(0.041)
N	35,336	35,336	18,452	18,452

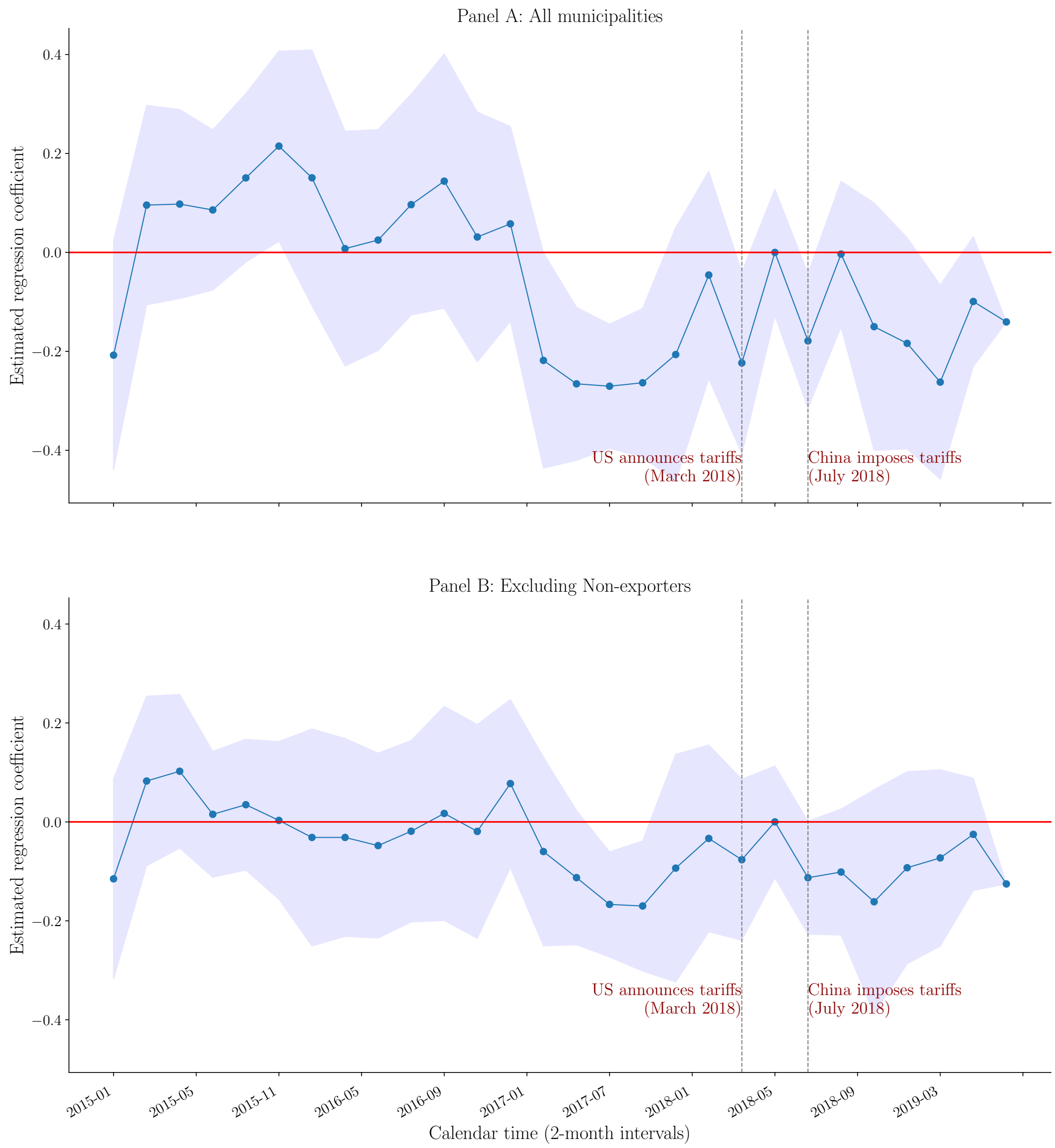
Notes: The dependent variable is log deforestation rate. All specifications include municipality and time fixed effects. Standard errors are clustered at the municipality level.

While the parallel trends assumption itself is not testable, I can test for the absence of pre-trends in the data, which is implied by parallel trends. I estimate the empirical specification

$$\log(\text{deforestation rate})_{mt} = \alpha_m + \alpha_t + \Sigma_t \gamma_t \text{exposure}_m + \epsilon_{mt} \tag{5}$$

where γ_t is the t-period specific exposure coefficient and α_m and α_t are municipality and time fixed effects, respectively. Figure 6 plots estimated coefficients with a 95% uniform confidence interval based on Montiel Olea and Plagborg-Møller (2019). As suggested by Freyaldenhoven et al. (2019), uniform confidence bands are designed for inference on the entire path of the coefficients, which is the relevant parameter for evaluating parallel trends. Panel A presents results for the entire sample and Panel B for the sample restricted to municipalities that export soy to China. In Figure 6, the estimated coefficients are closer to zero and the confidence intervals are tighter for the restricted sample (Panel B) than for the entire sample (Panel A). Therefore, the path of coefficients in the restricted sample is more consistent with the lack of pre-trends relative to the entire sample. This is theoretically plausible because, as discussed earlier, drivers of deforestation in municipalities that export to China might differ from those in municipalities that do not. Therefore, my preferred estimates are based on the restricted sample.

Figure 6 Effect of Exposure to Chinese Trade on Log Deforestation rate



Note: This figure presents the estimates for equation (5). The blue shaded areas are 95% uniform confidence intervals (as suggested by Freyaldenhoven et al. (2019); based on Montiel Olea and Plagborg-Møller (2019)). Panel A presents results for the entire sample and Panel B for the sample restricted to municipalities that export soy to China.

Another takeaway from Figure 6 is the largely flat path of coefficients even after the incidence of the trade shock. This lends credence to the zero economic effect of the trade war on deforestation. A potential concern for the parallel trends could be the apparent dip in coefficients in 2017 prior to the trade war, which raises the question of the appropriate counterfactual trend after the trade war. While the coefficients for 2017 being statistically different from zero, the economic difference is still very small.[6].

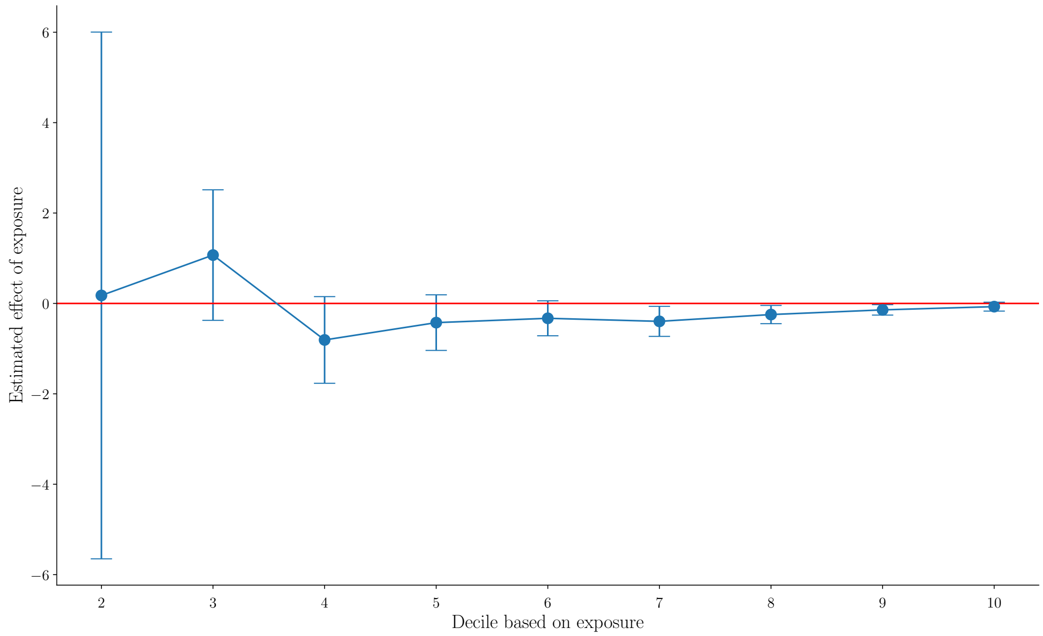
The zero effect is quite stark result and the average effect for all municipalities might mask heterogenous effects across subgroups. The remainder of this section investigates this heterogeneity.

I start by considering heterogeneity along exposure. Callaway et al. (2021) observe that the two-way fixed effects estimator is the weighted average of treatment effects at all intensity levels and these weights have unusual properties. In particular, the weights are maximized at the average treatment level and treatment effects "in the middle" get higher weights. I address these issue in two ways.

First, I plot the non-parametric relation between the effect of exposure with the exposure decile. I bin municipalities with *positive* exposure into deciles based on exposure. For each decile d except for the lowest, I restrict my sample to municipalities in decile d and the lowest decile. I estimate (4) for this restricted sample. Essentially, this uses the lowest decile of exporting municipalities as a benchmark group to evaluate the effect in decile d, $d \neq 1$. Results from this exercise are presented in Figure 7. Two observations stand out. One, the estimated effects are statistically not distinguishable from zero across all 9 deciles. Two, the effects are slightly larger in magnitude, but in different directions for lower deciles. Note that the mean exposure in my sample is 0.85 is higher than the median exposure of 0.30. This is consistent with the idea that the municipalities with average exposure have a higher weight in the average treatment effect. Nonetheless, it does not change my overall conclusion that the trade war had a muted effect on deforestation in the short run.

[6] To further mitigate concerns arising from parallel trends violations, we can think of alternative plausible assumptions for the counterfactual path of potential outcomes and evaluate the sensitivity of our estimates to these. Formal investigations of this type for the case of a binary treatment are discussed in Bilinski and Hatfield (2018) and Rambachan and Roth (2019). I do not attempt these here because they are not straightforward to implement for a continuous treatment.

Figure 7 Effect of Exposure to Chinese Trade for various Deciles based on Exposure



Note:

I also estimate (4) using first differences, instead of two-way fixed effects. This serves as another robustness check for the potential issues with the two-way fixed estimator. These estimates are shown in Table 3. First-differences estimates are closer to zero and do not invalidate the qualitative results from two-way fixed effects.

Table 3 Effect of Exposure to Chinese Trade on Log Deforestation rate: First Differences Estimation

	(1) All	(2) All	(3) Exporters only	(4) Exporters only
Exposure × 1(Post-retaliation)	-0.018***	-0.018***	-0.020***	-0.020***
	(0.005)	(0.005)	(0.006)	(0.006)
Exposure × 1(Post-US announcement)		0.012		0.014
		(0.029)		(0.032)
N	34,074	34,074	17,793	17,793

Notes: The dependent variable is log deforestation rate. All specifications include time fixed effects. Standard errors are clustered by municipality.

Next, I look at heterogeneity by other characteristics of municipalities. I start by looking at how the effect varies with areas under to alternative land uses. The three dominant land uses in this region are forests, agriculture (including but not limited to soy) and pasture. As discussed earlier, land can be diverted to soy not just from forests but also other land use types. To the extent, the high prevelance of a land use type in an area is correlated with its local benefits relative to switching to soy - this would be true under revealed preference models of land use such as Stavins (1999) and Scott (2014) - the effect of the trade war might vary by the ex-ante of land allocated to that land use.

To investigate this, first, using MapBiomas data, I find the fraction of area under each of the three dominant land uses at the end of 2014 for each municipality. Next, for each land use, I split my sample into municipalities with above median and below median allocation to that land use. Finally, I estimate (4) for each subsample. Based on the above analysis, I exclude non-exporting municipalities and restrict my sample to municipalities that export soy to China. These estimates are reported in Table 4. These results do not reveal substantial heterogeneity by ex-ante land use prevalence.

Table 4 Effect of Exposure to Chinese Trade: By Fraction of Area under Alternative Land Uses

	(1) > Median forest	(2) < Median forest	(3) > Median agri.	(4) < Median agri.	(5) > Median pasture	(6) < Median pasture
Exposure × 1(Post-retaliation)	-0.041	0.017	-0.095*	-0.098***	-0.103***	-0.054
	(0.030)	(0.060)	(0.050)	(0.032)	(0.038)	(0.042)
N	19,796	15,540	12,056	23,280	19,892	15,444

Notes: The dependent variable is log deforestation rate. All specifications include municipality and time fixed effects. Standard errors are clustered by municipality. The sample is restricted to municipalities that export soy to China.

I also check if the size of the effect varies with whether a municipality lies in the Amazon or the Cerrado. Historically, soy has been more prevalant in the Cerrado relative to the Amazon. The dominant non-forest land use in the Amazon is pasture. As returns to have gone up, soy farms have been replacing pre-existing pastures. Estimates for (4) for subsamples for each biomes are presented in Table 5. Again, I do not find evidence of different effects across biomes. However, the positive (even though statistically similar to zero) estimate for the Amazon suggests that Amazonian municipalities might have seen a mild uptick in deforestation due to the trade war. Unfortunately, I cannot conclude this with confidence due to lack of statistical power. I discuss the spillover issue further in the next section.

Table 5 Effect of Exposure to Chinese Trade: By Biome

	(1) Cerrado Only	(2) Amazon Only
Exposure × 1(Post-retaliation)	-0.109***	0.164
	(0.035)	(0.099)
N	16,968	1,484

Notes: The dependent variable is log deforestation rate. Standard errors are clustered by municipality. The sample is restricted to municipalities that export soy to China.

6 Economic Mechanisms

We know that soybean exports went up following the trade war. However, this did lead to detectable increase in deforestation. As suggested by the conceptual framework in Section 3, there are several reasons for why this might have happened. I discuss two plausible hypothesis for my finding.

First, it is possible that deforestation did in fact increase but my approach is unable to detect this change. This can happen due to leakage or spilling over of deforestation between municipalities[7]. This happens in two steps. One, in more exposed municipalities, soy should take over other anthropic land uses. Two, these displaced land uses move into municipalities without much prior soy production or exposure. This cross-municipality interaction would not be picked up by my empirical strategy.

I do not find strong evidence of this mechanism. In particular, MapBiomas data allows me to measure transitions into soy from other land uses. I can use this to investigate if municipalities with greater exposure saw higher transition rates from alternative land uses into soy. I compute transitions from forests, pasture and grasslands, and non-soy agriculture into soy for each municipality for every year starting 2014-15 to 2019-20. This is annual data so I only have two observations for each municipality for the post trade-war period. I estimate the following specification for each land use type k

$$\log(\text{transition from } k \text{ to soy})_{mt} = \alpha_m + \alpha_t + \Sigma_t \gamma_t \text{exposure}_m + \epsilon_{mt}$$

where

$$\text{transition from } k \text{ to soy}_{mt} = \frac{\text{Area in location } m \text{ that transitioned from } k \text{ to soy between } t \text{ and } t+1}{\text{Area in location } m \text{ under land use } k \text{ at } t}.$$

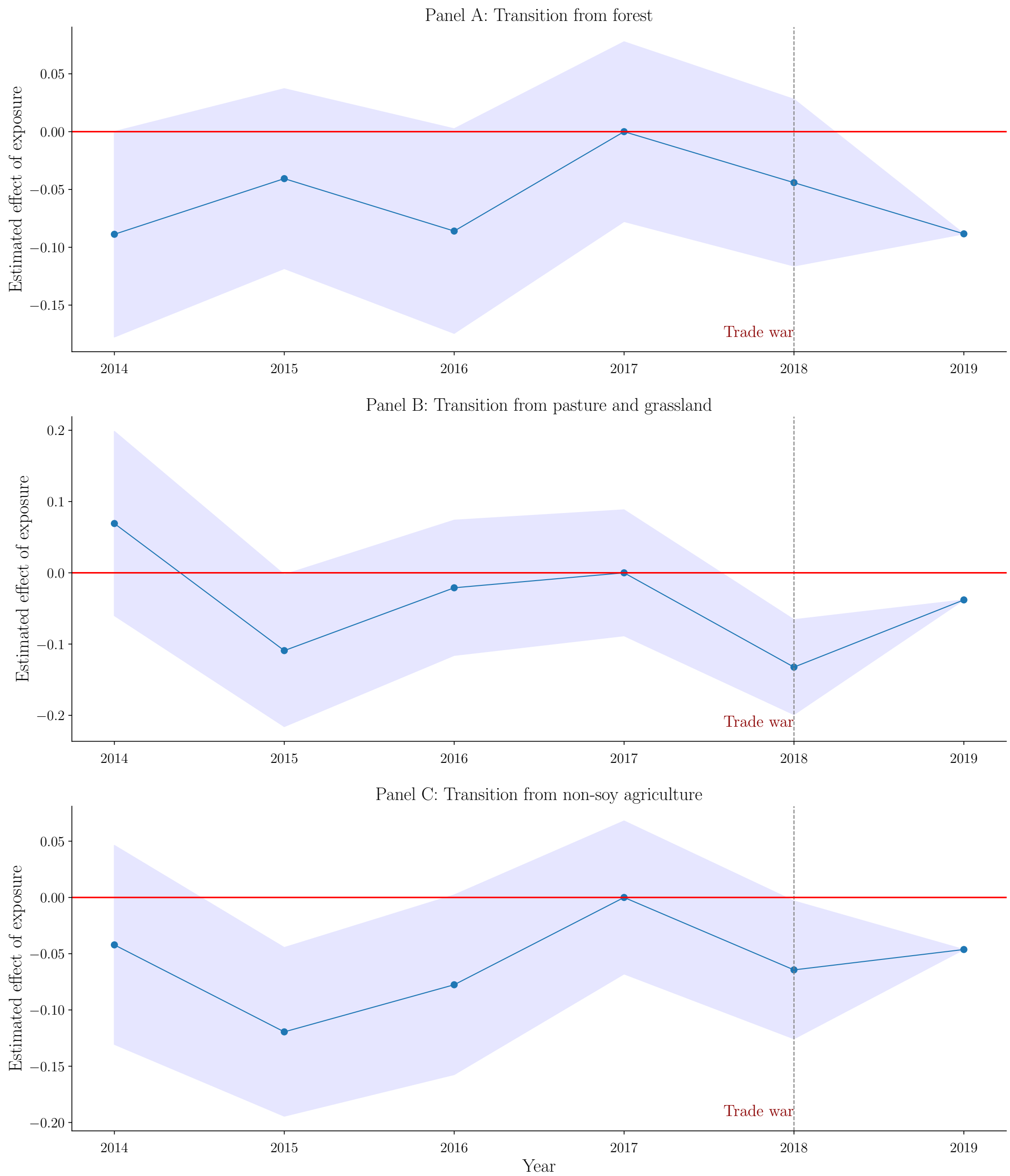
The remaining notation is analogous to (5). I plot the estimated coefficients in Figure 8. The transition out of forest to soy is consistent with earlier results using higher frequency

[7] My approach will capture within municipality leakage.

data. Transition out of all land use types into soy *decreased* following the trade shock. Therefore, I do not find evidence for the first part of the leakage hypothesis that more exposed municipalities see higher transition rates out of other land uses into soy.[8].

[8]To evaluate the second part of the leakage mechanism, I can also look at difference-in-difference style estimates comparing the change in deforestation rate for soy producers and non-producers (and also for soy exporters and non-exporters). I present these results in Table A1 in the Appendix. These results suggest that deforestation did indeed go up among non-producers and non-producers relative to their respective counterparts. However, these must be treated with a pinch of salt because a priori these groups are likely to be quite different and therefore have different drivers and trends in deforestation rates, bringing the validity of difference-in-differences estimates in question.

Figure 8 Effect of Exposure on Annual Transitions from Various Land Uses to Soy



The second hypothesis, which is supported by my finding of no effect, is that deforestation did not deviate from its pre-existing trend due to the trade war. The increased exports came from existing stocks of soy, which were plentiful to last for a few months. If the shock is expected to be short lived, exporters can find it optimal to deplete pre-existing stocks. While I did not find good data on stocks of soy, there is anecdotal evidence to support this hypothesis. News reports from mid-2017 indicate that stocks of soy had been building up in warehouses due a bumper harvest (Reuters, 2017). Moreover, storage capacity is often binding constraint in for agricultural traders in Brazil, especially with increasing agricultural productivity outpacing the construction of new warehousing (de Freitas et al., 2019). This would make it optimal to deplete stocks at higher rate relative to situation with slowing productivity or excess capacity. Finally, it is likely that market participants did not expect the trade war to last very long - especially because it was imposing a large cost on US farmers. The trade war did start winding down towards the end of 2019 and US exports to China have largely recovered (Kenner et al., 2021). In fact, soy was one of the first commodities on which China imposed a moratorium on increasing tariffs. The short-lived nature of the trade war in case of soy is also evident from prices of soy in the US and Brazil (Figure A.2. in the appendix). Prices of soy in US and Brazil diverged when tariffs took effect but converged to the same levels towards the beginning of January 2019. As shown in Scott (2014) and argued in the first chapter of this dissertation, short-run and long-run land use elasticities can be substantially different and depend on the anticipated longevity of the shock.

7 Conclusion

This paper investigates whether the US-China trade war, which led to a spike in Brazil's soy exports to China, caused deforestation in the Amazon and the Cerrado. The key empirical problem here is to disentangle the effects of multiple coincidental aggregate shocks. My conceptual framework suggests that municipalities with higher prior exposure to Chinese trade are likely to have a higher increase in deforestation. This allows me to infer the effect of the trade war by examining trends in deforestation rates, before and after the trade war, across municipalities with different degrees of trade exposure.

I find that in the short-run trade war did not lead to excess deforestation. This effect is relatively stable across subsamples based on municipality characteristics. Though my empirical approach does not capture deforestation spillovers between municipalities, I present some evidence to suggest that this was not the case. Instead, I argue that the anticipated short-lived nature of the trade war meant that it did not lead a significant changes in land use.

There are some caveats to my study. First, I only look at short run effects - upto one year from the start of the trade war. A longer time-horizon would be needed to retrospectively study if the trade war shifted the path of deforestation. Second, my study does not fully take into account spillovers between municipalities. Even though I present suggestive evidence against deforestation leaking into other municipalities, a more complete investigation of this issue would require an equilibrium model.

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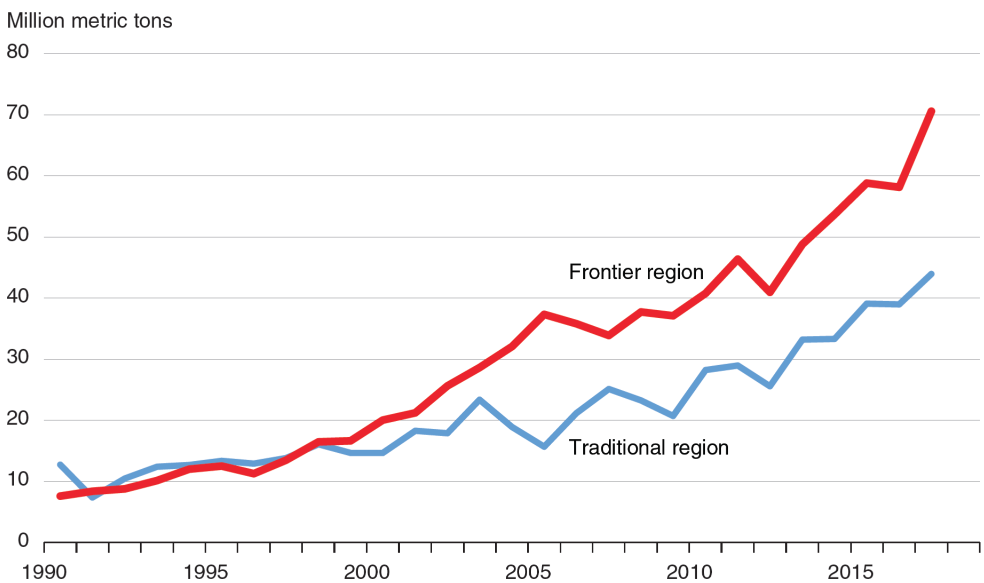
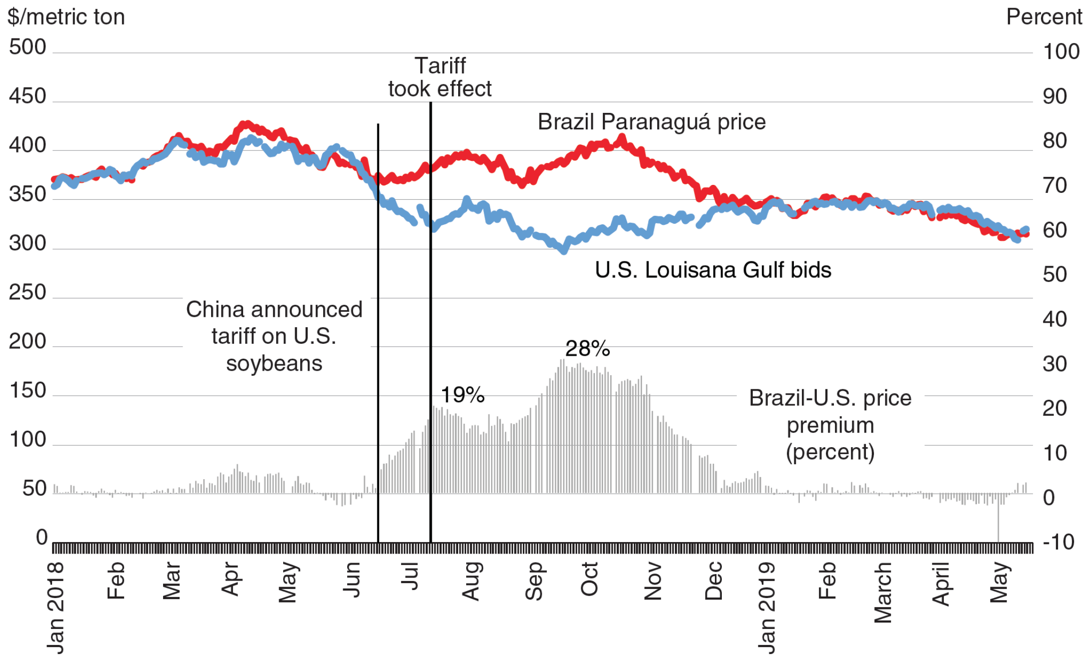
A Appendix

Figure A.1.: Brazil soybean production by region, 1990–2017



Notes: This figure is taken from Gale et al. (2019). "Frontier" region includes the States of Bahia, Piauí, Goiás, Mato Grosso do Sul, Mato Grosso, Maranhão, Minas Gerais, Acre, Amazonas, Amapá, Pará, Rondônia, and Roraima, encompassing nearly 93 percent of the Cerrado biome and the Amazônia biome. "Traditional" region includes the States of Paraná, Rio Grande do Sul, Santa Catarina, Espírito Santo, Rio de Janeiro, São Paulo, Alagoas, Ceará, Paraíba, Pernambuco, Rio Grande do Norte, and Sergipe. ERS calculations based on data from Brazil Ministry of Agriculture Marketing Agency (CONAB).

Figure A.2.: U.S.-Brazil soybean prices, 2018-19



Note: This figure is taken from Gale et al. (2019). Daily averages converted to dollars per metric ton. The Brazil price, obtained from Center for Advanced Studies on Applied Economics (CEPEA), is the soybean spot price in the Paranagua market. The US prices, obtained from USDA Agricultural Marketing Service, are the cash bids at the export elevators at US Gulf ports.

Table A.1.: Changes in Deforestation Rates between exporters/non-exporters and producers/non-producers

	(1)	(2)
1(Soy-producer) × 1(Post-retaliation)	0.231***	
	(0.038)	
1(Soy-exporter) × 1(Post-retaliation)		0.166***
		(0.040)
N	52,388	52,388

Notes: The dependent variable is log deforestation rate. All specifications include municipality and time fixed effects. Standard errors are clustered by municipality. The sample is restricted to municipalities that export soy to China.