The Spillover Effects of Environmental Transparency and Enforcement Regulation: Evidence From Commodity Trading Firms

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Abstract

This paper examines the effects of environmental transparency and enforcement regulation on the sourcing patterns of commodity trading firms (i.e., firms that source commodities from producers and distribute them downstream). By exploiting Brazil's priority regulation targeting producers in deforestation-intense municipalities, I find trading firms do not reallocate their sourcing from regulated to unregulated locations. However, treated trading firms reduce their exposure to deforestation and CO_2 emissions associated with their sourcing in both regulated and unregulated locations relative to control firms. Although the effect in regulated locations could reflect the first-order responses by producers, the positive spillover effect in unregulated locations suggests trading firms respond to upstream production shocks and enhance the sustainability of their firm-wide sourcing activities. Importantly, (i) local enforcement actions and (ii) voluntary commitments to zero-deforestation sourcing and third-party audits appear to be crucial factors in altering trading firms' behavior and thereby enhancing the sustainability of commodity sourcing.

Keywords: Deforestation, enforcement, environmental regulation, CO_2 emissions, supply chains, transparency.
JEL Classifications: F14, F18, G38, L14, O13, Q13, Q17, Q56.

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1. Introduction

To reduce the adverse environmental externalities associated with firm-level operating activities, regulatory entities have introduced various transparency and enforcement regulations. For example, California's cap-and-trade program sets limits on greenhouse gas emissions, requiring emission disclosures for covered firms and implementing enforcement and monitoring mechanisms. However, because regulations are typically limited in scope, firms can reallocate their operations that produce environmental externalities—such as carbon emissions and deforestation—to unregulated areas (e.g., Becker and Henderson [2000]; Copeland et al. [2021]). Therefore, without global and coordinated regulation that encompasses all firms, the overall efficacy of any environmental transparency and enforcement regulation critically depends on the decision-making of global supply chain actors.

Commodity trading firms are one such key supply chain actor.[1] These firms operate globally, with their primary role being to buy commodities from producers and distribute them further down the supply chain to markets where these commodities are used and consumed. Therefore, trading firms form a crucial link between supply and demand, facilitating the efficient flow and availability of essential commodities across the globe. In the global grain commodity market, a handful of trading firms dominate the majority of trade (Murphy et al. [2012]), collectively controlling assets exceeding $260 billion and generating over $430 billion in revenues in 2023. Yet, due to the opacity in which trading firms typically operate—most are privately held and do not provide typical disclosures—we lack a systematic understanding of their role in shaping the sustainability of global commodity supply chains. This study addresses the interplay between environmental transparency and enforcement regulation and trading firms' sourcing patterns (and the sustainability thereof).

Specifically, I examine changes in firms' sourcing activities in response to Brazil's priority regulation (formerly known as "blacklisting") that was first implemented in 2008 to combat deforestation. Notably, Brazil is home to the world's largest rainforest, which is threatened

[1] I refer to commodity trading firms as "trading firms" or "firms" throughout the paper.

by deforestation due to commodity production and trade (e.g., Barona et al. [2010]; Arima et al. [2011]). To combat deforestation, the priority regulation intensified transparency and enforcement in selected deforestation-intense locations, known as "priority municipalities." Transparency was increased through a government-disclosed list of these municipalities and the media widely disseminating this "blacklist," while enforcement was strengthened by imposing embargoes and issuing fines on producers in these areas (see e.g., Assunção and Rocha [2019]). I argue that this transparency and enforcement regulation can induce changes in trading firms' sourcing decisions (i.e., where and how they buy commodities), which can significantly enhance the sustainability of local production and trade flow.

Because trading firms source from various geographic locations simultaneously and are not dependent on any single source location, I examine their sourcing patterns in (i) regulated locations ("direct effect") and (ii) unregulated locations from which they source ("firm-wide spillover effect"). Note that changes in sourcing characteristics in regulated locations could reflect the first-order responses of both producers and trading firms to the priority regulation. However, any spillover effect in unregulated locations is likely to indicate changes in trading firms' sourcing strategies when they are exposed to the priority regulation elsewhere. Therefore, the firm-wide spillover effects are of key interest in this study.[2]

If trading firms are sensitive to regulatory shocks at the upstream production level, they could change where and how they source their commodities. First, because the regulation could introduce compliance and litigation costs and could increase external pressure if economic ties between trading firms and producers in deforestation-intense locations are revealed, firms may want to avoid sourcing from regulated locations. In fact, trading firms' supply chain networks are plausibly flexible enough to reallocate their sourcing activities across municipal borders and thereby avoid the costs associated with sourcing in regulated locations. Such avoidance strategies are documented in prior research examining the effects

[2] Prior research finds the priority regulation successfully lowered deforestation in regulated locations (e.g., Arima et al. [2014]; Cisneros et al. [2015]; Assunção and Rocha [2019]; Koch et al. [2019]; Harding et al. [2021]; Assunção et al. [2023]; Damm et al. [2024]). Yet, regulatory attention to the priority regulation diminished over time (Bizzo and De Farias [2017]).

of environmental regulation on firm behavior (e.g., Bartram et al. [2022]; Ben-David et al. [2021]). In contrast to this expectation and findings in prior empirical research, trading firms may not reallocate their operations due to the high switching costs associated with their business model. Particularly, the trade relationships between firms and their source locations tend to be sticky due to large capital investments in local storage facilities, processing plants, and transportation networks (e.g., dos Reis et al. [2020]).

Second, apart from any reallocation decision across regulated borders, the priority regulation could spur firms' active sustainability investments in their supply chain networks because of existing and expected costs related to local deforestation policies. Trading firms exposed to the priority regulation could be incentivized to adopt new technologies and policies that help mitigate deforestation outcomes. For instance, firms can establish traceability initiatives, implement audits, and provide credit to producers who engage in sustainable farming practices. Such outcomes are plausible because firms are already in close relationships with producers, who purchase trading firms' production inputs (e.g., seeds and fertilizer) and financing (Bicudo Da Silva et al. [2020]).

Using trade flow data from Trase, I capture trading firms' sourcing activities linked to soybean production in Brazil. By combining geospatial data and data from per-shipment customs declarations, bills of lading, taxation, logistics, production, and firms' assets and facilities, Trase links soybean-related land-use changes in Brazilian municipalities to trading firms' sourced volume from 2006 to 2019. This data allows me to observe several key variables at the firm-municipality-year level: (i) the tons of soybeans sourced (i.e., sourced volume), (ii) the hectares of deforestation resulting from soybean-related land-use changes associated with firms' sourced volume (i.e., deforestation exposure), and (iii) the tons of CO_2 emissions resulting from soybean-related land-use changes associated with firms' sourced volume (i.e., emissions exposure).[3] Observing deforestation and CO_2 emissions linked to firms' sourced

[3]For simplicity, I refer to firms' *exposure* to CO_2 emissions (and deforestation). Yet, one could also classify the emissions occurring at the producer level as upstream scope 3 emissions or indirect supply chain emissions for trading firms. When trading firms own local land and production, these farm-level emissions would represent firms' scope 1 emissions. Such instances of ownership are, however, unobservable in the data and

volume allows me to infer changes in the sustainability of their sourcing strategies. Although focusing on Brazil and the country's soybean production and exports may seem narrow, the setting is economically important as Brazil has been the largest exporter of soybeans globally since 2013 (Statista [2024]).

To examine the effects of the priority regulation on trading firms' sourcing patterns, I exploit the introduction of the priority regulation across various municipalities and perform a difference-in-differences analysis. I define firms as treated when they source from at least one regulated location. In my main specification, I include firm-municipality fixed effects and firm size-year fixed effects, and I separately assess changes in treated firms' sourcing activities in regulated locations (direct effect) and unregulated locations (spillover effect) from which they source, relative to changes for control firms that do not source in a regulated location. Utilizing this design, I first examine the effects of the priority regulation on treated firms' sourced volume in both regulated and unregulated locations and find no statistically significant effects. The lack of evidence on firms reallocating sourced volume away from or into regulated locations is consistent with the stickiness of the trade relationships between trading firms and source locations (dos Reis et al. [2021]).

I next study the effects of the priority regulation on firms' deforestation and CO_2 emissions exposures. On average, I find improvements in the sustainability of firms' sourcing strategies: firms' deforestation and CO_2 emissions exposures decrease in both regulated and unregulated locations, relative to control firms. First, the direct effect of firms' sustainability improvements in regulated locations is not per se surprising because the regulation incentivized producers to manage previously cleared land (e.g., MMA [2018]; Damm et al. [2024]), allowing trading firms to capitalize on improvements in local farming practices in regulated locations. Nonetheless, I learned from discussions with key stakeholders that trading firms play an important role in shaping local production practices even in regulated locations. For instance, in one priority-flagged location, firms established a new quality control entity that

anecdotally less common.

provides assurance services of the origins of soybeans.[4] Second, the documented positive spillover effects—that is, improvements in treated firms' deforestation and CO_2 emissions exposures in *unregulated* locations—suggest that firms whose sourcing network is impacted by the priority regulation adopt policies that improve the sustainability of their *firm-wide* operations. These findings are consistent with the following two non-mutually exclusive mechanisms: firms adjust their trade relationships (e.g., firms select new, responsible producers) or firms invest in existing trade relationships and sustainable sourcing practices (e.g., firms invest in quality control mechanisms).

Finally, I test whether two key forces moderate the observed positive spillover effects of firms' reduced deforestation and CO_2 emissions exposures in unregulated locations. First, I argue that upstream shocks are more costly for firms sourcing from locations where stringent enforcement actions occur. Because producers can face penalties and entire farming facilities can be embargoed by Brazil's environmental enforcement agency (i.e., "IBAMA," Brazilian Institute of Environmental and Renewable Resources), firms' supply chain operations can experience non-trivial pressure. To operationalize the concept of firms' vulnerability to local enforcement actions, I create a variable that is based on publicly available embargo data. Overall, I find that the positive spillover effect occurs only in the subsample of firms vulnerable to high levels of enforcement actions. These results cast doubt on the notion that transparency shocks alone—such as in the form of firm or governmental disclosures—can drive commodity trading firms to adopt more sustainable sourcing practices.

Although the above heterogeneous results are consistent with trading firms shaping their sourcing strategies in response to enforcement, they could also reflect, in part, the direct responses of producers to enforcement. To better isolate firm-level incentives and responses, I next focus on another moderating characteristic: firms' commitment to zero-deforestation sourcing and third-party audits. I hand-collect information on whether firms are signatories of the Amazon Soy Moratorium, the first voluntary zero-deforestation agreement concerning

[4] Further insights gathered from an interview with Adnan Demachki, former mayor of a municipality that was listed for priority action, are summarized in Internet Appendix Table IA.3.

soybeans. I argue that firms committed to zero-deforestation sourcing face additional pressures from external stakeholders when their sourcing operations are linked to deforestation-intense locations or producers and therefore adopt firm-wide policies to align their real activities with their stated commitments. In line with this expectation, I find that the positive spillover effects are concentrated in signatory firms, consistent with the notion that firms' commitments to zero deforestation are in line with their real activities. In addition, the estimated magnitudes of the positive spillover effects increase when firms also comply with the audit requirements of the Soy Moratorium, which includes spatial monitoring and supplier verification by a third party. In sum, I find that firms' commitments to zero-deforestation sourcing and the verification of their purchase activities through third-party audits are important factors for the observed firm-wide spillover effects.

The contributions of this paper are three-fold. First, I add to the literature on corporate sustainability in the context of supply chains (e.g., Dai et al. [2021]; She [2021]; Darendeli et al. [2022]; Koenig and Poncet [2022]; Baik et al. [2024]; Bisetti et al. [2024]). Prior research often classifies supply chains as the economic interactions between two broad groups: supplier firms and customer firms. Yet, global supply chains are typically more complex and comprise various actors with different incentives. I introduce the interactions between producers and commodity trading firms, previously understudied given the opacity in which both parties operate. Given the limited scope of current policies targeting supply chain sustainability—such as mandatory reporting regimes—understanding the decision-making of commodity trading firms is important. I find that trading firms, which often establish dependencies with producers and heavily invest in local infrastructure near their source locations, can play a crucial role in shaping global supply chain sustainability.

Second, I add to the literature on the real effects of non-financial regulation (e.g., Jin and Leslie [2003]; Leuz and Wysocki [2016]; Christensen et al. [2017]; Chen et al. [2018]; Rauter [2020]; Downar et al. [2021]; Bartram et al. [2022]; Fiechter et al. [2022]). Most prior research examines firms *directly* affected by a regulation, testing for changes in various firm outcomes.

I study the effect of a regulation implemented in the upstream supply chains on firms further downstream by examining changes in trading firms' sourcing patterns. Prior research in environmental economics studies the effects of the same regulation on municipality-level outcomes and finds improvements in deforestation in regulated locations (e.g., Arima et al. [2014]; Cisneros et al. [2015]; Assunção and Rocha [2019]; Koch et al. [2019]; Harding et al. [2021]; Assunção et al. [2023]; Damm et al. [2024]). I add to these papers by focusing on the role of trading firms, which allows me to assess firm-wide spillover effects in unregulated locations when firms are affected by the priority regulation elsewhere. My findings imply that local enforcement actions and commitments to zero deforestation combined with third-party purchase verification are key moderating forces in the direction and extent to which firms adopt more sustainable sourcing practices that contribute to the efficacy of the priority regulation.

Third, I add to the literature on the measurement of corporate sustainability performance. Prior research captures variation in firms' reported ESG activities or third-party ESG ratings (e.g., Berg et al. [2022]) or employs data on firms' (self-reported) emissions (e.g., Matsumura et al. [2014]; Griffin et al. [2017]; Ben-David et al. [2021]; Jouvenot and Krueger [2021]; Dai et al. [2022]; Tomar [2023]). I utilize data on the economic ties between trading firms and their source locations to capture their exposure to real environmental externalities associated with their sourced volume. A key benefit of using this data is to overcome the challenges caused by the opacity, complexity, and geographical dispersion of global supply chains.

2. Institutional background

2.1. History of Brazil's forest protection policies

This section describes the evolution of environmental regulation in Brazil, which is characterized by a shift to disincentivizing deforestation activities, and provides context for the specific regulation examined in this paper. During the military dictatorship between 1964 and 1985, public policies emphasizing large-scale infrastructure development and colonization projects led to major deforestation in the Amazon (e.g., Mahar and Banco [1989]; Fearnside [2005]). Today's patterns in deforestation can be linked to the development of infrastructure, with the majority of forest loss occurring near transportation networks such as roads and rivers (Barber et al. [2014]; MMA [2018]). Early forest protection was largely outlined in the Forest Code of 1965, which regulates land use and management and originally required private landowners to maintain 50% of their native land (e.g., Soares-Filho et al. [2014]). Yet, environmental laws were typically violated (Chaves [1979]), and Mahar and Banco [1989] declared the Forest Code as "unenforceable."

In the late 1980s, Brazil's government ended fiscal subsidies that incentivized deforestation and demarcated land for indigenous people and conservation. Deforestation remained high due to (i) high prices and international demand for agricultural commodities (Fearnside [2005]) and (ii) practices of illegal land grabbing and speculation (Stabile et al. [2020]). Subsequent policy changes that strengthened the Forest Code and stricter environmental regulations that govern deforestation on private lands did not reduce deforestation because of weak enforcement (Soares-Filho et al. [2014]). In response to internationally growing concerns about Amazon deforestation in the early 2000s, Brazil articulated new rainforest conservation policies in the Action Plan to Prevent and Control Deforestation in the Legal Amazon (PPCDAm). The PPCDAm is split into four phases and characterized by the following three pillars: (i) land planning policies, (ii) monitoring and control, and (iii) the promotion of sustainable activities (see West and Fearnside [2021] for a summary).

During the first phase of PPCDAm (2004–2008), the introduction of real-time satellite monitoring was a primary contributor to forest surveillance and protection. The high temporal resolution of the "DETER" satellite system allowed for quick deforestation detection and coordinated enforcement actions (Diniz et al. [2015]). Yet, the low spatial resolution meant deforestation of fewer than 25 hectares remained undetected (MMA [2018]). Consistent with local producers adjusting to this minimum mapping unit, the introduction of satellite monitoring was associated with an increase in small-patch deforestation and a decrease in large-patch deforestation (Rosa et al. [2012]; Kalamandeen et al. [2018]; MMA [2018]). Although the various policies helped to reduce deforestation, deforestation activities remained high and even increased from 2007 to 2008 (Brazil [2009]; West and Fearnside [2021]). To take further action, the government announced a key policy change that I exploit in this paper: the priority regulation.

2.2. Environmental transparency and enforcement regulation: priority regulation

Brazil's former president Luiz Inácio Lula da Silva established the legal basis for the priority regulation in the presidential Decree 6.312/2007. The decree allows the government to take priority command and control actions in locations with intense deforestation activities. The locations are selected according to three criteria: (i) cumulative deforestation area, (ii) cumulative deforestation area over the prior three years, and (iii) increased deforestation rates in three out of the five prior years. Brazil's National Institute for Space Research verifies the selection process and the Ministry of the Environment publishes the priority list periodically.[5] In 2008, the government targeted the first 36 municipalities, which accounted for 45%-50% of total prior-year Amazon deforestation (e.g., Arima et al. [2014]; Assunção and Rocha [2019]). The first list of regulated locations was announced in the Diário Oficial da União, the official federal government's newsletter, and then disseminated by all large local news providers (see Internet Appendix Figure IA.1), which initially referred to the list of the "worst deforesting municipalities" or the "blacklist."

In addition to disclosing deforestation-intense municipalities, the government strengthened enforcement actions in regulated locations. IBAMA, which acts as the federal environmental police, received more resources to enforce land-use policies by issuing fines, imposing embargoes, confiscating agricultural production, and seizing or destroying goods or machinery (Assunção et al. [2013]; Bizzo and De Farias [2017]). Also, private landowners must register their properties with Brazil's Rural Environmental Registry and need deforesta-

[5] See Appendix B for a summary of the list of priority municipalities.

tion permits to clear new land. Landowners self-declare the size and spatial boundaries of their landholdings, which enhances transparency about land management and enables environmental law enforcement (Soares-Filho et al. [2014]). Other rules include federal credit constraints for agriculture and other forestry activities (Arima et al. [2014]).

Prior research finds evidence consistent with the success of this regulation (e.g., Arima et al. [2014]; Cisneros et al. [2015]; Assunção and Rocha [2019]; Koch et al. [2019]; Harding et al. [2021]; Assunção et al. [2023]), which is largely attributed to the policy's intensified monitoring and enforcement actions. Yet, because of diminished attention to the priority regulation over time, the success of the regulation is attributed to its early years (Bizzo and De Farias [2017]). After the publication of the first list in 2008, seven municipalities were added in 2009 and 2010, respectively, and two municipalities were targeted in 2012, with no subsequent emphasis on the priority regulation. In 2016, the government announced a continuation and renewed focus on the priority regulation (Bizzo and De Farias [2017]; West and Fearnside [2021]) and updated the priority list again in 2017 and the years thereafter.

2.3. The effectiveness of environmental transparency and enforcement regulations

Extensive prior research examines the effectiveness of various public and private policies aimed at reducing environmental externalities, such as carbon emissions and deforestation. Prior empirical research documents negative environmental spillover effects in various settings (e.g., Becker and Henderson [2000]; Copeland et al. [2021]). For instance, Bartram et al. [2022] exploit California's cap-and-trade rule and examine firm-level emissions and resource reallocation decisions. They find that financially constrained firms reallocate their emissions away from regulated to unregulated areas because of excessive regulatory costs. Ben-David et al. [2021] find multinational firms export their emissions to countries with laxer environmental regulation when their home country implements stricter environmental regulation. In addition, using the staggered adoption of mandatory environmental, social, and governance (ESG) disclosure across various countries, Fiechter et al. [2024] find that downstream customer firms shift contracts from public to private suppliers. Given that mandatory ESG disclosure increases the level of supply chain transparency, their finding is consistent with the idea that firms have a preference for some level of supply chain opaqueness.

Concerning the regulation examined in this paper, prior research finds that the priority regulation is effective in reducing deforestation in regulated locations (e.g., Arima et al. [2014]; Cisneros et al. [2015]; Assunção et al. [2015]). In addition, the Amazon Soy Moratorium, a private zero-deforestation initiative, successfully reduced deforestation (e.g., Nepstad et al. [2014]; Gibbs et al. [2015]). At the same time, recent studies document a displacement of deforestation from areas protected (e.g., Amazon) to areas excluded from public and private policies (e.g., Cerrado) (e.g., Moffette and Gibbs [2021]). Several studies document the effects of this "leakage" due to policies that are limited in scope (e.g., Arima et al. [2011]; Alix-Garcia and Gibbs [2017]; Lima et al. [2019]).[6]

In sum, prior research documents carbon and deforestation leakage when regulations are limited in scope. However, little is known about the decision-making of trading firms in response to environmental regulation. Because trading firms operate in opaqueness and source from various geographic locations, it is unclear whether and the extent to which environmental transparency and enforcement regulation can induce changes in trading firms' sourcing behavior.

2.4. Commodity trading firms

In the most simplified form, a commodity supply chain can be dissected into upstream and downstream operations (see Figure 2). The upstream of supply chains forms the origins of any product. Upstream activities are typically described as production activities (e.g., extraction of minerals, plantation of soybeans, raising of cattle), while downstream activities encompass manufacturing and retail operations. In this paper, I focus on trading firms that

[6]In general, assessing the effectiveness of zero-deforestation commitments is challenging due to differences in the scope and ambition, a lack of data availability (e.g., Lambin et al. [2018]), and a lack of a globally consistent definition of what constitutes a forest (e.g., Leijten et al. [2020]). Definitions vary based on land span, tree density, tree height, and canopy cover (Chazdon et al. [2016]; Lambin et al. [2018]).

operate in the “midstream” of supply chains and thereby form an important linkage between upstream production and downstream consumption operations. Trading firms are typically responsible for the sourcing and distribution of commodities and thereby take on a strategic position by linking supply and demand (Murphy et al. [2012]). The regulation discussed in this paper shocks the upstream of supply chains but its forces can have transmission effects further downstream.

Even though a focus on trading firms may appear limited, their importance cannot be understated. Typically, a few multinational firms operating in the midstream (and downstream) of supply chains control the majority of global agricultural commodity supply chains (e.g., Murphy et al. [2012]; Craviotti [2016]) and create dependencies with producers who rely on their production inputs (e.g., seed and fertilizers) and financing (Bicudo Da Silva et al. [2020]). According to estimates, the largest multinational commodity trading firms account for more than 70% of global soybean trade, and in Brazil, they provide more than 60% of total financing to soybean producers, thereby assuming the role of a lender as well (Murphy et al. [2012]). Moreover, trading firms largely invest in local infrastructure, such as ports, railways, and highways, and the establishment of such trade networks is a key driver behind today’s deforestation patterns (Barber et al. [2014]). Therefore, trading firms’ influence over local production implies that they can meaningfully contribute to the efficacy of environmental regulation and improve global supply chain sustainability. Whether trading firms invest in the sustainability of their supply chain network is, however, unclear. Transparency across global commodity supply chains is scarce, as most trading firms are privately-held entities and do not provide typical disclosures. Combined with producer-level activities being mostly in the hands of small- and medium-sized family farms, the production and trade stages of commodity supply chains remain opaque and understudied.

3. Conceptual underpinnings

3.1. The priority regulation and commodity trading firms' sourcing patterns

The priority regulation increased transparency and enforcement in deforestation-intense locations from which trading firms source their commodities. Anecdotal evidence suggests this regulation exhibits pressures throughout the supply chain:

> The federal public prosecutor [. . .] followed the supply chain back from the supermarkets through the beef companies to the ranchers to find out which animals had been produced on illegally deforested land, and threatened the supermarket with prosecution. "They reacted fast," [. . .] "It was about their brand, their visibility to the public." Brazil's supermarket association—which includes Walmart and Carrefour—said its members would stop buying beef from recently deforested land. But [the public prosecutor] was not alone in applying economic pressure. The International Finance Corporation, the private-finance arm of the World Bank, withdrew a loan it had promised to Bertin [. . .] to expand its facilities in the Amazon.
>
> Although small farmers continue to clear land in areas where the authority of the state is weak, the big beef and soya companies that used to [deforest] themselves or buy produce from those [farmers] that [deforested land] no longer want anything to do with it. (Economist [2013])

The anecdotal evidence implies that trading firms may be sensitive to upstream supply chain shocks and therefore change their sourcing strategies. First, whether the priority regulation incentivizes firms to reallocate their operations across regulated borders is ex-ante unclear. On the one hand, firms could reallocate their operations if it becomes too costly to continue sourcing from regulated locations. The regulation can introduce compliance and litigation costs and can increase external pressure if economic ties between trading firms and producers in deforestation-intense locations are revealed. For instance, the Federal Public Ministry of Brazil (Ministério Público Federal)—which acts as the federal prosecutor—intensified the pressure on various supply chain actors after the priority regulation, finding some trading firms "complicit" in illegal deforestation and land grabbing activities (Arima et al. [2014]). Because agricultural commodities are grown by thousands of different producers, trading firms are not dependent on any single producer or source location and therefore could reallocate their activities to forgo the costs of the regulation.[7] Such avoidance and

[7]For instance, ADM states the following: "*The Company's raw materials are procured from thousands of*

substitution behaviors are documented in prior research examining the effects of regulation in several non-financial settings (e.g., Becker and Henderson [2000]; Jin and Leslie [2003]; Rauter [2020]).

On the other hand, firms may not reallocate their operations to unregulated locations if switching costs are relatively too high. For example, dos Reis et al. [2020] find that trade relationships between trading firms and sourcing locations can be sticky. This stickiness occurs due to firms making large capital investments in storage facilities, processing plants, and transportation networks (e.g., highways and river ports) at the local level. Notably, these investments are specific to trading firms' business model and can shape their business decisions. Despite the stickiness of trade relationships, producers operating in regulated locations are incentivized to invest in previously cleared land (MMA [2018]). If producers update their farming practices and avoid deforesting new areas, trading firms could benefit from producer-level improvements and must not reallocate their operations.

Second, apart from any reallocation decision across regulated borders, the priority regulation could spur firms' active sustainability investments in their supply chain networks because of existing and expected costs related to local deforestation policies.[8] Therefore, the priority regulation could incentivize firms to adopt new technologies and policies that help mitigate their exposure to deforestation. Because trading firms sell production inputs (e.g., seeds and fertilizer) and, in Brazil, provide most of the financing to producers (Bicudo Da Silva et al. [2020]), firms can closely monitor their producers and contract with those that farm sustainably. For example, firms can establish traceability initiatives and implement third-party audits to verify the sourcing of their commodities. A combination of active investments in firms' existing supply chain network and the selection of new producers could

growers, grain elevators, and wholesale merchants pursuant to short-term agreements (less than one year) or on a spot basis. The Company is not dependent upon any particular grower [...] as a source for its raw materials" (10-K, fiscal year 2008).

[8] Despite such pecuniary incentives to improve the sustainability of supply chain networks, trading firms can have non-pecuniary preferences for sustainable sourcing and learn about new conservation practices. However, these different incentives are non-mutually exclusive and difficult to disentangle, so I treat these incentives as bundled.

have firm-wide spillover effects beyond the borders of regulated locations.

On the contrary, firms may not improve the sustainability of their sourcing activities. If this is the case, my results would likely still capture a positive direct effect in regulated locations suggesting the sustainability of firms' sourcing strategies improves in these areas (because of the first-order responses of producers, and not trading firms, to the regulation). However, I should not document firm-wide spillover effects in unregulated locations if firms do not update and invest in their sourcing operations after being exposed to the priority regulation elsewhere. Therefore, the firm-wide spillover effects are of key interest when examining changes in trading firms' sourcing characteristics.

3.2. Heterogeneous effects

I next argue that the firm-wide spillover effects are likely heterogeneous in two firm characteristics: firms' vulnerability to local enforcement actions and firms' commitments to zero-deforestation sourcing. First, IBAMA, Brazil's environmental enforcement agency, received more financial resources to fight illegal deforestation and support the priority regulation (Assunção et al. [2013]; Bizzo and De Farias [2017]). The agency combats illegal deforestation activities by issuing fines, placing embargoes on production sites, and confiscating production equipment and agricultural output resulting from deforested land (Sousa [2016]). If producers receive negative sanctions and source locations are more frequently visited by the federal environmental enforcement agency, trading firms can experience costly disruptions to their sourcing activities when they source from these producers. Therefore, trading firms are likely to have strong incentives to limit their sourcing from deforestation-intense locations and deforestation-risky producers. To do so, firms can avoid those producers flagged for illegal forest activities by IBAMA and monitor areas using satellite imagery.

Second, firms that make zero-deforestation commitments could face additional pressures from external stakeholders when their sourcing operations can be linked to deforestation activities. For instance, Greenpeace [2006] shamed three soybean trading firms due to their (partially illegal) investments in the Brazilian Amazon and their contribution to deforestation. Therefore, I expect firms with such sustainability commitments to act according to their pledges, avoid deforestation-risky areas, and adopt firm-wide policies that foster deforestation-free supply chains. This argument builds on Bursztyn and Jensen [2017], who state that firms committed to sustainability have strong incentives to maintain their public image as "environmentally friendly." In fact, if external stakeholders reveal a firm's actions to be associated with poor environmental practices, that firm's reputation and market position can be negatively affected (Baron [2001]; Rueda et al. [2017]) and the firm risks being cut off by corporate customers (Gibbs et al. [2016]; Bisetti et al. [2024]). Therefore, the priority regulation can function as a disciplining force for firms with a zero-deforestation commitment to improve the sustainability of their firm-wide sourcing operations.

4. Data and research design

4.1. Data and sample on soybean commodity trading firms

I obtain trade flow data from Trase, a private initiative founded by the Stockholm Environment Institute and Global Canopy, that aims at improving transparency and accountability of commodity supply chains. I gather the dataset covering commodity trading firms sourcing soybeans from Brazilian municipalities.[9] Broadly, Trase creates trade flow maps by linking firms' sourcing to the smallest jurisdictional level in a country and year, and then calculating firms' exposure to environmental externalities (primarily deforestation). Regarding the Brazilian soybean supply chain, firms' sourcing operations are linked to municipalities from 2006–2019.[10] To capture the trading flow at the firm-municipality-year level, Trase collects data from per-shipment custom declarations, bills of lading, taxation, logistics, production, and other firm-level data (e.g., assets and facilities). In the case of Brazil, municipal tax numbers are recorded in all per-shipment custom declarations and used as key indicators

[9] The data was downloaded in 2021. For more information on the data construction and trade flow model, see Trase [2018] and Godar et al. [2015].

[10] Brazil is divided into 26 states and one federal district. The states are further divided into municipalities, which constitute the lowest level of political and administrative division in Brazil.

to create the firm-municipality-year linkages. Trase then combines maps of soybean production and deforestation to calculate firms' exposure to deforestation and related greenhouse gas emissions in a given municipality-year.

First, to capture deforestation at the municipality-year level, Trase calculates the hectares of deforestation that are due to soybean production.[11] To link deforestation events to soybean production, Trase considers a so-called allocation period and lag period that varies per commodity and country. In the case of soybeans, the allocation period is five years and the lag period is one year. The five-year allocation period acknowledges that land is typically not suitable for soybean plantations in the first years after deforestation (Osorio [2018]) and captures most crop-related deforestation in Latin America (Graesser et al. [2018]; zu Ermgassen et al. [2020]). The one-year lag period captures the time it takes to harvest soybeans. Second, to create linkages between trading firms and municipality-level deforestation estimates, Trase allocates the hectares of deforestation to firms based on the volumes of soybeans sourced from producing plants in that municipality. When multiple trading firms source from a municipality and no information on the exact trade relationships is available, soybean deforestation is allocated proportionally to trading firms using information on the firm's sourced volume from that municipality, the municipality's total soybean production, and the municipality's soybean-related deforestation. Finally, Trase estimates the total greenhouse gas emissions resulting from deforestation and the loss of biomass and translates this estimate into a gross carbon dioxide equivalent. I use these data to compute estimates for sourced volume, deforestation exposure, and CO_2 emissions exposure at the firm-municipality-year level. Sourced volume is defined as a trading firm's aggregate soybean volume (in tons, deflated by 10,000) sourced from plants in a given municipality and year ($Volume_{imt}$). Similarly, a firm's exposure to deforestation (in hectares, deflated by 100) and

[11] According to Trase, deforestation is defined as the primary loss of native vegetation including the loss of forests, savannahs, and grassland. The data captures both deforestation and advanced stages of forest degradation. While the data mostly captures *direct* deforestation, that is, deforestation that occurs with the intent to produce a certain commodity, it also partially captures *indirect* deforestation. Indirect deforestation occurs when a commodity is produced on deforested land but then replaced by another, typically more profitable, commodity.

CO_2 emissions (in tons, deflated by 10,000) are aggregated at the firm-municipality-year level ($DefExposure_{imt}$ and $CO2Exposure_{imt}$, respectively). Appendix A details the variable definitions.

When determining a firm's municipality source location is not possible, Trase labels the municipality as "unknown." I eliminate such instances of unknown firm-municipality linkages. I also drop observations linked to (i) missing treatment assignments, (ii) missing trading firm name identifiers (e.g., this typically occurs when the soybean volume is linked to domestic consumption rather than export), (iii) firms in the lowest two ranks of a firm size quintile rank variable, and (iv) data errors (i.e., when estimated soybean land use is smaller than the estimated deforestation for soybeans in that municipality-year). I obtain a final sample of 7,932 firm-municipality-year observations representing 319 unique trading firms (of which 45 are treated).

4.2. Research design

I exploit the staggered introduction of the priority regulation across various municipalities to identify the effects of environmental regulation on trading firms' sourcing patterns across their firm-wide operations. Because trading firms can source from plants in various locations simultaneously, I investigate changes in their sourcing patterns from (i) regulated locations and (ii) unregulated locations. Appendix B presents the list of priority municipalities and Figure 1 shows the geographical locations of regulated locations and unregulated locations from which treated firms source. I estimate the following two-way fixed effects difference-in-differences regression using OLS in a panel of firm-municipality-years, with standard errors clustered at the municipality level, in line with the implementation of the regulation:[12]

$$Y_{imt} = \beta_1 Treated\_Direct_{imt} + \beta_2 Treated\_Spillover_{imt} + \alpha_{im} + \alpha_{zt} + \varepsilon_{imt}, \tag{1}$$

where i indexes firm, m indexes municipality, t indexes year, and z indexes a tercile rank

[12]In a robustness test, I cluster standard errors at the firm level and calculate wild bootstrapped standard errors. My results remain similar (see Internet Appendix Table IA.2).

of firm size. Y is the dependent variable and is either $Volume_{imt}$, $DefExposure_{imt}$, or $CO2Exposure_{imt}$. $Treated\_Direct_{imt}$ is an indicator variable equal to 1 for firms' sourcing from plants in regulated locations when the priority regulation is in place, and 0 otherwise. $Treated\_Spillover_{imt}$ is an indicator variable equal to 1 for all unregulated locations when a firm sources from plants in at least one regulated location at that time and the priority regulation is in place, and 0 otherwise.[13] α_{im} and α_{zt} are sets of firm-municipality and firm size-year fixed effects, respectively.[14] β_1 measures the direct effect for firms sourcing from regulated locations. β_2 measures the firm-wide spillover effect for treated firms sourcing from unregulated locations.

My identification strategy can be illustrated as follows: suppose a firm sources soybeans from plants located in Alta Floresta, Novo Mundo, and Princesa in Brazil. In 2006 and 2007, the priority regulation did not exist, so no location was regulated and no soybean-producing plant was affected. In 2008, the government announced the priority list including Alta Floresta. Accordingly, the $Treated\_Direct_{imt}$ indicator variable switches from 0 to 1 only for the firm's sourcing linked to production in Alta Floresta, while the $Treated\_Spillover_{imt}$ indicator variable switches from 0 to 1 for all other firm-municipalities (i.e., Novo Mundo and Princesa). The control group includes firms that are not treated at that time. Figure 3 illustrates the research design graphically.

There are three identifying assumptions underlying my research design: (i) parallel trends, (ii) the Stable Unit Treatment Value Assumption, and (iii) no anticipation. First,

[13] Although both $Treated\_Direct_{imt}$ and $Treated\_Spillover_{imt}$ primarily switch "on" from 0 to 1, $Treated\_Spillover_{imt}$ can also switch "off" from 1 to 0. The latter case occurs when a location was initially identified as a "spillover" location but that location is later selected under the priority regulation. Also, in the infrequent cases when a firm observation is linked to a regulated location in year t but there are missing firm observations linked to that location in subsequent years, I set $Treated\_Spillover_{imt}$ to missing in those instances. Results are similar when I instead set $Treated\_Spillover_{imt}$ equal to 1 in these instances (untabulated).

[14] The inclusion of firm-municipality fixed effects allows for an interpretation of the estimates as the average within-firm-municipality change in sourcing characteristics for treated firms. Although this design is favorable in representing the granularity at which sourcing decisions are made and the level of implementation of the priority regulation, it limits the extent to which I capture firms' new trade relationships in new municipalities post-regulation in my estimates. I find that spillover estimates are similar when I employ panel fixed effects at a broader level, such as firm-*state* fixed effects (untabulated).

the key identifying assumption is that, absent the priority regulation, treated firms' sourced volume, deforestation exposure, and CO_2 emissions exposure evolve similarly to those of control firms. Figures 4 and 5 provide graphical support for this assumption by plotting the outcome variables for spillover-treated plants against the control group showing similar pre-trends in the outcome variables.[15] Also, it should be the case that no firm-municipality-level shocks are simultaneously correlated with the priority regulation and the outcomes of interest.[16] Second, I allow for spillover effects in my research design (across treated firms' regulated and unregulated locations) but assume no spillover effects occur from treated to control firms. This assumption can be violated when treated and control firms operate within the same location in so-called "spillover" locations. To address this concern, I re-define the control group and exclude observations linked to control firms in spillover locations in an alternative, "adjusted" main design.[17] Third, I assume no anticipation of the priority regulation. This assumption is plausible given that the regulation was not announced in advance. Since the producers operating in regulated locations did not anticipate the policy change, firms further down in supply chains are unlikely to have anticipated the regulation.[18]

[15] Internet Appendix Figure IA.2 provides graphical support for plotting the direct effects.

[16] Recent econometric advances in difference-in-differences designs with staggered treatment timing emphasize the potential pitfalls when heterogeneity exists in the treatment effect across units or over time (see, e.g., Callaway and Sant'Anna [2021]; Goodman-Bacon [2021]; Sun and Abraham [2021]; de Chaisemartin and D'Haultfoeuille [2022]; Roth et al. [2023]). I conduct robustness tests in which I drop the post-treatment observations for all later-treated firm-municipalities (i.e., trading firms that source from plants in regulated locations after 2008), therefore alleviating concerns associated with estimating treatment effects for later-treated cohorts. My results are similar (untabulated).

[17] In addition, prior studies suggest deforestation rates are correlated across space and over time (e.g., Pfaff [1999]). In the context of the priority regulation, Andrade [2016] documents spillover effects from regulated locations to unregulated neighboring locations, indicating producers experienced relatively higher levels of enforcement in neighboring source locations. Because the key goal of this study is to capture firm-wide spillover effects at the trading firm level, in a robustness test, I address such producer-level spillover effects that can introduce bias in my estimation. I exclude (i) direct neighbors and (ii) close locations of regulated municipalities. Using these alternative designs, I find similar results (see Internet Appendix Table IA.1).

[18] See e.g., www.ecosystemmarketplace.com. Even if this assumption is violated, note that prior forest protection policies typically failed to combat deforestation and several agencies lacked resources to effectively address deforestation occurrences (e.g., West and Fearnside [2021]). In addition, I assume no reversibility of the direct treatment. That is, once a firm sources from at least one regulated location, it remains "treated" for the entire sample period.

5. Empirical results

5.1. Descriptive statistics

Table 1 presents the summary statistics of the variables used in this paper. Panel A provides descriptive statistics for the full sample of firm-municipality-year observations. I find the average (median) firm sources about 56,130 (21,080) tons of soybeans from producers in a given municipality m and year t. This amount can be linked to firms' exposure to about 123 hectares of deforestation and 21,740 tons of CO_2 emissions due to soybean-related land-use changes at their source location. In Panel B, I present descriptive statistics for samples conditional on treatment status and the years before treatment starts (i.e., pre-2008). On average, I find firms source about 64,840 tons of soybeans from regulated locations and these sourcing activities are associated with high levels of deforestation and CO_2 emissions (1,128 hectares and 365,740 tons, respectively). Treated firms source similar tons of volume from unregulated locations but are less exposed to deforestation and CO_2 emissions in these locations. These descriptive insights are unsurprising given that the priority regulation targets locations with the historically highest deforestation levels and rates. Although control firms sourcing from unregulated locations acquire on average lower volumes of soybeans (19,120 tons), firms' exposure to deforestation and CO_2 emissions *per* sourced volume is similar across treated and control firms' source locations (untabulated).[19]

Next, about 72% of firm-municipality-year observations relate to firms with signatory status in the Amazon Soy Moratorium, which corresponds to about 20% of firm-year observations and about 9% of firm observations (untabulated). In addition, about 59% of firm-municipality-year observations relate to firms with signatory status *and* an additional commitment to verify their purchases and financing through third-party audits under the Soy Moratorium. This statistic relates to about 11% of firm-year observations and about 5% of firm observations (untabulated). The stark difference in the mean of $SoyM_i$ ($SoyM\_Audit_i$)

[19]Note that sourced volume, deforestation exposure, and CO_2 exposure exhibit sizeable standard deviations with large right-tail values. The empirical results are similar (but of smaller magnitude) when winsorizing outcome variables at the 1[st] and 99[th] percentiles of their distributions (untabulated).

between firm-municipality-year and firm observations highlights the dominance of a few large players in commodity supply chains that participate in zero-deforestation commitments and source large quantities of soybeans from multiple locations. In fact, trading firms source from 40 distinct municipalities in a given year, on average. Yet, substantial variation exists in firms' supply chain networks, as firms' sourcing activities range from a minimum of one location to a maximum of 117 distinct locations (untabulated). Moreover, an average firm-municipality-year observation receives 2 embargoes by IBAMA. At the municipality-year level, I find an average of 1.4 embargoes and in the 99^{th} percentile of its distribution 255 embargoes (untabulated). Finally, 6.7% of observations belong to firms' source locations that are listed for priority action ($Treated\_Direct_{imt}$) during my sample period, whereas 46% of observations belong to unregulated locations from which treated firms source ($Treated\_Spillover_{imt}$).

5.2. Commodity trading firms' sourced volume

Table 2 presents the effects on treated firms' sourced volume in regulated locations and unregulated locations from which they source.[20] Column (1) presents the basic design, comparing the sourcing activity of treated firm-municipalities with non-treated firm-municipalities and thereby pooling the direct and spillover effect estimates. In this design, I include firm, municipality, and year fixed effects separately. In column (2), I implement the main design as referred to in equation (1), presenting coefficient estimates for treated firms' sourcing from plants in regulated locations ($Treated\_Direct_{imt}$) and sourcing from plants in unregulated locations ($Treated\_Spillover_{imt}$) separately, while using firm × municipality and firm size × year fixed effects (relative to control firms). In columns (3) and (4), I implement the main design but make adjustments to the control group to reduce concerns about spillover effects

[20]Note that all firm-municipality-year observations have nonzero sourced volume. In my research design, I therefore only investigate intensive margin effects. It is difficult to impute zeros and therefore estimate extensive margin effects because I cannot observe the reason for missing firm-municipality-year linkages. For instance, a missing link can indicate that a firm exited a location but it can also indicate that producers rotated crops (e.g., soybeans and corn) or experienced a harvest failure in a given year (or simply, data limitations in gathering and linking the full scope of shipment data).

from treated to control firms. Particularly, I exclude observations linked to control firms operating simultaneously with treated firms in so-called spillover locations. Utilizing this adjusted main design, I also estimate the effect for treated firms (relative to control firms) only in unregulated locations in column (4).

Overall, I find no statistically significant evidence of changes in treated firms' sourced volume in either regulated or unregulated locations. Figure 4 supports these findings graphically for firms' spillover-treated locations. The "no-reallocation" finding is consistent with the stickiness of trading firms' source locations due to large capital investments at the local level (dos Reis et al. [2021]) and with trading firms capitalizing on the improvements in local farming practices in regulated locations (Damm et al. [2024]). Importantly, however, the point estimates are not precise enough to rule out firms' reallocation of sourced volume across regulated borders following the priority regulation. For instance, there could be heterogeneity in firms' strategies to contract with certain producers and source locations not accounted for in the research design. Finally, these findings do not preclude firms from adjusting their trade relationships *within* regulated locations or across unregulated locations (which could be reflected in my results below).

5.3. Commodity trading firms' deforestation and CO_2 emissions exposures

I next examine whether trading firms' sourcing activities become more sustainable in response to the priority regulation. Panels A and B of Table 3 present results for deforestation and CO_2 emissions exposures, respectively. Figure 5 supports these findings graphically. In my discussion of the results, I emphasize firms' deforestation exposure, as the interpretations using firms' CO_2 exposure as the outcome variable are the same.

In column (1) of Panel A, I find the overall effect on treated firms' deforestation exposure is negative and statistically significant. In column (2), I find deforestation exposure decreases by about 901 hectares in regulated locations.[21] This coefficient remains similar when using

[21] In terms of magnitude, 901 hectares is approximately half of a standard deviation in the within-fixed-effects variation of deforestation exposure in regulated municipalities before 2008. This large magnitude

the control-adjusted main design in column (3). Similar to the findings in prior research, my results suggest a positive impact of the priority regulation on the sustainability of firms' sourcing activities in regulated locations. Because this paper is concerned with firm-wide spillover effects, I next turn to the results related to firms' sourcing in unregulated locations. Overall, I find evidence for the existence of a positive firm-wide spillover effect. In column (2) of Panel A, I find that treated firms reduce their exposure to deforestation by about 163 hectares in unregulated locations when they are exposed to transparency and enforcement shocks in regulated locations, compared to control firms. The magnitude is economically meaningful: 163 hectares is approximately a quarter of a standard deviation in the within-fixed-effects variation of deforestation exposure in unregulated municipalities before 2008. Although the magnitude of the coefficient estimates on $Treated\_Spillover_{imt}$ attenuates when adjusting for contamination in the control group in columns (3) and (4), the findings remain statistically significant and economically important.

I conclude that treated firms reduce their deforestation and CO_2 emissions exposures in both regulated locations and unregulated locations. I reemphasize that the direct effect is likely to capture the first-order responses of both producers and trading firms. In the extreme, the direct effect may be entirely attributable to changes in producer-level farming practices. That is, instead of firms actively selecting their producers or investing in the sustainability of their existing supply chain network, firms could benefit only from improvements in local farming practices in regulated locations. Importantly, however, the positive spillover effect in *unregulated* locations suggests firms improve the sustainability of their sourcing practices when exposed to environmental regulation elsewhere. The documented firm-wide sustainability improvements can be attributed to firms exiting certain trade relationships (and selecting new, responsible producers within regulated locations or across unregulated locations), investing in the existing supply chain network, or both.

To further understand trading firms' role in improving the sustainability of upstream sup-

is unsurprising given that the priority regulation targets deforestation-intense municipalities (see Table 1, Panel B).

ply chain networks after the priority regulation, I interviewed the former mayor of Paragominas (a formerly designated regulated location). Although the interview confirms the first-order effect at the producer level (i.e., farmers in regulated locations started to grow soybeans on underutilized land), the mayor emphasized the importance of trading firms in shaping local production practices and deforestation patterns. For instance, after Paragominas was priority-flagged, Cargill—one of the main soybean commodity trading firms—put restrictions on the sourcing of soybeans in that location. Subsequently, trading firms established a new quality control entity that verifies the origins of soybeans and assures the sourcing of deforestation-free soybeans after the priority regulation (see Internet Appendix Table IA.3). The insights gathered in this interview, together with the results of a positive spillover effect in unregulated locations, confirm that trading firms take an active stake in enhancing the sustainability of their firm-wide operations when exposed to transparency and enforcement shocks at the producer level.

6. Heterogeneous effects

6.1. Vulnerability to local enforcement actions

I next examine whether the firm-wide spillover effect is moderated by firms that are vulnerable to local enforcement actions. To operationalize firm-level vulnerability to enforcement actions, I use publicly available data on embargoes imposed by IBAMA. Although producers in regulated locations are subject to other forms of enforcement actions (e.g., fines, credit constraints, limited deforestation permits), embargoes are a key enforcement tool that is likely to induce changes in trading firms' sourcing behavior. In short, embargoes are regulated by federal law and IBAMA can impose an embargo when an investigated property is found to illegally deforest land. After notifying the property owner of the embargo, the embargo comes into effect instantly. The property location, the full name of the property owner, and the owner's Individual Taxpayer Registration number are published online by the Ministry of the Environment (see Sousa [2016] for a summary). Therefore, trading firms can utilize the information on embargoed areas and producers and adjust their sourcing activities accordingly.

Utilizing the dataset of embargoes, I construct a firm-level split variable capturing firms' high versus low vulnerability to deforestation embargoes. In columns (1) and (2) of Table 4, embargo data from all locations (regulated and unregulated) is used to determine firm-level enforcement vulnerability, and in columns (3) and (4), embargo data from only unregulated locations is used to define the sample cutoffs. The reason to use the underlying distribution of the embargo variable in only unregulated locations is two-fold: First, because I am interested in firm-level spillover effects in unregulated locations, it is logical to zoom in on firms' embargo exposure in these locations. Second, IBAMA increased its enforcement actions across all regulated locations in line with the regulator's intent to combat deforestation in these locations. Consequently, all treated firms are exposed to similarly higher levels of enforcement strength in regulated locations, making enforcement actions in unregulated locations the primary source of meaningful variation for treated firms.[22]

Performing cross-sectional splits of equation (1) based on the high- versus low-enforcement partitions and zooming in on firm-wide spillover effects only, I find the positive spillover effect is concentrated in treated firms that face stringent local enforcement actions relative to control firms. I find a negative and statistically significant coefficient on $Treated\_Spillover_{imt}$ that is about -153 hectares and -254 hectares, respectively, in the high-enforcement partitions of Panel A. In contrast, firms facing no or low enforcement actions across their sourcing network do not improve their sourcing activities. The differences in spillover coefficients between the low- and high-enforcement partitions are statistically significant across all specifications. My results are similar when examining firms' CO_2 emissions exposure in Panel B. Overall, the findings cast doubt on the notion that transparency shocks alone—such as in the form of firm or governmental disclosures—can lead to meaningful changes in trading firms' behavior.

[22]In a panel of municipality-year observations, regulated locations experience an average of 10 embargoes in a given year, while non-regulated locations register an average of one embargo (untabulated).

6.2. *Voluntary zero-deforestation commitments*

To better isolate firm-level decisions that concern investments in sustainable sourcing strategies, I focus on firms' voluntary zero-deforestation commitments. A zero-deforestation commitment is broadly defined as a firm's voluntary promise to reduce or eliminate deforestation associated with the production, purchase, and sale of its commodities (e.g., Lambin et al. [2018]; Garrett et al. [2019]). I hand-collect information on firms' signatory statuses in the Amazon Soy Moratorium ($SoyM_i$). The Soy Moratorium is the first collective voluntary zero-deforestation agreement in a tropical rainforest concerning soybeans. This initiative was created in response to environmental activist pressure, which shamed multinational firms' "complicity" in Amazon deforestation (Greenpeace [2006]). Signatories agree not to buy soybeans planted on deforested land after 2008 in the Amazon. In addition, signatory firms can voluntarily undergo third-party audits to verify their purchases and financing in line with the Soy Moratorium requirements ($SoyM\_Audit_i$). Appendix A summarizes the variable definitions in detail.

I perform cross-sectional splits of equation (1) based on an indicator variable that is either $SoyM_i$ or $SoyM\_Audit_i$, and focus on firm-wide spillover effects only. Results are presented in Table 5. In both panels A and B, column (1) presents results for non-signatory firms ($SoyM_i = 0$) and columns (2) and (3) for signatory firms ($SoyM_i = 1$). In column (3), I further condition the sample on those firms that perform voluntary audits in line with the Soy Moratorium requirements ($SoyM\_Audit_i = 1$).

In line with expectations, treated firms who commit to zero-deforestation sourcing (and those performing third-party audits) contribute to the observed positive firm-wide spillover effects. For instance, columns (2) and (3) of Panel A show that treated firms with signatory status under the Soy Moratorium and those that additionally opt in for voluntary audits are associated with decreases in deforestation exposure by about -186 and -277 hectares, respectively. In contrast, treated firms without such voluntary commitments do not significantly reduce their deforestation exposure compared to control firms, on average. The differences in spillover coefficients are statistically significant across all specifications (column (1) versus column (2) or column (3)), and interpretations of the results are the same when examining firms' CO_2 emissions exposure in Panel B. I conclude that firms' zero-deforestation commitments and their efforts to verify their supply chain network through third-party audits can shape the direction and extent to which the observed firm-wide spillover effects occur. Because signatories of the Soy Moratorium are likely firms that face external pressure to comply with local deforestation practices, this result more broadly captures improvements in the sustainability of soybean sourcing when firms are exposed to NGO and/or downstream pressures.

7. Limitations

The following caveats apply to this research. First, I do not capture firms' entire *global* sourced volume, and the data on firms' sourced volume linked to Brazilian municipalities contains missing linkages (that is, some sourced volume cannot be linked to a municipality). Therefore, it is difficult to assess and speak to trading firms' total reallocation strategies. Yet, because Brazil is one of the largest soybean exporters globally and home to a crucial ecosystem, the setting is economically meaningful. In addition, my focus on the soybean supply chain can limit the external validity of this study. However, the findings likely generalize to other (agricultural) commodity supply chains that are characterized by high levels of opacity in the production and trade stages and that have production facilities located in countries with similar levels of environmental policy stringency. Although the specific upstream and downstream processes of commodity supply chains can vary, commodity trading firms are key players in any commodity supply chain and operate mostly in opacity. Therefore, understanding trading firms' incentives and decision-making is crucial in evaluating the efficacy of environmental transparency and enforcement regulation, and sustainability outcomes along supply chains.

Third, although the dataset used in this paper provides important insights into trading firms' sourcing strategies, the soybean supply chain—and other commodity supply chains—is characterized by a few large trading firms that dominate the sourcing and distribution of commodities. A leave-one-out analysis of the largest trading firms in my sample does not materially alter my results (untabulated). However, note that my results of positive firm-wide spillover effects are driven by trading firms in the largest size tercile (untabulated), suggesting that environmental transparency and enforcement regulation is most likely to influence the behaviors of global trading firms. Another data limitation is that I am unable to capture trading firm-*producer* linkages, which is why I cannot assess reallocation strategies across different producers *within* certain locations.

Finally, the use of the priority regulation as a plausibly exogenous shock to trading firms' sourcing strategies, combined with the data used in this study, introduces a few design challenges. The first published priority list in 2008 targeted the majority of deforestation-intense locations and thereby limits the length of the pre-period for that cohort. Moreover, the benefits of a staggered treatment design diminish at the firm level since most treated firms already had sourcing linkages to priority-flagged locations in 2008.

8. Conclusion

This paper examines the effects of environmental transparency and enforcement regulation on the sourcing patterns of commodity trading firms. I exploit Brazil's priority regulation first implemented in 2008 that increases transparency and enforcement actions in certain deforestation-intense Brazilian municipalities and argue that international trading firms exposed to that regulation are likely to change where and how they source their commodities. Because trading firms source commodities from various geographic locations simultaneously, I investigate changes in treated firms' sourcing in regulated locations (direct effect) and unregulated locations (firm-wide spillover effect) relative to control firms. Because the direct effects likely reflect the first-order responses of both producers and trading firms, the main focus of this study is on firm-wide spillover effects in unregulated locations.

Overall, I find trading firms reduce their exposure to deforestation and CO_2 emissions associated with their sourcing in both regulated locations and unregulated locations. Importantly, anecdotal evidence gathered in interviews with key stakeholders in Brazil suggests trading firms play an active and important role in influencing local production practices and deforestation dynamics even in regulated locations. In line with the anecdotal evidence gathered, my findings reflect the joint effects of (i) firms strategically selecting producers that farm their crops on previously cleared land and (ii) firms actively investing in the existing supply chain network after the priority regulation. Finally, I find heterogeneity in the firm-wide spillover effect. Firms vulnerable to high levels of local enforcement actions and those committed to zero-deforestation sourcing and third-party verification drive the positive spillover effect. The combined results suggest that local enforcement actions and voluntary sustainability commitments are important factors in determining the direction and extent to which trading firms update their sourcing strategies. As a consequence, the findings cast doubt on transparency alone being a sufficient driver behind changes in trading firms' behavior. I conclude that environmental transparency and enforcement regulation can have notable transmission effects through global supply chains, as evidenced by trading firms improving the sustainability of their sourcing activities and thereby shaping the efficacy of environmental regulation in upstream supply chains.

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Appendix A. Variable definitions

Variable	Definition and measurement
Main variables:	
$CO2Exposure_{imt}$	Trading firm i's exposure to gross CO_2 emissions (in tons, deflated by 10,000) that results from land-use changes linked to soybean deforestation in municipality m and year t due to their sourcing of soybean from plants in that municipality-year. For each i, m, t, I aggregate gross CO_2 emissions estimates. (Source: Trase)
$DefExposure_{imt}$	Trading firm i's exposure to soybean deforestation (in hectares, deflated by 100) in municipality m and year t due to their sourcing of soybean from plants in that municipality-year. For each i, m, t, I aggregate deforestation estimates. (Source: Trase)
$Treated_{it}$	An indicator variable equal to 1 for all firm observations in year t when trading firm i sources from plants in at least one regulated location and the priority regulation is in effect, and 0 otherwise. $Treated\_Direct_{imt}$ and $Treated\_Spillover_{imt}$ partition the $Treated_{it}$ variable.
$Treated\_Direct_{imt}$	An indicator variable equal to 1 for all firm observations in regulated locations in year t when the priority regulation is in effect, and 0 otherwise. Appendix B summarizes the full list of regulated locations during my sample period. "Year of Entry" is used to identify the onset of the treatment years. (Source: Various governmental and legal websites to identify regulated locations as part of the priority regulation incl. gov.br, ibama.gov.br, jusbrasil.com.br, and legisweb.com.br)
$Treated\_Spillover_{imt}$	An indicator variable equal to 1 for all firm observations in unregulated locations in year t when trading firm i operates in at least one regulated location when the priority regulation is in effect, and 0 otherwise.
$Volume_{imt}$	Trading firm i's sourced soybean volume (in tons, deflated by 10,000) linked to their sourcing from plants in municipality m at time t. For each i, m, t, I aggregate sourced volume. (Source: Trase)
Cross-sectional split variables:	
$Embargo_i$	Firm-level vulnerability to enforcement actions. First, I take the sum of all officially registered embargoes in municipality m at time t to estimate the strength of enforcement actions at the production level. The data is published by the Brazilian government through IBAMA. Then, I create a firm-level variable capturing firms' vulnerability to high enforcement actions that is equal to 1 when firms face a maximum of embargoes that is larger than or equal to the 90^{th} percentile of the firm-level maximums of $Embargo_{imt}$. (Source: ibama.gov.br)
$SoyM_i$	Firm-level voluntary zero-deforestation commitment. An indicator variable equal to 1 when a trading firm i is identified as a signatory under the Soy Moratorium (i.e., collective soybean zero-deforestation agreement), and 0 otherwise. (Source: soyontrack and insights collected in Villoria et al. [2022] and zu Ermgassen et al. [2020])
$SoyM\_Audit_i$	Firm-level commitment to conduct third-party audits. An indicator variable equal to 1 when a trading firm i is identified as a signatory under the Soy Moratorium and additionally opts in for voluntary third-party verification of their purchases and finances under the Soy Moratorium, and 0 otherwise. (Source: soyontrack and insights collected in Villoria et al. [2022] and zu Ermgassen et al. [2020])

Appendix B. Brazil's list of priority municipalities

State	Municipality	Year of entry	Year of exit
Amazonas	Lábrea	2008	–
Mato Grosso	Alta Floresta	2008	2012
Mato Grosso	Aripuanã	2008	–
Mato Grosso	Brasnorte	2008	2013
Mato Grosso	Colniza	2008	–
Mato Grosso	Confresa	2008	2017
Mato Grosso	Cotriguaçu	2008	–
Mato Grosso	Gaúcha do Norte	2008	–
Mato Grosso	Juara	2008	–
Mato Grosso	Juína	2008	–
Mato Grosso	Marcelândia	2008	2013
Mato Grosso	Nova Bandeirantes	2008	–
Mato Grosso	Nova Maringá	2008	–
Mato Grosso	Nova Ubiratã	2008	2017
Mato Grosso	Paranaíta	2008	–
Mato Grosso	Peixoto de Azevedo	2008	2018
Mato Grosso	Porto dos Gaúchos	2008	2017
Mato Grosso	Querência	2008	2011
Mato Grosso	São Félix do Araguaia	2008	2017
Mato Grosso	Vila Rica	2008	2017
Pará	Altamira	2008	–
Pará	Brasil Novo	2008	2013
Pará	Cumaru do Norte	2008	–
Pará	Dom Eliseu	2008	2012
Pará	Novo Progresso	2008	–
Pará	Novo Repartimento	2008	–
Pará	Paragominas	2008	2010
Pará	Rondon do Pará	2008	2021
Pará	Santa Maria das Barreiras	2008	2017
Pará	Santana do Araguaia	2008	2012
Pará	São Félix do Xingu	2008	–
Pará	Ulianópolis	2008	2012
Rondônia	Machadinho d'Oeste	2008	–
Rondônia	Nova Mamoré	2008	–
Rondônia	Pimenta Bueno	2008	–
Rondônia	Porto Velho	2008	–
Maranhão	Amarante do Maranhão	2009	–
Mato Grosso	Feliz Natal	2009	2013
Pará	Itupiranga	2009	–
Pará	Marabá	2009	–
Pará	Pacajá	2009	–
Pará	Tailândia	2009	2013
Roraima	Mucajaí	2009	–

Amazonas	Boca do Acre	2011	–
Maranhão	Grajaú	2011	–
Mato Grosso	Alto Boa Vista	2011	2017
Mato Grosso	Cláudia	2011	2017
Mato Grosso	Santa Carmem	2011	2017
Mato Grosso	Tapurah	2011	2017
Pará	Moju	2011	–
Pará	Anapu	2012	–
Pará	Senador José Porfírio	2012	–
Amazonas	Apuí	2017	–
Amazonas	Manicoré	2017	–
Amazonas	Novo Aripuanã	2017	–
Pará	Itaituba	2017	–
Pará	Portel	2017	–
Rondônia	Buritis	2017	–
Rondônia	Candeias do Jamari	2017	–
Rondônia	Cujubim	2017	–
Mato Grosso	Marcelândia∗	2018	–
Pará	Placas	2018	–
Pará	Uruará	2018	–

Notes: This table summarizes the full list of regulated locations as part of the priority regulation (so-called "priority municipalities") that is published by Brazil's government. I identify priority municipalities from various governmental and legal websites, incl. gov.br, ibama.gov.br, jusbrasil.com.br, and legisweb.com.br). In general, locations are added to the list of *"priority municipalities for actions to prevent, monitor, and control deforestation"*, formerly known as the "blacklist," when they violate certain deforestation thresholds. Regulated locations are released from the priority list when they comply with other criteria that commonly relate to the registration of private landholdings in an electronic land registry system and deforestation reduction activities. When regulated locations prove compliance and exit the priority list, they are added to the *"list of municipalities with monitored and controlled deforestation"*. Column one refers to the state. Column two refers to the municipality. Column three refers to the year in which the municipality was added to the priority list. Column four refers to the year in which the municipality proved compliance and exited the list. The asterisk (∗) indicates a location reentering the priority list. The use of data on soybean supply chains and commodity trading firm linkages to source locations leaves 30 regulated locations as part of my full sample.

Figure 1

Treated firms' sourcing from regulated locations and spillover locations

This figure shows the geographical locations of treated trading firms sourcing from regulated locations (dark blue) and unregulated locations (yellow) that are part of my sample from 2006 until 2018. Because commodity trading firms source commodities from plants in various geographic locations at the same time, I can identify the effect of the priority regulation on firms' sourcing in regulated locations and spillover locations. Firms that do not source from plants in any regulated location make up the control group.

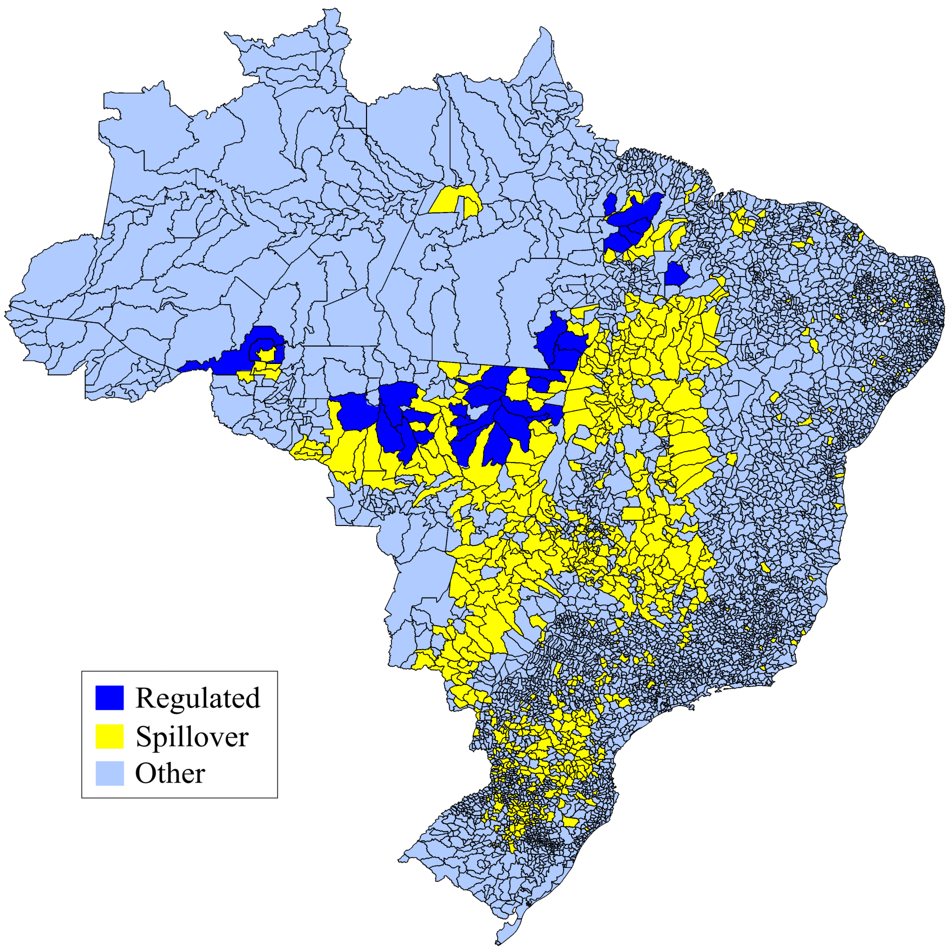


Figure 2

Commodity supply chain

This figure illustrates a commodity supply chain dissected into its two core commodity flow stages—(i) upstream and (ii) downstream. The commodity flow stages can also be dissected into three stages including the midstream of the supply chain. Note that some midstream activities overlap with upstream activities (e.g., storage) and other midstream activities overlap with downstream activities (e.g., transportation). The key value-added steps in a commodity supply chain are typically as follows: (i) production, (ii) storage, transportation, trade, and distribution, and (iii) manufacturing, retail, and consumption. I also identify the key actors in each of the value-added steps as (i) producers, (ii) commodity trading firms, and (iii) manufacturers, restaurants, and retailers. This illustration is broadly applicable to most commodity supply chains. Importantly, this paper focuses on a regulatory shock on upstream production activities and its transmission effects on firms further down in supply chains, i.e., commodity trading firms.

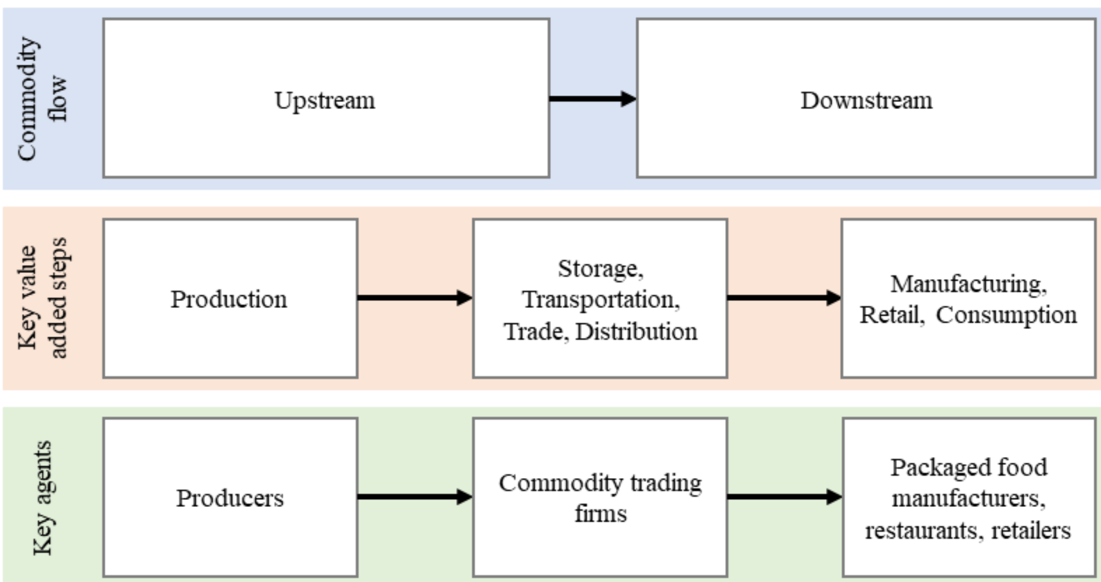


Figure 3
Research design

This figure illustrates the main research design of this study. There are a number of geographic locations from which trading firms can source commodities, and out of all locations, only a few are regulated by the government because of high deforestation levels and rates. Treated firms are identified when they source commodities from plants in at least one regulated location. I then split the firm-level treatment effect into a "direct effect" (that captures treated firms' sourcing from plants in regulated locations relative to control firms) and a "firm-wide spillover effect" (that captures treated firms' sourcing from plants in unregulated locations when they are exposed to the priority regulation elsewhere relative to control firms). Firms that do not source from any regulated location make up the control group. I also specify an adjusted version of this research design, in which I exclude control firms that source from plants in the same location as at least one treated firm (i.e., control firms operating in "Municipality B", a so-called spillover location). This adjusted main design reduces concerns about spillover effects from treated to control firms.

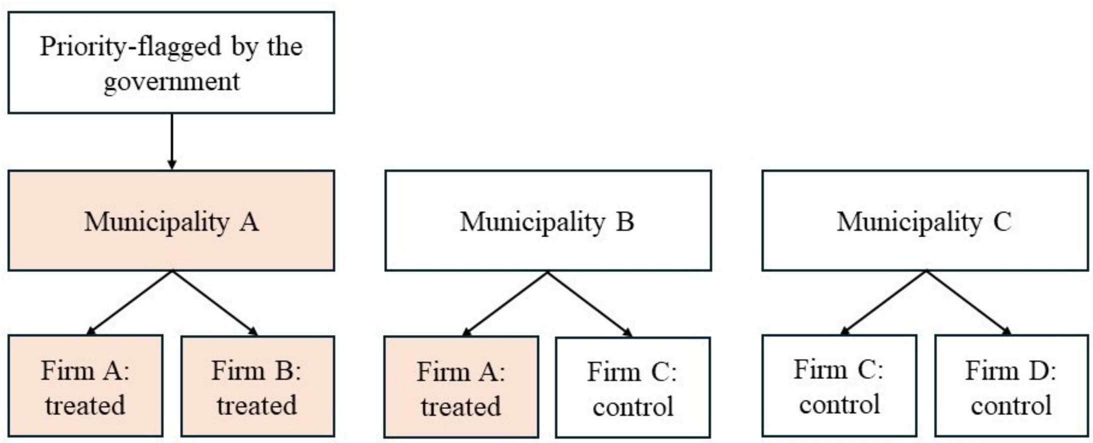


Figure 4

Firms' sourced volume in spillover locations in event time

This figure displays the difference in sourced volume (in tons, deflated by 10,000) ($Volume_{imt}$) between spillover-treated plants and control plants in event time. The coefficient estimates are based on an interaction between plant-level treatment status and 7 time indicators (i.e., from $--2$ to $++4$, where 0 represents the first year of the treatment). The year before the priority status (-1) is the relative difference estimate set to zero. The first time indicator represents 2 or more years before the priority regulation and the last time indicator represents 4 or more years after the priority regulation. Coefficient estimates are based on firm $\times$ municipality and firm size $\times$ year fixed effects. Confidence bands represent 90% confidence intervals and are based on standard errors clustered at the municipality level.

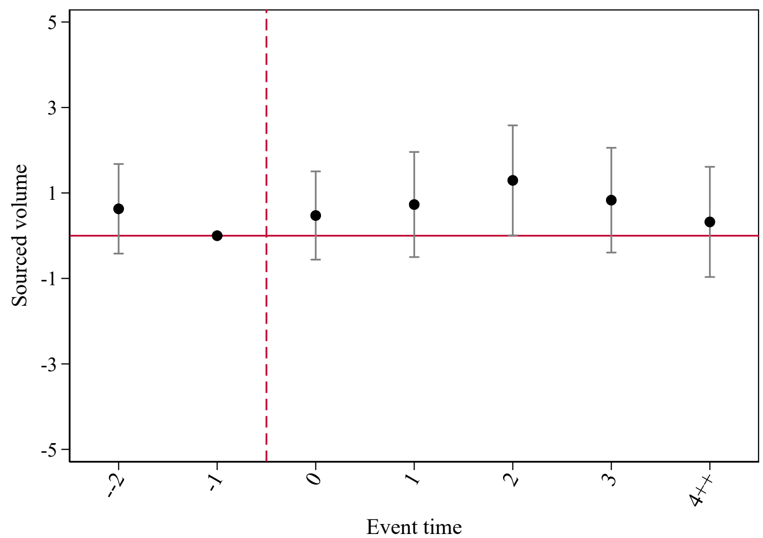
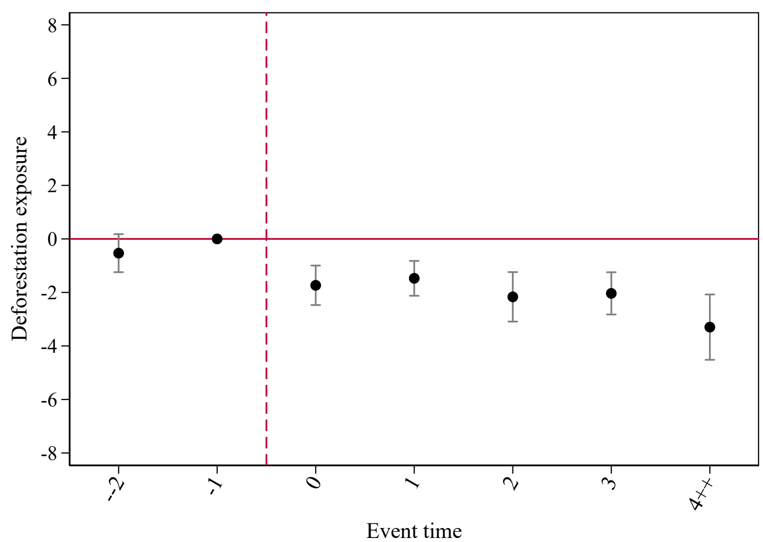


Figure 5

Firms' environmental exposures in spillover locations in event time

The top figure displays the difference in firms' exposure to deforestation (in hectares, deflated by 100) ($DefExposure_{imt}$) between spillover-treated plants and control plants in event time. The bottom figure replicates the graph using firms' CO_2 emissions exposure (in tons, deflated by 10,000) ($CO2Exposure_{imt}$) as the outcome variable. The coefficient estimates are based on an interaction between plant-level treatment status and 7 time indicators (i.e., from $--2$ to $++4$, where 0 represents the first year of the treatment). The year before the priority status (-1) is the relative difference estimate set to zero. The first time indicator represents 2 or more years before the priority regulation and the last time indicator represents 4 or more years after the priority regulation. Coefficient estimates are based on firm × municipality and firm size × year fixed effects. Confidence bands represent 90% confidence intervals and are based on standard errors clustered at the municipality level.



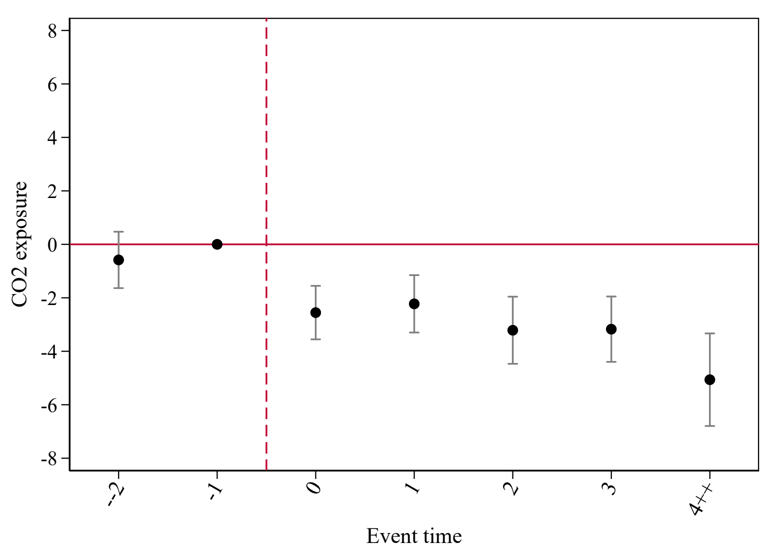


Table 1

Descriptive statistics

Panel A: Full sample

	N	Mean	Std. Dev	P5	Median	P95
Treatment indicator variables:						
$Treated_{it}$	7,932	0.527	0.499	0.000	1.000	1.000
$Treated\_Direct_{imt}$	7,932	0.067	0.249	0.000	0.000	1.000
$Treated\_Spillover_{imt}$	7,932	0.460	0.498	0.000	0.000	1.000
Outcome variables:						
$Volume_{imt}$	7,932	5.613	9.678	0.055	2.108	23.737
$DefExposure_{imt}$	7,932	1.232	5.470	0.000	0.066	5.407
$CO2Exposure_{imt}$	7,932	2.174	9.569	0.001	0.125	8.922
Cross-sectional split variables:						
$Embargo_{imt}$	7,932	1.943	7.241	0.000	0.000	9.000
$SoyM_{imt}$	7,932	0.717	0.451	0.000	1.000	1.000
$SoyM\_Audit_{imt}$	7,932	0.592	0.491	0.000	1.000	1.000

Panel B: Key variables split based on treatment statuses pre-2008

	Regulated location	Unregulated spillover location	Unregulated control location
$Volume_{imt}$	6.484	6.325	1.912
	(11.97)	(11.59)	(3.73)
$DefExposure_{imt}$	11.277	3.606	0.434
	(21.67)	(11.58)	(1.29)
$CO2Exposure_{imt}$	36.574	5.417	0.707
	(64.49)	(16.37)	(2.35)

Notes: Panel A presents descriptive statistics for the pooled sample of firm-municipality-year observations. Panel B presents the mean and standard deviation in parentheses for the outcome variables in the period before the onset of the priority regulation (i.e., before 2008) split into (i) observations related to eventually regulated locations in column one (directly treated), (ii) observations related to unregulated locations and conditional on a trading firm obtaining treatment status in the post period in column two (spillover treated), and (iii) observations related to unregulated locations and conditional on a trading firm never obtaining treatment status in column three (control). Appendix A summarizes the variable definitions in detail.

Table 2
Environmental regulation and firms' sourced volume

$Y = Volume_{imt}$	(1) Basic design	(2) Main design	(3) Main design adj.	(4) Spillover only
$Treated_{it}$	-0.171			
	(-0.39)			
$Treated\_Direct_{imt}$		-0.513	-0.150	
		(-0.31)	(-0.08)	
$Treated\_Spillover_{imt}$		0.211	0.507	0.641
		(0.37)	(0.84)	(1.05)
Fixed effects:				
Firm	Y	–	–	–
Firm × Municipality	–	Y	Y	Y
Municipality	Y	–	–	–
Year	Y	–	–	–
Firm Size × Year	–	Y	Y	Y
Observations	7,677	6,569	5,250	4,711
Adjusted R^2	0.330	0.548	0.533	0.567

Notes: This table reports results from estimating the relation between environmental transparency and enforcement regulation and firms' sourced volume (in tons, deflated by 10,000) using OLS. Appendix A summarizes the variable definitions in detail. In the basic design in column (1), I include firm, municipality, and year fixed effects separately. In the main design in column (2), I include firm × municipality and firm size × year fixed effects. Changes in the number of observations between columns (1) and (2) reflect the exclusion of singletons in this high-dimensional fixed effects design. In columns (3) and (4), I repeat the main design but adjust the control group by excluding observations linked to control firms in spillover locations. In column (4), I further limit the sample to unregulated locations and thereby estimate the coefficient on $Treated\_Spillover_{imt}$ only. Standard errors are adjusted for heteroskedasticity and clustered at the municipality level. T-statistics are presented in parentheses below the coefficient estimates, and *, **, *** reflect two-tailed significance levels at 0.1, 0.05, and 0.01, respectively.

Table 3

Environmental regulation and firms' deforestation and CO_2 emissions exposures

Panel A: Deforestation exposure

Y=$DefExposure_{imt}$	(1) Basic design	(2) Main design	(3) Main design adj.	(4) Spillover only
$Treated_{it}$	-1.955***			
	(-4.24)			
$Treated\_Direct_{imt}$		-9.012**	-9.163**	
		(-2.56)	(-2.39)	
$Treated\_Spillover_{imt}$		-1.633***	-1.328***	-1.368***
		(-3.99)	(-3.85)	(-4.30)
Fixed effects:				
Firm	Y	–	–	–
Firm × Municipality	–	Y	Y	Y
Municipality	Y	–	–	–
Year	Y	–	–	–
Firm Size × Year	–	Y	Y	Y
Observations	7,677	6,569	5,250	4,711
Adjusted R^2	0.252	0.492	0.498	0.548

Panel B: CO_2 emissions exposure

Y=$CO2Exposure_{imt}$	(1) Basic design	(2) Main design	(3) Main design adj.	(4) Spillover only
$Treated_{it}$	-4.238***			
	(-3.53)			
$Treated\_Direct_{imt}$		-30.737***	-32.072***	
		(-2.97)	(-2.87)	
$Treated\_Spillover_{imt}$		-2.269***	-1.900***	-2.199***
		(-2.78)	(-2.66)	(-3.79)
Fixed effects:				
Firm	Y	–	–	–
Firm × Municipality	–	Y	Y	Y
Municipality	Y	–	–	–
Year	Y	–	–	–
Firm Size × Year	–	Y	Y	Y
Observations	7,677	6,569	5,250	4,711
Adjusted R^2	0.162	0.346	0.356	0.464

Notes: This table reports results from estimating the relation between environmental transparency and enforcement regulation and firms' deforestation exposure (in hectares, deflated by 100) in Panel A and CO_2 emissions exposure (in tons, deflated by 10,000) in Panel B using OLS. Appendix A summarizes the variable definitions in detail. In the basic design in column (1), I include firm, municipality, and year fixed effects separately. In the main design in column (2), I include firm × municipality and firm size × year fixed effects. In columns (3) and (4), I repeat the main design but make adjustments to the control group by excluding observations linked to control firms in spillover locations. In column (4), I further limit the sample to unregulated locations and thereby estimate the coefficient on $Treated\_Spillover_{imt}$ only. Standard errors are adjusted for heteroskedasticity and clustered at the municipality level. T-statistics are presented in parentheses below the coefficient estimates, and *, **, *** reflect two-tailed significance levels at 0.1, 0.05, and 0.01, respectively.

Table 4

Heterogeneous effects using variation in enforcement actions

Panel A: Deforestation exposure

$Y = DefExposure_{imt}$	(1)	(2)	(3)	(4)
	Embargoes all locations		Embargoes unregulated locations	
	low	high	low	high
$Treated\_Spillover_{imt}$	0.132	-1.528***	0.176	-2.541***
	(0.76)	(-4.45)	(1.00)	(-4.43)
Fixed effects:				
Firm × Municipality	Y	Y	Y	Y
Firm Size × Year	Y	Y	Y	Y
Observations	1,631	4,387	1,632	4,387
Adjusted R^2	0.513	0.558	0.496	0.560

Panel B: CO_2 emissions exposure

$Y = CO2Exposure_{imt}$	(1)	(2)	(3)	(4)
	Embargoes all locations		Embargoes unregulated locations	
	low	high	low	high
$Treated\_Spillover_{imt}$	0.197	-2.403***	0.158	-4.022***
	(0.78)	(-3.79)	(0.56)	(-4.05)
Fixed effects:				
Firm × Municipality	Y	Y	Y	Y
Firm Size × Year	Y	Y	Y	Y
Observations	1,631	4,387	1,632	4,387
Adjusted R^2	0.551	0.472	0.534	0.474

Notes: This table reports results from estimating the relation between environmental transparency and enforcement regulation and firms' deforestation exposure (in hectares, deflated by 100) in Panel A and firms' CO_2 emissions exposure (in tons, deflated by 10,000) in Panel B, using cross-sectional splits of firms' vulnerability to local enforcement actions (estimated as separate OLS regressions). In each panel, columns (1) and (2) use the distribution of $Embargo_{imt}$ based on the pooled sample—regulated and unregulated locations—and conditional on unique trading firm observations. In columns (3) and (4), I use the distribution of $Embargo_{imt}$ based on unregulated locations only and conditional on unique trading firm observations. Because enforcement actions are increased in all regulated locations (albeit with varying levels), the latter approach allows me to capture firms' vulnerability to local enforcement actions in their unregulated locations. High enforcement strength is equal to 1 for firms that face a maximum of deforestation embargoes that is larger than or equal to the 90^{th} percentile of the firm-level maximums of $Embargo_{imt}$. In columns (1) and (2), the cutoff is 19 embargoes or more, while in columns (3) and (4) the cutoff is 13 embargoes or more. Appendix A summarizes the variable definitions in detail. I include firm × municipality and firm size × year fixed effects. Standard errors are adjusted for heteroskedasticity and clustered at the municipality level. T-statistics are presented in parentheses below the coefficient estimates, and *, **, *** reflect two-tailed significance levels at 0.1, 0.05, and 0.01, respectively.

Table 5

Heterogeneous effects using variation in sustainability commitments

Panel A: Deforestation exposure

Y=$DefExposure_{imt}$	(1)	(2)	(3)
	Soy Moratorium	Soy Moratorium	
	non-signatories	signatories	signatories with audit
$Treated\_Spillover_{imt}$	-0.036	-1.861***	-2.766***
	(-0.23)	(-4.68)	(-4.71)
Fixed effects:			
Firm × Municipality	Y	Y	Y
Firm Size × Year	Y	Y	Y
Observations	1,547	4,482	3,788
Adjusted R^2	0.486	0.557	0.560

Panel B: CO_2 emissions exposure

Y=$CO2Exposure_{imt}$	(1)	(2)	(3)
	Soy Moratorium	Soy Moratorium	
	non-signatories	signatories	signatories with audit
$Treated\_Spillover_{imt}$	-0.108	-2.906***	-4.332***
	(-0.43)	(-3.95)	(-4.14)
Fixed effects:			
Firm × Municipality	Y	Y	Y
Firm Size × Year	Y	Y	Y
Observations	1,547	4,482	3,788
Adjusted R^2	0.523	0.472	0.468

Notes: This table reports results from estimating the relation between environmental transparency and enforcement regulation and firms' deforestation exposure (in hectares, deflated by 100) in Panel A and firms' CO_2 emissions exposure (in tons, deflated by 10,000) in Panel B, using cross-sectional splits of firms' commitment to zero deforestation (estimated as separate OLS regressions). A firm's commitment to zero deforestation is estimated using an indicator variable equal to 1 when a firm is a signatory of the Soy Moratorium, and 0 otherwise ($SoyM_i$). In each panel, column (1) presents results for non-signatory firms, while columns (2) and (3) present results for signatory firms. In column (3), I limit the sample of signatory firms to those that additionally opt in for third-party audits in line with the Soy Moratorium's requirements. Appendix A summarizes the variable definitions in detail. I include firm × municipality and firm size × year fixed effects. Standard errors are adjusted for heteroskedasticity and clustered at the municipality level. T-statistics are presented in parentheses below the coefficient estimates, and *, **, *** reflect two-tailed significance levels at 0.1, 0.05, and 0.01, respectively.

Internet Appendix

The Spillover Effects of Environmental Transparency and Enforcement Regulation: Evidence From Commodity Trading Firms

Sandra G. Schafhäutle
August 2024

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Figure IA.1

Disclosure and news coverage of the list of priority municipalities

The first priority list was disclosed in Brazil's official federal newspaper in the section of the Ministry of the Environment (Ministério do Meio Ambiente) under "Portaria no 28" on the 24th of January in 2008 (see www.ibama.gov.br). This ordinance outlines the selection criteria that define when a location is added to the priority lists and records all locations that are considered for priority action (see Appendix B). A snapshot of the disclosure is provided below. The top newspapers with the highest daily number of circulations in Brazil immediately report about the priority regulation: (i) Extra, (ii) Folha de São Paulo, (iii) O Estado de São Paulo, (iv) O Globo (each with about 300,000 daily newspapers sold in 2008). For instance, Folha de São Paulo reports on the priority regulation either on the front page or in the Science section in each of the eleven days after the government's disclosure. Other large and small newspapers and NGO webpages also cover the story (e.g., articles are found in (i) Amazônia Real, (ii) Diário do Grande ABC, (iii) El País, (iv) Istoé, (v) ((o))eco, (vi) Portal R7, (vii) réporter brasil, (viii) Senado Notícias, (ix) Terra, and (x) VERDEfato). The priority regulation continued to be the focus of media attention when the Ministry of the Environment updated the priority list with new locations.

Ministério do Meio Ambiente

GABINETE DA MINISTRA

PORTARIA Nº 28, DE 24 DE JANEIRO DE 2008(*)

Dispõe sobre os municípios situados no Bioma Amazônia onde incidirão ações prioritárias de prevenção, monitoramento e controle do desmatamento ilegal.

A MINISTRA DE ESTADO DO MEIO AMBIENTE, no uso de suas atribuições, e tendo em vista o disposto nos Decretos nºs 6.101, de 26 de abril de 2007 e 6.321, de 21 de dezembro de 2007, resolve:

Art. 1º Ficam identificados no Anexo a esta Portaria, nos termos do art. 2º do Decreto nº 6.321, de 21 de dezembro de 2007, os municípios situados no Bioma Amazônia, selecionados conforme os seguintes critérios:

I - área total de floresta desmatada;

II - área total de floresta desmatada nos últimos três anos; e

III - aumento da taxa de desmatamento em pelo menos três dos últimos cinco anos.

Art. 2º Nos municípios que constam da lista anexa incidirão ações prioritárias relativas à proteção de áreas ameaçadas de degradação e à racionalização do uso do solo, de forma a prevenir, monitorar e controlar o desmatamento ilegal.

Art. 3º Esta lista será atualizada anualmente, com o ingresso de novos municípios, de acordo com o desempenho e a dinâmica de desmatamento verificados pelo Instituto Nacional de Pesquisas Espaciais-INPE do Ministério de Ciência e Tecnologia.

Art. 4º Esta Portaria entra em vigor na data de sua publicação.

MARINA SILVA

ANEXO

LISTA DE MUNICÍPIOS PRIORITÁRIOS PARA AÇÕES DE PREVENÇÃO, MONITORAMENTO E CONTROLE DO DESMATAMENTO NA AMAZÔNIA EM 2008

I - Amazonas: Lábrea;

II - Mato Grosso: Alta Floresta, Aripuanã, Brasnorte, Colniza, Confresa, Cotriguaçu, Gaúcha do Norte, Juara, Juína, Marcelândia, Nova Bandeirantes, Nova Maringá, Nova Ubiratã, Paranaíta, Peixoto de Azevedo, Porto dos Gaúchos, Querência, São Félix do Araguaia, Vila Rica;

III - Pará: Altamira, Brasil Novo, Cumaru do Norte, Dom Eliseu, Novo Progresso, Novo Repartimento, Paragominas, Rondon do Pará, Santa Maria das Barreiras, Santana do Araguaia, São Félix do Xingú, Ulianópolis; e

IV - Rondônia: Nova Mamoré, Porto Velho, Machadinho D'Oeste, Pimenta Bueno.

(*) Republicada por ter saído, no DOU de 25-1-2008, Seção 1, pág. 70, com incorreção no original.

Figure IA.2

Outcome variables in event time in regulated locations

The top figure displays the difference in sourced volume (in tons, deflated by 10,000) ($Volume_{imt}$) between regulated firm-municipalities and control firm-municipalities in event time. The subsequent figures display the difference in deforestation exposure (in hectares, deflated by 100) and CO_2 exposure (in tons, deflated by 10,000) between regulated firm-municipalities and control firm-municipalities in event time, respectively. The coefficient estimates are based on an interaction between plant-level treatment status and 7 time indicators (i.e., from $--2$ to $++4$, where 0 represents the first year of the treatment). The year before the priority status (-1) is the relative difference estimate set to zero. The first time indicator represents 2 or more years before the priority regulation and the last time indicator represents 4 or more years after the priority regulation. Coefficient estimates are based on firm × municipality and firm size × year fixed effects. Confidence bands represent 90% confidence intervals and are based on standard errors clustered at the municipality level.

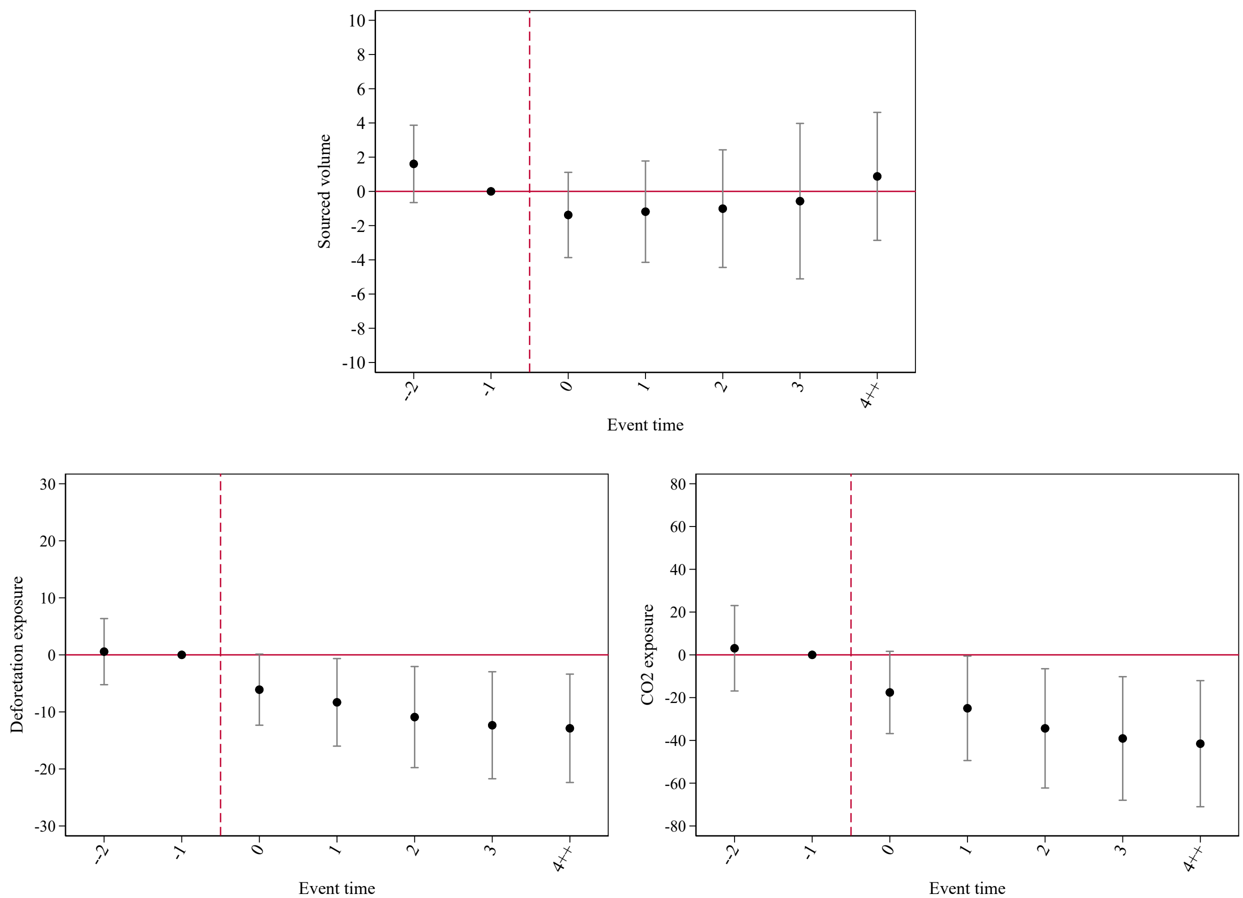

Table IA.1

Alternative design choices to address spatial spillover effects

	(1)	(2)	(3)	(4)	(5)	(6)
	Exclude neighboring locations			Exclude locations within 200 km distance		
	$Volume_{imt}$	$DefExposure_{imt}$	$CO2Exposure_{imt}$	$Volume_{imt}$	$DefExposure_{imt}$	$CO2Exposure_{imt}$
$Treated\_Direct_{imt}$	-1.342	-10.539***	-34.797***	-0.637	-9.391***	-31.524***
	(-0.85)	(-2.88)	(-3.25)	(-0.38)	(-2.65)	(-3.02)
$Treated\_Spillover_{imt}$	0.231	-1.678***	-1.687**	0.177	-1.740***	-1.709**
	(0.43)	(-3.34)	(-2.15)	(0.32)	(-3.35)	(-2.17)
Fixed effects:						
Firm × Municipality	Y	Y	Y	Y	Y	Y
Firm Size × Year	Y	Y	Y	Y	Y	Y
Observations	5,426	5,426	5,426	5,273	5,273	5,273
Adjusted R^2	0.574	0.512	0.384	0.568	0.509	0.373

Notes: This table presents the results of Tables 2 and 3 utilizing alternative research design choices to address concerns of spatial (producer-level or location-level) spillover effects. In columns (1) to (3), I exclude locations that are direct neighbors to regulated locations, by hand-collecting and identifying direct neighbors (i.e., locations that share a border with a regulated location). In columns (4) to (6), I exclude locations within a 200 km geodetic distance from regulated locations (the geodetic distance is the minimum distance along the surface of a mathematical representation of the Earth between two points). The 200 km cutoff is chosen because of the larger sizes of Brazilian municipalities in the Amazon. Appendix A summarizes the variable definitions in detail. I include firm × municipality and firm size × year fixed effects. Standard errors are adjusted for heteroskedasticity and clustered at the municipality level. T-statistics are presented in parentheses below the coefficient estimates, and *, **, *** reflect two-tailed significance levels at 0.1, 0.05, and 0.01, respectively.

Table IA.2

Alternative standard error clustering choices

	(1)	(2)	(3)	(4)	(5)	(6)
	Cluster at firm level			Wild bootstrap at municipality level		
	$Volume_{imt}$	$DefExposure_{imt}$	$CO2Exposure_{imt}$	$Volume_{imt}$	$DefExposure_{imt}$	$CO2Exposure_{imt}$
$Treated\_Direct_{imt}$	-0.513	-9.012**	-30.737***	-0.513	-9.012**	-30.737**
	(-0.18)	(-2.22)	(-2.84)	(-0.31)	(-2.56)	(-2.97)
				(0.790)	*(0.025)*	*(0.011)*
$Treated\_Spillover_{imt}$	0.211	-1.633**	-2.269*	0.211	-1.633***	-2.269***
	(0.36)	(-2.18)	(-1.73)	(0.37)	(-3.99)	(-2.78)
				(0.702)	*(0.000)*	*(0.009)*
Fixed effects:						
Firm × Municipality	Y	Y	Y	Y	Y	Y
Firm Size × Year	Y	Y	Y	Y	Y	Y
Observations	6,569	6,569	6,569	6,569	6,569	6,569
Adjusted R^2	0.548	0.492	0.346	0.548	0.492	0.346

Notes: This table presents the results of Tables 2 and 3 utilizing the main research design but with alternative standard error clustering choices. In my main specification, I cluster standard errors at the municipality level in line with the level of treatment effect. I implement two alternative strategies to examine whether my results are robust to the choice of standard error clustering. In columns (1) to (3), I cluster standard errors at the firm level. In columns (4) to (6), I calculate wild bootstrapped standard errors using municipality clustering and present estimated p-values in parentheses and italicized letters below the estimated t-statistics. Appendix A summarizes the variable definitions in detail. I include firm × municipality and firm size × year fixed effects. T-statistics are presented in parentheses below the coefficient estimates, and *, **, *** reflect two-tailed significance levels at 0.1, 0.05, and 0.01, respectively.

Table IA.3

Qualitative analysis: key stakeholder details and insights

Name	Description	Duration
Adnan Demachki	Former mayor of Paragominas, a municipality in the State of Pára, Brazil. Served from 2005–2008 and 2009–2012. Along with numerous other recognitions, Adnan received the Chico Mendes Environmental Prize in 2011 in recognition of his efforts in transforming Paragominas into a green municipality after Paragominas was initially targeted by the federal government and put on the list of priority action. The Green Municipality Program was subsequently expanded and applied to other municipalities and received recognition from the World Forum in Oxford, UK in 2012. In 2012, he was named among the top 100 influential persons in Brazil according to the Epocá magazine, and continues to contribute to the sustainable development of the Amazon.	1 hour 30 min

Key insights: (i) history of public policies that incentivize farmers to colonize and transform forests rather than preserve them is emphasized as a key problem for the effectiveness of environmental regulation in Brazil; (ii) priority regulation (henceforth, "regulation") was implemented because of high levels and rates of deforestation in certain locations mainly due to illegal fires and logging activities (exemplified in Paragominas); (iii) this regulation is the first regulatory attempt that includes all stakeholders along supply chains and the society at large and viewed as a 'milestone' forest protection policy; (iv) there was strong opposition to this regulation, as reflected by protests and violence against federal enforcement agencies such as IBAMA after a location's listing; (v) this regulation led to direct changes at the municipality level (e.g., changes to education and sustainable production practices through the use of abandoned and already cleared land, so-called "open areas") and changes in trading firms' economic activity in a regulated location (e.g., Cargill, one of the largest soybean trading firms, put restrictions on the sourcing of soybeans in Paragominas); (vi) the success of the regulation is due to both the public shaming of regulated locations and bundled enforcement actions in these locations; (vii) trading firms implemented additional quality controls in regulated locations by establishing an entity called "União das Empresas de Grãos", which is a warehouse that verifies the origins of soybeans; (viii) more trading firms increased their presence in regulated locations, because this allowed trading firms to source "clean" soybeans after the improvements in the sustainability of local production practices (exemplified in Paragominas); (xi) the regulation influenced the development of several other private sector initiatives such as the "Green Municipality Program" and the "Protocol of Green Grains", which established new rules for trading firms' sourcing activities (e.g., farmer properties must be registered on the electronic land registry system, there must be an official purchasing invoice, farmers cannot be on the list of embargoed areas by IBAMA and also not on the list of areas with slave labor reports)

Notes: These insights are gathered from an interview and several follow-up discussions with Adnan Demachki. I thank Stephan Freundt for transcribing and translating the interview. I disclose details about the interviewee in the above table. Adnan read and approved the details disclosed in this research paper. Other discussions with anonymous key stakeholders in Brazil supplement and support the insights gathered. Interview approval for research inclusion was provided by the "Ethics Committee Economics and Business" at the University of Amsterdam in 2022.