Making a Difference? The Impact of Environmental NGO Campaigns on Deforestation in Brazil

Christoph Oberthür[1]

Abstract

This paper provides causal evidence that NGO campaigns influence land-use decisions in Brazil's soy and beef supply chains, two of the leading drivers of tropical deforestation globally. Using a shift-share design that exploits quasi-exogenous variation in campaign timing and firm–municipality trade linkages, I find that municipalities more exposed to targeted firms through trade linkages experience significantly less deforestation, with aggregate estimates implying a reduction of approximately 19% of total forest and savanna loss in the sample. The effect operates primarily by deterring forest-to-cattle conversion. Campaigns also promote reallocation from cattle grazing to soy cultivation, indicating a shift toward more intensive and economically productive land uses, accompanied by measurable productivity gains. The results underscore the persistent influence of civil society in shaping environmental outcomes, and highlight the broader potential of reputational governance to complement state regulation in managing global environmental externalities.

JEL Codes: Q23, Q56, L31, F18, C26

[1]Paris School of Economics. E-mail: `christoph.oberthur@psemail.eu`.
I thank Lionel Fontagné, Michele Fioretti, Sandra Poncet, Jean-Stéphane Mésonnier, Ryan Abman, and François Bourguignon for helpful comments.

1 Introduction

In regions where formal environmental enforcement faces significant logistical and political challenges, environmental NGOs increasingly act as global 'watchdogs,' leveraging reputational risk to penalize firms associated with ecological damage. Theoretical models of 'private politics' suggest this scrutiny can serve as a critical complement to public governance, filling gaps where state capacity is constrained (Baron, 2001; Baron, 2003; Aldashev, Limardi and Verdier, 2015). Indeed, landmark cases in Brazil, such as the Soy Moratorium and the G4 Cattle Agreement, demonstrate that activists can catalyze sector-wide commitments that improve upon existing legislation. But does the underlying reputational pressure itself drive change, or does the effect mainly come from public enforcement and pushing for binding commitments? This paper investigates whether NGO campaigns causally reduce deforestation through reputational pressure, and whether this also triggers a restructuring of agricultural production, from extensive frontier clearing toward more intensive use of already-cleared land. This question is especially timely as NGOs face increasing political and economic pushback,[2] raising questions about the role and effectiveness of reputational enforcement.

A large theoretical literature examines NGOs' strategic use of reputational pressure. The 'private politics' framework of Baron (2001) and Baron (2003) argues that NGOs influence corporate behavior through campaigns rather than legal mandates; Daubanes and Rochet (2019) model NGOs as welfare-enhancing counterweights where regulators are captured or poorly informed. NGOs strategically target visible brands to maximize leverage (Baron and Diermeier, 2007), timing campaigns to coincide with global media events (Couttenier and Hatte, 2016) and corporate annual general meetings (Fioretti, Saint-Jean and Smith, 2026), and exploiting geographic and cultural proximity (Hatte and Koenig, 2020). Credible monitoring can compel firms to comply or exit high-risk markets, particularly where governance is weak (Aldashev, Limardi and Verdier, 2015), and firms may pre-empt pressure by adopting voluntary commitments (Baron, 2016;

[2] Associated Press; New York Times

Potoski and Prakash, 2013).

These dynamics are well-documented in Brazil. Greenpeace's campaigns catalyzed the Amazon Soy Moratorium (2006) and G4 Cattle Agreements (2009). The Moratorium sharply curtailed the direct conversion of forest to soy, cutting the share of new soy expansion occurring on recently cleared Amazon land from roughly 30% to near zero (Gibbs, Rausch et al., 2015; Macedo et al., 2012; Kastens et al., 2017), with the most rigorous causal evaluation attributing a 35% reduction in deforestation across soy-suitable areas to the agreement (Heilmayr et al., 2020); the cattle agreements, in turn, curtailed sourcing from recently deforested ranches (Gibbs, Munger et al., 2016). Yet significant leakage persists among indirect suppliers and in less regulated biomes (Moffette, Skidmore and Gibbs, 2021; Zu Ermgassen et al., 2020).

At the same time, Brazil's largest deforestation reductions during this period were driven by public enforcement: credit embargoes (Assunção, Gandour, Rocha and Rocha, 2021), the federal Priority List (Assunção, McMillan et al., 2023), and satellite-based monitoring (DETER) (Assunção, Gandour and Rocha, 2023), which together accounted for over half of observed reductions (Assunção, Gandour and Rocha, 2015). Public and private enforcement can interact positively: federal blacklisting affected larger ranches while G4 agreements improved smaller-ranch productivity (Moffette, Skidmore and Gibbs, 2021). However, enforcement gains weakened with declining political support after 2014 (Burgess, Costa and Olken, 2019), and local political dynamics can further undermine outcomes (Katovich and Moffette, 2025). Temporary reductions in fires following global attention to the 2019 Amazon crisis illustrate how public scrutiny complements formal enforcement (Araujo, Costa and Garg, 2024).

Despite this extensive body of work, there is no causal evidence linking targeted NGO campaign activity to localized environmental outcomes. As Brass et al. (2018) note, research on NGO environmental impact remains largely qualitative or based on case studies. Existing work documents where and why NGOs act, but not whether specific campaigns alter land-use behavior on the ground. This paper fills this gap by providing the first causal estimates of how NGO reputational pressure affects deforestation and land-use transitions at the municipal level.

To do so, I construct a novel panel dataset linking 169 distinct deforestation-related NGO campaigns to firm-level trade flows and satellite-derived land cover changes across 3,520 Brazilian municipalities from 2012 to 2023. I exploit the plausibly exogenous timing of campaign events and pre-campaign (2011) firm-municipality trade shares to construct a shift-share measure of local campaign exposure, controlling for concurrent policy changes, corporate commitments, and enforcement variation to isolate the effect of NGO campaigns.

I document three main findings. First, increased exposure to NGO campaigns through trade linkages with targeted firms causally reduces subsequent deforestation, primarily by deterring forest-to-cattle conversion. Second, NGO pressure shifts land use from extensive, low-productivity cattle grazing to more intensive soy cultivation, an effect concentrated in municipalities where forest is already depleted, revealing a displacement channel from the extensive to the intensive margin. Third, these shifts are accompanied by measurable productivity gains: cattle density increases modestly, while soy productivity initially declines on newly converted pasture but rebounds within two to four years, consistent with soil rehabilitation dynamics.

These results contribute to the literatures on private politics, deforestation, and supply-chain governance. By establishing reputational campaigns as a third pillar of forest governance, operating alongside public enforcement and binding supply-chain commitments rather than merely as the catalyst behind them, this paper provides empirical support for theories of private politics (Baron, 2001). The findings that campaigns not only deter harmful practices but also promote agricultural intensification challenge the view that environmental activism purely imposes economic costs, mirroring productivity gains observed under formal enforcement (Koch et al., 2019) but emerging in the absence of direct state intervention.

2 Institutional Background

Brazil has been at the epicenter of global tropical deforestation, primarily driven by the expansion of cattle ranching and soybean cultivation (see Appendix Figure A1). Since 2000, pasture and soybean expansion have collectively driven nearly three-quarters of Brazil's forest conversion[3]. The cattle and soy sectors represent Brazil's two largest agricultural exports, and are crucial to local and national economic performance. Brazil is currently the leading global exporter of soybeans, capturing around 59% of the global export market[4], and the top exporter of beef, accounting for approximately 27% of global trade[5].

Brazil's cattle supply chain comprises over 2.4 million ranching establishments, characterized by a highly fragmented upstream production structure, as 76.3% of ranches maintain fewer than 50 head of cattle (Instituto Brasileiro de Geografia e Estatística (IBGE), 2017). In contrast, downstream meatpacking operations are highly concentrated, with three major companies (JBS, Marfrig, and Minerva) controlling around half of the federally inspected slaughter capacity and dominating export markets (Alix-Garcia and Gibbs, 2017). This oligopsonistic market structure grants these downstream firms substantial leverage over pricing and sourcing practices (Domínguez-Iino, 2026).

Soybean farming involves far fewer producers than cattle, with around 236,000 establishments recorded in the 2017 Census (Instituto Brasileiro de Geografia e Estatística (IBGE), 2017), yet its upstream structure is similarly skewed: roughly 73% of soy farms are under 50 hectares, while output is heavily concentrated in large, capitalized operations on the Cerrado frontier, where average farm size exceeds 500 hectares in the Midwest. Downstream, exports are highly concentrated, with approximately 60% handled by the "ABCD" traders (ADM, Bunge, Cargill, Louis Dreyfus) and China's COFCO. The crop's expansion began in southern Cerrado regions, transitioned into the Amazon in the mid-1990s, and more recently returned predominantly to the Cerrado's

[3] Mapbiomas
[4] USDA
[5] USDA

MATOPIBA frontier, driven largely by global demand for animal feed and biofuels.

Cattle ranching occupies approximately 170 million hectares, an area exceeding the size of Mongolia, despite pasture areas remaining relatively stable between 2006 and 2023. Output has risen instead through intensification, with average yields climbing by roughly a fifth between 2012 and 2020 through rotational grazing, improved genetics and the spread of crop–livestock integration systems (Pereira et al., 2024).

Simultaneously, soybean cultivation has intensified, with national average yields rising from about 3.0 tonnes per hectare in 2012 to roughly 3.5 by the 2024 season[6]. Yet expansion has rarely involved removing primary forest directly: nearly 80% of all new crop area added since 2000 replaced pre-existing pasture rather than native vegetation (Zalles et al., 2019). Converting degraded pasture into cropland does require upfront investment in soil amendments, which can deter smaller farmers[7].

Converting degraded pasture to cropland is often more profitable than rehabilitating it for grazing, enabling crop expansion without new deforestation (Colussi et al., 2024), and soybean farming generates roughly four times the income per hectare of cattle ranching, giving producers a strong incentive to shift from cattle toward soy[8]. These incentives, combined with growing scrutiny of deforestation, have led NGOs and international buyers to advocate pasture conversion as a more sustainable alternative to forest-frontier expansion.

NGOs: Historical Influence and Current Role as Watchdogs Environmental NGOs have historically played pivotal roles in shaping the Brazilian cattle and soy sectors through targeted campaigns that leverage reputational risks and market access concerns. Greenpeace's landmark reports, notably *'Eating Up the Amazon'* (2006) and *'Slaughtering the Amazon'* (2009), catalyzed industry-wide agreements, namely the Amazon Soy Moratorium and the G4 Zero-Deforestation Cattle Agreements. These agreements prohibited sourcing commodities linked to new deforestation and implemen-

[6] Purdue
[7] Mongabay
[8] Mongabay

ted monitoring frameworks that significantly curtailed direct deforestation associated with these sectors (Macedo et al., 2012; Gibbs, Munger et al., 2016).

Today, NGOs continue to act as watchdogs by tracking land-use change and publicly naming companies linked to environmental harm. Greenpeace has launched the follow up report "Still Slaughtering the Amazon"[9] that targets JBS as well as other major buyers in the supply chain, whereas Global Witness published "Money to Burn" targeting banks and investors associated with the firms responsible of destroying the rainforests (examples shown in Appendix Figure A3). In addition, Organizations such as Mighty Earth publish recurring deforestation scorecards that trace commodity-driven clearing back to individual firms. Their 2023 report, for example, attributed roughly 94,000 hectares of recent deforestation (2021–2023) to indirect suppliers connected to the three largest meatpacking companies (JBS, Marfrig, and Minerva) [10]. These campaigns generate reputational pressure on firms by publicly exposing their links to deforestation, which can lead to consumer backlash, investor divestment, and regulatory scrutiny. By naming specific companies and quantifying their environmental impacts, NGOs create tangible incentives for firms to clean up their supply chains and avoid future reputational damage.

How NGOs build the evidence. The credibility of these campaigns rests on a forensic methodology that has become progressively more granular over the period of this study, combining satellite monitoring (PRODES, DETER, MapBiomas), property boundaries from the Cadastro Ambiental Rural (CAR), trade-flow records (Trase, customs and shipping data), and on-the-ground field evidence to attribute specific clearings to named ranches, traders, and end-markets. This layered approach makes modern campaigns far harder for firms to dismiss than the broader "name-and-shame" reports of the early 2000s, and is one reason why reputational pressure has become a credible threat. Appendix A.1 details the methodology with worked examples for soy and cattle.

At the same time, NGOs must also face a balancing act while advocating for forest pro-

[9] Greenpeace
[10] Mightyearth

tection, as it is crucial to consider that agriculture remains a vital source of income and poverty alleviation for millions of Brazilians. In this context, several NGOs have as their sole purpose the responsible use of existing land, such as The Nature Conservancy and Alianca da Terra. But also large NGOs such as WWF and Mongabay have increasingly shifted from blanket anti-expansion rhetoric toward promoting land-use intensification, encouraging more efficient production on already-cleared land rather than forest frontier expansion[11]. This includes supporting policies and private standards that reward the rehabilitation of degraded pasture, improved traceability, and investment in sustainable intensification practices. Framing zero-deforestation not as an anti-growth agenda but as a path to reconciling productivity with preservation has become central to NGO messaging, especially as it aligns sustainability incentives with those of the farmers, who might otherwise be opposed (Barbosa De Andrade Aragão et al., 2024).

Nonetheless, significant challenges remain. Indirect suppliers often escape regulation, resulting in deforestation "leakage" to less-monitored regions.[12] Thus, NGOs play a critical role in maintaining scrutiny and accountability within Brazil's key commodity sectors, especially given rising global demand for meat and soy, and the economic incentives that drive continued forest clearing.

2.1 Hypothesis Development

NGOs influence corporate behavior through several complementary channels:

1. **Market exclusion**, via buyer commitments and certification schemes (e.g., Soy Moratorium, G4 cattle agreement) (Baron and Diermeier, 2007);
2. **Regulatory reinforcement**, by supplying information to government monitoring systems (e.g., Brazil's DETER alerts) (Daubanes and Rochet, 2019);
3. **Reputational pressure**, generated through targeted public campaigns that threaten brand image and stakeholder trust (Aldashev, Limardi and Verdier, 2015).

[11]See i.a. Mongabay, or page 79 in WWF
[12]Greenpeace

While prior work has explored the first two mechanisms, this paper focuses mainly on the causal effect of the third, reputational risk from NGO campaigns on environmental outcomes, while controlling for the first two. Building on private politics theory (Baron, 2001; Baron, 2003), in which publicly visible firms respond strongly to reputational threats (Aldashev, Limardi and Verdier, 2015), I formalize this logic in a simple model of a profit-maximizing agricultural firm that trades off expanding cleared land (the extensive margin) against intensifying production on existing land (the intensive margin), where NGO campaigns raise the reputational cost of clearing. The model, sketched in Appendix A.2, yields the three predictions I take to the data:

H1: **NGO campaign exposure reduces deforestation.** Municipalities more commercially linked to firms recently targeted by NGO campaigns will experience a statistically significant decline in deforestation. This reflects firms' efforts to deter future reputational damage by cleaning up their supply chains.

H2: **NGO campaign exposure reallocates land use from cattle pasture toward soy on already-cleared land.** Because cattle ranching is land-intensive while soy is more capital-intensive (relying on mechanization, fertilizer, and improved seeds), firms under campaign pressure will curtail sourcing from pasture-driven frontier expansion and favor soy grown on previously deforested areas. H2 therefore predicts a compositional shift in agricultural land use, complementary to the level reduction in clearing posited by H1.

H3: **Campaign exposure induces intensification and increased land productivity.** Faced with reputational pressures and constraints on land expansion, firms and producers will intensify land-use practices, adopting yield-enhancing technologies or transitioning to higher-value crops (e.g., soybeans). This outcome aligns with economic theory predicting that when land expansion is constrained (e.g., by environmental or reputational pressures), producers substitute toward more capital-intensive, yield-enhancing practices to maintain or increase output.

These hypotheses imply heterogeneous treatment effects depending on the extent of a municipality's trade exposure to targeted firms. Municipalities with higher exposure to reputational pressure will experience larger environmental responses, generating spatial variation in deforestation and agricultural intensification outcomes that I exploit in my empirical analysis.

3 Data

My analysis draws on a panel of 3,520 Brazilian municipalities that exported soy or cattle between 2012 and 2023. To estimate the causal impact of NGO campaign pressure on local deforestation and land use, I combine three primary data sources: NGO campaign records (SIGWATCH), international commodity supply chains (TRASE), and high-resolution land cover data (MapBiomas). These are augmented with administrative records on municipal characteristics and enforcement actions. Together, these sources allow me to trace how reputational pressure targeted at firms transmits through trade linkages to land-use decisions at the municipality level.

NGO Campaigns (SIGWATCH) I use data from SIGWATCH, a commercial database that monitors global NGO activity across over 19,700 companies in 179 countries. Each observation records the date, target firm, campaign theme (e.g., deforestation, labour, pollution), and a sentiment score ranging from -2 (strongly negative) to $+2$ (strongly positive). I focus on negative campaigns targeting companies active in Brazil between 2011 and 2021, restricting attention to those explicitly linked to deforestation drivers, namely cattle industry, soy industry, logging, and agriculture. This yields 169 distinct deforestation-related campaigns, corresponding to 423 firm-campaign pairs across 36 targeted firms. Each campaign is matched to a global ultimate parent company. Figure 1 shows the distribution of these campaigns over time. Campaigns rise sharply from 2018 onward, reflecting three forces: the 2019 Amazon wildfires and the global attention they attracted, the election of Jair Bolsonaro and his administration's rollback of environmental protections, and a broader secular increase in NGO campaigning worldwide over the same period, documented in Appendix Figure A5.

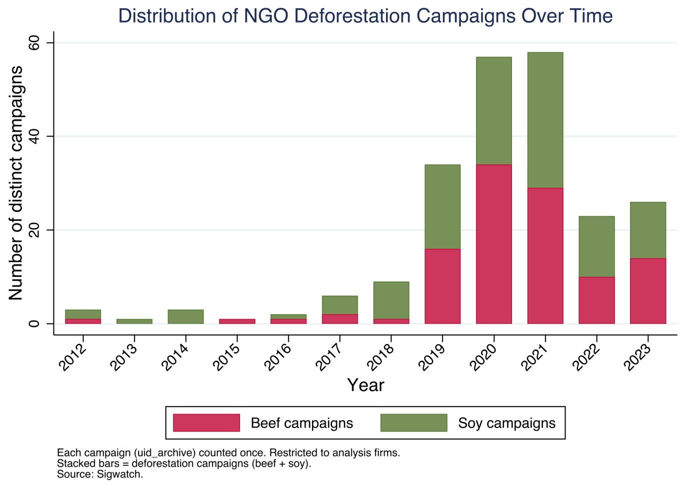


Figure 1: Distribution of deforestation-related NGO campaigns over time, by sector. Stacked bars show the number of distinct beef and soy campaigns per year, restricted to firms in the analysis sample. Source: SIGWATCH.

Commodity Supply Chains (TRASE) To trace supply chains from firms to municipalities, I use TRASE data on Brazilian soy and beef exports from 2011 to 2022. TRASE reconstructs supply chains from customs and shipment-level trade data, covering 95–98% of official export volumes (Domínguez-Iino, 2026). Each shipment includes the exporter identity, producing municipality, destination, volume (in tonnes), and export value; Appendix Figure A4 illustrates the resulting beef supply-chain mapping across states, biomes, and exporters. I aggregate shipments to the municipality-exporter-year level and link exporters to targeted firms in the SIGWATCH data. This enables construction of a shift-share exposure measure: the share of a municipality's export volume in a given year routed through exporters subject to NGO campaigns. This measure captures the transmission of firm-level reputational pressure to local economic actors. As shown in Figure A8, NGO-targeted firms account for approximately 75–85% of beef export volume and 60–75% of soy export volume throughout the sample period, underscoring the economic relevance of the targeted exporters. Beyond trade flows, TRASE also records whether a given volume is traded under a zero-deforestation supply-chain commitment: the Amazon Soy Moratorium for soy, and, for beef, the voluntary G4 Cattle Agreement together with the legally binding *Termos de Ajustamento de Conduta*, TACs. This lets me distinguish volumes already governed by binding commitments from those exposed only to reputational pressure.

Land Use and Deforestation (MapBiomas) I use satellite-based land cover data from MapBiomas, which provides annual 30-meter resolution LANDSAT classifications for Brazil from 2011 to 2023. I classify land cover into three categories: natural forest (including savannah and rainforest[13], pasture, and soy cultivation). From these, I derive three sets of municipality-level outcomes:

- *Land-use shares*: Changes in the proportion of municipal land allocated to soy, pasture, or forest in period t
- *Land-use transitions*: Transitions between land uses, i.a. forest-to-cattle, forest-to-soy, and cattle-to-soy as a percentage of the municipal area in period t
- *Productivity*: Output per hectare in the cattle and soy sectors, combining sector export volumes from TRASE (in tonnes) with the corresponding MapBiomas land area (pasture for cattle, soy for soy), capturing land-use intensity rather than allocation

These measures allow me to capture land use, land conversion, and land-use intensity dynamics in response to NGO pressure.

Other Economic Data I include controls for municipal characteristics from IBGE and administrative records: log GDP per capita, adult literacy rate, and rural population share. To account for enforcement heterogeneity, I include the number of environmental fines issued by IBAMA per municipality-year (Assunção, Gandour and Rocha, 2023), and a binary indicator for "Priority Municipality" status based on the federal blacklist policy (Assunção, McMillan et al., 2023). These controls help isolate the effect of NGO campaigns from confounding institutional and economic factors.

[13] This classification follows standard practice in the literature. While rainforest and savannah differ in vegetation density and structure, both are typically grouped under "natural forest" due to their ecological significance and relevance for deforestation monitoring. The Amazon biome is predominantly rainforest, whereas the Cerrado is primarily savannah.

3.1 Sample Construction and Temporal Aggregation

To ensure meaningful variation in both treatment intensity and land-use outcomes, I exclude municipalities in the bottom 5th percentile of baseline forest cover and cumulative trade volume. These areas are characterized by negligible deforestation pressure and limited exposure to NGO campaigns due to minimal trade integration. The resulting sample comprises 3,520 municipalities observed between 2012 and 2023. The summary statistics can be seen in Table 1

To reduce high-frequency noise and better capture the medium-term dynamics of NGO campaign effects, I aggregate the data into non-overlapping two-year periods. This temporal structure reflects a trade-off between statistical precision and responsiveness to treatment timing. Firm-level local projection estimates using annual Trase-based deforestation exposure, and municipal level regressions using the shift-share exposure variable suggest that campaign effects emerge gradually and persist beyond a single year, supporting the choice of two-year intervals. As a robustness check, I re-estimate the main specifications using three-year aggregations to assess the sensitivity of the results to temporal smoothing.

4 Empirical Strategy

To estimate the causal effect of NGO campaigns on land-use outcomes, I use a shift-share design that maps firm-level shocks to the municipality level. The core intuition is that local land-use dynamics are driven in part by firm-level campaign shocks, NGO campaigns shaming major traders on deforestation-related grounds, that in turn put pressure on local actors or cut trade relations with them.

Table 1: Summary Statistics by Campaign Exposure

Variable	**All**	**Exposed**	**Not exposed**
Outcomes: $\Delta Y^{(\ell)}_{m,t}$ *— Land-use change (% of mun. area)*			
Δ Forest	−0.22 (1.04)	−0.19 (0.82)	−0.25 (1.18)
Δ Cattle	−0.39 (1.71)	−0.68 (1.74)	−0.16 (1.65)
Δ Soy	0.68 (1.99)	0.64 (1.72)	0.71 (2.24)
Outcomes: Land-use transitions (%)			
Forest $\to$ Cattle	0.29 (0.65)	0.29 (0.57)	0.30 (0.71)
Cattle $\to$ Soy	0.09 (0.27)	0.14 (0.33)	0.03 (0.16)
Forest $\to$ Soy	0.01 (0.06)	0.01 (0.05)	0.01 (0.06)
Outcomes: Productivity (log)			
Cattle (heads/ha)	−1.93 (0.39)	−1.77 (0.31)	−2.12 (0.45)
Soy (t/ha)	1.08 (0.26)	1.10 (0.22)	1.05 (0.31)
Treatment: Campaign exposure $\mathit{Exposure}^{(s)}_{m,t}$ *(shift-share)*			
$\text{Exposure}^{(\text{Cattle})}$	0.11 (0.20)	0.26 (0.23)	–
$\text{Exposure}^{(\text{Soy})}$	0.04 (0.14)	0.08 (0.21)	–
Exposure (combined)	0.15 (0.23)	0.34 (0.23)	–
$\text{Exposure}^{(\text{Unrelated})}$	0.13 (0.22)	0.29 (0.21)	–
Controls: $\mathbf{X}_{m,t-1}$			
Zero-deforest. commitments (cattle)	0.07 (0.18)	0.14 (0.22)	0.02 (0.10)
Zero-deforest. commitments (soy)	0.31 (0.39)	0.39 (0.42)	0.25 (0.36)
Rural population share	0.34 (0.22)	0.30 (0.21)	0.37 (0.22)
Adult literacy rate	0.12 (0.07)	0.11 (0.06)	0.13 (0.08)
GDP per capita (log)	2.46 (0.64)	2.58 (0.58)	2.38 (0.67)

Note: Mean values with standard deviations in parentheses. "Exposed" = positive shift-share exposure to deforestation-related campaigns; "Not exposed" = zero exposure. 3,520 municipalities, 2-year periods. $\text{Exposure}^{(s)}_{m,t} = \sum_f \theta^{(s)}_{m,f} \text{Camp}_{f,t}$, where $\theta^{(s)}_{m,f}$ is firm f's share of municipality m's 2011 exports in sector s and $\text{Camp}_{f,t}$ counts deforestation campaigns targeting firm f.

4.1 Estimating Equation

I estimate the effect of campaign exposure on municipal land-use outcomes using the following specification:

$$\Delta Y_{m,t} = \sum_s \beta^{(s)} \, \text{Exposure}^{(s)}_{m,t-1} + \gamma^\top \mathbf{X}_{m,t-1} + \alpha_m + \delta_t + \varepsilon_{m,t}, \qquad (1)$$

where $\text{Exposure}^{(s)}_{m,t-1}$ is the shift-share measure of municipality m's exposure to NGO campaigns targeting firms in sector s, formally defined in the next subsection. $\mathbf{X}_{m,t-1}$ is a vector of lagged controls including municipal GDP per capita, literacy rate, rural population share, environmental fines (Assunção, Gandour and Rocha, 2023), and priority-list status (Assunção, McMillan et al., 2023). Municipality fixed effects α_m absorb time-invariant geographic and institutional factors; period fixed effects δ_t capture aggregate changes in enforcement, global commodity prices, and the general rise in NGO activism. In the sector-specific regressions, because shares $\theta^{(s)}_{m,f}$ do not sum to one within each sector, I follow Borusyak, Hull and Jaravel (2025) in including an incomplete-share control $\sum_f \theta^{(s)}_{m,f}$ to avoid upward bias.

I apply equation (1) to three families of outcomes. First, **area shares**: $\Delta Y_{m,t}$ is the two-year change in the share of municipal land allocated to forest, pasture, or soy. Second, **land-use transitions**: $\Delta Y_{m,t}$ captures the change in the share of land transitioning between specific uses (e.g., forest-to-pasture, pasture-to-soy), providing direct evidence on whether campaigns deter deforestation or trigger reallocation across uses. Third, **productivity**: $\Delta Y_{m,t}$ is the change in logged output per hectare in the cattle and soy sectors, testing whether reputational pressure affects land-use intensity rather than just land allocation.

4.2 Shift-Share Exposure Construction

Let $\text{Camp}^{(s)}_{f,t}$ denote whether firm f has been targeted in year t by an NGO campaign concerning sector $s \in \{\text{Cattle, Soy}\}$, that is, a campaign about cattle ranching or soy farming displacing forest, respectively. I translate these firm-level shocks to the municipal level through *predetermined* sector-specific export shares from 2011,

$$\theta^{(s)}_{m,f} \;=\; \frac{\text{Exports of firm } f \text{ from municipality } m \text{ in sector } s \text{ in 2011}}{\sum_j \text{Exports of firm } j \text{ from municipality } m \text{ (all sectors) in 2011}}, \tag{2}$$

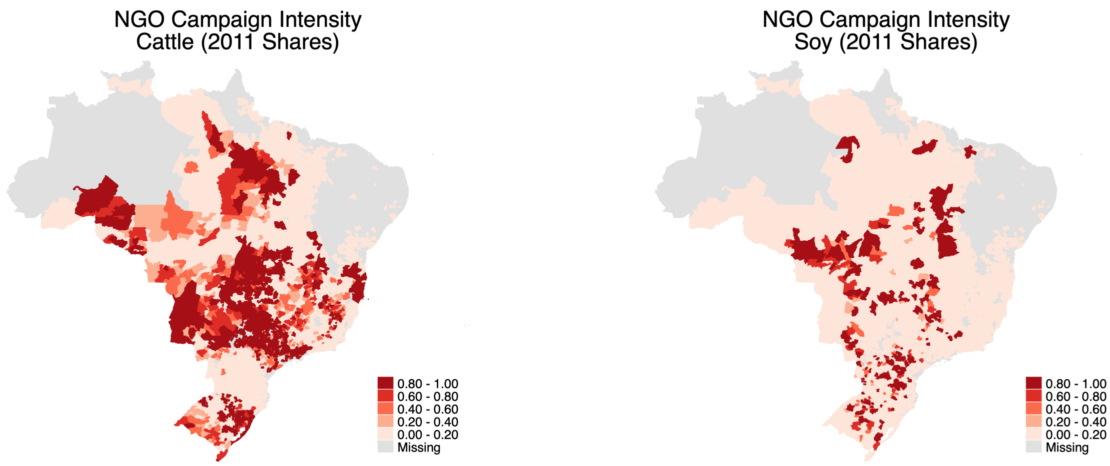
and define municipality m's sector-specific exposure in year t,

$$\text{Exposure}^{(s)}_{m,t} \;=\; \sum_f \theta^{(s)}_{m,f}\,\text{Camp}^{(s)}_{f,t}, \qquad s \in \{\text{Cattle, Soy}\}. \tag{3}$$

The sector specificity arises from two channels: the sector-specific export shares $\theta^{(s)}_{m,f}$ and the sector-specific classification of the campaign shock $\text{Camp}^{(s)}_{f,t}$. For the combined measure, I pair the broad shock $\text{Camp}_{f,t}$ with the pooled share $\theta_{m,f} = \sum_s \theta_{m,f}^{(s)}$, yielding

$$\text{Exposure}_{m,t} = \sum_f \theta_{m,f}\, \text{Camp}_{f,t}. \tag{4}$$

Therefore equation (4) represents the share of trade with targeted firms. The maps of the exposure of the municipalities to these variables can be seen in Figure 2.



(a) Cattle campaign exposure (b) Soy campaign exposure

Figure 2: Municipal-level campaign exposure in Brazil, shown for cattle (left) and soy (right). Maps are a snapshot for 2020, the peak year of NGO campaign activity in the sample. Exposure combines each firm's predetermined 2011 share of a municipality's exports with an indicator for whether that firm was targeted by a sector-specific NGO campaign in 2020; darker shading denotes higher exposure. Cattle exposure concentrates in Pará and Mato Grosso, soy exposure in Mato Grosso and the MATOPIBA frontier.

4.3 Identification

The identification strategy follows the "exogenous shifts" logic of shift-share designs: a share-weighted average of quasi-random shocks is itself as-good-as-random (Borusyak, Hull and Jaravel, 2025). Identification therefore does not require the export shares $\theta_{m,f}^{(s)}$ to be exogenous, only that the timing of NGO campaign shocks $\text{Camp}_{f,t}$ is uncorrelated with unobserved determinants of deforestation, conditional on firm observables that drive targeting by NGOs (corporate commitments, proxied by voluntary zero-deforestation pledges; firm visibility, proxied by non-deforestation-related campaigns against the firm outside Brazil; and firm size, proxied by trade volume (Baron and Diermeier, 2007)) and on period fixed effects, which absorb aggregate trends of increased NGO activity (see Appendix Figure A5). The resulting variation in $\text{Exposure}^{(s)}_{m,t}$ is plausibly exogenous to unobserved determinants of land-use change at the municipal level.

Several institutional features reinforce this claim. NGO campaigns involve extensive preparatory work: investigative reports such as Greenpeace's *Still Slaughtering the Amazon* (2020) require months or years of undercover investigation, satellite analysis, and legal review before publication, a production lag that decouples launch dates from contemporaneous local deforestation. Campaign timing is moreover driven by external focal events that command public attention (UN Climate Conferences, the EU Deforestation Regulation, firms' annual general meetings, and major sporting events) rather than by real-time changes in municipality-level forest cover. Finally, because the typical firm in the sample sources from roughly 700 municipalities over the sample period, a campaign is a blunt reputational instrument whose timing cannot respond to idiosyncratic deforestation trends in individual sourcing locations.

To reinforce this assumption, I follow two standard practices. First, I include the shift-share analogues of these firm-level covariates, $\sum_f \theta_{m,f}\,\text{Controls}_{f,t}$, to absorb potential confounders at the firm level. Second, I lag the treatment variable to ensure temporal precedence and to accommodate the diffusion of reputational pressure through supply chains (Gibbs, Munger et al., 2016).

I verify this assumption with a formal shift-level balance test (Borusyak, Hull and Jaravel, 2025), regressing the deforestation-campaign shock that enters the instrument on the firm's exposure-weighted pre-treatment municipality characteristics (initial forest cover, pasture and soy shares, enforcement intensity). Conditional on firm size and year fixed effects, none predict campaign timing: the joint test on the pre-treatment land-use shares is insignificant ($p = 0.46$, rising to $p = 0.80$ with enforcement and priority-status controls), and lagged changes in exposure-weighted deforestation do not predict campaign onset ($p = 0.45$). Results appear in Appendix Tables A2–A3.

Two placebo exercises, both reported in Figure 4, provide further support. First, I re-estimate the main specification with leads and lags of campaign exposure: only the lagged treatment should matter, while leads should not predict outcomes. Second, I replace deforestation campaigns with unrelated NGO campaigns against the same firms (e.g., labor or animal-welfare campaigns), which should not affect land use if the effects reflect deforestation-specific rather than generic reputational pressure. Results are also robust to constructing $\theta^{(s)}_{m,f}$ from lagged export shares, which strengthens the instrument without changing the core estimates (Appendix Table A4) (Borusyak, Hull and Jaravel, 2025).

Standard errors are clustered at the municipality level to account for serial correlation within municipalities over time. Regressions are weighted by municipality size so that large and small municipalities do not carry equal weight in the estimates. The Sigwatch campaign data cover the period 2011–2023, and because the estimating equations use one-period lags of campaign exposure, effective treatment variation extends through 2021. Appendix Figure A6 summarizes this identification logic.

5 Results

5.1 H1: Area

In this section, I examine whether municipalities more exposed to deforestation-related NGO campaigns subsequently reduced their rate of forest loss and the expansion of agricultural areas.

Before turning to formal estimates, Figure 3 provides descriptive evidence of the relationship. It plots indexed forest-plus-savanna area for municipalities in the top and bottom quartiles of NGO campaign exposure, normalized to 100 in 2012. The two groups track closely through 2015, consistent with parallel pre-trends. After 2016, when deforestation-related campaigns accelerate, the trajectories diverge: bottom-quartile municipalities lose roughly twice as much forest cover as top-quartile municipalities by

2023. While purely descriptive, this pattern motivates the formal analysis below.

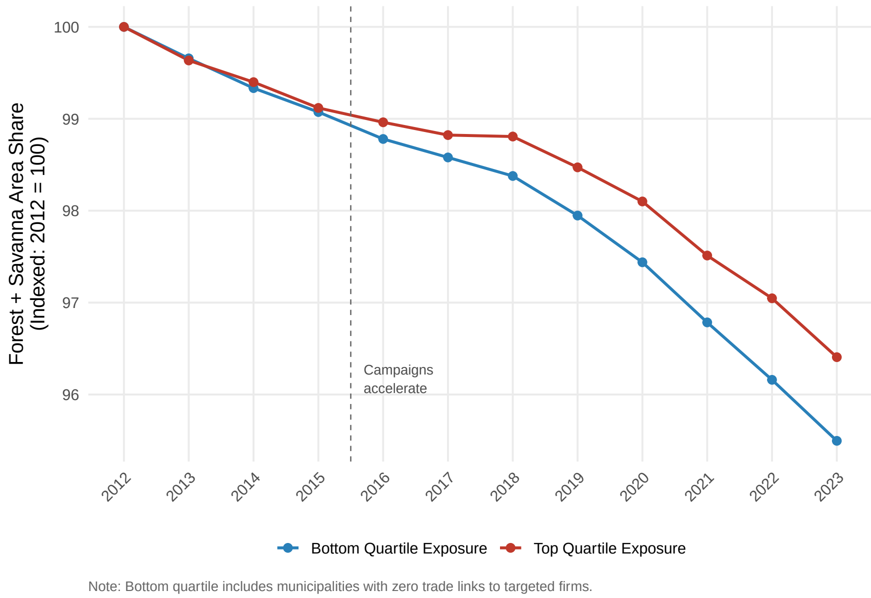


Figure 3: Forest-plus-savanna area (indexed: 2012 = 100) for municipalities in the top and bottom quartiles of NGO campaign exposure. Bottom quartile includes municipalities with zero trade links to targeted firms. The dashed line marks 2016, when deforestation-related campaigns accelerate.

Table 2 presents the estimated effect of NGO campaign exposure on municipal land-use change over two-year periods. Panel A reports the combined deforestation campaign exposure. The results show a strong and statistically significant relationship between campaign exposure and reduced deforestation: the estimated effect on forest preservation is +0.515 pp ($p < 0.01$). Given the mean change in forest cover of -0.244%, this effect is economically substantial.

Panel B decomposes the exposure into sector-specific campaigns. Cattle-related campaigns have an estimated effect of +0.620 pp ($p < 0.01$) on forest preservation, while soy-related campaigns contribute +0.409 pp ($p < 0.05$). The sector-specific decomposition also reveals significant reallocation effects. Cattle campaigns significantly reduce pasture area (-0.397 pp, $p < 0.05$), while soy campaigns substantially reduce soy area (-0.736 pp, $p < 0.01$). These patterns suggest that each campaign type constrains expansion of the sector it directly targets, consistent with reputational pressure operating through sector-specific supply chains. These dynamics are explored more directly in the transition regressions under Hypothesis 2.[14]

Table 2: Effect of NGO Campaign Exposure on Land-Use Shares (2-Year Periods)

	Forest	**Cattle**	**Soy**
Panel A: Combined Deforestation Campaign			
Deforestation Campaign	0.515***	−0.243	−0.225
	[0.151]	[0.169]	[0.184]
Observations	16,500	16,175	13,260
R^2	0.696	0.666	0.372
Panel B: Sector-Specific Campaigns			
Cattle Campaign	0.620***	−0.397**	0.342*
	[0.162]	[0.170]	[0.207]
Soy Campaign	0.409**	−0.115	−0.736***
	[0.163]	[0.205]	[0.238]
Observations	16,500	16,175	13,260
R^2	0.696	0.666	0.377
Dependent variable mean (%)	−0.244	−0.408	0.741
Municipality FE	Yes	Yes	Yes
Period FE	Yes	Yes	Yes
Controls	Yes	Yes	Yes

Note: Each column reports the two-year percentage-point change in the share of land under the indicated use. Panel A uses a combined municipality-level shift–share measure based on trade linkages to firms targeted by any deforestation-related NGO campaign. Panel B decomposes the exposure into cattle- and soy-specific campaigns. All regressions include municipality and period fixed effects and control for literacy rate, log GDP per capita, rural population share, priority-municipality status, number of environmental fines, share of exports under zero-deforestation commitments, firm size, and firm-level exposure to unrelated campaigns. Standard errors (in brackets) are clustered at the municipality level. $^{*}\, p < 0.10$, $^{**}\, p < 0.05$, $^{***}\, p < 0.01$.

A back-of-envelope calculation translates the combined coefficient into aggregate deforestation avoided. Multiplying each municipality's predicted effect ($\hat{\beta} \times \text{Exposure}_{m,t} \times \text{area}_m$) and summing across all municipality-periods yields approximately 32,275 km^2 of forest and savanna loss avoided over 2014–2023, an area approximately the size of Belgium. This represents roughly 19.0% of the total forest and savanna loss observed

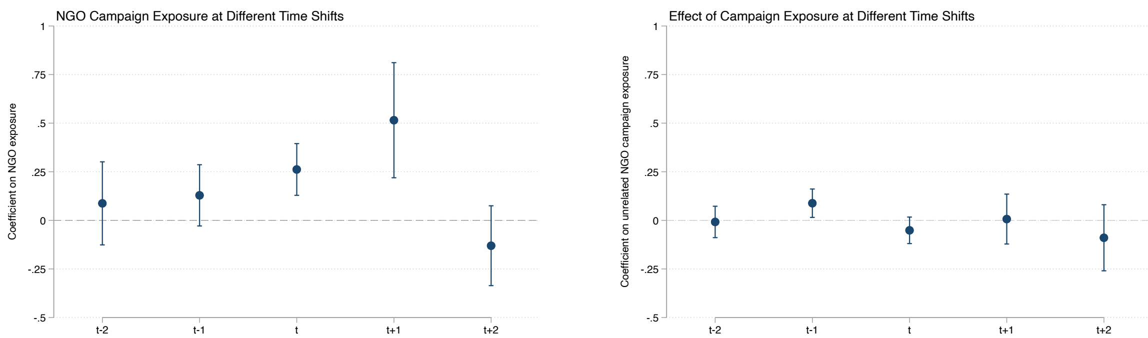
[14] Results are robust to using updated (time-varying) export shares, with a mild attenuation that is expected when shares are allowed to respond to the campaigns (Appendix A.4), and to jointly estimating the three compositional area-change equations as a seemingly unrelated regression (Appendix Table A5).

in the sample.

To put this magnitude in perspective, it is useful to benchmark the campaign effect against published estimates for Brazil's principal deforestation-control interventions. The Soy Moratorium, under which major traders agreed not to purchase soy grown on land cleared after 2008, sharply reduced the direct conversion of forest to soy: the share of new soy expansion occurring on recently deforested Amazon land fell from roughly 30% to about 1% (Gibbs, Rausch et al., 2015; Macedo et al., 2012; Kastens et al., 2017), and the most rigorous causal evaluation attributes a 35% reduction in deforestation across soy-suitable areas to the agreement (Heilmayr et al., 2020). The priority-municipality program, which subjects blacklisted municipalities to intensified monitoring and credit restrictions, has been estimated to cut deforestation by about 43% in targeted municipalities (Assunção, McMillan et al., 2023). More broadly, the bundle of federal policies under the PPCDAm action plan is estimated to have avoided on the order of 73,000 km^2 of clearing between 2005 and 2009, roughly 56% of the counterfactual, with satellite-based DETER monitoring and conditional rural credit identified as the key operative channels (Assunção, Gandour and Rocha, 2015; Assunção, Gandour and Rocha, 2023; Assunção, Gandour, Rocha and Rocha, 2020). These figures are not directly comparable, as they differ in outcome definition, geographic scope, and estimation window, but they help situate the order of magnitude of the campaign effect. Against these benchmarks, the 19.0% reduction attributable to NGO campaigns is economically meaningful yet smaller in scale, consistent with reputational pressure operating as a complementary, private channel of environmental governance that works alongside, rather than in place of, formal public enforcement and binding supply-chain agreements.

To validate the identifying assumption, I conduct two placebo exercises following Borusyak, Hull and Jaravel (2025). Figure 4(a) plots coefficients from lead-lag specifications, where the deforestation outcome is regressed on campaign exposure shifted forward and backward in time. The coefficient at $t-1$ (the actual lagged treatment) is large and significant, while all leads and distant lags are statistically indistinguishable from zero, ruling out pre-trends and anticipatory responses.

Figure 4(b) repeats this exercise using exposure to NGO campaigns unrelated to deforestation (e.g., labor practices, food safety).[15] All coefficients are small and insignificant, confirming that the main effects are specific to deforestation-related campaigns rather than reflecting general NGO activity or reputational exposure.



(a) Deforestation campaigns

(b) Unrelated campaigns (placebo)

Figure 4: Lead-lag placebo tests. Each point shows the estimated coefficient on NGO campaign exposure shifted k periods relative to outcome year t. Panel (a): deforestation-related campaigns: the sharp spike at $t-1$ confirms the treatment effect, with no pre-trends. Panel (b): unrelated (non-deforestation) campaigns targeting the same firms, zero effect at all horizons, confirming that only deforestation-specific pressure matters. 95% confidence intervals; standard errors clustered at municipality level.

Together, these tests support the causal interpretation: effects are driven by plausibly exogenous, deforestation-specific campaign shocks rather than by pre-existing trends, serial correlation, or broader NGO scrutiny. The decay in significance beyond the first lag suggests that campaign effects are relatively short-lived, consistent with Araujo, Costa and Garg (2024), who document a similar pattern for media attention during Brazil's 2019 Amazon fires.

The estimate is further supported by a battery of shift-share diagnostics reported in

[15]For example, in 2017 Oxfam America ran a campaign targeting JBS over workplace safety at its US meat-packing plants, while in 2012 the Rainforest Action Network campaigned against Cargill over speculation in agricultural commodities. These campaigns concern labor conditions and commodity market practices, respectively, and have no plausible channel through which they could affect land-use decisions in Brazilian municipalities where these firms source cattle or soy. Yet because JBS and Cargill are in the treatment group, these campaigns contribute to the placebo exposure measure. If the shift-share instrument merely captured general media attention to targeted firms rather than deforestation-specific pressure, such campaigns should also predict land-use change in Brazil. They do not.

the Appendix: a Rotemberg weight decomposition (Goldsmith-Pinkham, Sorkin and Swift, 2020) and leave-one-firm-out and leave-one-NGO-out jackknives confirm that no single firm or NGO drives the result (Appendix Table A6 and Figures A10–A11). A randomization-inference test that reshuffles campaign assignments across firms places the headline estimate in the right tail of its permutation distribution (RI $p = 0.062$); the marginal significance reflects the small number of targeted firms rather than a weak effect, as detailed in Appendix A.6.

5.2 Difference-in-Differences: Threshold-Based Staggered Design

The shift-share IV estimates presented above capture the average effect of campaign exposure across the full distribution of trade linkages. A complementary question is whether a discrete event, the first targeting of a municipality's dominant trading partner, produces a comparable treatment effect under a more straightforward difference-in-differences design. This section implements a threshold-based staggered DiD following Baker, Larcker and Wang (2022), which avoids the negative-weighting problems of conventional two-way fixed effects estimators under staggered adoption. I adopt the stacked estimator rather than the alternative heterogeneity-robust approaches (Callaway and Sant'Anna, 2021; Sun and Abraham, 2021; De Chaisemartin and D'Haultfœuille, 2020; Borusyak, Hull and Jaravel, 2022) because it transparently accommodates the rich set of time-varying controls and the area weighting used throughout the paper within a single regression, keeping the design directly comparable to the shift-share specification while delivering the same robustness to treatment-effect heterogeneity.

A municipality is classified as treated in year g if any firm accounting for more than 50% of its 2011 combined beef and soy FOB exports is first targeted by a deforestation-related NGO campaign in year g. Municipalities whose dominant partners are never targeted serve as never-treated controls. Using the predetermined 2011 trade shares ensures that treatment assignment is not endogenous to post-campaign supply-chain adjustments. The 50% threshold identifies municipalities where a single firm exercises substantial economic influence, making them most directly exposed to that firm's reputational shocks. I drop cohorts treated in 2011 or 2012, as the panel begins in 2012 and these municipalities lack a pre-treatment period.

For each treatment cohort g, I construct a sub-experiment comprising the cohort's treated municipalities, all never-treated municipalities, and not-yet-treated municipalities whose own treatment date falls beyond the event window. Each sub-experiment spans three pre-treatment years and five post-treatment years. The sub-experiments are then stacked and estimated jointly with cohort-specific unit fixed effects and cohort × year fixed effects, ensuring that comparisons are always made within the same cohort-specific time window. Specifically, I estimate Equation (5):

$$Y_{m,g,t} = \alpha_{m,g} + \delta_{g,t} + \sum_{k \neq -1} \beta_k \cdot \mathbf{1}[\text{event time} = k] \times \text{Treated}_m + \mathbf{X}'_{m,t-1}\boldsymbol{\gamma} + \varepsilon_{m,g,t} \quad (5)$$

where $\alpha_{m,g}$ denotes unit-within-cohort fixed effects, $\delta_{g,t}$ denotes cohort × year fixed effects, and the event-time coefficients β_k are estimated relative to the omitted period $k = -1$. Controls include lagged unrelated campaign exposure, enforcement variables, corporate commitments, and the incomplete-share control of Borusyak, Hull and Jaravel (2022). All regressions are weighted by municipality area and cluster standard errors at the municipality level.

Figure 5 presents the event-study estimates for the 50% threshold. Because the outcome is the annual *change* in forest share, each coefficient measures a flow effect: this effect emerges at treatment, builds through t+3, and then stabilises at a positive plateau, with pre-treatment coefficients at $t-2$ and $t-3$ small and statistically insignificant. The pasture share declines progressively after treatment, with stable pre-trends (Appendix Table A9). Soy area changes are small and statistically insignificant throughout, consistent with campaign effects operating primarily through reduced pasture expansion rather than changes in soy cultivation. Results are robust to alternative threshold choices.[16]

[16] Raising the threshold to 75% yields near-identical post-treatment effects with better pre-trends.

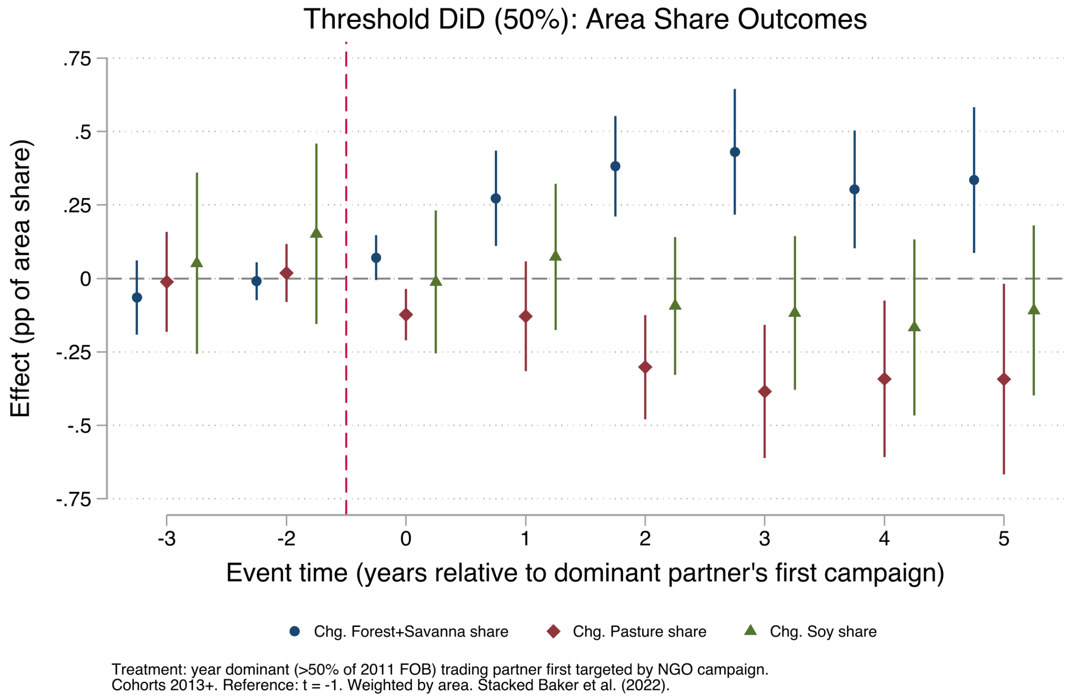


Figure 5: Threshold-based staggered DiD (50% trade share threshold). Treatment: year in which a municipality's dominant trading partner (>50% of 2011 combined beef and soy FOB) is first targeted by an NGO campaign. Reference period: $t = -1$ (omitted). 95% confidence intervals; standard errors clustered at municipality level. Stacked Baker, Larcker and Wang (2022) estimator. Weighted by municipality area.

The saturating profile is noteworthy: the per-period effect rises through t+3 and then settles at a positive plateau rather than fading to zero. Because this flow effect stays positive, the *cumulative* (level) protection keeps accumulating over the horizon. Unlike the shift-share placebo tests, which show effects concentrated at a single lag, the DiD design reveals that being linked to a targeted firm delivers sustained, not one-off, protection. This is consistent with repeated targeting in the sample (see Figure A7): once a firm enters the campaign spotlight, it faces sustained NGO scrutiny.

These DiD results serve two purposes. First, they provide an independent validation of the shift-share IV findings under a simple event-study framework with a dummy variable. The magnitudes are broadly comparable: the DiD estimate of t+1 + t+2 $\approx$ 0.6, comparable to the shift-share IV estimate of 0.515 pp for a two-year period. Second, they reveal the dynamic profile of campaign effects: a flow effect that builds and then saturates, so that cumulative protection accumulates, which the two-period shift-share design cannot identify. This offers evidence that the cumulative pressure of repeated targeting, rather than a single campaign shock, drives the sustained reduction in deforestation.

5.3 H2: Land Use Transitions

To examine whether campaigns trigger reallocation from low-productivity grazing toward soy, I estimate transition-level regressions using sector-specific campaign exposure. Table 3 reports the results.

Both campaign types strongly deter forest-to-cattle conversion: beef-related campaigns reduce the forest-to-pasture transition rate by 0.420 pp ($p < 0.01$, column 1), while soy-related campaigns show an even larger effect (-0.589 pp, $p < 0.01$). Beef campaigns also increase direct forest-to-soy conversion ($+0.025$, $p < 0.05$, column 3), suggesting that some land is redirected toward higher-value uses. Neither campaign type significantly affects pasture-to-soy reallocation in this specification (column 2), though the mechanism analysis below reveals important heterogeneity by forest cover.

The dominant pattern is one of deterrence: both campaign types strongly curtail pasture-driven deforestation, consistent with cattle ranching being the primary margin of adjustment. The finding that soy campaigns also deter forest-to-cattle conversion, despite targeting a different sector, is most plausibly explained by the interaction between NGO pressure and the Amazon Soy Moratorium. Signed in 2006 and enforced through property-level satellite monitoring, the Moratorium commits the ABCD traders and COFCO to not purchase soy grown on Amazon land deforested after the cutoff date (Heilmayr et al., 2020). Because Brazilian forest frontier expansion typically follows a sequential pattern (forest $\rightarrow$ pasture $\rightarrow$ soy) (Macedo et al., 2012), the Moratorium mechanically forecloses the soy option for any newly cleared land: a farmer clearing forest for cattle today cannot later convert that plot to soy because it is permanently ineligible under the Moratorium. Intensified NGO pressure on the soy sector tightens

Table 3: Effect of NGO Campaigns on Land-Use Transitions (2-Year Periods)

	Forest→Cattle	**Cattle→Soy**	**Forest→Soy**
Lagged Campaign Intensity			
Cattle Campaign	-0.420^{***}	0.064	0.025^{**}
	[0.114]	[0.069]	[0.011]
Soy Campaign	-0.589^{***}	−0.059	−0.011
	[0.125]	[0.069]	[0.009]
Dependent variable mean (%)	0.309	0.088	0.009
Municipality FE	Yes	Yes	Yes
Period FE	Yes	Yes	Yes
Controls	Yes	Yes	Yes
Observations	16,100	12,780	12,215
R^2	0.800	0.609	0.592

Note: Each column reports the two-year change (in percentage points) in the share of land transitioning between uses following NGO campaign exposure. Campaign intensity is a lagged municipality-level shift–share measure based on trade linkages to firms targeted by cattle- or soy-related campaigns. All regressions include municipality and period fixed effects and control for literacy rate, log GDP per capita, rural population share, priority-municipality status, number of environmental fines, share of exports under zero-deforestation commitments, firm size, and firm-level exposure to unrelated campaigns. Standard errors (in brackets) are clustered at the municipality level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

this enforcement margin, raising monitoring credibility, blacklist stringency, and trader compliance costs, which in turn reduces the expected return to frontier cattle clearing even though no cattle-specific pressure is applied. Legacy pasture cleared before the Moratorium cutoff, by contrast, remains freely convertible to soy, which explains why the displacement toward soy documented below is concentrated on likely pre-existing cleared land.

5.3.1 Land-Use Displacement: Pasture-to-Soy Conversion in Low-Forest Municipalities

NGO campaigns strongly reduce forest-to-pasture conversion, but this margin requires forest left to clear. Where forest is already depleted, campaign pressure cannot deter clearing and may instead operate on a different margin: redirecting existing pasture into soy. The descriptive gradient is consistent with this (Appendix Figure A14): municipalities below 5% forest cover lose roughly 1.1 pp of pasture per period while gaining a nearly identical amount in soy, whereas high-forest municipalities gain both from ongoing deforestation.

To test whether campaigns *cause* this substitution, I interact sector-specific beef and soy exposure with an indicator for low initial forest cover (<10% forest-plus-savanna in 2012). Table 4 reports the results. Without the interaction (column 1), neither sector has a significant average effect on pasture-to-soy transitions (beef +0.066; soy −0.057), matching the pooled results. The interaction in column 2 reveals the heterogeneity: the soy interaction with low forest is large and significant (+0.207, $p < 0.01$), while the beef interaction is small and insignificant (+0.049, $p = 0.19$). This asymmetry follows the Moratorium logic: in low-forest municipalities most pasture was cleared before the cutoff and remains legally eligible for soy, so soy campaigns cannot deter clearing but do redirect expansion onto legacy pasture, the one margin the Moratorium leaves open.

This displacement complements the main results: where campaigns have exhausted the deforestation margin, pressure shifts to the intensive margin, accelerating pasture-to-soy conversion in already-cleared areas.

5.4 H3: Productivity

The previous sections show that NGO campaigns reduce deforestation and shift land use from low-productivity cattle grazing toward more intensive soy cultivation. Here I test whether campaign exposure also raises land-use intensity, defined as output per hectare, as would be expected if reputational costs constrain expansion and push firms and producers toward yield-enhancing investment on already-cleared land.

Table 5 reports the estimated effect of NGO campaign exposure on production intensity, measured as log output per hectare. Column (1) provides tentative evidence that exposure to cattle-related campaigns raises cattle productivity, though the effect is significant only at the 90% level and should be read as suggestive. Full exposure implies a gain of 3.6% over a two-year period; since full exposure is rare in practice, a more

Table 4: Pasture-to-Soy Transition: Heterogeneity by Initial Forest Cover

	Pasture → Soy transition	
	(1)	(2)
Lagged Campaign Intensity		
Beef Campaign	0.066	0.062
	(0.068)	(0.070)
× Low forest		0.049
		(0.038)
Soy Campaign	−0.057	−0.063
	(0.068)	(0.068)
× Low forest		0.207***
		(0.058)
Dependent variable mean	0.086	0.086
Municipalities	2,767	2,767
Municipality FE	Yes	Yes
Period FE	Yes	Yes
Controls	Yes	Yes
Forest interaction	No	Yes
Observations	13,835	13,835
R^2	0.607	0.608

Note: Both columns report the two-year change (in percentage points) in the share of land transitioning from pasture to soy following sector-specific NGO campaign exposure. Low forest denotes municipalities with less than 10% forest-plus-savanna cover in 2012; the interaction is absorbed into the municipality fixed effects in level form, so only the interaction terms are reported. All specifications include municipality and period fixed effects, enforcement, commitment, and trade controls, firm-level exposure to unrelated campaigns, and the BHJ incomplete-share control. Regressions are area-weighted; standard errors (in parentheses) are clustered at the municipality level.
$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

realistic benchmark at mean exposure intensity implies roughly 0.36% per two-year period, or about 1.1% extrapolated linearly to six years. For comparison, Brazil's G4 Cattle Agreement and Priority List policies have been estimated to raise productivity by 5.4% and 13.5%, respectively, over six years (Koch et al., 2019; Moffette, Skidmore and Gibbs, 2021). Given that over half of Brazil's 177 million hectares of pastureland are classified as degraded (Bolfe et al., 2024), even informal pressure transmitted through lead firms could in principle accelerate pasture rehabilitation, but in light of the weak significance I treat this channel as suggestive rather than conclusive.

Table 5: Effect of NGO Campaigns on Production Intensity (2-Year Periods)

	Cattle Productivity	**Soy Productivity**
Lagged Campaign Intensity		
Cattle Campaign	0.036^{*}	-0.134^{***}
	[0.019]	[0.036]
Soy Campaign	0.011	-0.080^{**}
	[0.027]	[0.040]
Dependent variable mean[a]	−1.900	1.092
Municipality FE	Yes	Yes
Period FE	Yes	Yes
Controls	Yes	Yes
Observations	11,886	9,029
R^2	0.993	0.448

[a] Natural logarithm of average carcass weight per hectare (cattle) or grain yield per hectare (soy) in tonnes.

Note: Campaign intensity is a lagged municipality-level shift–share measure based on trade linkages to firms targeted by cattle- or soy-related NGO campaigns (Trase data). All regressions include municipality and period fixed effects and control for literacy rate, log GDP per capita, rural population share, priority-municipality status, number of environmental fines, share of exports under zero-deforestation commitments, firm size, and firm-level exposure to unrelated campaigns. Standard errors (in brackets) are clustered at the municipality level. $^{*}\, p < 0.10$, $^{**}\, p < 0.05$, $^{***}\, p < 0.01$.

In contrast, column (2) reveals that both cattle- and soy-focused campaigns significantly reduce soy productivity, with coefficients implying declines of 13.4% ($p < 0.01$) and 8.0% ($p < 0.05$), respectively. These negative effects are not necessarily inconsistent with campaign-induced efficiency: they likely reflect transitional dynamics in land reallocation. Remote-sensing studies show that the vast majority of soy expansion in Brazil occurs on former pasture (91% in Mato Grosso during 2006–2010 alone (Macedo et al., 2012)). Newly converted soy plots on degraded pasture typically suffer from poor soil quality, requiring intensive rehabilitation (e.g., liming, deep-tillage, fertilization) before achieving full yield potential. Field studies of soybean grown on recently converted degraded pasture document such initial yield constraints, driven by low phosphorus availability that responds to corrective phosphate fertilization over the first few seasons (Caires et al., 2017). The observed decline in soy productivity may therefore capture the short-run costs of transitioning onto lower-quality land, rather than a long-run efficiency loss.

To examine whether the negative productivity impacts of soy-related campaign exposure are temporary, Figure 6 presents dynamic treatment effects estimated using multiple leads and lags of the soy-specific exposure variable. The insignificant coefficients for all lead variables confirm the robustness of the identification strategy, indicating no pre-existing trends.

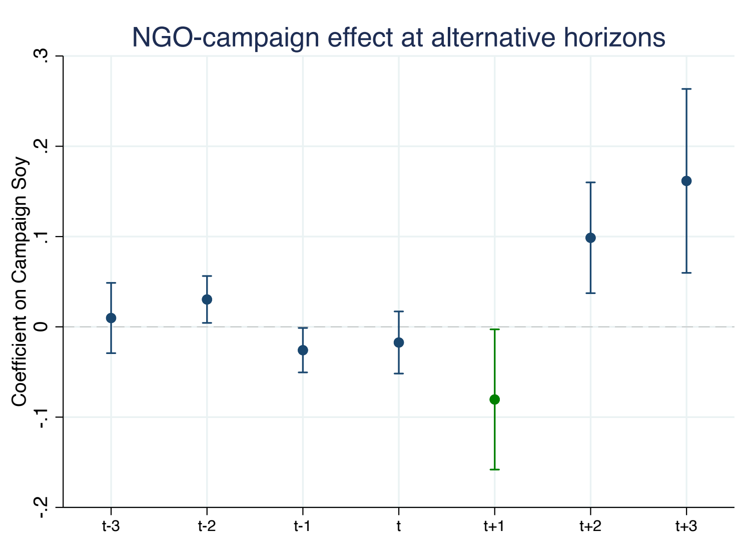


Figure 6: Event study: soy production response to NGO campaign exposure. Each point plots the estimated coefficient when the treatment is shifted by k two-year periods relative to outcome year t (labels denote $t+k$). 95% confidence intervals; standard errors clustered at municipality level. Absence of significant pre-trends ($t+1$ to $t+3$) supports the parallel-trends assumption.

The subsequent coefficients reveal a pronounced rebound effect. Immediately following campaign exposure ($t-1$), soy productivity initially declines, consistent with the short-term disruption caused by transitioning cultivation onto previously degraded pastureland. However, this productivity decline is short-lived. Coefficients for periods $t-2$ and $t-3$ turn positive and statistically significant, indicating that soy productivity not only recovers but improves significantly over time as farmers invest in soil rehabilitation methods such as liming, deep tillage, and fertilization.

Taken together, these patterns support a convergence interpretation: campaigns trigger a temporary productivity drop by raising the cost of expansion into forest and pushing cultivation onto lower-quality, degraded pasture, followed by net gains as the soil is restored and farming intensifies. These findings complement Koch et al., 2019 and Kastens et al., 2017, who document modest productivity improvements following regulatory pressure and commitments, suggesting that both formal regulation and informal NGO campaigns can stimulate efficiency-enhancing land-use change.

5.5 Heterogeneity of Campaign Effects

To assess whether the headline effect is driven by particular kinds of campaigns, I decompose the aggregate exposure variable along six dimensions: severity, prominence, geographic scope, NGO power, pressure channel, and export destination. For each, I replace the single campaign variable with sub-indicators constructed using the same shift-share logic on the relevant subset, retaining the full set of controls, fixed effects, and clustering from the baseline. Figure 7 and Appendix Table A10 report the forest-cover results.

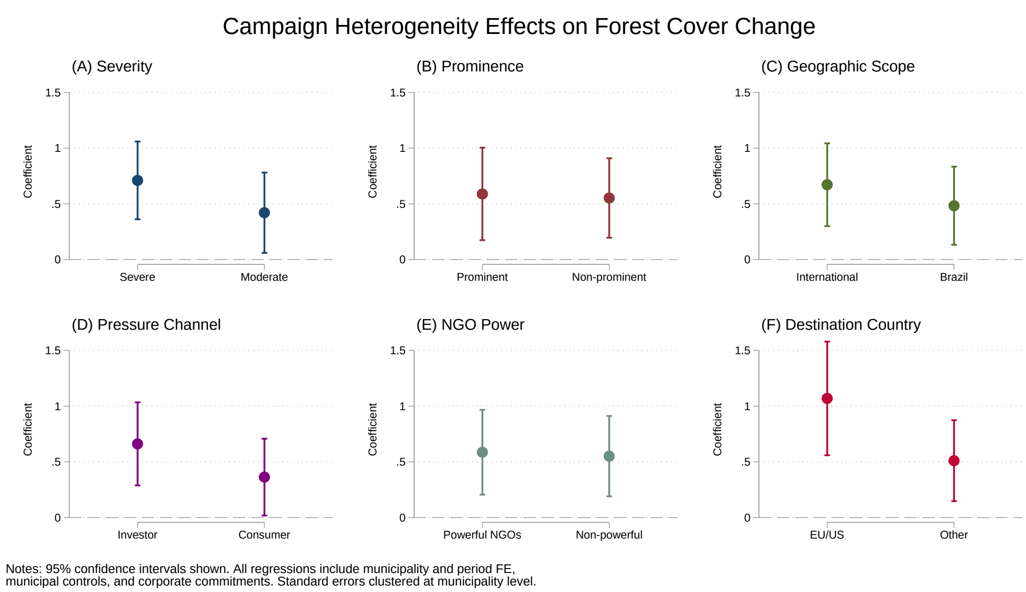


Figure 7: Heterogeneous effects of NGO campaign exposure on forest cover change across six campaign dimensions. Each panel decomposes the aggregate campaign variable into sub-categories and reports the estimated coefficient with 95% confidence intervals. All specifications include municipality and period FE, municipal controls, corporate commitments, and enforcement controls. Standard errors clustered at municipality level.

The point estimates line up with a market-mediated reputational-pressure mechanism: effects are larger for severe than moderate campaigns (0.71 vs. 0.42 pp), for internationally than domestically targeted campaigns (0.69 vs. 0.44 pp), for investor- than consumer-targeted campaigns (0.66 vs. 0.36 pp), and for trade links to EU and US rather than other markets (1.07 vs. 0.51 pp), while differences by prominence and NGO power are negligible (around 0.55–0.59 pp throughout). These contrasts are only suggestive: the 95% confidence intervals overlap for every pair, so none of the differences is statistically significant. The most defensible reading is that the deforestation-reducing effect is broadly stable across campaign characteristics, with the directional gaps pointing toward, without confirming, a stronger role for financially salient, internationally oriented pressure.

Biome Heterogeneity. Brazil's major biomes differ substantially in ecological characteristics, land-use patterns, and the intensity of agricultural frontier activity. The Amazon is the primary deforestation frontier, where cattle ranching drives large-scale forest clearing. The Cerrado, a tropical savanna, has experienced rapid soy expansion, often on already-cleared land. The Mata Atlântica, largely deforested historically, retains little native forest but supports intensive agriculture. These differences shape how much campaign pressure each biome effectively receives. Deforestation campaigns are overwhelmingly framed around the Amazon, the subject of the landmark NGO reports and of Brazil's binding supply-chain agreements, so a given firm's exposure translates above all into pressure to clean up its Amazon sourcing. The Cerrado, by contrast, remained a comparatively unregulated and less-targeted frontier over the sample period (Moffette, Skidmore and Gibbs, 2021), while the Mata Atlântica retains too little forest for clearing to be the relevant margin at all.

Panel A reports effects on the combined forest-and-savanna share. In the Amazon, both cattle and soy campaigns produce large and highly significant reductions in forest loss (0.87 and 1.43 pp, respectively), consistent with the Amazon being the active deforestation frontier where campaign pressure has the most scope to deter clearing. In the Cerrado, neither campaign type significantly affects forest cover. Given the standard errors (0.22 and 0.23), these are imprecise estimates rather than precisely-estimated zeros, so they indicate the absence of a *detectable* effect rather than evidence of no effect, in a biome where native-vegetation loss has been historically rapid and more diffuse. In the Mata Atlântica, both campaign types are significant but small in absolute terms (0.21 and 0.25 pp); relative to the biome's near-zero average forest change (a mean of -0.02%) they are not negligible, though the limited remaining forest stock caps how much preservation is physically possible.

Panel B turns to pasture, the margin through which Amazon clearing primarily operates. In the Amazon, both campaign types are associated with large reductions in pasture area (-1.64 and -1.30 pp), the mirror image of the forest-retention effects in Panel A and consistent with campaigns deterring the forest-to-cattle pathway where pressure is most concentrated. In the Cerrado, cattle campaigns reduce pasture moderately (-0.68 pp, $p < 0.05$) while soy campaigns have no significant effect, a sector-specific pattern. In the Mata Atlântica both campaign types are associated with *increases* in pasture area (+0.61 and +0.48 pp), which I read below as production relocating to an already-deforested biome rather than as a forest-clearing effect.

Panel C shows that soy-campaign effects on soy area are concentrated in the Amazon, where soy campaigns significantly reduce soy expansion (-0.67 pp, $p < 0.05$). Neither the Cerrado nor the Mata Atlântica shows a significant soy-campaign effect. Read together, the three Amazon panels are mutually consistent and roughly reconcile in land-accounting terms: the forest retained under campaign pressure (Panel A) is of the same order of magnitude as the combined pasture and soy expansion that is averted (Panels B and C), exactly the pattern expected if campaigns hold land in forest that would otherwise have entered the sequential forest $\rightarrow$ pasture $\rightarrow$ soy conversion chain described earlier in this section. In the Mata Atlântica, by contrast, the forest, pasture, *and* soy effects are all positive. It is consistent with cross-biome sourcing reallocation: firms under pressure on their Amazon supply chains shift purchases toward the deforestation-safe Atlantic biome, so more exposed municipalities there expand pasture and soy. Because little native forest remains, however, this relocated production trans-

Table 6: Biome Heterogeneity: Campaign Effects on Land-Use Change

	Amazon (1)	**Cerrado** (2)	**Mata Atlântica** (3)
Panel A: Forest and Savanna (Δ Forest)			
Cattle Campaign	0.869***	0.351	0.213***
	[0.313]	[0.222]	[0.053]
Soy Campaign	1.431***	−0.300	0.253***
	[0.330]	[0.231]	[0.062]
Observations	1,575	4,475	9,350
R^2	0.740	0.671	0.636
Dep. var. mean (%)	−0.959	−0.489	−0.023
Panel B: Pasture (Δ Cattle)			
Cattle Campaign	−1.639***	−0.676**	0.607***
	[0.475]	[0.269]	[0.204]
Soy Campaign	−1.298***	0.274	0.478**
	[0.491]	[0.254]	[0.206]
Observations	1,575	4,475	9,360
R^2	0.723	0.693	0.589
Dep. var. mean (%)	0.610	−0.615	−0.501
Panel C: Soy (Δ Soy)			
Cattle Campaign	0.179	0.659	0.674**
	[0.286]	[0.466]	[0.273]
Soy Campaign	−0.671**	−0.421	0.092
	[0.267]	[0.492]	[0.318]
Observations	1,330	4,315	6,770
R^2	0.618	0.442	0.177
Dep. var. mean (%)	0.429	0.684	0.839
Municipality FE	Yes	Yes	Yes
Period FE	Yes	Yes	Yes
Controls	Yes	Yes	Yes

Note: Each panel reports the effect of sector-specific NGO campaign exposure on the two-year percentage-point change in the indicated land-use share, estimated separately for each biome. All regressions use the shift–share instrument with predetermined 2011 trade shares and include municipality and period fixed effects, corporate commitments, enforcement controls, municipal characteristics, and unrelated campaign exposure. Standard errors (in brackets) are clustered at the municipality level. Weighted by municipality area. $^{*}\,p < 0.10$, $^{**}\,p < 0.05$, $^{***}\,p < 0.01$.

lates into little new clearing; the small, if anything positive, forest coefficients confirm that exposed Mata Atlântica municipalities do not lose more forest. The displacement therefore reflects where production occurs rather than additional deforestation that would offset the Amazon gains.

Taken together, the biome pattern tracks where deforestation campaigns are actually directed. Effects are largest in the Amazon, the focus of NGO pressure and of Brazil's binding supply-chain agreements; they are undetectable in the Cerrado, the country's most active soy frontier today but one that has drawn comparatively little campaign attention and regulation over the sample period (Moffette, Skidmore and Gibbs, 2021); and in the already-cleared Mata Atlântica they appear as relocated production rather than averted clearing. The policy-relevant reading is twofold. Reputational pressure bites hardest where the deforestation frontier and campaign attention coincide, and it leaves the Cerrado, now the leading edge of soy expansion, largely untouched. Because the production that relocates to the Mata Atlântica lands on terrain with little forest left, it does not offset the forest preserved in the Amazon.

6 Conclusion

This paper provides the first causal evidence that reputational campaigns by NGOs significantly influence land-use decisions in Brazil's soy and cattle sectors, even after major supply-chain agreements have been implemented. Exploiting exogenous variation conditional on controls in campaign timing and predetermined firm-municipality trade linkages, I show that NGO pressure operates through two complementary mechanisms. On the extensive margin, campaigns deter frontier cattle expansion, preserving forest, an effect concentrated in the Amazon, where both cattle and soy campaigns produce large reductions in forest loss. On the intensive margin, campaign pressure accelerates pasture-to-soy conversion in municipalities where forest is already depleted, redirecting land use toward more productive agriculture. These effects are accompanied by modest but meaningful gains in cattle productivity, while soy productivity initially declines on newly converted pasture but rebounds within two to four years as farmers invest in soil rehabilitation.

A decomposition of campaign exposure along six dimensions, severity, prominence, geographic scope, NGO power, pressure channel, and export destination, points consistently in one direction: estimated effects are somewhat larger for severe, internationally targeted, and investor-oriented campaigns, and for municipalities tied to EU and US markets. None of these contrasts is statistically significant, however, so the most defensible reading is that the deforestation-reducing effect is broadly stable across campaign types, with the directional gaps hinting at, without confirming, a stronger role for financially salient, internationally oriented pressure. Reassuringly, effectiveness does not depend on the size or prominence of the targeting NGO, consistent with a broad-based accountability mechanism rather than one concentrated among a few powerful organizations.

Because reputational pressure can relocate rather than eliminate the targeted activity, spatial displacement across biomes is a natural concern. The biome-level analysis is reassuring on this front, and it points to a specific transmission channel. Campaign effects on forest preservation are concentrated in the Amazon, the active deforestation frontier and the one biome where the Soy Moratorium legally binds, while the Cerrado, where the Moratorium does not apply, shows no significant response. This contrast is what one would expect if firm-level reputational pressure operates by reinforcing existing supply-chain commitments rather than through diffuse public awareness. The only sign of possible relocation is a modest rise in pasture in the heavily deforested Mata Atlântica, but with little remaining forest there this cannot offset the clearing avoided on the frontier.

These findings highlight a broader role for civil society in shaping sustainable development outcomes. Rather than simply restricting harmful practices, NGOs appear to redirect economic behavior toward more productive as well as less environmentally damaging practices. By empirically linking NGO campaign exposure to both environmental and economic outcomes, this paper contributes to the literature on non-state environmental governance, global supply-chain accountability, and land-use transformation. It offers new insights for policy design, suggesting that the facilitation of non-state monitoring, e.g. through transparency infrastructure and stakeholder engagement, are key levers in addressing commodity-driven deforestation.

References

Aldashev, Gani, Limardi, Michela and Verdier, Thierry (2015). 'Watchdogs of the Invisible Hand: NGO monitoring and industry equilibrium'. en. *Journal of Development Economics* 116, pp. 28–42. ISSN: 03043878. DOI: 10.1016/j.jdeveco.2015.03.006. URL: https://linkinghub.elsevier.com/retrieve/pii/S0304387815000425 (visited on 18th May 2025).

Alix-Garcia, Jennifer and Gibbs, Holly K. (2017). 'Forest conservation effects of Brazil's zero deforestation cattle agreements undermined by leakage'. en. *Global Environmental Change* 47, pp. 201–217. ISSN: 09593780. DOI: 10.1016/j.gloenvcha.2017.08.009. URL: https://linkinghub.elsevier.com/retrieve/pii/S095937801730170X (visited on 20th May 2025).

Araujo, Rafael, Costa, Francisco and Garg, Teevrat (2024). 'Going Viral: Public Attention and Environmental Action in the Amazon'. en. *Journal of the Association of Environmental and Resource Economists* 11.6, pp. 1605–1627. DOI: 10.1086/731080.

Assunção, Juliano, Gandour, Clarissa and Rocha, Romero (2023). 'DETER-ing Deforestation in the Amazon: Environmental Monitoring and Law Enforcement'. en. *American Economic Journal: Applied Economics* 15.2, pp. 125–156. ISSN: 1945-7782, 1945-7790. DOI: 10.1257/app.20200196. URL: https://pubs.aeaweb.org/doi/10.1257/app.20200196 (visited on 30th June 2024).

Assunção, Juliano, Gandour, Clarissa, Rocha, Romero and Rocha, Rudi (2020). 'The Effect of Rural Credit on Deforestation: Evidence from the Brazilian Amazon'. en. *The Economic Journal* 130.626, pp. 290–330. ISSN: 0013-0133, 1468-0297. DOI: 10.1093/ej/uez060. URL: https://academic.oup.com/ej/article/130/626/290/5637860 (visited on 18th May 2025).

– (2021). 'Does Credit Affect Deforestation? Evidence from a Rural Credit Policy in the Brazilian Amazon'. en.

Assunção, Juliano, Gandour, Clarissa and Rocha, Rudi (2015). 'Deforestation slowdown in the Brazilian Amazon: prices or policies?' en. *Environment and Development Economics* 20.6, pp. 697–722. ISSN: 1355-770X, 1469-4395. DOI: 10.1017/S1355770X15000078. URL: https://www.cambridge.org/core/product/identifier/S1355770X15000078/type/journal_article (visited on 24th May 2024).

Assunção, Juliano, McMillan, Robert, Murphy, Joshua and Souza-Rodrigues, Eduardo (2023). 'Optimal Environmental Targeting in the Amazon Rainforest'. en. *The Review of Economic Studies* 90.4, pp. 1608–1641. ISSN: 0034-6527, 1467-937X. DOI: 10.1093/restud/rdac064. URL: https://academic.oup.com/restud/article/90/4/1608/6747968 (visited on 30th June 2024).

Baker, Andrew C., Larcker, David F. and Wang, Charles C.Y. (2022). 'How much should we trust staggered difference-in-differences estimates?' en. *Journal of Financial Economics* 144.2, pp. 370–395. ISSN: 0304405X. DOI: 10.1016/j.jfineco.2022.01.004. URL: https://linkinghub.elsevier.com/retrieve/pii/S0304405X22000204 (visited on 15th July 2024).

Barbosa De Andrade Aragão, Rafaela, Bastos Lima, Mairon G., Burns, Georgette Leah, Ross, Helen and Biggs, Duan (2024). ''Greenlash' and reactionary stakeholders in environmental governance: An analysis of soy farmers against zero deforestation in Brazil'. en. *Forest Policy and Economics* 166, p. 103267. ISSN: 13899341. DOI: 10.1016/j.forpol.2024.103267. URL: https://linkinghub.elsevier.com/retrieve/pii/S1389934124001217 (visited on 20th May 2025).

Baron, David P. (2001). 'Private Politics, Corporate Social Responsibility, and Integrated Strategy'. en. *Journal of Economics & Management Strategy* 10.1, pp. 7–45. ISSN: 1058-6407, 1530-9134. DOI: 10.1111/j.1430-9134.2001.00007.x. URL: https://onlinelibrary.wiley.com/doi/10.1111/j.1430-9134.2001.00007.x (visited on 18th May 2025).

– (2003). 'Private Politics'. en. *Journal of Economics & Management Strategy* 12.1, pp. 31–66. ISSN: 1058-6407, 1530-9134. DOI: 10.1111/j.1430-9134.2003.00031.x. URL: https://onlinelibrary.wiley.com/doi/10.1111/j.1430-9134.2003.00031.x (visited on 18th May 2025).

– (2016). 'Self-Regulation and the Market for Activism'. en. *Journal of Economics & Management Strategy* 25.3, pp. 584–607. ISSN: 1058-6407, 1530-9134. DOI: 10.1111/jems.12162. URL: https://onlinelibrary.wiley.com/doi/10.1111/jems.12162 (visited on 18th May 2025).

Baron, David P. and Diermeier, Daniel (2007). 'Strategic Activism and Nonmarket Strategy'. en. *Journal of Economics & Management Strategy* 16.3, pp. 599–634. ISSN: 1058-6407, 1530-9134. DOI: 10.1111/j.1530-9134.2007.00152.x. URL: https://onlinelibrary.wiley.com/doi/10.1111/j.1530-9134.2007.00152.x (visited on 18th May 2025).

Bolfe, Édson Luis, Victoria, Daniel De Castro, Sano, Edson Eyji, Bayma, Gustavo, Massruhá, Silvia Maria Fonseca Silveira and De Oliveira, Aryeverton Fortes (2024). 'Potential for Agricultural Expansion in Degraded Pasture Lands in Brazil Based on Geospatial Databases'. en. *Land* 13.2, p. 200. ISSN: 2073-445X. DOI: 10.3390/land13020200. URL: https://www.mdpi.com/2073-445X/13/2/200 (visited on 19th May 2025).

Borusyak, Kirill, Hull, Peter and Jaravel, Xavier (2022). 'Quasi-Experimental Shift-Share Research Designs'. en. *The Review of Economic Studies* 89.1. Ed. by Dirk Krueger, pp. 181–213. ISSN: 0034-6527, 1467-937X. DOI: 10.1093/restud/rdab030. URL: https://academic.oup.com/restud/article/89/1/181/6294942 (visited on 5th May 2025).

Borusyak, Kirill, Hull, Peter and Jaravel, Xavier (2025). 'A Practical Guide to Shift-Share Instruments'. en. *Journal of Economic Perspectives* 39.1, pp. 181–204. ISSN: 0895-3309, 1944-7965. DOI: 10.1257/jep.20231370. URL: https://pubs.aeaweb.org/doi/10.1257/jep.20231370 (visited on 8th Apr. 2025).

Brass, Jennifer N., Longhofer, Wesley, Robinson, Rachel S. and Schnable, Allison (2018). 'NGOs and international development: A review of thirty-five years of scholarship'. en. *World Development* 112, pp. 136–149. ISSN: 0305750X. DOI: 10.1016/j.worlddev.2018.07.016. URL: https://linkinghub.elsevier.com/retrieve/pii/S0305750X18302456 (visited on 18th May 2025).

Burgess, Robin, Costa, Francisco J M and Olken, Benjamin A (2019). 'The Brazilian Amazon's Double Reversal of Fortune'. en.

Caires, E. F., Sharr, D. A., Joris, H. A. W., Haliski, A. and Bini, A. R. (2017). 'Phosphate fertilization strategies for soybean production after conversion of a degraded pastureland to a no-till cropping system'. *Geoderma* 308, pp. 120–129. DOI: 10.1016/j.geoderma.2017.08.032.

Callaway, Brantly and Sant'Anna, Pedro H. C. (2021). 'Difference-in-Differences with multiple time periods'. en. *Journal of Econometrics* 225.2, pp. 200–230. ISSN: 0304-4076. DOI: 10.1016/j.jeconom.2020.12.001. URL: https://www.sciencedirect.com/science/article/pii/S0304407620303948.

Colussi, Joana, Paulson, Nick, Schnitkey, Gary, Baltz, Jim and Zulauf, Carl (2024). *Potential for Crop Expansion in Brazil Based on Pastureland and Double-Cropping*. en. Tech. rep. farmdoc daily (14):69, Department of Agricultural and Consumer Economics, University of Illinois at Urbana-Champaign. URL: https://farmdocdaily.illinois.edu/2024/04/potential-for-crop-expansion-in-brazil-based-on-pastureland-and-double-cropping.html.

Couttenier, Mathieu and Hatte, Sophie (2016). 'Mass media effects on non-governmental organizations'. en. *Journal of Development Economics* 123, pp. 57–72. ISSN: 03043878. DOI: 10.1016/j.jdeveco.2016.07.001. URL: https://linkinghub.elsevier.com/retrieve/pii/S0304387816300463 (visited on 18th May 2025).

Daubanes, Julien and Rochet, Jean-Charles (2019). 'The Rise of NGO Activism'. en. *American Economic Journal: Economic Policy* 11.4, pp. 183–212. ISSN: 1945-7731, 1945-774X. DOI: 10.1257/pol.20180027. URL: https://pubs.aeaweb.org/doi/10.1257/pol.20180027 (visited on 18th May 2025).

De Chaisemartin, Clément and D'Haultfœuille, Xavier (2020). 'Two-Way Fixed Effects Estimators with Heterogeneous Treatment Effects'. en. *American Economic Review* 110.9, pp. 2964–2996. ISSN: 0002-8282. DOI: 10.1257/aer.20181169. URL: https://pubs.aeaweb.org/doi/10.1257/aer.20181169 (visited on 15th July 2024).

Domínguez-Iino, Tomás (2026). 'Efficiency and Redistribution in Environmental Policy: An Equilibrium Analysis of Agricultural Supply Chains'. en. *Journal of Political Economy* 134.4, pp. 1075–1109. DOI: 10.1086/739325.

Fioretti, Michele, Saint-Jean, Victor and Smith, Simon C. (2026). *NGO Activism: Exposure vs. Influence*. en. DOI: 10.48550/arXiv.2411.06875. URL: http://arxiv.org/abs/2411.06875.

Gibbs, H. K., Rausch, L., Munger, J., Schelly, I., Morton, D. C., Noojipady, P., Soares-Filho, B., Barreto, P., Micol, L. and Walker, N. F. (2015). 'Brazil's Soy Moratorium'. en. *Science* 347.6220, pp. 377–378. ISSN: 0036-8075, 1095-9203. DOI: 10.1126/science.aaa0181. URL: https://www.science.org/doi/10.1126/science.aaa0181 (visited on 18th May 2025).

Gibbs, Holly K., Munger, Jacob, L'Roe, Jessica, Barreto, Paulo, Pereira, Ritaumaria, Christie, Matthew, Amaral, Ticiana and Walker, Nathalie F. (2016). 'Did Ranchers and Slaughterhouses Respond to Zero-Deforestation Agreements in the Brazilian Amazon?: Brazil's zero-deforestation pacts'. en. *Conservation Letters* 9.1, pp. 32–42. ISSN: 1755263X. DOI: 10.1111/conl.12175. URL: https://onlinelibrary.wiley.com/doi/10.1111/conl.12175 (visited on 21st May 2025).

Goldsmith-Pinkham, Paul, Sorkin, Isaac and Swift, Henry (2020). 'Bartik Instruments: What, When, Why, and How'. en. *American Economic Review* 110.8, pp. 2586–2624. ISSN: 0002-8282. DOI: 10.1257/aer.20181047. URL: https://pubs.aeaweb.org/doi/10.1257/aer.20181047 (visited on 6th May 2025).

Hatte, Sophie and Koenig, Pamina (2020). 'The Geography of NGO Activism against Multinational Corporations'. en. *The World Bank Economic Review* 34.1, pp. 143–163. ISSN: 0258-6770, 1564-698X. DOI: 10.1093/wber/lhy007. URL: https://academic.oup.com/wber/article/34/1/143/5049878 (visited on 18th May 2025).

Heilmayr, Robert, Rausch, Lisa L., Munger, Jacob and Gibbs, Holly K. (2020). 'Brazil's Amazon Soy Moratorium reduced deforestation'. en. *Nature Food* 1.12, pp. 801–810. ISSN: 2662-1355. DOI: 10.1038/s43016-020-00194-5. URL: https://www.nature.com/articles/s43016-020-00194-5 (visited on 30th June 2024).

Instituto Brasileiro de Geografia e Estatística (IBGE) (2017). *Censo Agropecuário 2017: Resultados Definitivos*. Censo Agropecuário. Rio de Janeiro: IBGE. URL: https://sidra.ibge.gov.br/pesquisa/censo-agropecuario/censo-agropecuario-2017/resultados-definitivos.

Kastens, Jude H., Brown, J. Christopher, Coutinho, Alexandre Camargo, Bishop, Christopher R. and Esquerdo, Júlio César D. M. (2017). 'Soy moratorium impacts on soybean and deforestation dynamics in Mato Grosso, Brazil'. en. *PLOS ONE* 12.4. Ed. by Krishna Prasad Vadrevu, e0176168. ISSN: 1932-6203. DOI: 10.1371/journal.pone.0176168. URL: https://dx.plos.org/10.1371/journal.pone.0176168 (visited on 21st May 2025).

Katovich, Erik and Moffette, Fanny (2025). 'Does Local Politics Drive Tropical Land-Use Change? Property-Level Evidence from the Amazon'. en. *The Economic Journal*. Advance access. DOI: 10.1093/ej/ueaf123.

Koch, Nicolas, Zu Ermgassen, Erasmus K.H.J., Wehkamp, Johanna, Oliveira Filho, Francisco J.B. and Schwerhoff, Gregor (2019). 'Agricultural Productivity and Forest Conservation: Evidence from the Brazilian Amazon'. en. *American Journal of Agricultural Economics* 101.3, pp. 919–940. ISSN: 0002-9092, 1467-8276. DOI: 10.1093/ajae/aay110. URL: https://onlinelibrary.wiley.com/doi/10.1093/ajae/aay110 (visited on 21st May 2025).

Macedo, Marcia N., DeFries, Ruth S., Morton, Douglas C., Stickler, Claudia M., Galford, Gillian L. and Shimabukuro, Yosio E. (2012). 'Decoupling of deforestation and soy production in the southern Amazon during the late 2000s'. en. *Proceedings of the National Academy of Sciences* 109.4, pp. 1341–1346. ISSN: 0027-8424, 1091-6490. DOI: 10.1073/pnas.1111374109. URL: https://pnas.org/doi/full/10.1073/pnas.1111374109 (visited on 21st May 2025).

Moffette, Fanny, Skidmore, Marin and Gibbs, Holly K. (2021). 'Environmental policies that shape productivity: Evidence from cattle ranching in the Amazon'. en. *Journal of Environmental Economics and Management* 109, p. 102490. ISSN: 00950696. DOI: 10.1016/j.jeem.2021.102490. URL: https://linkinghub.elsevier.com/retrieve/pii/S0095069621000656 (visited on 3rd July 2024).

Pendrill, Florence, Persson, U. Martin, Godar, Javier, Kastner, Thomas, Moran, Daniel, Schmidt, Sarah and Wood, Richard (2019). 'Agricultural and forestry trade drives large share of tropical deforestation emissions'. en. *Global Environmental Change* 56, pp. 1–10. ISSN: 09593780. DOI: 10.1016/j.gloenvcha.2019.03.002. URL: https://linkinghub.elsevier.com/retrieve/pii/S0959378018314365 (visited on 28th May 2024).

Pereira, Mariana De A., Bungenstab, Davi J., Euclides, Valeria P. B., Malafaia, Guilherme C., Biscola, Paulo H. N., Menezes, Gilberto R. O., Abreu, Urbano G. P. De, Laura, Valdemir A., Nogueira, Ériklis, Mauro, Rodiney De A., Da Silva, Marta P., Nicacio, Alessandra C., Almeida, Roberto G. De, Gomes, Rodrigo Da C., Silva, Juliana C. B. and De Souza, Vanessa F. (2024). 'From Traditionally Extensive to Sustainably Intensive: A Review on the Path to a Sustainable and Inclusive Beef Farming in Brazil'. en. *Animals* 14.16, p. 2340. ISSN: 2076-2615. DOI: 10.3390/ani14162340. URL: https://www.mdpi.com/2076-2615/14/16/2340 (visited on 21st May 2025).

Potoski, Matthew and Prakash, Aseem (2013). 'Green Clubs: Collective Action and Voluntary Environmental Programs'. en. *Annual Review of Political Science* 16.1, pp. 399–419. ISSN: 1094-2939, 1545-1577. DOI: 10.1146/annurev-polisci-032211-211224. URL: https://www.annualreviews.org/doi/10.1146/annurev-polisci-032211-211224 (visited on 18th May 2025).

Sun, Liyang and Abraham, Sarah (2021). 'Estimating dynamic treatment effects in event studies with heterogeneous treatment effects'. en. *Journal of Econometrics* 225.2, pp. 175–199. ISSN: 0304-4076. DOI: 10.1016/j.jeconom.2020.09.006. URL: https://www.sciencedirect.com/science/article/pii/S030440762030378X.

Zalles, Viviana, Hansen, Matthew C., Potapov, Peter V., Stehman, Stephen V., Tyukavina, Alexandra, Pickens, Amy, Song, Xiao-Peng, Adusei, Bernard, Okpa, Chima, Aguilar, Ricardo, John, Nicholas and Chavez, Selena (2019). 'Near doubling of Brazil's intensive row crop area since 2000'. en. *Proceedings of the National Academy of Sciences* 116.2, pp. 428–435. ISSN: 0027-8424, 1091-6490. DOI: 10.1073/pnas.1810301115. URL: https://pnas.org/doi/full/10.1073/pnas.1810301115 (visited on 21st May 2025).

Zu Ermgassen, Erasmus K. H. J., Godar, Javier, Lathuillière, Michael J., Löfgren, Pernilla, Gardner, Toby, Vasconcelos, André and Meyfroidt, Patrick (2020). 'The origin, supply chain, and deforestation risk of Brazil's beef exports'. en. *Proceedings of the National Academy of Sciences* 117.50, pp. 31770–31779. ISSN: 0027-8424, 1091-6490. DOI: 10.1073/pnas.2003270117. URL: https://pnas.org/doi/full/10.1073/pnas.2003270117 (visited on 21st May 2025).

A Appendix

A.1 Background and Data

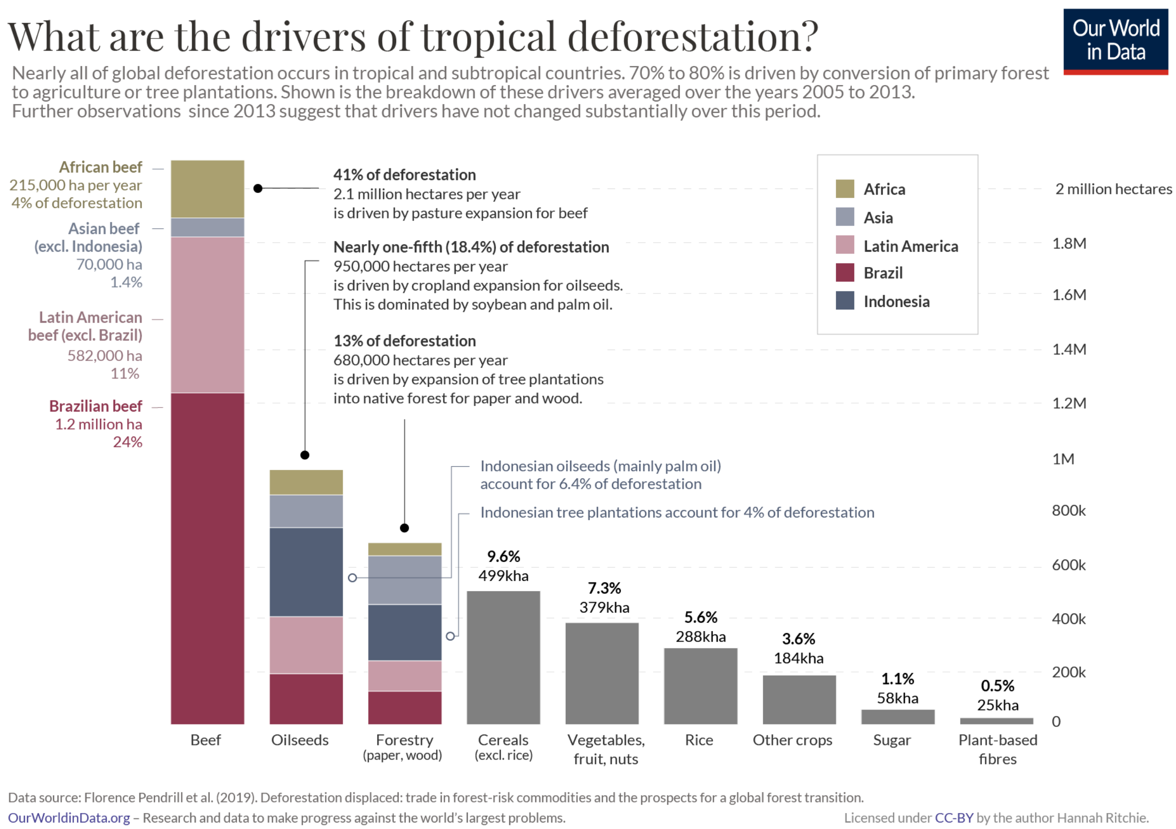


Figure A1: Drivers of Tropical Deforestation by Commodity and Region (2005–2013). This figure shows the average annual tropical deforestation attributed to major commodity groups between 2005 and 2013, disaggregated by region. Nearly 41% of tropical deforestation is linked to pasture expansion for beef, particularly in Brazil. A further 18.4% results from cropland expansion for oilseeds, such as soy and palm oil, and 13% is driven by tree plantations for paper and wood. Source: Pendrill et al. (2019), via Our World in Data.

NGO campaign methodology: worked examples. Contemporary investigations typically combine four layers of evidence: (i) satellite monitoring (PRODES, DETER, MapBiomas, or Imazon's SAD) to flag recent clearance; (ii) overlay of the cleared pixels on property boundaries from the Cadastro Ambiental Rural (CAR), which allows NGOs to attribute deforestation to a specific ranch or farm; (iii) trade-flow evidence from Trase, federal customs data, port records and private shipping manifests to link the property to a trader, port of exit and end-market; and (iv) on-the-ground field evidence, together with, for cattle, Animal Transport Permits (*Guia de Trânsito Animal*, GTA) issued by state veterinary agencies, which record the origin farm, destination slaughterhouse and number of animals in every transfer. Greenpeace's *Under Fire*[17] (2019) illustrates this approach for soy: its investigation of the 305,000-hectare Agronegócio Estrondo estate in Formosa do Rio Preto (Bahia) overlays SICAR property polygons on INEMA clearance permits and community-land mappings (Figure A2), then traces the estate's soy through Bunge and Cargill silos inside its boundaries and via Trase and Panjiva shipping records to named vessels delivering soy meal to Brake (Germany), Sète (France) and Pyeongtaek (South Korea). The cattle-side analogue is Amnesty International and Repórter Brasil's *From Forest to Farmland*[18] (2020), which uses GTA data obtained through Freedom-of-Information requests to the state agency IDARON to trace cattle grazed illegally inside the Uru-Eu-Wau-Wau Indigenous Territory and the Rio Ouro Preto and Rio Jacy-Paraná Reserves through intermediary "clean" fazendas and onward to JBS slaughterhouses in Rondônia.

[17] Greenpeace

[18] Amnesty International

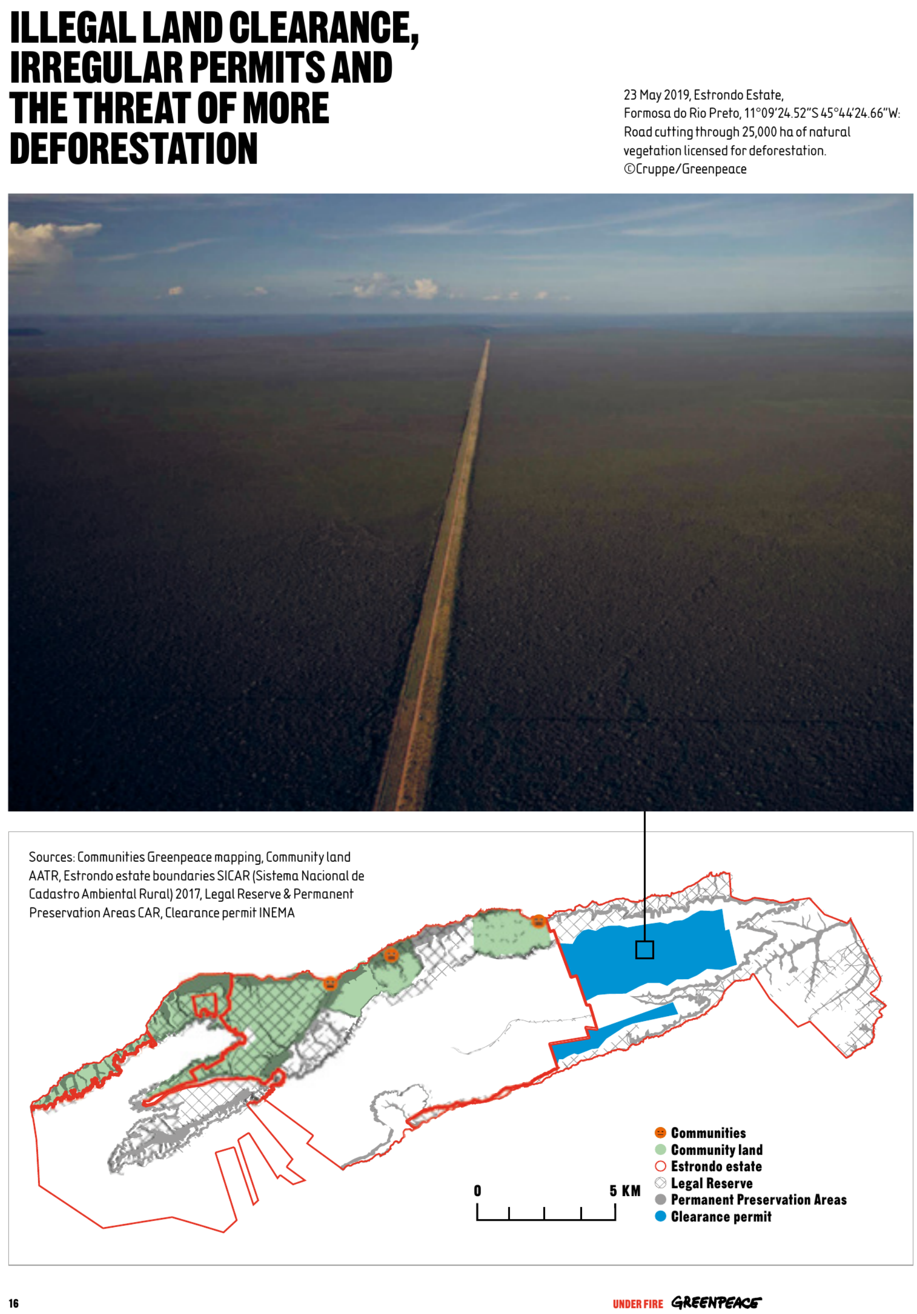


Figure A2: Example of property-level NGO investigation methodology. Greenpeace's overlay of the Agronegócio Estrondo estate (Formosa do Rio Preto, Bahia) using property boundaries from the national rural-property registry (SICAR, 2017), community land identified by the Associação de Advogados dos Trabalhadores Rurais, Legal Reserve and Permanent Preservation Areas declared in the CAR, and the May 2019 clearance permit issued by INEMA for an additional 25,000 ha of native vegetation (blue polygon). Source: Greenpeace, *Under Fire* (2019), p. 16.

Figure A3: Examples of NGO campaigns illustrating different channels of influence. Left: Greenpeace's 'Still Slaughtering the Amazon' (2020) names both the producer JBS and consumer-facing firms such as Burger King, KFC, Tesco and Aldi. Right: Global Witness's 'Money to Burn' (2019) pressures banks and investors financing deforestation-linked firms.

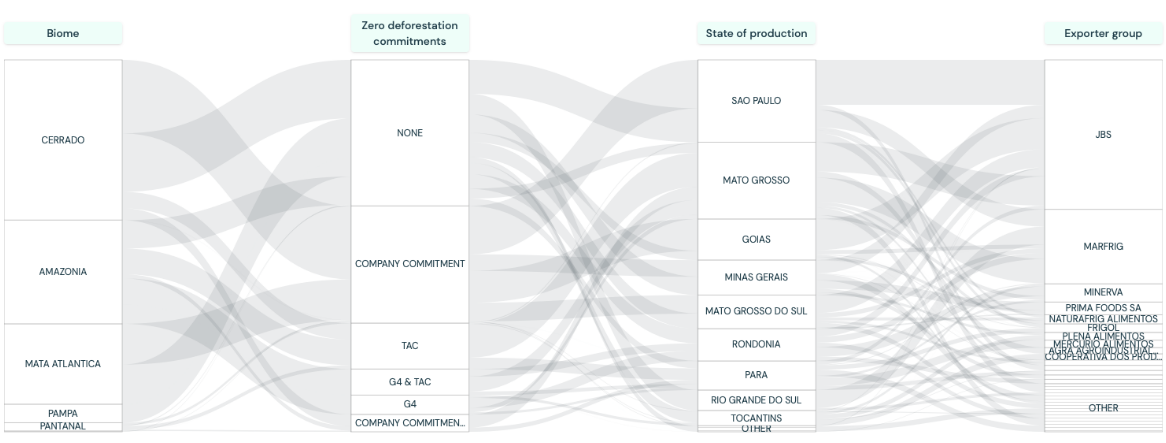


Figure A4: Beef supply chain in Brazil by State, commitment, Biome and Exporter in 2020

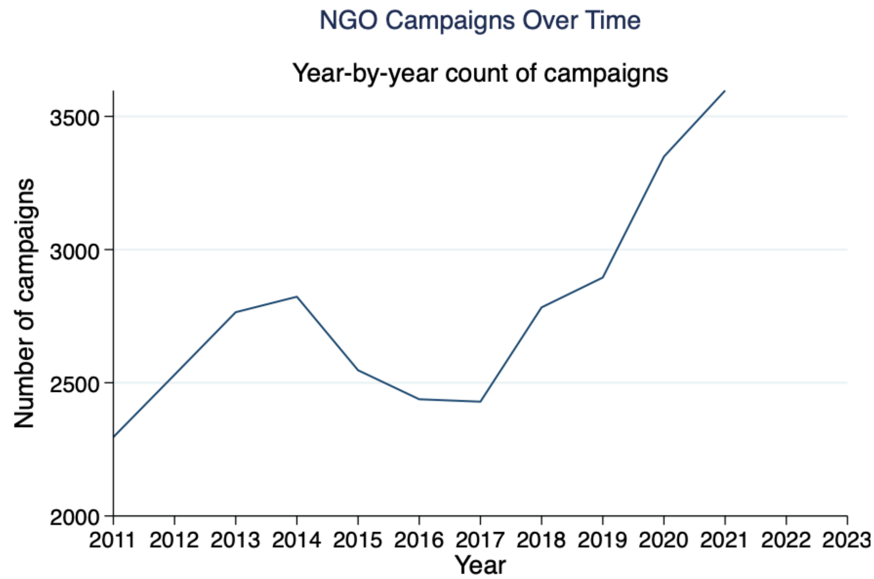


Figure A5: Trends in deforestation-related NGO campaigns worldwide

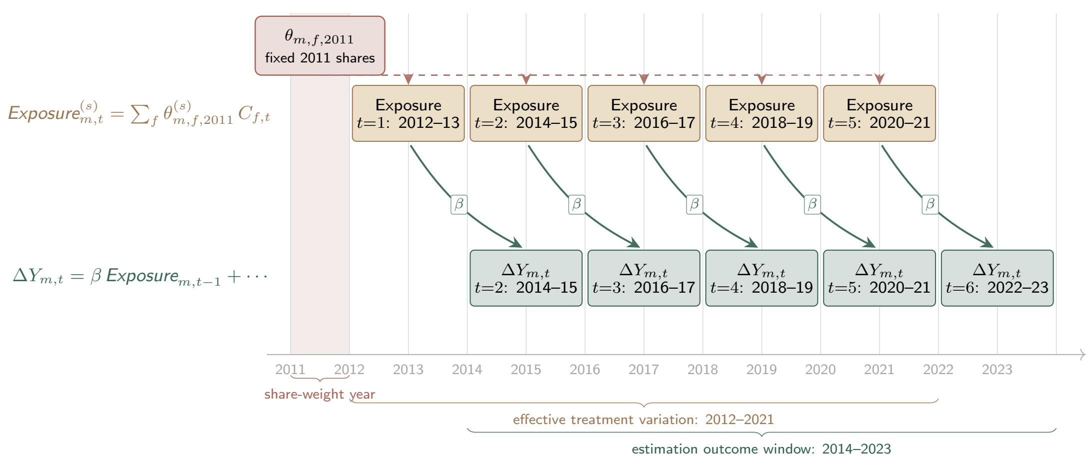
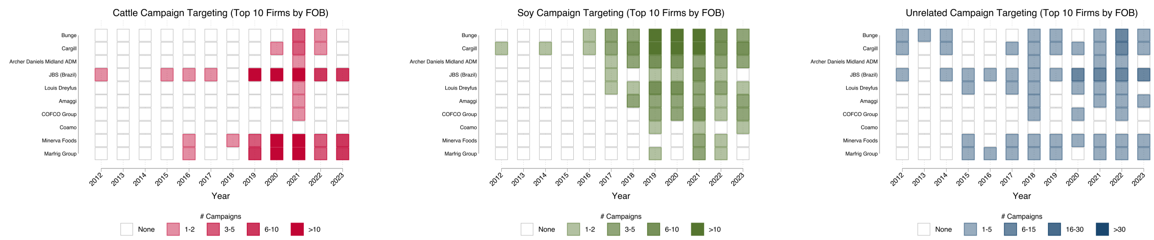


Figure A6: Identification timeline. NGO campaigns originate from long investigative cycles and external political windows at the firm level, and are transmitted to municipalities through predetermined (2011) sourcing shares. Campaign timing is decoupled from contemporaneous, municipality-level deforestation, which underpins the quasi-random assignment of the shift-share exposure.



(a) Cattle campaigns (b) Soy campaigns (c) Unrelated campaigns

Figure A7: Campaign targeting intensity by firm and year for the top 10 firms by FOB export value. Panel (a) shows cattle-related campaigns; Panel (b) shows soy-related campaigns; Panel (c) shows unrelated campaigns (e.g., labor, animal welfare). Darker shading corresponds to higher campaign intensity. Source: SIGWATCH.

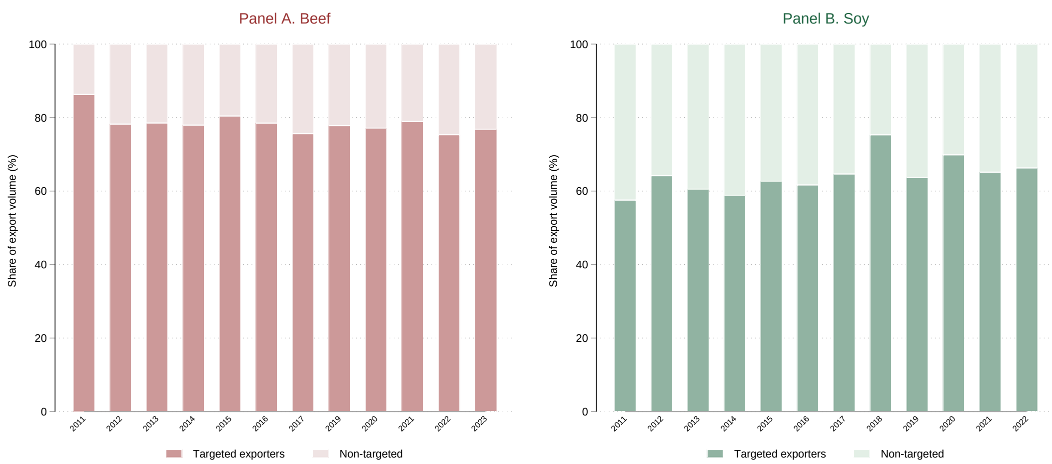


Figure A8: Share of Brazilian export volume handled by SIGWATCH-targeted versus non-targeted exporters, shown separately for beef (Panel A) and soy (Panel B). Each panel is a stacked histogram by year; the darker segment is the targeted share. Targeted firms consistently account for the majority of trade in both sectors. Source: TRASE and SIGWATCH.

A.2 A Simple Model of NGO Pressure and Land Use

This appendix sketches the simple framework that motivates hypotheses H1–H3 in Section 2.1. The model is deliberately stylized: it isolates the single margin through which NGO campaigns are assumed to operate, namely the reputational cost of clearing forest, and shows that the three empirical predictions follow from a standard cost-minimizing firm.

Production: two margins of output. A representative agricultural firm produces output Y from two inputs: land L, the area under cultivation, which is expanded by clearing forest (the *extensive* margin), and capital K, covering machinery, fertilizer, soil correction, and improved seeds (the *intensive* margin). Production is

$$Y = F(L, K), \qquad F_L, F_K > 0, \quad F_{LL}, F_{KK} < 0. \tag{6}$$

Land and capital are substitutes: a given level of output can be reached either by clearing more land or by raising yields on existing area. On the Brazilian frontier these two margins map onto the two main activities: cattle ranching is land-intensive, expanding chiefly by clearing pasture, whereas soy is capital-intensive, relying on mechanization, fertilizer, and improved seeds and grown largely on already-cleared land. I therefore assume *soy is more capital-intensive than cattle*, so that a rise in K relative to L corresponds to a shift in land use from pasture toward soy.

Costs. Intensification costs $C_K = r\,K$, where r is the cost of capital, historically high in Brazilian agriculture and thus a structural barrier to intensification. Land expansion costs

$$C_L = \Big[\, c + \rho(M) \,\Big]\, L, \tag{7}$$

where c is the physical cost of clearing, M is NGO monitoring effort, and $\rho(M)$ is a reputational risk premium with $\rho'(M) > 0$. NGO campaigns enter the firm's problem *only* through $\rho(M)$: an endogenous risk premium on the marginal cost of clearing, capturing the expected loss from boycotts, ESG exclusion, or retail delisting.

Firm's problem. The firm chooses L and K to maximize profit, given the commodity price P:

$$\max_{L,K}\ \Pi = P \cdot F(L, K)\ -\ \underbrace{rK}_{\text{intensity}}\ -\ \underbrace{[\, c + \rho(M)\,]\, L}_{\text{expansion}}. \tag{8}$$

The first-order conditions equate the marginal revenue product of each input to its marginal cost:

$$P \cdot F_K(L, K) = r, \qquad\qquad P \cdot F_L(L, K) = c + \rho(M). \tag{9}$$

Comparative statics. Totally differentiating the first-order conditions around the optimum (with F concave and $F_{LK} > 0$) gives the response to an increase in NGO monitoring effort M:

$$\frac{\partial L^*}{\partial M} < 0, \qquad \frac{\partial K^*}{\partial M} > 0, \qquad \frac{\partial (Y/L)^*}{\partial M} > 0. \tag{10}$$

Intuitively, NGO monitoring raises the shadow price of land. The firm reallocates along two margins: it clears less forest (extensive) and substitutes toward capital (intensive). Because soy is the more capital-intensive activity, this substitution shows up as a shift in land use from cattle pasture toward soy, and output per hectare rises through both channels.

From predictions to hypotheses. Table A1 maps the comparative statics into the three hypotheses tested in the paper.

Table A1: Model predictions and their empirical counterparts.

Prediction	**Mechanism**	**Empirical counterpart**
H1. Reduced deforestation $\partial L^*/\partial M < 0$	$\rho(M)$ raises the marginal cost of clearing, so the firm scales back L.	Deforestation falls in exposed municipalities; cattle pasture stops expanding into forest.
H2. Reallocation to soy $\partial K^*/\partial M > 0$	Because soy is more capital-intensive than cattle, substituting K for L shifts land from pasture toward soy.	Cattle-to-soy transitions on already-cleared land rise.
H3. Intensification $\partial(Y/L)^*/\partial M > 0$	L falls and K rises, so output per hectare increases.	Yields rise (cattle heads per ha, soy tonnes per ha) in exposed areas.

A.3 Shift-Level Balance Tests

Following the shift-share balance logic of Borusyak, Hull and Jaravel (2025), I test whether firm-level campaign targeting is predicted by the pre-treatment characteristics of the firm's sourcing municipalities. The unit of observation is the firm × year. For each firm I construct exposure-weighted averages of municipality characteristics, $\overline{X}_f = \sum_m \omega_{mf} X_m$, using the firm's 2011 FOB sourcing weights ω_{mf}, and regress an indicator for whether the firm is targeted by at least one deforestation-related campaign in a given year on these weighted characteristics with year fixed effects, estimated as a linear probability model. This binary shock is the object that enters the shift-share instrument. Table A2 uses pre-treatment levels (initial forest, pasture and soy shares, enforcement intensity, and priority-municipality share); Table A3 uses the lagged annual changes in the weighted land-use shares, testing whether recent deforestation in the sourcing area predicts campaign onset. In both panels the raw correlations (columns without firm controls) vanish once firm size (log 2011 FOB) and the number of sourcing municipalities are included: the joint F-tests on the pre-treatment land-use shares are far from significant ($p = 0.46$ for levels, rising to $p = 0.80$ once enforcement and priority-status controls are added, and $p = 0.45$ for changes), consistent with campaign timing being as-good-as-random conditional on firm observables.

Table A2: Shift-Level Balance Test: Pre-Treatment Municipality Characteristics

	(1)	(2)	(3)	(4)
Wtd. forest share (2012)	-0.251	-0.758**	0.385	0.261
	(0.250)	(0.318)	(0.348)	(0.373)
Wtd. pasture share (2012)		-0.874**	0.174	0.457
		(0.383)	(0.401)	(0.466)
Wtd. soy share (2012)		-0.982**	0.127	0.356
		(0.363)	(0.376)	(0.414)
Wtd. enforcement (2012)				0.133*
				(0.068)
Wtd. priority mun. share				-0.435***
				(0.149)
Log firm FOB (2011)			0.064***	0.074***
			(0.019)	(0.020)
N sourcing municipalities			0.000*	0.000*
			(0.000)	(0.000)
Year FE	Yes	Yes	Yes	Yes
Firm controls	No	No	Yes	Yes
Joint F (mun. chars.)		2.56	0.88	0.34
Joint p-value		0.077	0.462	0.799
Observations	324	324	324	324
R^2	0.221	0.244	0.362	0.376

Standard errors clustered at firm level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Observations are firm × year (balanced panel 2012–2023). Linear probability model; dependent variable equals one if firm f is targeted by at least one deforestation-related campaign in year t. Municipality characteristics weighted by firm's 2011 FOB share across sourcing municipalities.

Table A3: Shift-Level Balance Test: Lagged Deforestation Changes

	(1)	(2)	(3)
L.Δ Wtd. forest share	16.669*	2.266	-4.902
	(8.737)	(10.091)	(10.276)
L.Δ Wtd. pasture share		-25.230**	-1.015
		(11.765)	(12.190)
L.Δ Wtd. soy share		-1.418	-1.522
		(1.072)	(0.963)
Log firm FOB (2011)			0.070***
			(0.018)
N sourcing municipalities			0.000***
			(0.000)
Year FE	Yes	Yes	Yes
Firm controls	No	No	Yes
Joint F (land-use chg.)		4.23	0.91
Joint p-value		0.015	0.448
Observations	270	270	270
R^2	0.187	0.210	0.354

Standard errors clustered at firm level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Observations are firm × year (2013–2023). Linear probability model; dependent variable equals one if firm f is targeted by a deforestation campaign in year t. Regressors are the lagged annual change in firm-weighted municipality land-use shares (municipalities weighted by the firm's 2011 export share), testing whether recent deforestation in sourcing areas predicts campaign targeting.

A.4 Additional Robustness: Time-Varying Shares and Joint Estimation

Table A4 re-estimates the headline area-change specification using previous-period (time-varying) trade shares in place of the predetermined 2011 shares. The forest-preservation effects remain positive and statistically significant, confirming that the results are not an artifact of the 2011 weighting. Their magnitude is, however, somewhat smaller than in the headline specification (the cattle-campaign forest effect, for example, falls from 0.62 to 0.39 percentage points). This attenuation is expected for two reasons. First, time-varying shares are themselves endogenous to the campaigns: when reputational pressure leads a targeted firm to cut or clean up its sourcing from a municipality, that firm's export share there falls precisely where the campaign has bite, so the exposure measure mechanically reverts in the places and periods where the effect operates. Predetermined 2011 shares, fixed before any campaign, instead hold the trade linkage at its pre-treatment level and so capture intended exposure rather than its post-response realization. Second, annual firm-municipality shares are noisy relative to the stable 2011 anchor, and classical measurement error attenuates the coefficient toward zero. That the effect survives, with the anticipated downward shift, under this more demanding weighting both corroborates the supply-chain reallocation channel and supports the use of predetermined shares in the main specification.

Because the three area-change outcomes are compositional (forest, pasture and soy shares are mechanically linked), their regression residuals are correlated. Table A5 therefore re-estimates the system jointly as a seemingly unrelated regression. A Breusch–Pagan test strongly rejects independence of the equation residuals ($p < 0.001$), and the joint estimates confirm the main findings: campaign exposure preserves forest, and soy campaigns are associated with a large contraction in soy area, underscoring that part of the forest gain operates through the compositional reallocation of land use.

Table A4: Robustness: time-varying (lagged) trade shares

	(1) Δ Forest	(2) Δ Cattle	(3) Δ Soy
L. Cattle campaigns	0.392***	-0.089	0.553***
	(0.131)	(0.180)	(0.180)
L. Soy campaigns	0.310**	-0.368**	-0.302
	(0.137)	(0.183)	(0.190)
Dep. var. mean	-0.244	-0.408	0.741
Observations	16,500	16,175	13,260
R^2	0.695	0.662	0.374

Notes: Each column re-estimates the headline area-change specification using previous-period (time-varying) FOB trade shares in place of the predetermined 2011 shares. All columns include municipality and period fixed effects, the full set of municipal controls, corporate commitments, enforcement, and the unrelated-campaign placebo. Standard errors clustered at the municipality level in parentheses.

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

Table A5: Robustness: seemingly unrelated regressions of area changes

	(1) Δ Forest	(2) Δ Pasture	(3) Δ Soy
Cattle campaigns	0.618***	-0.340*	0.266
	(0.177)	(0.176)	(0.210)
Soy campaigns	0.385**	-0.024	-0.841***
	(0.173)	(0.202)	(0.244)
Observations	12,885	12,885	12,885

Notes: The three compositional area-change outcomes are estimated jointly as a seemingly unrelated regression system on the common sample, using 2011 FOB shares and the full control set. A Breusch–Pagan test strongly rejects independence of the equation residuals ($p < 0.001$), confirming the compositional correlation. Standard errors clustered at the municipality level in parentheses.

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

A.5 Shift-Share Diagnostics

Table A6: Rotemberg Weight Decomposition

Firm	α_f	$\hat{\beta}_f^{JID}$	SE	First-stage F
Amaggi	0.1941	0.3464	0.3130	310.83
Minerva Foods	0.1751	1.6346	0.6835	217.71
Vale Grande (food)	0.1749	1.1589	0.1656	2275.91
Bunge	0.1270	0.3703	1.2706	39.57
Marfrig Group	0.0923	3.2678	0.6553	265.98
JBS (Brazil)	0.0458	-5.4605	4.0417	8.02
Cargill	0.0397	-0.6272	3.0302	8.60
Archer Daniel Midlands ADM	0.0326	-7.0493	4.7969	11.97
Coamo	0.0225	0.3242	0.1442	3691.96
Frigol	0.0199	-0.0421	0.5889	354.59
Granol (soy)	0.0197	-0.2964	0.5843	267.56
Caramuru	0.0160	0.6958	0.3862	723.17
Louis Dreyfus Holding	0.0125	6.1792	1.9413	27.61
CHS	0.0093	0.4411	0.3447	230.73
Frigon Frigorifico Irmaos Goncalves	0.0076	–	–	–
COFCO Group	0.0061	0.5213	5.9967	8.03
Selecta (Brazil)	0.0028	0.9746	0.1730	442.97
Mitsui & Co	0.0007	–	–	–
Mitsubishi	0.0007	–	–	–
Glencore Plc	0.0006	0.0407	0.5661	41.96
Masterboi	0.0000	–	–	–
Agra Agroindustrial de Alimentos (Agra Foods)	0.0000	–	–	–

Notes: Rotemberg weights following Goldsmith-Pinkham, Sorkin & Swift (2020). α_f is the firm's weight in the shift-share estimand. $\hat{\beta}_f^{JID}$ is the just-identified estimate using only firm f's shares as an instrument. Firms with first-stage F < 5 are shown with missing $\hat{\beta}_f^{JID}$. Positive weights: 20. Negative weights: 0.

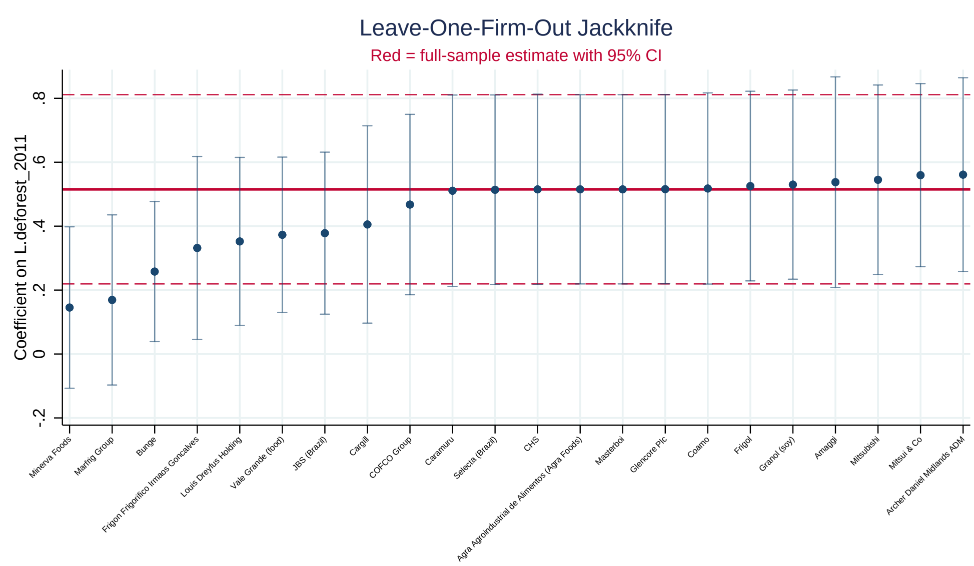


Figure A9: Leave-One-Firm-Out Jackknife Estimates. Each point shows the estimated coefficient on NGO campaign exposure when the named firm is excluded from the shift-share instrument. The dashed line indicates the full-sample estimate. All specifications include municipality and period fixed effects, municipal controls, and corporate commitments.

Table A7: Leave-One-Firm-Out Jackknife: Top 10 Most Influential Firms

Firm	Full $\hat{\beta}$	Leave-out $\hat{\beta}$	$\Delta\hat{\beta}$	Δ (%)	SE	N
Minerva Foods	0.5152	0.1455	-0.3698	-71.8	0.1288	16500
Marfrig Group	0.5152	0.1692	-0.3461	-67.2	0.1358	16500
Bunge	0.5152	0.2580	-0.2572	-49.9	0.1119	16500
Frigon Frigorifico Irmaos Goncalves	0.5152	0.3317	-0.1835	-35.6	0.1461	16500
Louis Dreyfus Holding	0.5152	0.3523	-0.1630	-31.6	0.1341	16500
Vale Grande (food)	0.5152	0.3730	-0.1423	-27.6	0.1240	16500
JBS (Brazil)	0.5152	0.3781	-0.1372	-26.6	0.1293	16500
Cargill	0.5152	0.4053	-0.1099	-21.3	0.1575	16500
COFCO Group	0.5152	0.4676	-0.0477	-9.2	0.1440	16500
Archer Daniel Midlands ADM	0.5152	0.5611	0.0458	8.9	0.1548	16500

Notes: Each row shows the coefficient when the named firm is excluded from the shift-share instrument. Full-sample coefficient: 0.5152. Firms sorted by absolute deviation.

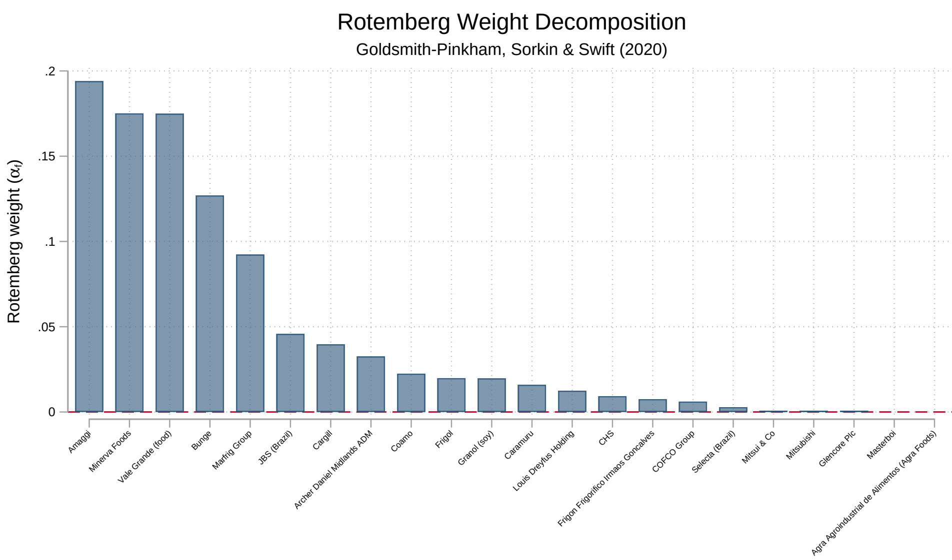


Figure A10: Rotemberg Weights by Firm. Bar heights show each firm's contribution (α_f) to the shift-share estimand following Goldsmith-Pinkham, Sorkin and Swift (2020). Positive weights indicate firms whose campaign shocks are positively correlated with the aggregate estimate.

Table A8: Leave-One-NGO-Out Jackknife: Top 10 Most Influential NGOs

NGO	Full $\hat{\beta}$	Leave-out $\hat{\beta}$	$\Delta\hat{\beta}$	Δ (%)	SE	N
Reporter Brasil	0.5152	0.2790	-0.2362	-45.8	0.1207	16500
Greenpeace International	0.5152	0.3107	-0.2046	-39.7	0.1749	16500
Danwatch	0.5152	0.3304	-0.1849	-35.9	0.1450	16500
Earthsight (aka Timberleaks)	0.5152	0.3716	-0.1437	-27.9	0.1253	16500
Greenpeace UK	0.5152	0.3869	-0.1284	-24.9	0.1624	16500
Greenpeace Brazil	0.5152	0.3887	-0.1266	-24.6	0.1096	16500
Regnskogfondet (Rainforest Foundation Norway)	0.5152	0.4215	-0.0938	-18.2	0.1497	16500
Greenpeace Spain	0.5152	0.4220	-0.0933	-18.1	0.1464	16500
Deutsche Umwelthilfe DUH	0.5152	0.5895	0.0742	14.4	0.1478	16500
Mighty Earth	0.5152	0.4603	-0.0550	-10.7	0.1571	16500

Notes: Each row shows the headline coefficient when the named NGO's attribution mass is dropped from the shift-share instrument. Attribution uses the equal-split rule ($1/K$ of each event across listed NGOs, renormalized within firm-year). Full-sample coefficient: 0.5152. NGOs sorted by absolute deviation from the full-sample estimate.

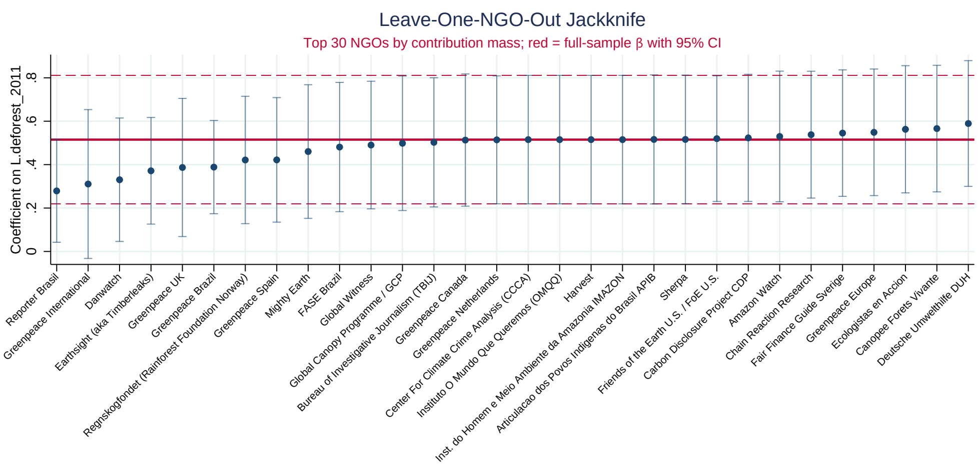


Figure A11: Leave-One-NGO-Out Jackknife Estimates. Each point shows the estimated coefficient on NGO campaign exposure when the named NGO's attribution mass is dropped from the shift-share instrument. Attribution uses the equal-split rule: each campaign event contributes $1/K$ of a unit to each of its K listed NGOs (up to five), renormalized within firm-year so that summing across NGOs reproduces the firm-year deforestation indicator. The dashed line indicates the full-sample estimate. Sample restricted to the top 30 NGOs by total contribution mass; the top 10 by $|\Delta\hat{\beta}|$ appear in Table A8.

A.6 Randomization Inference

We complement the conventional cluster-robust inference with a randomization-inference (RI) procedure in the spirit of Borusyak, Hull and Jaravel (2022). Whereas cluster-robust standard errors rely on asymptotic approximations that treat the number of clusters as large, RI is a finite-sample test that constructs the sampling distribution of the estimator directly from the data under the sharp null of no effect, and is therefore the more honest benchmark when the underlying variation comes from a small number of shocks.

Procedure. The test repeatedly shuffles the firm–year NGO campaign assignments across the set of trade-active firms (1,000 permutations) while holding the 2011 municipality–firm trade shares fixed, reconstructs the shift-share instrument under each permutation, and re-estimates the headline regression. Permuting the shocks rather than the outcome is the natural design for a shift-share instrument: it preserves the exposure structure (the 2011 sourcing weights) and the full panel of municipal outcomes and controls, and tests only whether the particular alignment of *which* firms were campaigned against *when* carries explanatory power for land use, which is precisely the variation the identification rests on. The deforestation-campaign treatment and the unrelated-country placebo are co-permuted under the same firm shuffle, so that any mechanical within-firm correlation between the two is preserved and the placebo control continues to absorb what it absorbs in the actual data. The two-sided RI p-value is the share of permuted coefficients with $|\hat{\beta}_{\text{perm}}| \geq |\hat{\beta}|$. Under the null that campaign assignment is exchangeable across firms (i.e. as-good-as-random conditional on a firm being trade-active), the permutation distribution should be centred at zero and the actual estimate should land in its tails.

Result and interpretation. Figure A12 reports the result for the deforestation outcome under the canonical specification. The permutation distribution is centred near zero (mean -0.018, SD 0.262), confirming that the design is not mechanically biased toward finding an effect, and the actual headline estimate ($\hat{\beta} = 0.5152$) lies roughly two permutation standard deviations above the centre, in the right tail, with a two-sided RI p-value of 0.062. This estimate is significant at the conventional cluster-robust level, and the RI test corroborates that the result is unlikely to be an artifact of a few influential firm–year shocks, but it falls just short of the 5% threshold under the permutation distribution. We read this marginal RI p-value as a feature of the data rather than a weakness of the result. The effective number of shocks that can be permuted is small: only the 36 trade-active firms that are ever targeted contribute identifying variation, and the campaign histories are concentrated in time (rising sharply from 2018 onward). With so few exchangeable units, the permutation distribution is coarse and the test has inherently limited power. Its dispersion is wide relative to the point estimate, so even a true effect of the magnitude estimated here would be expected to clear the 5% RI threshold only intermittently. The fact that the headline nonetheless lands clearly in the right tail, with the placebo co-permuted, is reassuring: it indicates that the alignment of campaign timing with subsequent forest preservation is improbable under random reshuffling, notwithstanding the limited resolution of a test built on a few dozen firms. Power would mechanically improve with a larger set of campaigned firms or a longer panel of campaign shocks, neither of which is available over the present sample window.

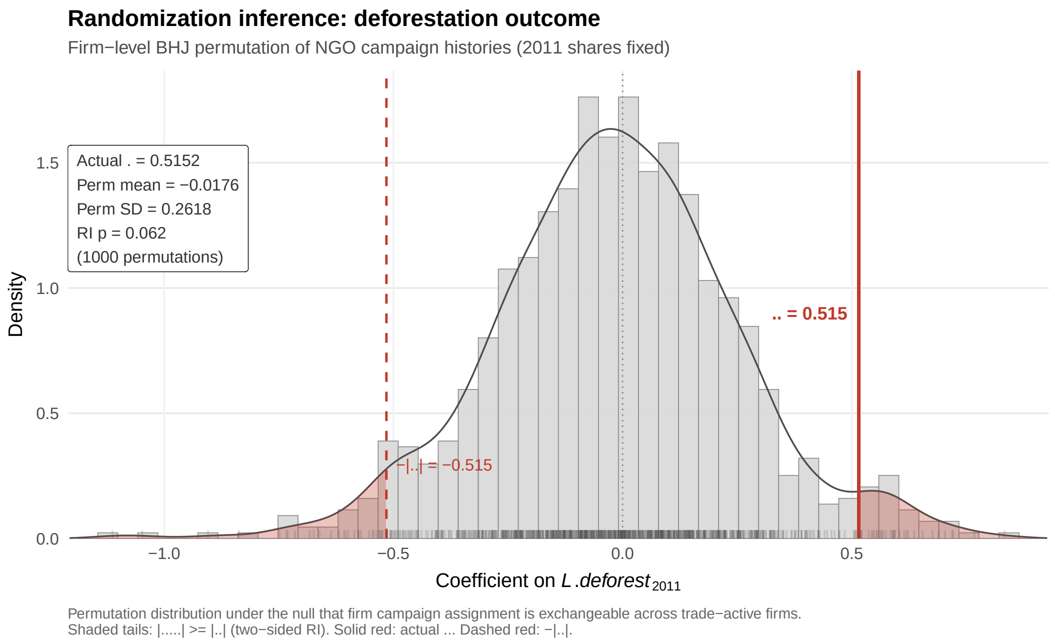


Figure A12: Randomization inference for the deforestation outcome. The grey histogram (with smoothed density overlay) is the distribution of 1,000 permuted coefficients on $L.deforest_{2011}$ under firm-level reshuffling of NGO campaign histories; 2011 trade shares are held fixed. The solid red line marks the actual estimate ($\hat{\beta} = 0.5152$); the dashed red line marks the reflected $-|\hat{\beta}|$. Shaded red tails are the permutations with $|\hat{\beta}_{\text{perm}}| \geq |\hat{\beta}|$; their combined mass equals the two-sided RI p-value. The permutation distribution is centred near zero (mean -0.018, SD 0.262), so the actual headline lies $\approx$2 permutation standard deviations above the centre, with RI $p = 0.062$.

A.7 Threshold-Based Staggered DiD

Table A9: Stacked DiD — Threshold 50%: Area Share Changes

	(1) Δ Forest+Savanna	(2) Δ Pasture	(3) Δ Soy
t = −3	-0.0647	-0.0112	0.0517
	(0.0645)	(0.0868)	(0.157)
t = −2	-0.00869	0.0184	0.152
	(0.0328)	(0.0504)	(0.157)
t = 0	0.0706*	-0.123***	-0.0118
	(0.0389)	(0.0444)	(0.124)
t = 1	0.273***	-0.129	0.0738
	(0.0827)	(0.0951)	(0.127)
t = 2	0.382***	-0.302***	-0.0930
	(0.0869)	(0.0906)	(0.120)
t = 3	0.431***	-0.385***	-0.118
	(0.109)	(0.116)	(0.133)
t = 4	0.303***	-0.342**	-0.167
	(0.102)	(0.136)	(0.153)
t = 5	0.335***	-0.343**	-0.109
	(0.127)	(0.165)	(0.148)
Observations	83276	82613	55882
Adj. R-sq	0.644	0.591	0.308

Treatment: municipality is treated when its dominant trading partner
(>50% of 2011 combined beef+soy FOB) is first targeted by an NGO campaign.
Cohorts 2013+ only (2011/2012-treated dropped for lack of pre-period).
Note: $t = 4$ and $t = 5$ identified from early cohorts only (later cohorts lack full post-periods).
Reference period: t = −1 (omitted).
FE: unit-within-cohort, cohort × year. Clustered by municipality.
Weighted by municipality area.

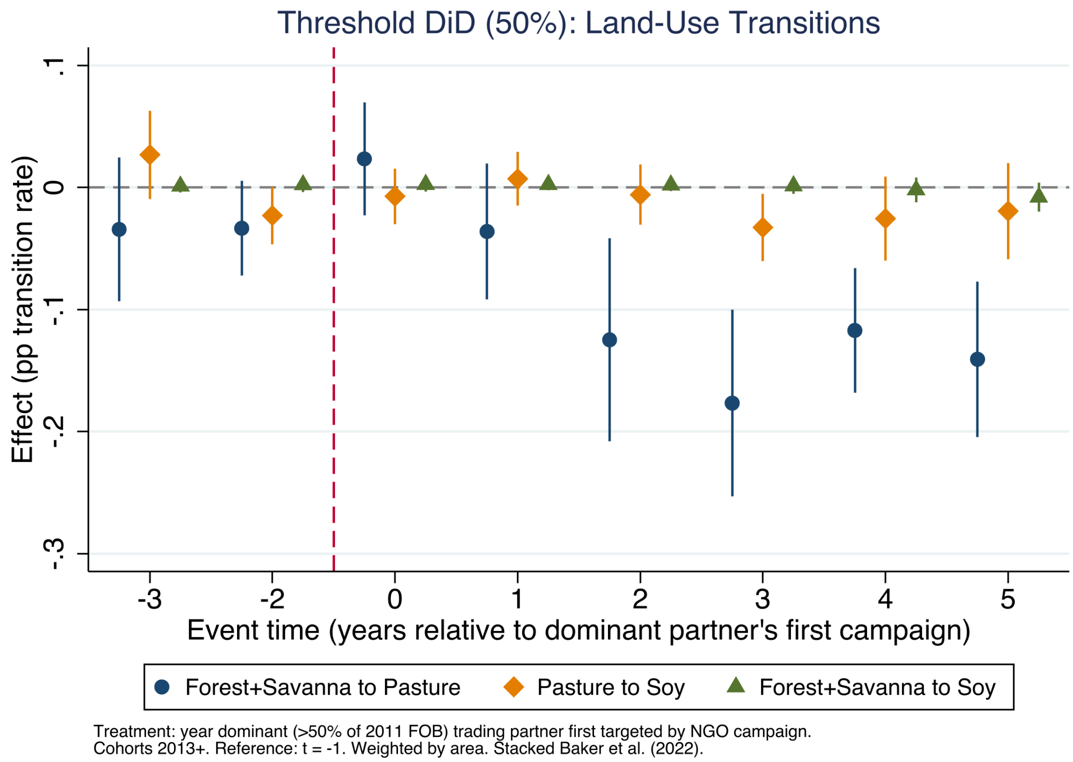


Figure A13: Threshold-based staggered DiD (50% trade share threshold): land-use transitions. Treatment: year in which a municipality's dominant trading partner (>50% of 2011 combined beef and soy FOB) is first targeted by an NGO campaign. Reference period: $t = -1$ (omitted). 95% confidence intervals; standard errors clustered at municipality level. Stacked Baker, Larcker and Wang (2022) estimator. Weighted by municipality area.

A.8 Heterogeneity of Campaign Effects

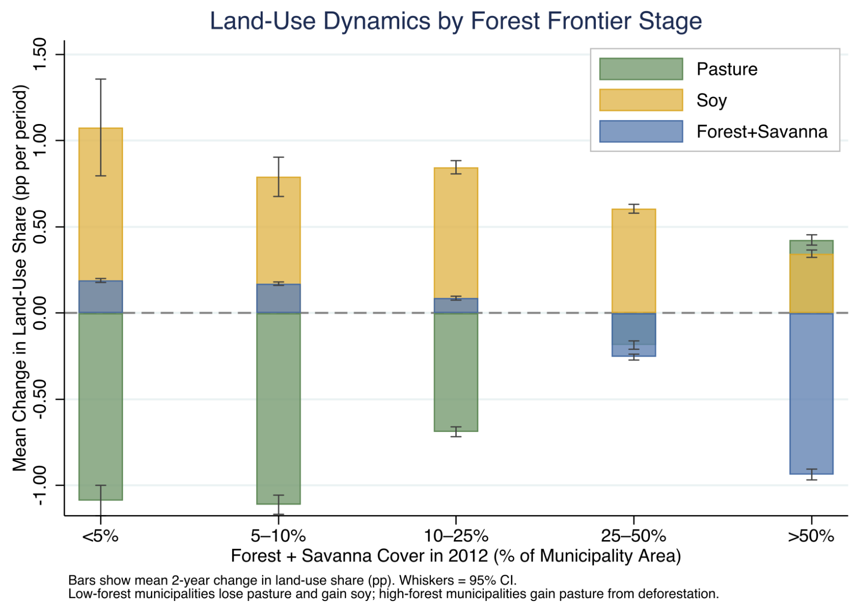


Figure A14: Mean change in land-use share (percentage points per two-year period) by initial forest-plus-savanna cover in 2012. Bars show pasture (green), soy (gold), and forest+savanna (blue) changes. Whiskers denote 95% confidence intervals. Low-forest municipalities exhibit pasture-to-soy substitution; high-forest municipalities gain pasture from deforestation.

Table A10: Campaign Heterogeneity Effects on Forest Cover Change

	(1) Severity	(2) Prominence	(3) Audience	(4) NGO Power	(5) Channel	(6) Destination
L.Moderate	0.4205**					
	[0.1837]					
L.Severe	0.7105***					
	[0.1780]					
L.Non-prominent		0.5526***				
		[0.1818]				
L.Prominent + severe		0.5890***				
		[0.2118]				
L.Brazil-targeted			0.4367***			
			[0.1678]			
L.International-only			0.6887***			
			[0.1833]			
L.Powerful NGO				0.5860***		
				[0.1940]		
L.Non-powerful NGO				0.5510***		
				[0.1839]		
L.Investor-targeted					0.6614***	
					[0.1902]	
L.Consumer-targeted					0.3631**	
					[0.1758]	
L.EU/US destination						1.0694***
						[0.2602]
L.Other destination						0.5102***
						[0.1854]
Dep. var. mean (%)	−0.245	−0.245	−0.245	−0.245	−0.245	−0.245
Mun. FE	Yes	Yes	Yes	Yes	Yes	Yes
Period FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	16,500	16,500	16,500	16,500	16,500	16,500
R^2	0.696	0.696	0.696	0.696	0.696	0.696

Standard errors in brackets. All specifications include municipality and period fixed effects, municipal controls, corporate commitments, and enforcement controls. SE clustered at municipality level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$