Indiscriminate Barriers: The Trade Effects of Due Diligence Policies in Global Timber Markets

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Abstract

This paper offers the first comprehensive assessment of the trade effects of due diligence policies (e.g., the U.S. Lacey Act and the EU Timber Regulation) which are motivated by concerns over illegal logging in global timber markets. We use a structural gravity model with global timber trade data from 2000 to 2021 to estimate the impact of these regulations, differentiating effects based on an exporter's risk profile and product type. We find that these policies act as a significant non-tariff barrier. Crucially, this deterrent effect is indiscriminate: we find no evidence that high-risk exporters are more constrained than compliant, low-risk exporters. This suggests that the substantial compliance costs and procedural barriers limit trade for all suppliers. We also show that these policies most strongly affect raw and lightly processed timber. Counterfactual simulations confirm that a risk-targeted policy—one that restricts due diligence obligations to high-risk exporters—could preserve overall trade volumes while effectively shifting sourcing away from risky suppliers. Our findings offer timely insights for global trade and environmental policy design, highlighting the need for refined, risk-calibrated mechanisms in emerging regulations such as the EU Deforestation Regulation (EUDR).

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1. Introduction

Forests play a crucial role in mitigating climate change by acting as carbon sinks; however, the world loses an estimated 10 million hectares annually, primarily due to agricultural expansion and trade (Curtis et al., 2018; Pendrill et al., 2019, 2022; Carreira et al., 2024). This alarming rate of deforestation not only threatens biodiversity and the livelihoods of millions who depend on forests but also undermines global efforts to combat climate change. While the international community has made efforts to address deforestation, illegal logging remains a key driver of deforestation, contributing to both environmental destruction and economic loss. Illegal logging stands as a pervasive problem (Goncalves et al., 2012), particularly in tropical regions, where enforcement of environmental regulations is often weak (Assunção et al., 2023). By exploiting low governance levels and taking advantage of corruption, illegal loggers deplete valuable forest resources, often with devastating long-term consequences for ecosystems and communities. This illicit activity undermines national economies by depriving governments of tax revenues, while legal timber companies face unfair competition, as illegal operators undercut prices by avoiding compliance costs.

In response to these challenges, international due diligence policies have been introduced to curb the entry of illegally sourced timber in domestic markets. Notable among these are the European Union Timber Regulation (EUTR)[1] and the Lacey Act in the United States. Although adopted in different legal contexts, these policies share a common logic: they shift responsibility for legality verification to importers, requiring them to gather information about the origin of timber, assess sourcing risks, and maintain documentation before placing products on the market. By raising the expected cost of trading illegally harvested wood through compliance obligations and potential liability, they operate through a broadly similar mechanism even if their precise legal formulations differ across jurisdictions.

This paper investigates the global trade effects of due diligence policies. Beyond the average effect of these policies across exporters, we differentiate exporters according to their risk profile, identifying "risky" countries where illegal logging is more prevalent or regulatory enforcement is weak. This distinction allows us to test whether existing due diligence policies are efficiently targeted or whether their compliance burden extends indiscriminately across all exporters. Furthermore, we examine the effects across timber product types, assuming that raw and lightly processed timber may be more susceptible to illicit sourcing than industrialized products. To quantify these effects, we employ state-of-the-art structural gravity models (Head and Mayer, 2014; Yotov et al., 2016) that account for the full set of due diligence policies implemented

[1](*Regulation (EU) No 995/2010 of the European Parliament and of the Council of 20 October 2010 laying down the obligations of operators who place timber and timber products on the market*, 2010)

globally. Using this framework, we first estimate the average impact of due diligence policy adoption on timber trade, and then assess heterogeneous effects across exporter risk profiles, country-specific policies, and product types. Finally, we use counterfactual simulations (i) to measure the aggregate impact of existing regulations on trade flows and (ii) to explore alternative, more targeted policy designs, assessing whether restricting due diligence obligations to risky exporters could reduce trade distortions while maintaining enforcement effectiveness.

Main Results. The analysis highlights five main results.

First, due diligence policies exert a negative average impact on timber exports. Across all countries and policies, due diligence regulations consistently exhibit a negative effect on export values. Our estimates imply that the implementation of due diligence policies by importers leads to a decline of around 45% in treated bilateral trade flows relative to their counterfactual, indicating a strong effect on specific trade margins. This finding is robust across different specifications and to various control variables, including bilateral tariffs and trade integration. In addition to their adverse impact on total export volumes, policies alter the geographic distribution of exports. Specifically, for a risky country, the share of exports directed toward regulating countries declines, while exports to non-regulating countries increase. Thus, the policy influences both the overall magnitude and the composition (i.e., destination) of exports.

Second, the analysis reveals that due diligence policies do not differentially impact exporters based on their risk profile. Both risky exporters (i.e., countries with a higher likelihood of illegal logging) and non-risky exporters experience similar trade reductions, with estimates indicating a roughly 45% decline in exports for both groups. While one might expect the burden to fall more heavily on risky exporters, the results indicate that non-risky exporters—despite not being the primary policy target—are equally constrained. This conclusion holds across various classifications of "risky" countries. Results imply that compliance costs and procedural barriers introduced by due diligence requirements extend beyond high-risk suppliers, limiting trade opportunities for all exporters and underscoring the potential over-inclusiveness of such regulations. In turn, this raises concerns about the efficiency of current policy design, as it may disproportionately penalize compliant actors without effectively targeting the sources of illegality.

Third, disaggregating the analysis across individual due diligence policies confirms that all major regulations significantly reduce timber trade, though with varying magnitudes. The Lacey Act exerts the strongest deterrent effect, followed by the EUTR, while Korea's and Australia's impact is weaker and Japan's regulation shows no significant effect, likely due to differences in design and enforcement. Similarly, when broken down by product categories, due diligence measures most strongly affect raw and sawn timber, which are more prone to illegal sourcing on site, such as illegal logging or artisanal sawing, whereas industrialized products such as plywood face smaller impacts thanks to more formal supply chains and substitution possibilities. Overall, the results show that heterogeneity exists across policies and products, but the consistent negative average effects suggest that such differences are of second-order importance relative to the broader deterrent impact of due diligence requirements.

Fourth, we build on recent advances in structural gravity modeling and we quantify the global trade effects of due diligence policies. Using counterfactual simulations, we first construct a "no-policy" baseline to measure the aggregate trade impact of existing regulations. The results confirm that (i) current policies have significantly reduced imports into regulating countries, consistent with their role as trade barriers and (ii) policies have also altered the composition of imports into regulating countries, increasing the relative share of imports from risky suppliers. While effective in curbing trade, these outcomes highlight a key limitation: the current design is indiscriminate and penalizes compliant exporters, suggesting that the policies are not sufficiently targeting high-risk suppliers.

Fifth, and motivated by this insight, we examine an alternative policy scenario that targets only risky exporters, excluding non-risky exporters from due diligence obligations. The results show that such a design maintains overall import volumes, shifts sourcing away from risky suppliers, and strengthens trade integration among low-risk partners. Importantly, risky exporters experience reduced access to global markets under this targeted regime, consistent with the intended deterrent effect.

Related Literature. This paper builds on three main strands of the literature. First, it builds on evidence relating trade flows and trade policies to forest cover and deforestation, and hence climate change issues. Whereas the question of deforestation is far-reaching and includes a wide set of determinants (Balboni et al., 2023), agricultural trade flows and policies have been shown to be quantitatively important in affecting forest cover changes over time in many contexts (Leblois et al., 2017; Abman and Lundberg, 2020; Abman et al., 2024; Harstad, 2024b,a; Carreira et al., 2024).

Second, this paper builds on methodological contributions allowing to credibly assess trade policy effects. The use of gravity models to study trade patterns and the impact of non-discriminatory policies gained traction in the literature. Among others, Yotov et al. (2016) and Heid et al. (2021) provide a comprehensive framework for understanding how trade policies affect the volume and direction of international trade, in particular in the context of non-discriminatory trade policies, such as due diligence policies. In our setup, gravity models offer a useful tool for examining how changes in regulatory requirements influence timber trade flows.

Finally, our paper is related to recent evidence studying the impact of due diligence policies. More specifically, many recent studies examine the effectiveness of various regulatory measures aimed at combating illegal logging. Moral-Pajares et al. (2020), Rougieux and Jonsson (2021) and Kim et al. (2024) discuss the effects of the EU Timber Regulation (EUTR) and similar policies on the European timber market, providing a foundation for understanding the regulatory landscape, also investigated in Giurca et al. (2013) and McDermott and Sotirov (2018). Furthermore, recent studies by Apeti and N'Doua (2023) and Cosimo et al. (2024) explore the impact of voluntary certification schemes and due diligence requirements on the global timber trade. Recently, Abman et al. (2026) –also focusing on certification schemes included in the EU FLEGT program – identified the trade diversion and leakage effect of these policies. Importantly, our analysis identifies changes in trade flows induced by these policies, but does not allow us to disentangle whether these effects reflect reductions in illegal timber trade, increased compliance costs, or other exporter adjustments.

While these studies primarily focus on timber-related regulations and certification schemes, a broader strand of the literature examines the systemic implications of due diligence and supply-chain traceability policies. The existing EUTR and the upcoming European Union Deforestation Regulation (EUDR) have been compared, with potential effects on private sector compliance and regulatory enforcement across supply chains (Köthke et al., 2023). Deforestation-free supply chain regulation in Brazil has been analyzed, with challenges in implementing compliance measures and the potential for trade distortions (Cesar De Oliveira et al., 2024). Beyond timber, the effects of the EUDR in the soy sector have been studied using a gravity model, showing that stricter trade restrictions can induce trade diversion toward unregulated markets and may have limited effectiveness in reducing deforestation (Sharma and Villoria, 2025).

Contributions. The present paper makes several significant contributions to the literature. First, we evaluate the full set of due diligence policies implemented globally, rather than focusing on a single regulation. This broad coverage allows us to identify the structural and common component of these policies and provides insights relevant for emerging regulations, such as the European Union Deforestation Regulation (EUDR), enhancing the external validity of our findings. Second, our framework allows us to assess the effects of due diligence across all exporters, including non-risky countries not directly targeted by the regulations. Unlike prior work, such as Apeti and N'Doua (2023), which focuses on risky exporters, we document the global trade barrier effect of these policies. While the regulations achieve their goals by reducing trade from risky exporters, they also impose substantial compliance costs on legal suppliers. Third, unlike most prior literature, which focuses on licensing systems such as FLEGT (Apeti and N'Doua, 2023;

Cosimo et al., 2024; Abman et al., 2026), we study the broader regulatory frameworks governing these systems, including mandatory operator due diligence. Our results hence complement existing evidence on voluntary versus mandatory compliance mechanisms and highlights the broader implications of regulatory design for global timber trade.

Fourth, by employing a structural gravity framework with multilateral resistance terms, our estimates are fully consistent with trade theory, complementing previous difference-in-differences approaches and improving identification by exploiting bilateral and temporal variation. This setup also accounts for general equilibrium effects and trade diversion, which a canonical difference-in-differences setup cannot capture.

Finally, we leverage the structural gravity model to quantify the total trade reshuffling induced by these policies. This approach moves from micro bilateral flows to the aggregate level, providing a comprehensive picture of trade reallocation. Importantly, we identify a credible alternative, risk-targeted policy design that mitigates costs for non-risky exporters—an effect not documented in prior work.

Taken together, our paper provides novel empirical evidence on both intended and unintended effects of due diligence policies and offers guidance for designing more efficient and targeted regulatory interventions. Our analysis identifies the net trade effect of due diligence, yet without being able to separate compliance costs, deterrence of illegal sourcing, and exporter behavioural responses.

The timber due diligence experience provides valuable lessons for the regulation of other deforestation-prone commodities, despite important differences between legality-based frameworks (e.g., EUTR) and broader deforestation-free regulations (e.g., EUDR). In line with our main findings, due diligence policies tend to reduce trade in targeted (high-risk) flows, but do so in a largely undifferentiated way, generating compliance costs for both high-risk and low-risk exporters, affecting trade integration, and inducing trade diversion (Köthke et al., 2023; Vasconcelos et al., 2024; Sharma and Villoria, 2025). These adjustments operate through changes in sourcing patterns and in the distribution of regulatory burdens across exporters. Our counterfactual simulations further suggest that more targeted policy designs can mitigate these inefficiencies by limiting exposure to risky suppliers while preserving trade integration with non-risky partners. However, differences in the exact scope and policy design between EUTR and EUDR imply that the magnitude and distribution of trade impacts may not fully align with those identified in our analysis, particularly given differences in supply-chain structures and substitution patterns across commodities.

The present paper is structured as follows. The next section establishes the context for the analysis. Section 3 details the data at hand and the main econometric strategy. Section 4 presents the main econometric results, highlighting the average and heterogeneous trade effects of the due diligence policies. We then conduct a

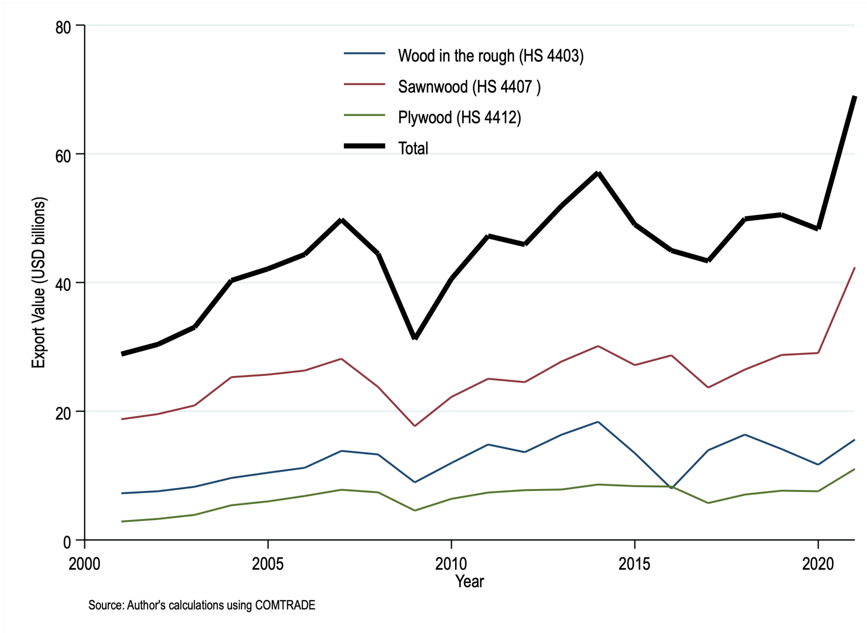


Figure 1: Evolution of timber trade flows

series of counterfactual analysis to quantify the impact of due diligence policies on international trade in Section 5, before discussing and concluding.

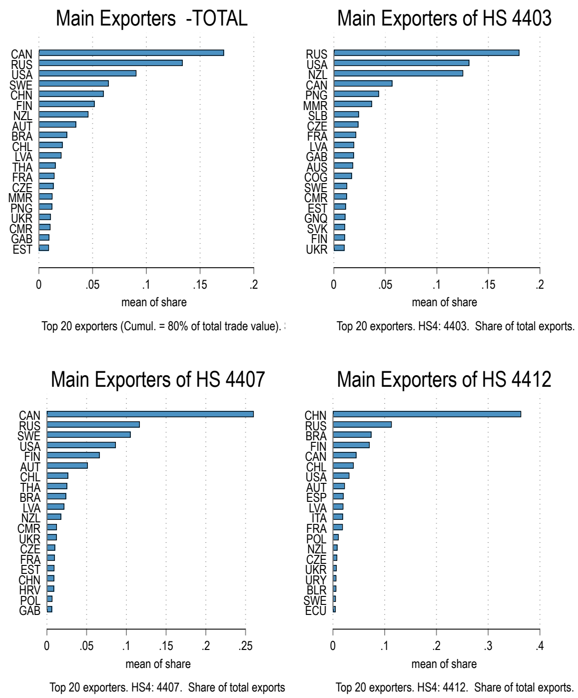
2. Context

This section establishes the context for the analysis, describing (i) a growing timber market in which (ii) regulation is progressively expanding, while the (iii) prevalence of potentially high-risk suppliers remains considerable.

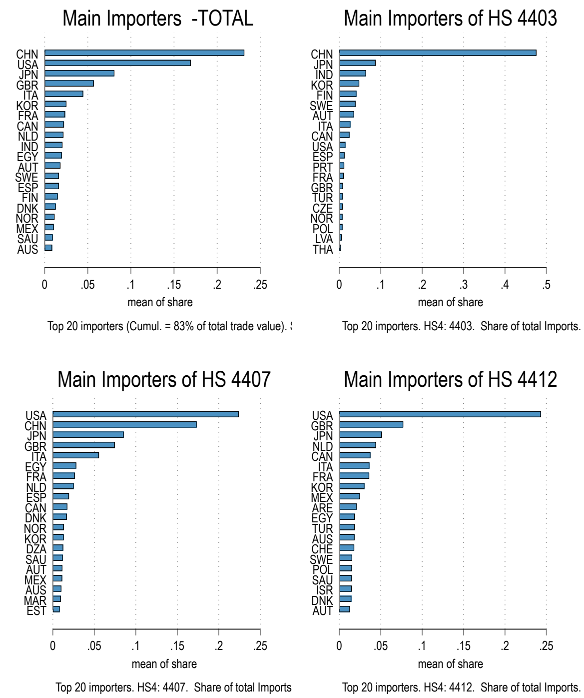
2.1. Timber Trade Values Over Time

Figure 1 presents the evolution of global trade flows for the three timber product categories considered in this analysis, both individually and in aggregate over the sample period. Overall, trade in all products exhibits a slight upward trend, reflecting gradual growth in global timber markets. A clear hierarchy emerges in trade values, with sawnwood consistently dominating, followed by timber in the rough, and then plywood and panels. Trade flows also respond to broader economic conditions, with observable contractions during periods of financial stress, such as the 2008 global financial crisis, highlighting the sensitivity of timber trade to macroeconomic shocks.

Distribution, Main Exporters, and Main Importers The distribution of global timber trade is highly concentrated (Figure 2). Among exporters (Figure 2a), Canada, Russia, and the United States dominate,



(a) Timber exporters



(b) Timber importers

Figure 2: Distribution of exporters and importers.

accounting for the largest shares of global shipments. Trade is notably skewed, with a small set of countries responsible for the majority of exports. Risky countries (see section 2.3) primarily appear in the exports of raw timber and sawntimber, whereas plywood and panels are almost exclusively exported by non-risky countries.

On the import side (Figure 2b), China emerges as the largest destination, particularly for raw timber, reflecting its central role in the global timber supply chain (Zhang et al., 2020, 2023; Wang et al., 2023). China imports large volumes of raw timber and sawnwood that are subsequently processed domestically and re-exported as higher value-added wood products such as furniture and panels. As a result, China acts both as a major importer of upstream timber products and as a key processing hub in global wood-based value chains. This highlights China's central position in linking upstream timber exporters with downstream manufacturing and consumer markets worldwide. Import flows are even more skewed than exports, with a few major importers accounting for the bulk of global demand.

2.2. Due Diligence Policies

Over the past two decades, a clear trend has emerged towards increasing regulation on the import side.

Table 1 lists the set of policies considered in the analysis: the list includes all jurisdictions that imple-

Table 1: List of due diligence policies in the timber industry

Country	Year of adoption	Year of implementation	Name of the legislation
USA	1900	2008	Lacey Act
European Union	2010	2013	European Union Timber Regulation
Australia	2012	2012	Illegal Logging Prohibition Act
Japan	2016	2016	Clean Timber Act
Republic of Korea	2018	2018	Act on the Sustainable Use of Timber

mented mandatory due diligence regulations for timber imports during our study period, to the best of our knowledge. The list was compiled using official legislative sources and international regulatory overviews such as the Timber Trade Portal. [2] Some major timber exporters, such as Canada, do not appear in the table because they have not adopted import due diligence requirements comparable to those implemented in the jurisdictions listed above. Similarly, New Zealand adopted the Illegal Logging and Associated Trade Amendment Act, but the due diligence obligations for importers are scheduled to enter into force only in 2027. Finally, while China has introduced timber legality initiatives, these do not establish a mandatory importer due diligence system comparable to the policies considered here.

While the policies considered in the analysis were adopted in different jurisdictions and at different points in time, their general objective and regulatory logic are largely shared. All of them seek to reduce illegal logging by restricting market access for wood products derived from illegally harvested timber. Rather than relying on border inspections in producing countries, they shift responsibility to importers in consuming markets. In all cases, firms are required—either explicitly or implicitly—to exercise some form of due diligence before placing timber products on the domestic market. The core mechanism is therefore common across policies: increase the expected cost of trading illegal timber by raising verification requirements and legal exposure for importers.

These measures operate as non-tariff measures as they increase information, verification, and compliance costs. Importers must collect information on species and country of harvest, assess sourcing risks, and maintain documentation. This generates both fixed costs (e.g., establishing traceability systems, staff training, supplier screening) and variable costs (e.g., shipment-level documentation and risk assessment). These costs apply regardless of whether illegality is ultimately detected. As a result, the main trade channel is not the detection of illegal shipments but the increase in costs associated to market access.

Some institutional differences exist across jurisdictions, though they are variations around a broadly similar

[2] See https://www.timbertradeportal.com/en/p/25/regulations.

framework. The European Union and Australia explicitly require structured due diligence procedures prior to market entry, whereas the United States relies on a flexible "due care" standard combined with import declaration requirements. Japan's framework places greater emphasis on registration and documentation incentives. Enforcement structures and implementation timelines also differ. However, these distinctions primarily affect the intensity and expected compliance burden rather than the underlying regulatory principle. For the purposes of our empirical analysis, the key common feature is that all policies introduce importer-side obligations aimed at excluding illegal timber products, thereby increasing the cost of accessing regulated markets.

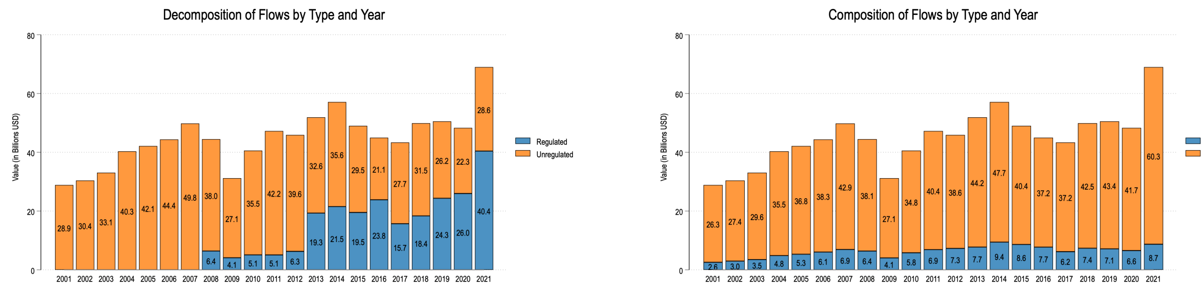
Our sample focuses on three upstream timber products: HS 4403 (wood in the rough), 4407 (sawnwood), and 4412 (plywood). These categories correspond to early stages of the timber supply chain where illegal logging risks are highest and where due diligence regulations are expected to operate most directly. Importantly, these HS codes are also consistently covered across all timber due diligence policies examined in our paper (e.g., EUTR and similar frameworks), which makes them a natural and comparable treatment group across countries and regulations. We will check the robustness of our result to this sample restriction.

In this context, a trade flow from country i to country j is classified as regulated if the importing country j has enacted a due diligence policy in a given year t. As shown in Figure 3a, the volume of trade occurring between regulated countries has risen consistently over time, reflecting the progressive introduction of successive due diligence policies. The observed growth in regulated trade highlights the expanding reach of these policies, both geographically and in terms of the volume of timber they cover. Today, more than half of the total global timber trade value falls under due diligence regulations, underscoring the importance of understanding their economic and trade-related consequences.

2.3. High-Risk Origins in Timber Trade

Another key feature of the global timber trade is the presence of countries where illegal logging is more prevalent or where the enforcement of forestry regulations is weak. Identifying these risky exporters allows us to analyze the differential effects of due diligence policies on trade flows and to assess whether regulatory interventions effectively target high-risk suppliers.

We therefore classify exporters from these countries as "risky," reflecting the higher probability that timber products may originate from illicit sources. Our analysis relies on a binary classification of countries associated with potentially illegal logging from Hoare (2015), which is also used in Apeti and N'Doua (2023). According to this classification, risky countries include Brazil, Cameroon, China, the Democratic Republic of the Congo, Ghana, Indonesia, Laos, Malaysia, Papua New Guinea, the Republic of the Congo, Thailand,



(a) Evolution of regulated trade

(b) Evolution of risky trade

Figure 3: Trends in timber trade: regulated vs. risky sources

Vietnam (see Table 2 for details). These exporters are primarily located in Asia, followed by Africa, with fewer in South America and Oceania.

Table 2: List of risky timber-exporting countries, by continent

Africa	**Asia**	**South America**	**Oceania**
Democratic Republic of the Congo	Indonesia	Brazil	Papua New Guinea
Ghana	Malaysia		
Republic of the Congo	Laos		
Cameroon	China		
	Thailand		
	Vietnam		

Trade value from "risky" countries has remained relatively low but has increased over time (Figure 3b). This pattern suggests that, despite the introduction of regulations, these countries continue to play a role in global timber markets. Importantly, this measure does not capture illegal trade per se. Rather, it reflects the growing integration of these countries into global supply chains, particularly for raw timber and sawnwood products, which are more likely to originate from regions with weaker governance or enforcement.

Our classification should therefore be interpreted as an approximation of relative illegality risk rather than a direct measure of illegal trade. While due diligence regulations operate at the firm and shipment level, our approach relies on a country-level categorization, which necessarily abstracts from within-country heterogeneity. As a result, some exports from “risky” countries are legal, while illegal activity may also occur in countries classified as non-risky. This implies that our estimates capture average country-pair effects and should not be interpreted as uniform effects across all firms within exporting countries. In addition, the use of a fixed country list does not capture potential changes in governance, enforcement capacity, or sectoral risk over time.

Nevertheless, such country-level classifications are widely used in the literature on timber legality and supply-chain regulation, given the limited availability of systematic and comparable indicators at the firm or product level (Hoare, 2015; Apeti and N'Doua, 2023). Our approach thus captures structural differences in the likelihood that timber products originate from jurisdictions with weaker enforcement. Consequently, some major exporters, such as Russia or Gabon, are not included because they are not classified as high-risk in the underlying source. To address this limitation, we will conduct robustness checks using alternative specifications, which yield qualitatively similar results.

3. Methods

To estimate the impact of these policies on trade flows, we use structural gravity modelling (Head and Mayer, 2014; Yotov et al., 2016; Larch et al., 2025) and we estimate a general relationship:

$$TimberFlow_{ijt} = \lambda_{ij} + \lambda_{it} + \beta Policy_{jt} + \delta_1 \mathbf{X}_{jt} + \delta_2 \mathbf{X}_{ijt} + \varepsilon_{ijt} \tag{1}$$

using a PPML estimator (Santos Silva and Tenreyro, 2006, 2022) and including all (potentially zero) trade flows from an exporter i to a market j in year t. Estimations hence account for both the intensive (conditional on exporting, the value of flows) and the extensive (the probability to export) margins.

The data used in this analysis covers the period from 2000 to 2021. Trade flow data is obtained from the *UN Comtrade* database, which provides detailed information on global trade transactions.[3]

We focus on trade values for the main timber products covered by the all due diligence policies in our setup, namely HS4 codes 4403, 4407, and 4412. The analysis is conducted both on the aggregate of these products and at the product level. Restricting the sample to these core products ensures consistency with the policy scope and results including a broader set of products are presented in the robustness checks.

We regress the value of exports $TimberFlow_{ijt}$ on a dummy variable capturing whether the importer j has implemented a due diligence regulation regarding timber imports at time t ($Policy_{jt}$) (see Table 1). The indicator equals zero before the policy is implemented and switches to one in the year of implementation, remaining equal to one in all subsequent years. The estimated coefficient therefore captures the average post-implementation effect of the regulation on bilateral timber trade.

The main coefficient of interest is hence β: if $\beta < 0$, due diligence policies have a trade-deterring effects. Our specification exploits variation in policy adoption across countries and over time. Specifically, we compare the trajectory of trade flows before and after the implementation of due diligence policies, across

[3]We use UN COMTRADE as our primary trade dataset instead of BACI because BACI's reconciliation procedure can introduce distortions. BACI adjusts reported exports (FOB) and imports (CIF) across reporters by weighting observations based on reliability and resolving discrepancies. While this improves consistency in some cases, many timber flows are reported by only one partner, meaning BACI's adjustments may alter the reported values or reduce coverage. COMTRADE preserves the original reports, providing a larger number of observations and greater statistical power for our analyses.

countries that adopted the policy versus those that did not, accounting for the fact that each exporter may divert trade to multiple destinations.

Fixed Effects. The estimation includes a bilateral fixed effect (λ_{ij}) which captures the time-invariant country-pair determinants of trade such as distance between countries, or cultural proximity. This set of fixed effects ensures that the main effects are estimated across time for a given exporter-importer relationship. Second, the main estimations include a set of exporter-year fixed effects, that crucially control for supply shocks in origin country.[4] This fixed effect also absorb governance and institutions in the exporting countries (Rodrik et al., 2004), in which logging occurs. However, this comes at the cost of absorbing the variation that would allow us to speak to the precise channels through which exporters respond to due diligence requirements. This set of fixed effects finally ensures the estimation identifies the export variation across destinations (i.e., importers) for a given exporter. Such variation is particularly informative, as it represents one of the main channels through which these policies operate. For any given exporter, these policies may act as reducing the relative share of exports to "regulated" countries, while potentially shifting exports toward unregulated countries. Note that including this fixed effect implicitly assumes the overall level of exports remains constant. More generally, specifications that include more granular fixed effects (e.g., importer–year FE) have slightly fewer observations, since the estimation requires within–fixed-effect variation. That is, adding fixed effects may reduce the sample.

In a fully specified trade estimation, one would ideally include importer-year (jt) fixed effects. However, these fixed effects are perfectly collinear with the policy variable of interest. A solution proposed in the recent literature (Yotov et al., 2016) is to include domestic trade flows. Unfortunately, to our knowledge, no intra-national trade data exist for timber products. To address this limitation, our main estimations rely exclusively on international trade data. We conduct sensitivity analyses in robustness checks, using two alternative approaches: (i) approximating national flows with FAO production and export data, which are subsequently incorporated into the estimation sample (Heid et al., 2021), acknowledging that this introduces some measurement noise; and (ii) applying a recent two-stage methodology (Freeman et al., 2025) to identify the effect of these policies as a country-specific characteristic.

Controls. We include a set of control variables in the specification, in line with Head and Mayer (2014). First, we include a set of importer-year variables $\mathbf{X}_{jt}$, capturing the demand-side mechanisms: importer population, importer forest rents (as a share of GDP), and (in robustness) governance. These variable altogether capture import demand for timber products and act as trade determinants. Importer population accounts

[4] Including this set of fixed effects naturally absorbs exporter-year controls, such as GDP or supply.

for the size of the economy and overall market demand, with larger or more populous countries typically importing greater volumes of timber regardless of policy interventions. Importer forest rents, measured as a share of GDP, proxy for domestic resource availability and the opportunity cost of imports, since countries with abundant forest resources may rely less on imported timber. For robustness, we consider further control variable, and we use data from the *World Governance Indicators* (Kaufmann and Kraay, 2024) to measure both importer and exporter governance quality. This data includes key indicators such as political stability, governance quality, and regulatory effectiveness, which help control for institutional factors affecting trade enforcement and influencing the costs or frictions associated with trading timber products and hence trade flows.

The bilateral-time-varying controls $\mathbf{X}_{ijt}$ capture trade-specific factors that may influence timber flows between exporter i and importer j over time. This includes applied tariffs on the relevant HS codes,[5], which directly affect the cost of trading timber and can influence the volume of imports. We also account for participation in regional trade agreements (Conte et al., 2022) which may reduce trade barriers and facilitate bilateral flows. These controls help isolate the effect of due diligence policies by adjusting for time-varying, country-pair-specific determinants of trade, ensuring that estimated policy effects are not confounded by shifts in trade costs or preferential trade arrangements.

Our specification is closely related to a difference-in-differences design but it also addresses general equilibrium effects in trade: a policy in one importing country can induce exporters to redirect shipments to other destinations, meaning untreated countries are indirectly affected. Such spillovers violate the SUTVA assumption required for a canonical difference-in-differences design, but are naturally accounted for by the fixed effects structure.

Regarding inference, we follow standard practice and cluster the standard errors at the importer-year level, in particular because due diligence policies are mainly set at this level. We check the robustness of inference to this choice.

4. Empirical evidence

4.1. Main effect of the policy

Table 3 reports the results from estimating equation 1 on the full sample, capturing the average trade impact of due diligence policies across all countries. In all specifications, the coefficient on the "policy" variable

[5] Tariff data is sourced from the *MAcMap* database (Guimbard et al., 2012) which provides information on tariff rates for the relevant HS6 codes. We compute average tariffs for the HS codes under consideration (HS 4403, 4407 and 4412), by weighting corresponding trade flows, and by interpolating the value across years, as data is available only 1 year out of 3.

is negative and highly significant, consistently pointing to a reduction in timber export values following the introduction of due diligence requirements. Point estimates range from –0.62 to -0.68: the imposition of due diligence policies by importers reduces exports by approximately 45% for treated bilateral flows relative to their counterfactual, highlighting a strong impact on these trade margins.

The specifications differ by their treatment of unobserved heterogeneity and by the inclusion of additional controls. Column (1) presents results with a parsimonious set of fixed effects, while column (2) introduces more demanding fixed effects that improve comparability between regulated and unregulated trade flows. Importantly, the magnitude of the estimated policy effect remains stable across these different setups, reinforcing the robustness of the findings. Besides, when including an exporter-year fixed effects,[6] the remaining negative coefficient on the policy implies that, besides the negative effect of the policy on total exports, the policy shifts exports away from the regulating countries: the share of exports that go into regulating countries decreases with the policy, in the favor of the non-regulating countries. Both the total amount and the composition (i.e., the direction) of exports are affected by the policy.

The negative and significant coefficient on log distance (col.1) confirms the standard result from trade theory and gravity models: larger distances are associated with higher transportation costs and thus lower trade flows (Disdier and Head, 2008; Head and Mayer, 2014; Borchert and Yotov, 2017). We also consider importer population as a trade determinant, in line with the gravity literature (Head and Mayer, 2014). Column 3 extends the specification by including additional bilateral (exporter-importer-year) controls that are not absorbed by the fixed effects. The coefficients on regional trade agreements (RTA) indicate that preferential agreements systematically promote timber trade, consistent with expectations and prior evidence. By contrast, bilateral tariffs do not appear to exert a significant influence once RTAs are controlled for, suggesting that much of the tariff effect is already internalized through broader trade agreements (Fontagné et al., 2022). Finally, we include measures of forest dependence in the importing country, capturing the extent to which forests contribute to national income. Results indicate that higher forest dependence in importing countries is associated with lower timber imports, highlighting the importance of domestic demand factors in shaping trade flows. This suggests that import reliance is influenced not only by regulatory and trade costs but also by the economic role and strength the forest sector plays in the importing economy.

Columns (4) and (5) decompose the overall trade effect of due diligence policies into intensive and extensive margins. Column (4) restricts the sample to strictly positive timber trade flows and estimates the impact of

[6] Including this fixed effect implicitly assumes the overall level of exports remains constant. What remains for identification is the variation in shares across destinations. See Methods.

the policy on the log of export values using a linear fixed-effects estimator. This specification captures how the policy affects the volume of existing trade relationships. Column (5), by contrast, focuses on the extensive margin by regressing a dummy equal to one if bilateral timber trade X_{ijt} is positive in year t, and zero otherwise, using a linear probability model (LPM). This setup identifies whether policies affect the likelihood that a trade relationship exists at all.

The results are consistent across the two margins: due diligence policies are associated with reductions both in the value of ongoing timber trade relationships and in the probability that such relationships exist. Quantitatively, roughly two-thirds of the total estimated trade effect is driven by the intensive margin, while the remaining one-third reflects contractions along the extensive margin. This decomposition indicates that the primary channel through which due diligence policies affect global timber trade is by reducing the intensity of existing flows, though they also contribute meaningfully to the disappearance of some bilateral trade links altogether.

4.2. Differential Effects across Exporters

Beyond their average impact, Table 4 investigates whether the effects of due diligence policies effects differ across exporters depending on their risk profile. Exporters can be classified as risky when they are known to be associated with illegal logging and weak enforcement of forestry regulations. Using the classification presented earlier (Table 2), Table 4 first replicates the baseline estimation separately for non-risky exporters (Column 1) and risky exporters (Column 2), before pooling the full sample and explicitly estimating the interaction between policy implementation and exporter risk status (Columns 3–5). The main effect of policy captures the average effect across all exporters, while the interaction term measures any additional impact for risky exporters. Coefficient on the interaction terms checks whether any difference between policy effect across risky and non risky exporters is statistically significant. This approach is important because comparing risky and non-risky subsamples in isolation may suffer from limited sample size, which can bias inference. By estimating the interaction term directly in the specification, we mitigate these concerns and obtain a more reliable test of differential effects.

Two main findings emerge. First, due diligence policies significantly reduce timber exports for both risky and non-risky countries. Columns (1) and (2) show large and negative coefficients on the policy variable in both subsamples,[7] even after accounting for controls such as tariffs, RTAs, and forest rents. This result highlights that the compliance burden created by due diligence requirements constrains all exporters, including those with a low probability of illegality.

[7] Comparing the point estimates in cols. 1 and 2 is tricky because estimates are produced on different samples.

Table 3: Average Effects

	Dep. Variable: X_{ijt}				Log X_{ijt}	$(X_{ijt} > 0)$
	(1)	(2)	(3)	(4)	(5)	(6)
Policy	-0.643***	-0.644***	-0.682***	-0.621***	-0.420***	-0.0580***
	(0.0822)	(0.0821)	(0.0770)	(0.0737)	(0.0475)	(0.00725)
Log Distance	-1.211***					
	(0.0282)					
Importer Population	-0.311	-0.306	-0.176	-0.338	0.0387	0.118***
	(0.251)	(0.251)	(0.246)	(0.211)	(0.108)	(0.0286)
Bilateral Tariff (wood)				-1.057	-1.361***	-0.0987
				(0.718)	(0.341)	(0.0608)
RTA (0/1)				0.257***	-0.0379	0.00754
				(0.0571)	(0.0380)	(0.00609)
Importer Forest Rents				-0.328***	-0.232***	-0.0221***
				(0.0594)	(0.0374)	(0.00601)
# Observations	188,741	188,741	188,741	182,578	76,627	182,919
Exporter FE	x					
Importer FE	x					
Year FE	x	x				
Importer-Exporter FE		x	x	x	x	x
Exporter-Year FE			x	x	x	x

Standard errors in parentheses.

Clustered standard errors at importer-year level.

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

Second, we find no systematic evidence that risky exporters are disproportionately affected. In the pooled estimations with interaction terms (Columns 3–5), the coefficient on the interaction between policy and risk is small and statistically insignificant across all specifications.[8] The negative trade effects of due diligence policies are hence broadly similar for risky and non-risky exporters. Importantly, this result is robust to the inclusion of additional control variables (Column 4) and to more demanding fixed effects (Column 5).

Taken together, the evidence suggests that while due diligence policies achieve their deterrent effect on timber trade, they do so in a largely non-discriminatory way: both risky and safer exporters face comparable reductions in trade flows. This raises the possibility that the current policy design may impose substantial compliance costs on exporters with low illegality risk, potentially reducing the efficiency of the intervention. We interpret this as evidence for considering that due diligence policies act as a non-tariff barrier to trade.

4.3. Heterogeneous Effects across Policies

Some institutional differences exist across policies, particularly in terms of the formal structure of due diligence requirements, enforcement arrangements, liability standards, and implementation timelines. While these differences do not alter the common regulatory objective or underlying mechanism, they may influence the intensity of the trade effects of these policies.

Whereas the benchmark results above pooled together all existing due diligence regulations (see Table 1), Table 5 disaggregates the analysis and reports the trade impacts of each individual policy. Specifically, we estimate the effect of policies implemented by the United States (Lacey Act), the European Union (EUTR), Australia, Japan, and Korea. Across almost all cases, the policy variable is negative and significant, confirming that the adoption of due diligence requirements reduces timber trade flows regardless of the implementing country.

The magnitudes, however, differ. The U.S. Lacey Act shows the strongest trade-deterring effect, with an estimated coefficient close to -1, suggesting a pronounced reduction in timber imports. The EUTR also displays a sizable and highly significant effect, underlining the deterrent power of the EU's regulatory framework. In contrast, Australia's regulation produces a weaker effect, which could reflect differences in policy stringency, enforcement intensity, or trade structure. The Japanese measure does not yield a significant effect, pointing to either weaker enforcement or limited coverage of the policy. Korea, meanwhile, shows a moderate but robust negative effect, indicating that its more recent regulation is already associated with reduced imports.

[8] Note that both unconditional terms are controlled for in this specification: the unconditional policy effect is included in the specification, whereas the unconditional effect of risk (i.e., the trade effect of being a "risky" exporter) is absorbed in the exporter-year set of fixed effects.

Table 4: Differential Effects across Exporters (Risky vs safer exporters)

	Sample				
	(1)	(2)	(3)	(4)	(5)
Sample	Risky ==0	Risky ==1	Full	Full	Full
Policy	-0.728***	-0.516***	-0.704***	-0.624***	
	(0.0857)	(0.0649)	(0.0843)	(0.0796)	
Policy × Risky			0.0824	0.0153	0.0870
			(0.0707)	(0.0673)	(0.0657)
Importer Population	-0.750***	1.367***	-0.306	-0.448**	
	(0.280)	(0.269)	(0.247)	(0.215)	
Bilateral Tariff (wood)				-1.213	-2.899***
				(0.755)	(0.812)
RTA (0/1)				0.226***	0.0340
				(0.0590)	(0.0391)
Importer Forest Rents				-0.289***	
				(0.0615)	
# Observations	148,101	13,355	161,456	161,456	161,456
Importer-Exporter FE	x	x	x	x	x
Exporter-Year FE	x	x	x	x	x
Importer-Year FE					x

Standard errors in parentheses.

Clustered standard errors at importer-year level.

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

Several factors could potentially explain the heterogeneous impacts of different policies. First, differences in the structure or importance of importing markets could play a role, for example in terms of market size, centrality in global trade, or substitution possibilities. Our empirical specification, however, mitigates this concern: exporter–importer fixed effects capture persistent bilateral trade patterns, and we include time-varying importer characteristics such as population, tariffs, RTAs, and forest rents. As a result, structural differences across importing markets are unlikely to drive the observed heterogeneity. Second, differences in timing of implementation could influence the estimated effects. While more recent policies might plausibly have weaker impacts, our analysis (see Figure A.2 and subsection 4.6.4) shows that the trade response is immediate, so timing alone does not explain the variation. Third, what remains as the most plausible explanation is differences in policy stringency, legal requirements, and enforcement arrangements across jurisdictions. In other words, the variation in estimated magnitudes is consistent with differences in the design and implementation of due diligence frameworks rather than market characteristics or timing.

Taken together, these results demonstrate that the average negative impacts reported in the pooled estimations are not driven by one particular policy but are instead a consistent feature across most regimes. At the same time, the variation in estimated magnitudes likely highlights heterogeneity in how different due diligence frameworks operate in practice, both in terms of their design and their enforcement.

Table 5: Heterogeneous Impact of the different policies

	(1)	(2)	(3)	(4)	(5)
Importing country:	USA	EUTR	Australia	Japan	Korea
Policy	-0.976***	-0.514***	-0.204*	0.122	-0.336***
	(0.143)	(0.0635)	(0.124)	(0.0943)	(0.0974)
Importer Population	0.313*	-0.0331	0.305*	0.339*	0.279
	(0.174)	(0.191)	(0.175)	(0.174)	(0.177)
Bilateral Tariff (timber)	-2.743***	-1.810**	-3.567***	-3.583***	-3.657***
	(0.676)	(0.759)	(0.750)	(0.748)	(0.755)
RTA (0/1)	0.163***	0.207***	0.214***	0.211***	0.246***
	(0.0528)	(0.0556)	(0.0556)	(0.0548)	(0.0571)
Importer Forest Rents	-0.543***	-0.376***	-0.417***	-0.441***	-0.414***
	(0.0676)	(0.0608)	(0.0650)	(0.0696)	(0.0651)
Observations	361780	361780	361780	361780	361780

Note: All columns include Exporter-Importer and Exporter-Year fixed effects.
Standard errors in parentheses
Clustered standard errors at importer-year level
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

4.4. Heterogeneous Effects across Products

Figure 4 presents the estimated impact of due diligence policies on timber trade flows, disaggregated by HS4 product categories. Across all products, the main effect of due diligence policies is negative, but the magnitude varies with the degree of industrialization.

Roundwood (HS 4403) and sawnwood (HS 4407) experience the largest declines.[9] These products are potentially most vulnerable to illegal sourcing on site, such as illegal logging and artisanal sawing, and may therefore be more directly affected by stringent regulations. Legal exporters also see reductions in these categories, though to a slightly lesser extent, reflecting both compliance costs and the ability of established supply chains to partially mitigate regulatory impacts.

By contrast, industrialized products such as plywood and panels (HS 4802) are less affected by due diligence policies. This pattern arises for three complementary reasons. First, industrialized products generally rely on formalized and traceable supply chains, which facilitate compliance with regulatory requirements. Second, raw timber can be transformed into more processed goods, effectively obscuring its origin and mitigating the regulatory burden. Third, the extent to which timber can be processed into industrialized products influences the degree of this effect—countries or firms with greater processing capacity can more easily shift production toward industrialized goods, reducing the impact of due diligence policies. As a result, exporters associated with illegal logging experience minimal negative effects on industrialized products, consistent with both this substitution mechanism and suggesting that due diligence policies are primarily better suited to target timber products that are illegally sourced on site.

4.5. Complementary to trade policies

Table 6 investigates the interaction between due diligence policies and trade policies, namely tariff levels and regional trade agreements (RTAs). To do so, we augment the baseline specification with interaction terms between the due diligence policy variable—whose average effect is negative—and the two main trade policy variables considered in the analysis. These interaction terms capture whether the effect of due diligence policies differs across trade regimes, that is, between low- versus high-tariff contexts and between integrated versus non-integrated trading partners. The results indicate a positive interaction between policy and tariff levels, but a negative interaction between policy and RTA membership. In other words, the trade-deterring effect of due diligence policies is strongest when tariffs are low and when trade integration is in place, a result that holds even under demanding specifications with stringent fixed effects.

[9] Although the confidence intervals for roundwood are large.

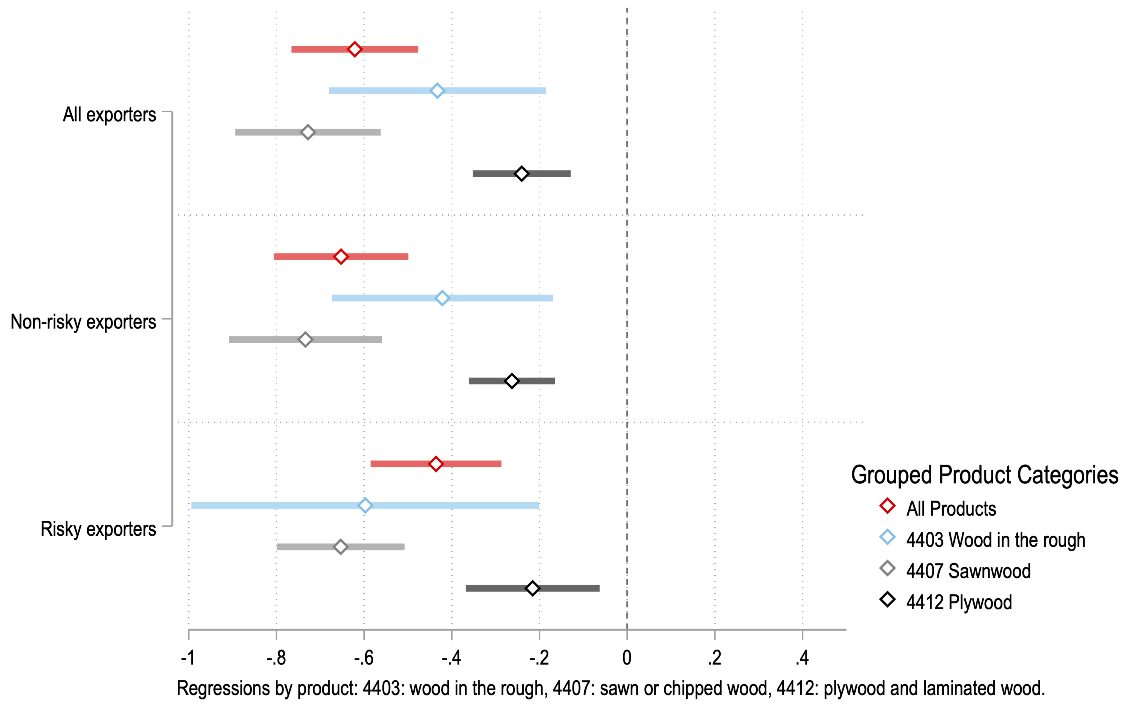


Figure 4: Heterogeneous effects across timber products

The interaction between due diligence policies and trade liberalization reveals one major tension. On the one hand, regional trade agreements and tariff reductions are designed to facilitate market access, yet our results suggest that the negative effect of due diligence policies on timber trade is particularly pronounced for these trade patterns. This indicates that, in economic terms, the two instruments act as substitutes: regulatory frictions offset some of the trade gains generated by liberalization. At the same time, they may be viewed as complementary from a policy perspective, since environmental regulations are introduced precisely to temper the risks of unsustainable practices that liberalized trade could exacerbate. Taken together, this suggests that due diligence and liberalization are not independent, but at the same time dampen trade volumes and enhance the sustainability of global timber markets.

4.6. Robustness checks and extensions

This subsection provides supporting results that address potential concerns in the estimations.

4.6.1. Robustness of risk classification

The classification of risky and non-risky exporter is based on a binary, country-level list, which does not vary over time or by product and may not fully capture firm-level or evolving illegality risks. We acknowledge potential measurement error and complement our analysis with robustness checks.

Table 6: Complementarity with trade policy

	Dep. Variable: X_{ijt}			
	(1)	(2)	(3)	(4)
Policy	-0.735***		-0.528***	
	(0.0825)		(0.0826)	
Policy × Bilateral Tariff (wood)	12.88***	8.201***		
	(3.536)	(2.358)		
Policy × RTA (0/1)			-0.176**	-0.268***
			(0.0748)	(0.0511)
Bilateral Tariff (wood)	-1.054	-3.557***	-1.012	-3.373***
	(0.716)	(0.771)	(0.720)	(0.801)
Importer Population	-0.311		-0.318	
	(0.208)		(0.209)	
RTA (0/1)	0.256***	0.0397	0.293***	0.0986**
	(0.0570)	(0.0385)	(0.0608)	(0.0393)
Importer Forest Rents	-0.360***		-0.344***	
	(0.0596)		(0.0606)	
# Observations	182,578	175,504	182,578	175,504
Importer-Exporter FE	x	x	x	x
Exporter-Year FE	x	x	x	x
Importer-Year FE		x		x

Standard errors in parentheses.

Clustered standard errors at importer-year level.

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

We construct three alternative measures of illegal logging risk potential at the country level to test the robustness of our baseline classification. First, we capture weak institutional quality. A country is classified as risky if its Rule of Law index, from the World Bank's Worldwide Governance Indicators, lies below the 25th percentile of the distribution. It reflects the main idea that countries with weak institutions and limited enforcement capacity are more likely to permit or tolerate illegal logging activity. Second, we combine governance weakness with forest exposure. A country is classified as risky if it simultaneously ranks in the top 25% of the forest share distribution and in the bottom 25% of the Rule of Law distribution. It rests on the idea that illegal logging risk is highest where both the resource is abundant and the institutional capacity to regulate its extraction is limited. Countries with large forest cover but strong governance, or with weak governance and little forest, are not classified as risky under this measure. Third, we use trade discrepancies as a proxy for low governance. A country is classified as risky if it exhibits a discrepancy index below -0.5 and belongs to the top 25% of the forest share distribution. The discrepancy index captures systematic gaps between reported export values and mirror import values in bilateral trade data: large negative values mean that a country's exports are systematically under-reported relative to what its trading partners declare as imports, a pattern consistent with potential illegal timber flows (Mitikj and Kaushik, 2024). As above, combining this measure with high forest cover ensures that we identify countries where the opportunity for illegal logging is present. Figure A.1 displays the risk classification of the top 100 exporting countries across the four alternative measures, where orange squares indicate countries classified as risky and grey squares indicate non-risky countries, allowing direct visual comparison of agreement and disagreement across measures.

We estimate two sets of robustness checks using these alternative risk classifications. First, we include interaction terms between each risk measure and the policy variable, see Table A.1. Second, we split the sample into risky and non-risky exporters, see Table A.2. Both exercises are conducted twice: using time-invariant risk measures (as country means over sample), and using time-varying measures that exploit annual variation in governance and forest cover indicators, see Tables A.3 and A.4.

Across all specifications and risk definitions, no significant differences are observed between risky and non-risky exporters. If anything, the estimated trade-reducing effect of due diligence policies is slightly smaller for risky exporters than for non-risky ones, suggesting that the policy burden is broadly distributed and not concentrated on high-risk sources.

4.6.2. *Random Treatment*

One key result from our analysis is the idea that both risky and non-risky exporters are almost equivalently affected by the policies. To complement and support this result, we conduct a placebo test in which we randomly assign exporter risk status, drawing from a Bernoulli distribution with a 5% probability to be risky. If our baseline results are meaningful, the estimated coefficient on the interaction between policy and this random risk indicator should be statistically indistinguishable from zero across repeated randomizations. We replicate this placebo exercise 500 times. In each iteration, a random risk indicator is independently drawn for each exporter country and interacted with the policy variable. We estimate a PPML specification with exporter-pair, exporter-time, and importer-time fixed effects, clustering standard errors at the importer-year level. We record the coefficient and 95% confidence interval for the interaction term across all replications. Figure A.4 plot the estimate on the interaction term for each iteration, the fit and the average coefficient across replications.

Across all replications, the average coefficient on the random interaction is statistically insignificant, supporting that our baseline results of indiscriminate barriers across exporters do not arise spuriously.

4.6.3. *Placebo Trade Outcome*

We then conduct a placebo test to examine whether due diligence policies affect trade in products outside the timber sector. If our empirical strategy correctly identifies the impact of these policies on timber trade, we should not observe any systematic effect on unrelated product categories. Estimating the same specification on a set of non-targeted products therefore provides a useful validation of the identification strategy and helps rule out the possibility that our results capture broader trade shocks unrelated to timber regulations.

One challenge is that some due diligence policies, particularly the Lacey Act, can apply to a wide range of products, making it difficult to identify a sector that is clearly unaffected a priori. To address this issue, we construct a placebo group consisting of all trade flows in products from Section II of the HS classification ("Vegetable products") that are not covered by the due diligence policies examined in our study. This includes products from Chapters 06, 07, 08, 09, 10, and 11. Estimating our baseline specification on this placebo sample yields no significant effect of due diligence policies on trade in these products, see Table A.5, supporting the interpretation that our main results capture timber-specific policy effects rather than broader trade dynamics.

4.6.4. *Estimation issues*

Estimations using trade volumes instead of values. We replicated main estimations using trade volumes (net weight in tonnes) reported in the UN Comtrade database, instead of trade values. The models and specification remain identical, with trade values simply replaced by trade volumes. These volume-based estimations, displayed in Table A.6, confirm that our results between volume and values are consistent in direction and magnitude, indicating that the trade effects of due diligence policies are not driven by price fluctuations.

Estimations using BACI as trade data source. To verify that our results are not sensitive to the choice of trade database, we replicate the benchmark regressions using BACI data. As expected, the sample size is somewhat smaller due to BACI's reconciliation and coverage rules, but the qualitative conclusions remain unchanged (Table A.7). Due diligence policies continue to reduce trade substantially across both extensive and intensive margins, and no systematic differences are observed between risky and non-risky exporters. Notably, the estimated coefficients are slightly larger with BACI, further confirming the robustness of our main findings.

Identifying country-specific policy variables in a gravity framework. A central challenge in gravity estimation is to properly identifying the impact of country-specific policies on bilateral trade flows. Recent advances in the structural gravity literature have proposed two-stage methods that use exporter-time and importer-time fixed effects to recover multilateral resistance terms, which can then be incorporated directly into the main estimating equation (Freeman et al., 2025).

We adopt this recent methodology and apply it to our setting ; results are displayed in Table A.8. Results confirm the direction of the policy effect obtained in the baseline specification, though the point estimates are smaller once the multilateral resistance terms are explicitly included. This suggests that part of the raw policy effect is absorbed by general equilibrium adjustments, underscoring the value of this framework for evaluating country-specific trade policies.

Including internal trade flows. Recent studies in structural gravity highlight the importance of including domestic trade flows in the analysis, not only as additional information, but also as a means to identify the impact of non-discriminatory trade policy measures (Heid et al., 2021), such as due diligence measures. Indeed, due diligence such as timber regulation apply to all foreign suppliers for a given regulating importer. Unfortunately, no data source provides these internal trade flows directly. We hence approximate them.[10]

[10] The absence of data on internal trade flows explains why we do not use this specification as our benchmark.

First, we approximate the internal timber flows (i.e., flows from i to i) by using FAO data on total production and by subtracting all (foreign) export flows from UN Comtrade. Second, we include these flows in the estimation sample, and a dummy variable equal to 1 indicating foreign trade flows and 0 otherwise, *Foreign*. Results in Table A.9 confirm the benchmark estimates: we estimate a negative and significant effect of the policy, with trade reductions of around 50% in column 2, for instance. Column 3 confirms that the policy homogeneously affects risky and non-risky exporters.

Effect over time Figure A.2 plots the effects of the policies over time since their implementation. It complements the average post-treatment effect estimated earlier. It is plausible that these policies have both direct, immediate effects (acting as trade barriers) and delayed effects (through general equilibrium mechanisms). We find that the effects of due diligence policies are largely immediate and persist over time, up to 10 years after implementation. One caveat is that some policies analyzed in this paper are less than 10 years old, limiting our ability to observe their full long-term impact. Nonetheless, the results suggest that the average effect of the policies does not conceal substantial delayed effects, nor any strong anticipation effects.

Staggered treatments. Given the staggered introduction of due diligence regulations across importing countries, we complement our baseline two-way fixed effects specification with recent difference-in-differences methods adapted to structural gravity settings that account for heterogeneous treatment timing. Following the approach proposed by Nagengast and Yotov (2025), we re-estimate the average treatment effect allowing for dynamic responses and avoiding potential biases associated with staggered adoption. The estimated average effect is -0.325 (sd = 0.10), which corresponds to a decline of approximately 27.8% in treated trade flows. The magnitude is smaller than in our benchmark estimates, consistently with recent findings that two-way fixed effects estimators in gravity settings can place different weights on observations across cohorts and time periods in the presence of heterogeneous treatment effects, leading to differences in aggregated treatment effects. We use these estimates in our counterfactual simulations as a robustness check, confirming that our main conclusions are not driven by the estimation method. The dynamic specification, plot in Figure A.3, indicates that the effect materializes immediately after implementation, with no evidence of anticipation effects in the pre-treatment periods.

4.6.5. Endogeneity concerns

Reverse causality is a potential issue for identification of the effect of the policy. Reverse causality may arise if the adoption of due diligence policies by importer countries is influenced by pre-existing trends or patterns in timber trade. For example, an importer country with growing imports from regions associated with illegal deforestation or unsustainable timber production practices might be more likely to implement due diligence policies. If this is the case, causality could be bi-directional: while policies may reduce imports, the volume or nature of imports might also prompt policy adoption.

To address this reverse causality, we need an instrument that predicts the adoption of due diligence policies but does not directly affect timber trade flows except through these policies. One candidate instrument is the participation to International Environmental Agreements (IEA) by the importing country (e.g., participation in the Paris Agreement, CITES). Indeed, countries that are active in global environmental initiatives such as IEAs are often more likely to implement domestic policies in line with international standards, including due diligence for environmentally sensitive goods. This variable reflects environmental commitment without directly influencing specific timber trade flows, except through the trade policies. The excluded instrument is the number of IEA in force signed by the importer up to year $t-1$.

In the first stage, we regress the endogenous variable, (policy), on the excluded instrument, along with a set of control variables and fixed effects for bilateral pairs and exporter-year. This regression aims to isolate the variation in the policy variable that is driven by the instrument. The second stage of the IV-S2SLS estimation runs the PPML estimation from eq. 1 using the first-stage results. Table A.10 shows the IV results of the analysis. Column 1 (first-stage results) shows that the number of IEA is a strong predictor of the policy dummy. Countries that signed many IEAs are more likely to adopt these due diligence policies in the timber industry, ceteris paribus. Columns 2, 3 and 4 show the second-stage results for the whole sample, for "safe" exporters, and for "risky" exporters, respectively. Results are in line with the benchmark conclusions, even though magnitudes are almost twice as large. We estimate a strong negative impact of the policy on the average trade flows, with both groups of exporters being affected by the policy. We also note that confidence intervals are larger than in the benchmark PPML estimations.

4.6.6. Additional specifications

Regarding specification choice, we re-estimate our main specification dropping the exporter-year fixed effects, and augmenting by a rich set of time-varying exporter controls including institutional quality and governance indicators. This specification allows time-varying exporter characteristics to enter directly. The results, reported in Table A.11, are qualitatively unchanged, confirming that the trade reductions we document are not driven by omitted time-varying exporter conditions.

4.6.7. Domestic Environmental Policies

A potential concern is that the negative trade effect attributed to due diligence policies may reflect correlated domestic environmental policies in importing countries rather than due diligence requirements themselves. While domestic environmental policies on the exporter side are absorbed by exporter-year fixed effects, their effect on the importer side is less straightforward, as such policies primarily target domestic production and consumption rather than trade flows.

Nevertheless, we provide a direct empirical test by including the OECD Environmental Policy Stringency index (Kruse et al., 2022) (EPS) as a time-varying importer control, however restricting the sample to OECD importers. Results are reported in Table A.12. The trade reduction we document is not driven by correlated domestic environmental regulations: as the policy effect remains stable across specifications, ranging from -0.570 to -0.628 and close to benchmark estimates. Interestingly, the EPS index itself enters positively and significantly, reflecting the fact that high-income OECD importers with stricter environmental frameworks are also larger timber importers, somehow consistent with an income effect.

4.6.8. Wider product scope

Our baseline focuses on three upstream timber products which are most directly linked to logging and sourcing decisions. These products capture the stages where illegal logging risks are highest and where due diligence is expected to operate, allowing us to identify trade adjustments associated with timber legality policies.

To check for potential substitution toward other timber goods, we conduct robustness analyses using broader coverage. First, we include in our sample all products in HS Chapter 44 (wood and wood products), see Table A.13. Second, we extend to Chapters 47, 48 (pulp and paper) as well as some products in Chapter 94 to capture downstream goods, see Table A.14. Results remain similar in signs and magnitudes, indicating that our main findings are not driven by product substitution.

4.6.9. Alternative clustering of standard errors

Regarding inference, we re-estimate the main specifications using bilateral exporter–importer clustering of standard errors (Table A.15), which allows for arbitrary correlation of shocks within each trading pair over time. The results remain unchanged, with levels of statistical significance very close to the baseline estimates. This confirms that our main inferences are not driven by restrictive assumptions about the error structure and are robust to alternative clustering strategies.

5. Counterfactuals

In this section, we conduct a counterfactual analysis to quantify the impact of due diligence policies on international trade. Our approach builds on recent advances in structural gravity modeling (Yotov et al., 2016; Zylkin, 2019; Campos et al., 2024) and consists of two exercises.

First, we estimate trade flows in the absence of regulation, generating a "no-policy" baseline. Comparing these simulated flows with observed trade outcomes enables us to quantify the overall effect of existing policies on global trade. Second, we evaluate an alternative, more targeted policy design that applies due diligence obligations only to exporters from "risky" countries. This exercise is motivated by our earlier findings, which showed that the current policy effectively applies to all exporters, regardless of the underlying risk of illegality. We therefore simulate a narrower policy—consistent in principle with the existing framework, but with a restricted scope informed by external evidence on illegality. Comparing trade flows under this targeted scenario with the no-policy baseline allows us to assess whether such a design could mitigate the trade reshuffling observed under the current policy regime.

5.1. Setup

To implement the counterfactual analysis, we combine the estimated structural gravity model with a general equilibrium simulation framework following Yotov et al. (2016), Baier et al. (2019), Zylkin (2019), and Campos et al. (2024). The counterfactuals are solved using the universal gravity framework of Allen et al. (2020).

First, we convert the estimated policy coefficient into a bilateral trade cost shock. The gravity estimations yield a policy effect of $\beta = -0.68$ (Table 3, column 3), which captures the partial equilibrium reduction in trade flows associated with due diligence policies.

Second, these bilateral trade cost shocks are introduced into a general equilibrium gravity model. The model allows countries to reallocate trade across partners while accounting for adjustments in production and prices. The simulations rely on two key structural parameters: an elasticity of substitution across timber varieties of $\theta = 8.55$, following estimates for the relevant HS4 products in Fontagné et al. (2022), and a supply elasticity of $\psi = 0.7$, capturing medium-term adjustments in timber production.

Third, the model solves for the new equilibrium of the global trading system under alternative policy scenarios. In the first scenario ("no-policy"), due diligence policies are removed, allowing us to compute the level of trade that would prevail in the absence of these regulations. In the second scenario ("partial policy"), the policy is applied only to exporters classified as risky. Comparing these simulated equilibria with the observed trade flows allows us to quantify the overall trade impact of existing due diligence policies and the

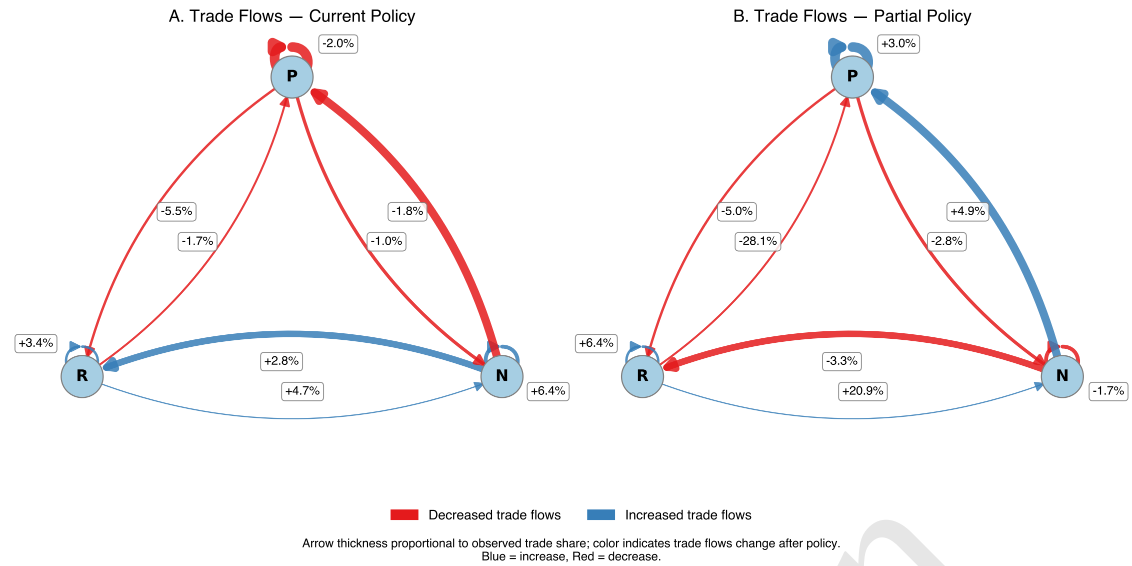


Figure 5: Estimated Effect of Due Diligence Policies on Timber Trade Flows
Note: P denotes regulating (policy-imposing) countries, R denotes risky exporting countries, and N denotes non-regulating, non-risky countries.

potential gains from more targeted regulatory designs.

To represent the distributional effects of these scenarios, countries are classified into three groups: (i) regulating countries imposing due diligence policies, denoted P; (ii) risky exporting countries where illegal logging risks are documented, denoted R; and (iii) the remaining non-regulating and non-risky countries, denoted N. Predicted bilateral trade flows are aggregated across these groups (within and between P, R, and N) to highlight how different policy designs reshape the structure of global timber trade.

5.2. Results

Current Policy Effects. The estimated effects of the policies are presented in Figure 5. Panel A plots the estimated trade effects of the current due diligence policies. Our counterfactual analysis yields several key findings.

First, the current policy reduced imports of timber products in the regulating countries (P), regardless of the origin of the goods. All incoming flows into P are negatively affected, consistent with the interpretation of due diligence obligations as a non-tariff trade barrier imposed uniformly on foreign suppliers. Interestingly, even flows from P to P are estimated to decline, despite the negligible risk of illegality in such transactions. Second, the policy altered the composition of imports into regulating countries. Specifically, it increased the relative share of imports sourced from risky countries (R), at the expense of non-risky suppliers, despite the stated objective of reducing dependence on illegal or high-risk sources. Importantly, this result should not be interpreted as an increase in the importance of risky exporters, nor as evidence that the policy benefits or favors these suppliers in absolute terms.

Rather, it reflects a compositional reallocation generated by broadly symmetric increases in trade costs across supplier groups, combined with differences in initial market shares. In particular, due diligence obligations operate as non-discriminatory trade cost shocks that reduce imports from both risky and non-risky exporters. Because non-risky suppliers initially account for a substantially larger share of imports into regulating countries, proportional contractions translate into larger absolute reductions for these exporters. As a consequence, the relative share of risky suppliers in remaining imports may mechanically increase even when imports from risky exporters also decline in absolute terms. This follows directly from the uniform nature of the policy design documented in our empirical results.

Additional mechanisms, related to heterogeneous adjustment across suppliers, may also be consistent with this pattern, although they are not directly identified in our analysis. In particular, pre-existing trade relationships between regulating countries and risky exporters, as well as dependence on specific timber resources supplied by these exporters, may affect the margin of adjustment for certain trade flows. While the literature also highlights potential compliance responses such as certification, adaptation, or misreporting (UNODC, 2025), our data do not allow us to disentangle these channels. We therefore interpret these mechanisms as suggestive rather than causal.

Third, the policy reshaped the export patterns of risky countries. Exports from *R* to *P* fell sharply, as expected, but these declines were partially offset by trade diversion toward non-regulating, non-risky countries (*N*) and between risky countries (*R*). As a result, the overall level of exports from *R* decreased only marginally, while the share of their exports directed to *N* increased—consistent with the evidence in Abman et al. (2026).

Fourth, the bulk of global trade in timber products is conducted by non-risky, non-policy countries (*N*). These countries collectively dominate both exports and imports. On the export side, *N* countries increasingly direct shipments to other *R* countries and to themselves, rather than to *P*. On the import side, however, the policy indirectly increased their reliance on risky suppliers, another unintended effect.

In summary, the current policy regime effectively isolates the regulating countries (*P*) from the rest of the world, including from non-risky partners. At the same time, it has fostered closer integration between risky countries (*R*) and non-policy countries (*N*), as a result of both trade diversion for risky countries' exports and increased trade costs for non-risky countries' exports.

In terms of magnitudes, our estimates imply that the aggregate effect of due diligence policies on total global timber trade amounts to 50 million USD. Amount are negligible as they correspond to less than 0.1%

of global timber trade in the sample period. Yet it hides composition changes: while imports into regulating countries (*P*) decline by roughly 500 USD million (around 1% of total trade), this reduction is largely offset by increases in exports from non-risky (*N*, +330 million, 0.6% of total trade) and risky (*R*, +150 million, 0.3% of worldwide trade) countries toward alternative destinations.

These adjustments are the general equilibrium counterpart of the estimated bilateral effects, which show average reductions of approximately 45% in treated trade flows. The apparent discrepancy between large bilateral declines and modest aggregate effects arises from trade diversion: the estimated 45% reductions apply to affected bilateral pairs, but general equilibrium substitution reallocates a substantial share of these flows toward alternative destinations, leaving global trade volumes largely unchanged.

In other words, the policy reshapes the geography of trade rather than substantially reducing its overall level. The bilateral contraction observed in treated pairs is therefore consistent with relatively modest net aggregate changes once trade diversion and substitution across destinations is allowed. This pattern highlights the presence of leakage and diversion effects, which redistribute timber trade across country groups (*P*, *R*, and *N*) rather than eliminating it.

Alternative Policy Effects. Figure 5, Panel B reports the estimated effects of an alternative "partial policy" that applies due diligence obligations only to exports originating from risky countries (*R*). This differentiated regime is modeled as an increase in trade costs for *R*, while trade costs between other partners remains unaffected.

Three main results emerge. First, unlike the baseline policy, the partial policy does not reduce total imports into the regulating countries (*P*). The decline in imports from *R* is offset by increased flows from non-risky exporters (*N*). In addition, trade between *P* countries themselves increases, which is important given the size of these flows in global trade.

Second, the composition of imports into *P* changes in a direction consistent with the intended policy goal. The share of imports from *N* increases, while the share from *R* declines, suggesting that the narrower scope reduces exposure to risky suppliers without penalizing other partners. Third, as in the baseline, risky countries adjust by redirecting exports away from *P* toward *N*. These diversion effects are sizable, but they occur alongside a clear contraction in *R*'s overall access to global markets. In sum, the partial policy reduces *R*'s integration into global markets while strengthening trade linkages between *P* and *N*. Although some diversion persists, the overall pattern is closer to the policy's intended goals.

Taken together, these exercises suggest that the current design of due diligence policies is effective at curbing trade but at the cost of penalizing legal exporters and reshaping trade flows in unintended ways. A more targeted approach appears to reduce these inefficiencies: it limits exposure to risky suppliers while maintaining trade integration with non-risky partners. These findings underscore the importance of policy design in balancing enforcement against the broader functioning of global timber markets.

5.3. Sensitivity to policy effects and to parameters

We examine the sensitivity of the counterfactual results to alternative assumptions regarding the magnitude of policy effects and key structural parameters. Overall, the main conclusions remain robust across a range of plausible values.

First, the counterfactual simulations depend on the estimated magnitude of the policy shock obtained in the micro-level gravity estimations. As discussed in the previous sections, our estimations provide a range of possible values for the policy coefficient. To assess how sensitive the counterfactual outcomes are to this magnitude, we conduct simulations using an alternative policy effect of $\beta = -0.2$. Figure A.5 reports the resulting counterfactual outcomes. The qualitative patterns remain similar to those obtained in the benchmark specification.

Second, we test the sensitivity of the results to the elasticity of substitution across timber products. While the benchmark calibration uses an elasticity of substitution of $\theta = 8.55$, we consider two alternative values commonly used in the trade literature for timber products (Fontagné et al., 2022), $\theta = 6$ and $\theta = 10$. The corresponding counterfactual results are presented in Figures A.6 and A.7. The overall structure of trade adjustments and the direction of the effects remain largely unchanged.

Finally, we examine the role of the supply elasticity parameter. The benchmark simulations assume a supply elasticity of $\psi = 0.7$. To test the robustness of the results, we conduct an alternative simulation using a higher supply elasticity of $\psi = 1$. Figure A.8 presents the corresponding counterfactual outcomes. Once again, the main conclusions remain qualitatively similar.

Taken together, these exercises indicate that the counterfactual results are not driven by specific parameter choices and remain stable across a range of plausible policy effects and structural parameters.

6. Conclusion and Discussion

Main results. This paper investigates the impact of due diligence policies on global timber trade, assessing their effectiveness in curbing the entry of illegal timber into domestic markets while examining potential unintended consequences for legal exporters. Using a structural gravity model and trade data spanning from 2000 to 2021, we find that these regulations generate substantial reductions in treated bilateral trade flows, with an average decline of around 45% relative to the counterfactual following policy implementation. Importantly, this effect is not limited to high-risk exporters; even countries with strong governance and legal forestry sectors experience trade declines. Among the policies analyzed, the US Lacey Act and the European Union Timber Regulation (EUTR) exhibit the strongest trade-deterring impact, particularly on raw timber products. However, our findings also highlight the broad costs associated with compliance, which may disproportionately burden legal exporters and create unintentional trade distortions. Our counterfactual exercises suggest that the current design of due diligence policies is effective at curbing trade but at the cost of penalizing legal exporters and reshaping trade flows in unintended ways. A more targeted approach appears to reduce these inefficiencies: it limits exposure to risky suppliers while maintaining trade integration with non-risky partners.

These results provide an assessment of the effectiveness of these due diligence policies.
First, timber due diligence regulations function in practice as non-tariff barriers, isolating implementing countries from the global timber market. They impose significant compliance costs and generate trade frictions for all exporters, with effects that are largely undifferentiated, affecting both high-risk and low-risk suppliers. From this perspective, if the primary objective is to restrict trade only with high-risk countries, current policies appear to miss their intended target. Second, the evidence of trade diversion effects, also highlighted in, *e.g.*, Vasconcelos et al. (2024); Abman et al. (2026), suggests that risky countries are able to circumvent these regulations, which in turn limits their capacity to mitigate illegal logging. In fact, illegal logging remains a major channel of environmental crime (UNODC, 2025), and our results indicate that complementary policy instruments, stronger enforcement modalities, and international cooperation may be required to address this illegality more effectively. In particular, the participation of major importers, such as China, is key in hindering this diversion effect (Vasconcelos et al., 2024).
Finally, since most of the countries that regulate imports are Western, these policies result in reduced imports at a time when many Western nations are becoming more reliant on biomass to meet sustainability and climate goals. This raises important trade-offs: while illegal timber imports are reduced, import reduction may conflict with national energy, bioeconomy, or sustainability strategies. At the same time, these regulations shift responsibility away from Western countries, limiting their direct exposure to illegally harvested timber products (Vasconcelos et al., 2024). Our counterfactual exercises suggest, however, that a more targeted policy design could isolate risky suppliers from the market without reducing overall imports in regulating countries, therefore reducing this trade-off.

Implications for EUDR. The timber due diligence experience provides valuable lessons for the regulation of other deforestation-prone commodities. A prominent example is the EU Deforestation Regulation

(EUDR), which will eventually replace the former EUTR and extends the due diligence principle to a wider set of agricultural commodities including soy, beef, palm oil, cocoa, coffee, and rubber, in addition to wood products (Köthke et al., 2023). This new regulation differs from EUTR by at least three paradigm shifts: (i) extension to agricultural commodities to target deforestation drivers rather than illegal logging; (ii) implementation of more stringent controls and registration processes; (iii) application of due diligence procedures to intra-EU trade flows. Indirectly, by reducing demand for deforestation-linked agricultural commodities, the EUDR may also reduce pressure on forest resources, though this channel remains speculative and its quantification lies beyond the scope of the present analysis. Importantly, illegal logging represents a narrow slice of the global deforestation problem: recent studies suggest that over 60% of tropical deforestation is linked to agricultural activities (Dummett and Blundell, 2021), meaning that regulations targeting the broader set of deforestation drivers could achieve greater environmental effectiveness than timber-specific due diligence alone.

In light of our findings, we see four concrete implications for the EUDR's design and expected effects. While the EUDR differs from timber due diligence both in its focus on deforestation-free supply chains and in its broader commodity coverage, our results provide a relevant benchmark for understanding potential trade effects of similar due diligence-type mechanisms.

First, due diligence regulations function in practice as non-tariff barriers, generating compliance costs and trade frictions that extend beyond high-risk suppliers. Applied to the EUDR context, that suppose more stringent due diligence obligations and, therefore, higher compliance costs, commodity exporters with strong governance and legal land-use frameworks may nonetheless face significant and higher trade penalties. Second, EUDR framework targets intra-EU trade flows of agricultural and wood products, which will eventually reduce trade flows in the European domestic market. This may further hinder the European Union's capacity to achieve energy, bioeconomy, and sustainability goals, while increasing demand pressure on European forest resources. Third, our evidence of trade diversion suggests that stricter EU import requirements may simply shift deforestation-linked trade toward less regulated markets, limiting overall environmental effectiveness. Monitoring and coordinating with non-EU importing countries will be essential to avoid leakage. Fourth, our counterfactual exercises show that risk-targeted policy design preserves trade volumes while isolating risky exporters, providing direct empirical support for the EUDR's country benchmarking approach. Differentiation along exporter risk profiles can mitigate the adverse trade effects we document for undifferentiated due diligence frameworks, while maintaining enforcement effectiveness against high-risk suppliers.

Future research. Several avenues for further research remain. First, we leave for future research the theoretical investigation of due diligence policies in trade theory. Traditional trade models emphasize tariff and non-tariff barriers, but due diligence introduces a novel form of regulation that affects trade flows through compliance costs and risk assessments. Future work could explore the integration of due diligence policies into standard trade model.

Second, future research could study the different strategies set up by actors to deal with these due diligence policies. The present paper documents that trade diversion to other destinations is adopted by exporting countries facing due diligence requirements. Yet, the precise channels and the full range of adjustment strategies remain to be investigated. Beyond diverting trade to non-regulated markets, exporters may adapt in multiple ways: they may increase trade in processed wood products, for which it is more difficult to guarantee traceability along the value chain; misclassify timber products to circumvent regulations (UNODC, 2025); redirect production to domestic consumption; invest in certification or governance improvements to reduce compliance costs; or exit regulated markets entirely. Our reduced-form framework does not allow us to separate these responses as the estimated trade reduction reflects the net effect of compliance costs borne by importers, deterrence of illegal sourcing, and exporter-side behavioural adjustments simultaneously. Importantly, our analysis identifies changes in trade flows induced by these policies, but does not allow us to disentangle whether these effects reflect reductions in illegal timber trade, increased compliance costs, or other exporter adjustments. As such, we do not identify the effect of these policies on illegal timber imports. Providing an overview and a hierarchy of these existing strategies is all the more important as due diligence policies expand in the policy sphere, and as policymakers need to anticipate whether exporters respond by improving legality systems or by circumventing regulations through other means.

Relatedly, our analysis does not capture detailed environmental, or social outcomes at the level of exporting industries, forest-dependent communities, or individual workers. While our framework quantifies how trade volumes and sourcing patterns respond to regulatory interventions, it abstracts from within-country heterogeneity in firm behavior. In particular, firms within the same exporting country may differ substantially in their exposure to due diligence requirements, compliance capacity, and ability to adjust, through certification or product transformation. As a result, our estimates reflect aggregate responses and do not distinguish heterogeneous effects across firms or supply-chain actors. Assessing more localized impacts—such as compliance costs, changes in illegal logging, or effects on livelihoods—would require complementary firm-level, sectoral, or environmental data. Future research could extend our work by integrating such micro-level information to provide a more complete understanding of the broader consequences of due diligence policies.

Finally, the ultimate objective of due diligence regulations is to combat illegal logging and, to a certain extent, deforestation. While reduced timber exports may suggest lower demand for illegally sourced timber, they could also lead to unintended shifts in deforestation drivers, such as increased domestic consumption or displacement effects. Future research could investigate the direct impact of due diligence regulations on deforestation rates, considering variations across regions, policy enforcement levels, and alternative land-use pressures.

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CRediT authorship contribution statement

Valentin Mathieu: Conceptualization, Data curation, Formal analysis, Investigation, Methodology, Resources, Software, Validation, Visualization, Writing – original draft, Writing – review & editing.
Clément Nedoncelle: Conceptualization, Data curation, Formal analysis, Investigation, Methodology, Resources, Software, Validation, Visualization, Writing – original draft, Writing – review & editing.

Declaration of competing interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Data availability

Data and code will be made available on Github.

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Appendix

A. Additional Results

Table A.1: Alternative Risk Measures

	Time-invariant risk measures		
	(1)	(2)	(3)
	Weak Governance	Gov. + Forest	Discrepancy + Forest
Policy	-0.642***	-0.665***	-0.619***
	(0.0731)	(0.0730)	(0.0670)
Policy $\times$*Risky*	0.124*	0.294***	0.138
	(0.0697)	(0.0817)	(0.100)
Importer Population	-0.135	-0.138	-0.139
	(0.254)	(0.255)	(0.254)
Bilateral Tariff (wood)	-0.676	-0.706	-0.647
	(0.719)	(0.721)	(0.718)
RTA (0/1)	0.268***	0.281***	0.276***
	(0.0461)	(0.0470)	(0.0462)
Importer Forest Rents	-0.342***	-0.349***	-0.338***
	(0.0551)	(0.0560)	(0.0542)
Observations	180500	180500	180500

Standard errors in parentheses

Clustered standard errors at the jt level

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

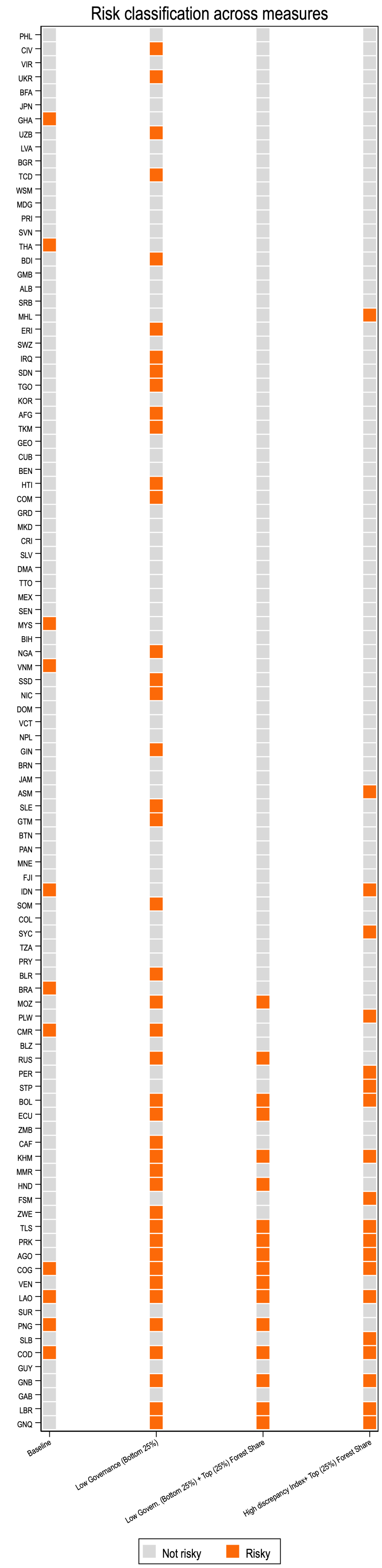

Figure A.1: Risk classification across alternative measures

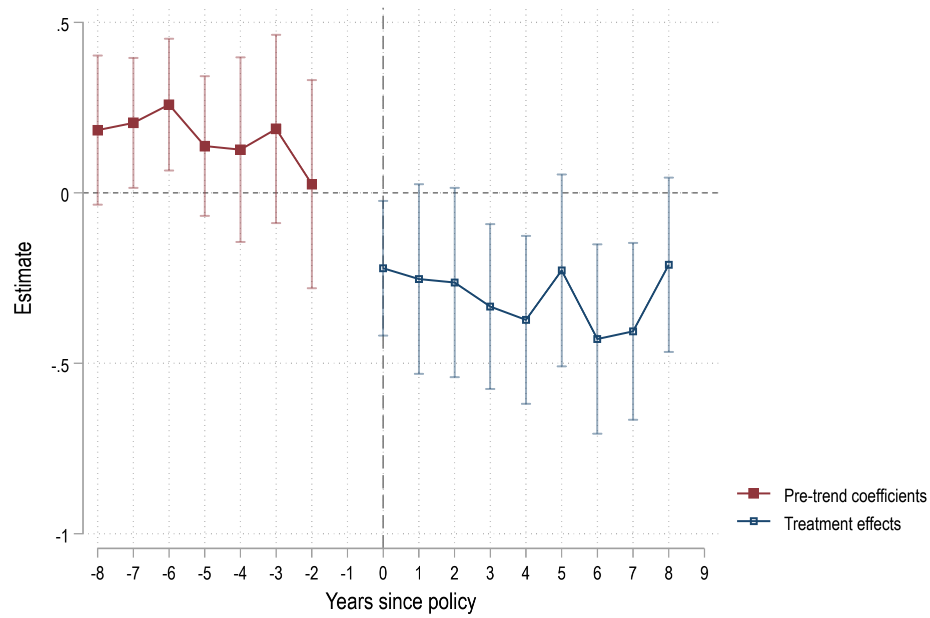

Figure A.2: Effect over time

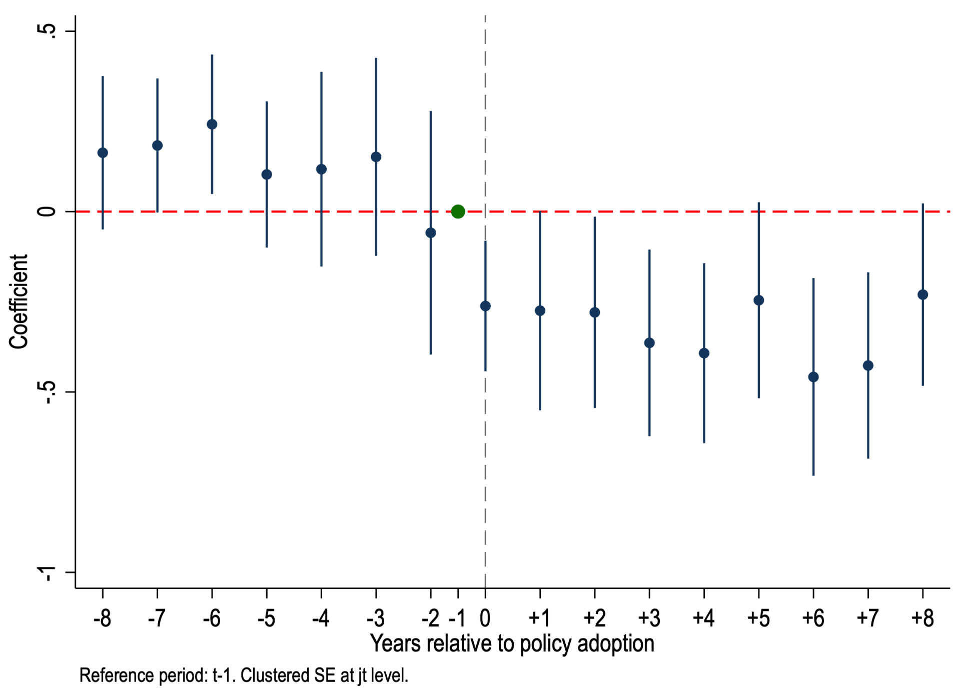

Figure A.3: Staggered Diff-in-Diff Setup (Nagengast and Yotov, 2025)

Table A.2: Alternative Risk Measures - Across subsamples

	Weak Governance		Gov. + Forest		Discrepancy + Forest	
	(1) Not Risky	(2) Risky	(3) Not Risky	(4) Risky	(5) Not Risky	(6) Risky
Policy	-0.638***	-0.490***	-0.663***	-0.394***	-0.619***	-0.370***
	(0.0728)	(0.0751)	(0.0729)	(0.0784)	(0.0672)	(0.110)
Importer Population	-0.406	0.902***	-0.374	1.194***	-0.138	0.293
	(0.268)	(0.343)	(0.263)	(0.423)	(0.255)	(0.671)
Bilateral Tariff (wood)	-1.685**	2.387**	-1.443*	3.196***	-0.773	0.924
	(0.834)	(0.931)	(0.802)	(1.065)	(0.755)	(1.458)
RTA (0/1)	0.334***	-0.0139	0.257***	0.570***	0.269***	0.396***
	(0.0578)	(0.0864)	(0.0500)	(0.129)	(0.0485)	(0.139)
Importer Forest Rents	-0.376***	-0.282***	-0.382***	-0.259***	-0.333***	-0.707***
	(0.0668)	(0.0551)	(0.0658)	(0.0580)	(0.0541)	(0.134)
Observations	142240	38260	163803	16697	168419	12081

Standard errors in parentheses

Clustered standard errors at the jt level

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

Table A.3: Alternative Time-Varying Risk Measures

	Time-varying risk measures		
	(1) Weak Governance	(2) Gov. + Forest	(3) Discrepancy + Forest
Policy	-0.632***	-0.638***	-0.609***
	(0.0701)	(0.0699)	(0.0666)
Policy $\times$*Risky*	0.0960	0.163**	-0.730***
	(0.0648)	(0.0751)	(0.126)
Importer Population	-0.138	-0.137	-0.128
	(0.254)	(0.254)	(0.254)
Bilateral Tariff (wood)	-0.664	-0.668	-0.626
	(0.718)	(0.719)	(0.716)
RTA (0/1)	0.272***	0.278***	0.276***
	(0.0461)	(0.0465)	(0.0460)
Importer Forest Rents	-0.341***	-0.343***	-0.338***
	(0.0548)	(0.0551)	(0.0540)

Standard errors in parentheses

Clustered standard errors at the jt level

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

Table A.4: Alternative Time Varying Risk Measures - Across subsamples

	Weak Governance		Gov. + Forest		Discrepancy + Forest	
	(1)	(2)	(3)	(4)	(5)	(6)
	Not Risky	Risky	Not Risky	Risky	Not Risky	Risky
Policy	-0.625***	-0.574***	-0.644***	-0.491***	-0.620***	-0.618***
	(0.0716)	(0.0848)	(0.0716)	(0.0926)	(0.0672)	(0.106)
Importer Population	-0.327	0.846**	-0.330	1.189**	-0.143	3.031***
	(0.264)	(0.390)	(0.262)	(0.467)	(0.255)	(0.838)
Bilateral Tariff (wood)	-1.589*	3.107***	-1.284*	2.739**	-0.707	-2.991**
	(0.827)	(1.034)	(0.773)	(1.283)	(0.747)	(1.320)
RTA (0/1)	0.318***	0.0327	0.263***	0.858***	0.269***	0.605***
	(0.0554)	(0.0935)	(0.0496)	(0.118)	(0.0485)	(0.112)
Importer Forest Rents	-0.370***	-0.247***	-0.373***	-0.242***	-0.332***	-1.010***
	(0.0648)	(0.0600)	(0.0638)	(0.0652)	(0.0541)	(0.135)
Observations	143002	31398	165369	13192	169096	11066

Standard errors in parentheses

Clustered standard errors at the jt level

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

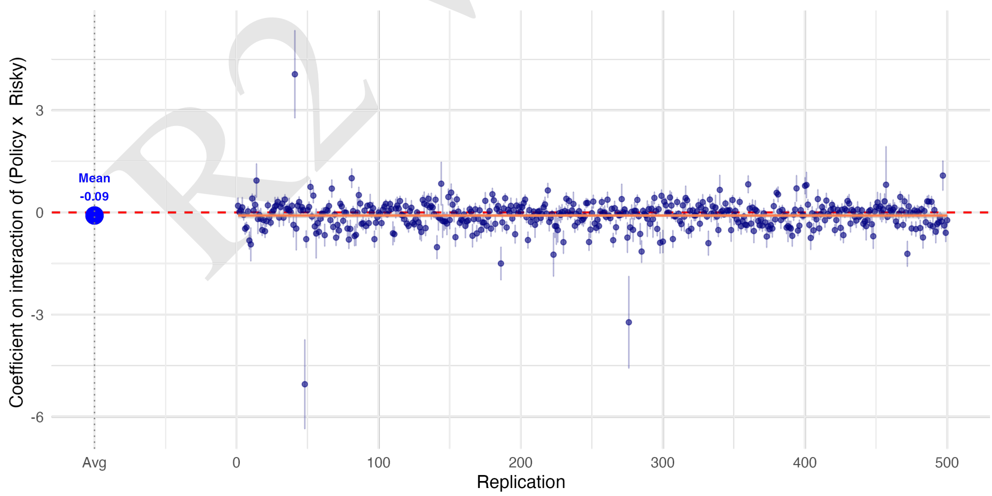

Figure A.4: Random assignment into risk - Main effect

Note: Each estimation randomly assigns countries to "risky" or "non-risky" status, then estimates the differential effect of the policy on trade flows between these placebo groups. The resulting coefficient is recorded across 500 replications, each shown as a spike with its 95% confidence interval. The left panel reports the average differential effect across all replications. The orange line shows the linear fit across replications.

Table A.5: Placebo Trade Outcome

	Dep. Variable: Placebo X_{ijt}			Placebo Q_{ijt}		
	(1)	(2)	(3)	(4)	(5)	(6)
Policy	-0.00224	-0.0331	-0.0237	-0.00170	-0.0784	-0.0738
	(0.0262)	(0.0265)	(0.0270)	(0.0451)	(0.0488)	(0.0515)
Policy × Risky			-0.0674			-0.0394
			(0.0425)			(0.101)
# Observations	175,566	175,380	175,380	187,078	175,291	175,291
Importer-Exporter FE	x	x	x	x	x	x
Year FE	x			x	x	
Exporter-Year FE		x	x			x

Standard errors in parentheses

Clustered standard errors at the jt level

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

Products from Chapters 06, 07, 08, 09, 10, and 11.

Table A.6: Replicating the analysis using trade volumes

	Dep. Variable: Q_{ijt}				Log Q_{ijt}	$(Q_{ijt} > 0)$
	(1)	(2)	(3)	(4)	(5)	(6)
Policy	-0.642***	-0.767***	-0.562***	-0.547***	-0.407***	-0.0655***
	(0.165)	(0.152)	(0.147)	(0.152)	(0.0817)	(0.0108)
Importer Population	-0.950**	-1.276***	-1.297***	-1.312***	-0.172	0.0560*
	(0.383)	(0.363)	(0.335)	(0.334)	(0.142)	(0.0297)
Bilateral Tariff (wood)			-2.186	-2.188	-1.554*	-0.130**
			(1.373)	(1.373)	(0.920)	(0.0585)
RTA (0/1)			0.0618	0.0634	-0.0376	-0.00201
			(0.120)	(0.120)	(0.0591)	(0.00688)
Importer Forest Rents			-0.518***	-0.520***	-0.272***	-0.0319***
			(0.119)	(0.119)	(0.0659)	(0.00721)
Policy × Risky				-0.191		
				(0.122)		
# Observations	184,700	184,214	178,381	178,381	71,905	182,919
Importer-Exporter FE	x	x	x	x	x	x
Year FE	x					
Exporter-Year FE		x	x	x	x	x

Standard errors in parentheses

Clustered standard errors at the jt level

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

Table A.7: Replicating the analysis using BACI data

	Dep. Variable: X_{ijt}			Log X_{ijt}	$(X_{ijt} > 0)$	
	(1)	(2)	(3)	(4)	(5)	(6)
Policy	-0.854***	-1.051***	-0.970***	-0.545***	-0.339***	0.0336***
	(0.0960)	(0.122)	(0.117)	(0.121)	(0.0650)	(0.0121)
Importer Population	-2.025***	-1.983***	-2.239***	-2.093***	-0.661***	0.0830**
	(0.450)	(0.482)	(0.481)	(0.563)	(0.196)	(0.0376)
Bilateral Tariff (wood)			-2.043	0.131	-3.282***	-0.0872
			(2.064)	(1.024)	(0.969)	(0.0707)
RTA (0/1)			0.167	-0.415***	-0.0542	0.0167
			(0.131)	(0.149)	(0.0673)	(0.0139)
Importer Forest Rents			-0.382***	-0.325***	-0.323***	0.0233**
			(0.0719)	(0.0667)	(0.0505)	(0.00949)
Policy $\times$ Risky				0.517		
				(0.330)		
# Observations	29,298	29,298	29,298	10,563	10,563	29,298
Year FE	x					
Importer-Exporter FE	x	x	x	x	x	x
Exporter-Year FE		x	x	x	x	x

Standard errors in parentheses

Clustered standard errors at the jt level

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

Table A.8: Identifying country-specific policy variables in a gravity framework

	Dep. Variable: X_{ijt}		
	(1)	(2)	(3)
Policy	-0.487***	-0.490***	-0.223***
	(0.0709)	(0.0718)	(0.0375)
$ln\widehat{P_{jt}}$	0.185**	0.293***	0.640***
	(0.0742)	(0.0743)	(0.0426)
Importer Population	-0.320	-0.392**	-0.0990
	(0.197)	(0.191)	(0.102)
Bilateral Tariff (wood)	-0.290	-0.335	-1.454***
	(0.680)	(0.663)	(0.398)
RTA (0/1)	0.270***	0.316***	0.167***
	(0.0599)	(0.0648)	(0.0462)
Importer Forest Rents	-0.223***	-0.189***	-0.111***
	(0.0655)	(0.0639)	(0.0304)
$ln\widehat{\Pi}_{it}$		-0.433***	
		(0.0709)	
# Observations	175,609	175,609	175,609
Importer FE	x	x	x
Year FE	x	x	x
Importer-Exporter FE	x	x	x

Col.3 offsets the impact of Π_{it}. Standard errors in parentheses

Clustered standard errors at the jt level

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

Table A.9: Including internal trade flows

	Dep. Variable: X_{ijt}		
	(1)	(2)	(3)
Policy	-0.479***	-0.574***	-0.581***
	(0.0565)	(0.0549)	(0.0614)
Foreign (0/1)	0.0816	-0.0331	-0.0338
	(0.0867)	(0.109)	(0.109)
Bilateral Tariff (wood)		-0.887	-0.887
		(0.722)	(0.723)
RTA (0/1)		0.164***	0.164***
		(0.0547)	(0.0548)
Policy× Risky			0.0438
			(0.0823)
# Observations	243,554	243,554	243,554
Importer-Exporter FE	x	x	x
Year FE	x		
Exporter-Year FE FE		x	x

Clustered standard errors at the importer-year level

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

Table A.10: IV Results

	(1)	(2)	(3)	(4)
	First-stage	Second-stage		
Dep. Variable:	Policy (0/1)	Exports (Value)		
Sample		Whole	Risky == 0	Risky == 1
Policy		-0.711***	-0.750***	-0.469***
		(0.0846)	(0.0912)	(0.0683)
Importer Population	-0.559***	0.327	-0.0155	1.753***
	(0.0442)	(0.219)	(0.238)	(0.266)
Bilateral Tariff (wood)	0.296***	-0.906	-0.475	-4.706***
	(0.0622)	(0.773)	(0.804)	(1.235)
RTA (0/1)	0.147***	0.129**	0.142**	0.0268
	(0.00825)	(0.0633)	(0.0575)	(0.147)
Number of IEAs in force, counting terminated IEAs	0.00229***			
	(0.000178)			
Observations	174929	174929	160576	14353

Standard errors in parentheses

Clustered standard errors at the jt level

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

Table A.11: Specification without exporter-year fixed effects.

	Dep. Variable: X_{ijt}			
	(1)	(2)	(3)	(4)
Policy	-0.587***	-0.562***	-0.556***	-0.501***
	(0.0808)	(0.0805)	(0.0819)	(0.0856)
Importer Population	-0.354	-0.368	-0.365	-0.338
	(0.221)	(0.225)	(0.225)	(0.225)
Bilateral Tariff (wood)	0.00841	0.321	0.334	0.269
	(0.771)	(0.823)	(0.823)	(0.822)
RTA (0/1)	0.310***	0.295***	0.297***	0.292***
	(0.0638)	(0.0680)	(0.0682)	(0.0681)
Importer Forest Rents	-0.272***	-0.247***	-0.248***	-0.251***
	(0.0697)	(0.0696)	(0.0696)	(0.0688)
Exporter: Governance		0.325***	0.328***	0.282***
		(0.0935)	(0.0930)	(0.0988)
Exporter: Rule of law		-0.565***	-0.558***	-0.530***
		(0.118)	(0.120)	(0.121)
Policy × Risky			-0.0471	-0.120
			(0.0704)	(0.0764)
Policy × Exporter Rule of Law				-0.0656
				(0.0934)
Policy × Exporter Governance Quali.				0.0183
				(0.119)
# Observations	182,685	168,049	168,049	168,049
Importer-Exporter FE	x	x	x	x
Year FE	x	x	x	x

Standard errors in parentheses.

Clustered standard errors at exporter-importer level.

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

Table A.12: Omitted Variable: Domestic Env. Policy Stringency (EPS)

	Dep. Variable: X_{ijt}				Log X_{ijt}	$(X_{ijt} > 0)$
	(1)	(2)	(3)	(4)	(5)	(6)
Policy	-0.603***	-0.628***	-0.570***	-0.599***	-0.511***	-0.0365***
	(0.0853)	(0.0782)	(0.0758)	(0.0794)	(0.0751)	(0.0103)
Importer Population	-1.326	-1.109	-2.744***	-2.773***	-1.018*	-0.0529
	(0.846)	(0.755)	(0.738)	(0.740)	(0.616)	(0.0739)
Env. Policy Stringency	0.284***	0.362***	0.294***	0.317***	0.142***	0.00108
	(0.0589)	(0.0527)	(0.0490)	(0.0519)	(0.0456)	(0.00694)
Bilateral Tariff (wood)			-10.45***	-10.50***	-7.879***	0.0595
			(1.848)	(1.843)	(1.252)	(0.290)
RTA (0/1)			0.169**	0.150**	-0.160***	0.0156*
			(0.0698)	(0.0685)	(0.0533)	(0.00929)
Importer Forest Rents			-0.286***	-0.280***	-0.313***	-0.0389***
			(0.0846)	(0.0843)	(0.0726)	(0.0118)
Policy $\times$ Risky				0.205***		
				(0.0676)		
EPS $\times$ Risky				-0.218***		
				(0.0621)		
# Observations	73,191	71,370	70,246	70,246	37,895	72,638
Importer-Exporter FE	x	x	x	x	x	x
Year FE	x					
Exporter-Year FE		x	x	x	x	x

Standard errors in parentheses.

Clustered standard errors at exporter-importer level.

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

Table A.13: Alternative sample: HS chapter 44

	Dep. Variable: X_{ijt}				Log X_{ijt}	$(X_{ijt} > 0)$
	(1)	(2)	(3)	(4)	(5)	(6)
Policy	-0.474***	-0.472***	-0.428***	-0.412***	-0.467***	-0.0463***
	(0.0594)	(0.0566)	(0.0504)	(0.0563)	(0.0391)	(0.00721)
Importer Population	0.401	0.768***	0.478**	0.479**	0.630***	0.192***
	(0.244)	(0.239)	(0.198)	(0.198)	(0.111)	(0.0375)
Bilateral Tariff (wood)			-1.622***	-1.617***	-1.657***	-0.133**
			(0.577)	(0.574)	(0.449)	(0.0567)
RTA (0/1)			0.208***	0.209***	0.0751**	0.00437
			(0.0408)	(0.0409)	(0.0317)	(0.00524)
Importer Forest Rents			-0.319***	-0.320***	-0.203***	-0.0228***
			(0.0409)	(0.0406)	(0.0298)	(0.00601)
Policy × Risky				-0.0702		
				(0.0505)		
# Observations	289,725	289,725	279,378	279,378	135,388	279,593
Importer-Exporter FE	x	x	x	x	x	x
Year FE	x					
Exporter-Year FE		x	x	x	x	x

Standard errors in parentheses.

Clustered standard errors at exporter-importer level.

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

Table A.14: Alternative sample: HS chapters 44, 47, 48 and 94

	Dep. Variable: X_{ijt}				Log X_{ijt}	$(X_{ijt} > 0)$
	(1)	(2)	(3)	(4)	(5)	(6)
Policy	-0.356***	-0.372***	-0.333***	-0.307***	-0.429***	-0.0436***
	(0.0379)	(0.0391)	(0.0381)	(0.0382)	(0.0324)	(0.00728)
Importer Population	0.553***	0.617***	0.461***	0.467***	0.574***	0.177***
	(0.165)	(0.171)	(0.162)	(0.162)	(0.0895)	(0.0370)
Bilateral Tariff (wood)			-2.249***	-2.263***	-1.483***	-0.0617
			(0.447)	(0.446)	(0.327)	(0.0565)
RTA (0/1)			0.117***	0.119***	0.0652***	-0.000773
			(0.0339)	(0.0346)	(0.0249)	(0.00529)
Importer Forest Rents			-0.236***	-0.238***	-0.161***	-0.0264***
			(0.0308)	(0.0306)	(0.0222)	(0.00673)
Policy × Risky				-0.0992**		
				(0.0388)		
# Observations	368,579	368,579	354,645	354,645	188,335	354,884
Importer-Exporter FE	x	x	x	x	x	x
Year FE	x					
Exporter-Year FE		x	x	x	x	x

Standard errors in parentheses.

Clustered standard errors at exporter-importer level.

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

Table A.15: Alternative clustering (importer-exporter) of standard errors

	Dep. Variable: X_{ijt}			Log X_{ijt}	($X_{ijt} > 0$)	
	(1)	(2)	(3)	(4)	(5)	(6)
Policy	-0.643***	-0.644***	-0.682***	-0.621***	-0.420***	-0.0580***
	(0.109)	(0.109)	(0.102)	(0.102)	(0.0426)	(0.00576)
Log Distance	-1.211***					
	(0.103)					
Importer Population	-0.311	-0.306	-0.176	-0.338	0.0387	0.118***
	(0.330)	(0.330)	(0.302)	(0.249)	(0.103)	(0.0146)
Bilateral Tariff (wood)				-1.057	-1.361***	-0.0987**
				(0.885)	(0.364)	(0.0479)
RTA (0/1)				0.257**	-0.0379	0.00754
				(0.113)	(0.0529)	(0.00718)
Importer Forest Rents				-0.328***	-0.232***	-0.0221***
				(0.0712)	(0.0320)	(0.00428)
# Observations	188,741	188,741	188,741	182,578	76,627	182,919
Exporter FE	x					
Importer FE	x					
Year FE	x	x				
Importer-Exporter FE		x	x	x	x	x
Exporter-Year FE			x	x	x	x

Standard errors in parentheses.

Clustered standard errors at exporter-importer level.

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

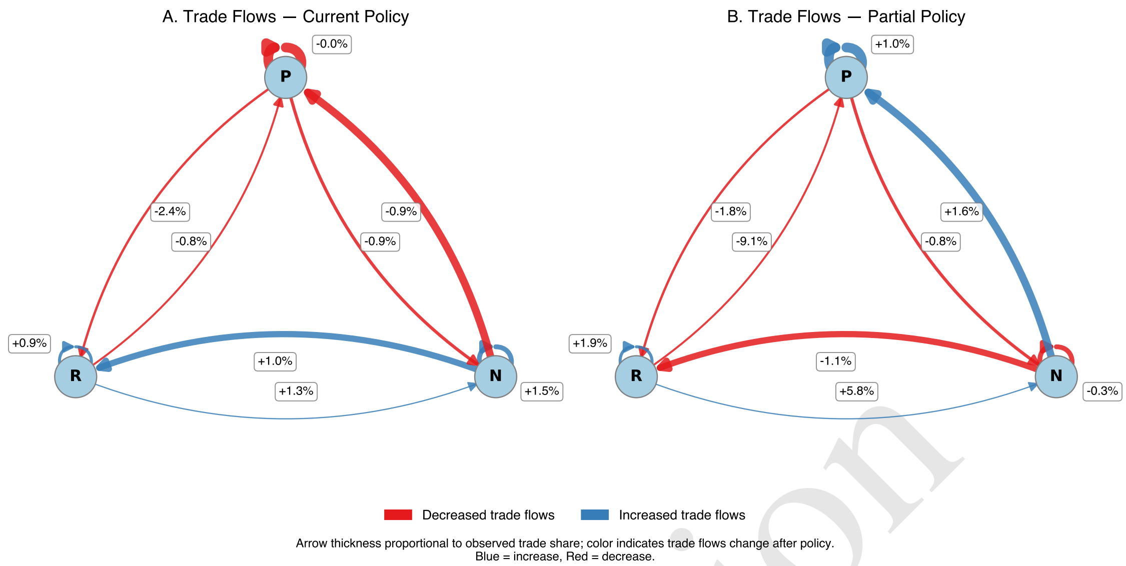

Figure A.5: Counterfactuals with Policy effect (β) = -0.2

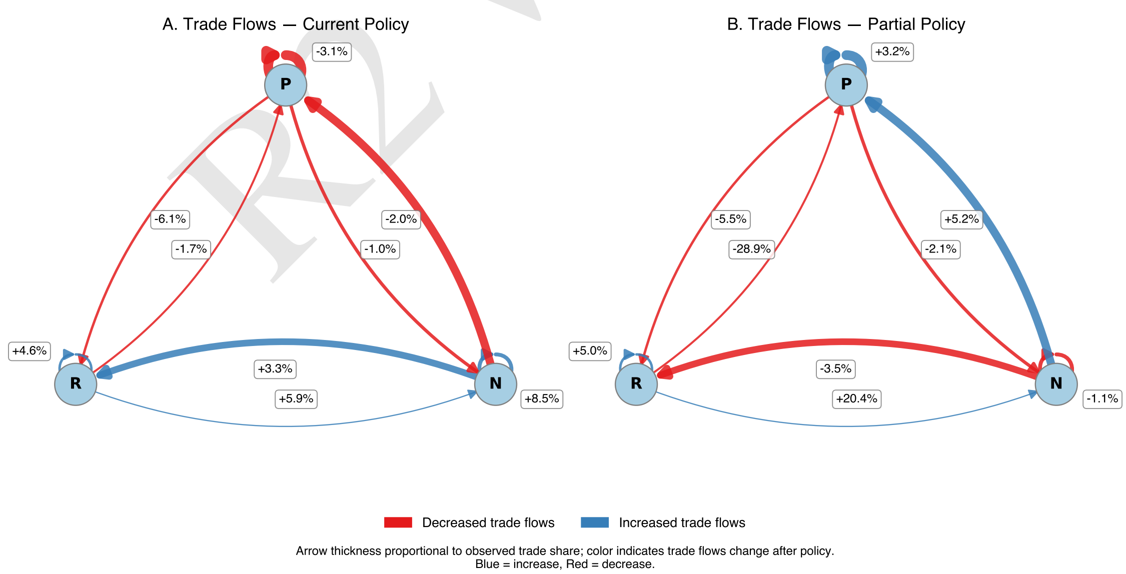

Figure A.6: Counterfactuals with elasticity of substitution (θ) = 6

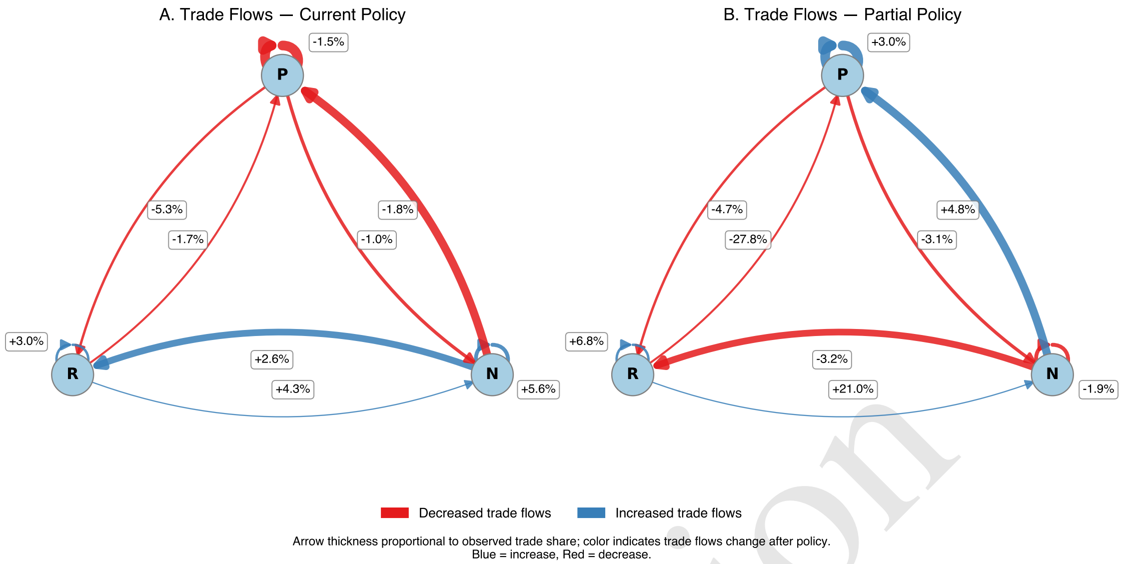

Figure A.7: Counterfactuals with elasticity of substitution (θ) = 10

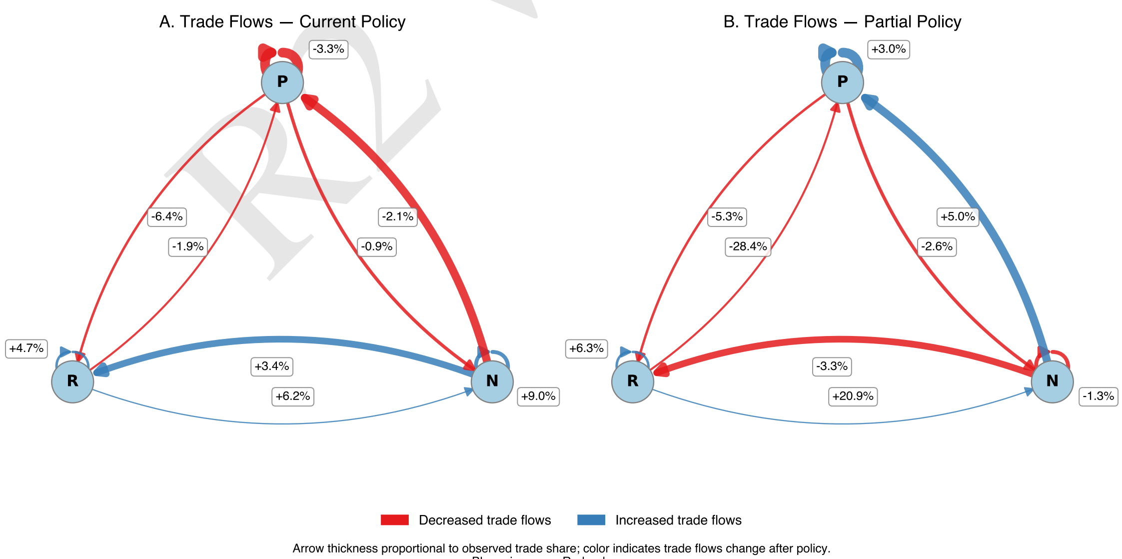

Figure A.8: Counterfactuals with supply elasticity (ψ) = 1