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Watching for Rerouting: Evaluating Vietnam's Trade Remedy Early Warning System

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Research Article

Keywords: Anti-circumvention policy, US–China trade war, Transshipment, Vietnam, Trade Remedy

Posted Date: November 17th, 2026

DOI: https://doi.org/10.21203/rs.3.rs-10983861/v1

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Additional Declarations: No competing interests reported.

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Abstract

The United States' 2018 tariff escalation against China created strong incentives for Chinese exporters to reroute goods through other countries to mitigate resulting tariff burden. Vietnam has since attracted growing scholarly attention as one of the key conduits for Chinese-origin goods tariff circumvention, which may draw heightened scrutiny from the United States and thus trigger country-wide antidumping investigations, whereby evidence of circumvention could expose Vietnamese exporters to punitive trade measures. Since 2019, Vietnam's Ministry of Industry and Trade has maintained an early-warning system, listing products judged to carry elevated circumvention risk. Listed products face enhanced customs scrutiny and stricter origin-certification requirements according to the Decision. This paper provides the first causal evaluation of this instrument, using a monthly Harmonized System 6-digit level panel of Vietnam's exports to the United States and imports from China spanning January 2018 to September 2025. A central identification concern is that authorities may preferentially flag products subject to larger increases in United States' tariffs against China. We correct for this endogeneity by limiting estimation to those products for which the underlying tariff exposure is above an objective threshold for inclusion, and estimate a Poisson pseudo-maximum-likelihood specification on this restricted sample. We do not detect a clear effect of Trade Remedy early-warning system on either the export or import margin.

Keyword Anti-circumvention policy · US–China trade war · Transshipment · Vietnam · Trade Remedy

JEL Classification codes F13 · F14 · F51 · C23

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1 Introduction

The US–China trade war, initiated by the Trump administration's imposition of tariffs on Chinese imports in 2018, fundamentally reshaped bilateral and global trade flows at a scale Fajgelbaum and Khandelwal (2022) describe as the most significant unilateral trade policy shock in the post Cold war era. United States' tariff increases in 2018 alone led to a persistent 14.4% contraction in China's market share for Made in China 2025 (MIC 2025) in the United States (Xu, Mao and Zhang 2026). As the conflict escalated through successive rounds of punitive tariffs, patterns of global trade were further reshaped, creating strong incentives for both legitimate trade diversion and illicit trade circumvention via third countries (Iyoha et al. 2024). In this context, bystander economies such as Vietnam, Thailand, South Korea, and Mexico emerged as key beneficiaries, recording average export growth of approximately 6.5% in affected sectors by capturing displaced demand in segments where United States and China trade had contracted (Fajgelbaum et al. 2024). Vietnam stands out among them. Over 2017–2022, the country absorbed nearly half of the United States' import market share that China lost (Alfaro and Chor 2023). Crucially, Varma and Toh (2025) argue that these gains are not fully attributable to genuine supply chain diversification. After adjusting for circumvention through third countries in Southeast Asia and Mexico, China's direct and indirect exports to the United States comprised 18.5% of its total exports in 2023, down only modestly from 20.6% in 2017, significantly higher than the decline indicated by direct export data alone, and thus suggesting the existence of hidden trade.

Within the ASEAN region, Vietnam has emerged as arguably the most notable case study of this transshipment phenomenon. This structural shift is reflected in recent enforcement efforts and related policy statements. Recently, the White House Office of Trade and Manufacturing Policy report titled "The Great Transshipment Scam" (2026) referred to a Commerce Department analysis which identified Vietnam, Mexico, and India as the principal transshipment hubs. The report estimates that these jurisdictions collectively accounted for approximately $67 billion in rerouted goods and $28 billion in foregone United States' tariff revenue over the preceding year, though these figures and their underlying methodologies have been the subject of public criticism, including rebuttals by the governments targeted in the report. Freund (2025) constructed a "China Wash" index to capture synchronized shifts, including a structural decline in China's American market share paired and a corresponding rise in both Chinese exports to third countries and those third countries' market shares in the United States within identical product categories. Applied across fifteen identified transit economies, this framework confirms Vietnam as one of the primary conduits for trade rerouting, concentrated heavily in electronics, furniture, solar panels, and steel products.

Vietnam's exposure to this risk is not evenly shared with its regional peers. The country combines unusually heavy trade dependence on the United States, together with this market's enforcement regime in which a single finding can expose an entire product category to duties. This vulnerability has surfaced repeatedly over the past decade, as United States' authorities have found goods routed through Vietnam to be circumventing existing trade remedies, thus extending liability to compliant exporters alongside the firms actually responsible. Vietnamese authorities have themselves acknowledged this vulnerability as a policy concern. Since 2019, Vietnam has strengthened monitoring, cross-border data reconciliation, and customs

enforcement to reduce the risk of its domestic economy being used as a transit point for Chinese goods destined for the United States (Rotunno et al. 2024). The institutional backbone of this effort is Decision No. 824/QĐ-TTg, signed by the Prime Minister on 4 July 2019, which established a multi-agency framework for combating trade-remedy circumvention and origin fraud. Central to this decision, the Ministry of Industry and Trade's Trade Remedies Authority (TRAV) compiles and updates an early-warning list of Vietnamese export products judged to be suspected of of being used to circumvent United States' trade remedies on Chinese goods and therefore carry heightened risk of triggering United States' anti-circumvention scrutiny. Listed products would face closer customs verification and stricter audit requirements before receiving Certificates of Origin (C/O). Whether this measure has actually curbed the circumventing risk it targets, however, remains an open empirical question.

Despite the richness of available monthly bilateral trade data, empirical research on the effectiveness of Vietnam's Trade Remedy early-warning system remains limited. While a growing literature has examined the trade diversion effects of the US–China trade war on intermediary-country economies (Agarwal, Barattieri and Mattoo 2026; Choi and Nguyen 2023; Hayakawa 2024; Hayakawa and Keola 2025; Lim and Nguyen 2024; Schulze and Xin 2025; Yanuar et al. 2026), and notwithstanding well-documented prevalence of trade circumvention behaviour (Hayakawa 2022; Iyoha et al. 2024; Lee, Nguyen and Rhee 2025), remarkably little empirical attention has been devoted whether anti-circumvention policies implemented by non-sanctioned countries are effective in mitigating such practices. This gap is non-trivial. If early-warning list mechanisms effectively reduce circumvention-driven trade flows, they offer a replicable policy model for other developing economies exposed to rerouting pressures. If not, they risk imposing compliance costs on legitimate exporters without generating meaningful deterrence.

This paper seeks to fill this gap by evaluating the causal effect of placing a product on Vietnam's Trade Defence early-warning list on the trade flows it is meant to influence. This study employs a Poisson Pseudo-Maximum-Likelihood specification with product and time fixed effects (a difference-in-differences style comparison of treated and untreated products over time) on a monthly, product-level panel spanning January 2018 to September 2025. The early-warning list's effect is estimated separately on both margins: products' export to the tariff-imposing market and import from the sanctioned exporter, following Hayakawa's (2022) diagnostic for distinguishing genuine trade diversion from transshipment. Since Vietnamese authorities may be more likely to list products subject to the United States' tariff hike against Chinese goods, listing status may be systematically related to tariff exposure, which could itself also affect trade outcomes directly. To address, our estimation sample is restricted to products whose exposure to the underlying tariff shock meets an objective threshold for inclusion.

This paper's findings contribute to a growing literature on the effectiveness of transit-country enforcement against tariff-driven circumvention (e.g., Hayakawa and Sudsawasd 2025; Yang 2008a; Javorcik and Narciso 2017; Chalendard et al. 2020, 2023). A product's placement on the Trade Defence early-warning list shows no association with either trade margin in our baseline specifications, a result stable across estimators, sample constructions, treatment-timing specifications, and is precise enough to rule out proportional effects larger than roughly 45 percent on the export margin and 25 percent on the import margin. This conclusion rests on different footing across the two margins: it follows directly from the

identifying assumption for exports, but only conditionally on the Rambachan and Roth (2023) sensitivity analysis for imports, where parallel trend is not satisfied outright. This null result holds unchanged once we account for the negative weighting that staggered treatment timing can introduce into two-way fixed effects. For policymakers, this cautions against treating the existence of a Trade Defence early-warning system as evidence of deterrence, while leaving open, a possibility we return to in the conclusion, that such instruments serve an evidentiary rather than a deterrent function under United States' trade-remedy law.

2 Context and Literature Review

2.1 Context

Vietnam faces substantial exposure to the risks associated with trade circumvention. In response to the growing use of third countries as transshipment hubs for Chinese exports (a practice commonly referred to as a transshipment export strategy), the United States has progressively strengthened its trade-remedy framework through measures such as anti-circumvention investigations and the application of the Third-Country Particular Market Situation (PMS) (Jiang and Xing, 2025). These mechanisms enable United States' authorities to impose trade restrictions or adjust duty calculations when production conditions in an intermediary country are deemed to be distorted by external influences, such as the inflow of underpriced Chinese inputs. Within this context, Vietnam occupies a particularly vulnerable position. Antràs and Presbitero (2026) construct a Rerouting Potential Index from global trade-flow patterns and rank Vietnam ninth among all economies worldwide in terms of exposure to trade rerouting risks. Ferrier (2021), studying an earlier and independent episode of circumvention finds that countries with higher corruption scores were systematically more likely to exhibit the suspicious trade patterns associated with origin fraud in the years following tariff imposition. Vietnam's own corruption score of 3.00, on the five-point scale used in that literature, places it toward the higher-risk end of this distribution. These factors position Vietnam among the economies most susceptible to United States' origin-verification inquiries and anti-circumvention enforcement actions. The situation is more compounded by how American trade remedy law is structured. The country's enforcement determinations are made at the country level, not the firm level. Since 2002, Vietnam has been classified as a non-market economy for antidumping purposes–a designation the Department of Commerce reaffirmed in August 2024 after formal review (International Trade Administration 2024a), and Commerce's default practice under this classification is to apply a single, country-wide antidumping rate to all Vietnamese exporters, unless an individual firm can demonstrate sufficient independence from state control to earn a separate rate of its own (International Trade Administration 2005). The result is a form of collective liability categorically different from ordinary trade-policy risk. Circumvention by a subset of firms can generate duty exposure for compliant exporters in the same sector who had no role in the underlying misconduct.

This concern is not merely theoretical. Over the past decade, Vietnam has faced repeated affirmative circumvention determinations, followed by extensions of the applicable duties. Between 2018 and 2019, the United States Department of Commerce issued a series of country-wide affirmative determinations that corrosion-resistant and cold-rolled steel products

completed in Vietnam using Chinese, Korean, or Taiwanese steel substrate were circumventing existing antidumping (AD) and countervailing (CVD) duty orders (International Trade Administration 2018a, 2018b, 2019). In early 2024, Commerce reached a similar determination for collated steel staples exported from Vietnam using Chinese-made wire and wire bands, setting a country-wide antidumping cash-deposit rate of 112.01% and a countervailing rate of 12.32% for any exporter without its own individually established rate (International Trade Administration 2024b). In August 2023, United States department of Commerce issued a country-wide affirmative determination that crystalline silicon photovoltaic cells and modules completed in Cambodia, Malaysia, Thailand, and Vietnam using Chinese-made components were circumventing existing antidumping and countervailing duty orders on Chinese solar products, triggered the extension of AD/CVD duties exceeding 238.95% (antidumping) plus 15.24% (countervailing). Among the exporters examined individually, only three were found not to have circumvented the orders and were therefore granted separate exemptions. One of these was the Vietnamese producer Boviet Solar (International Trade Administration, 2023). Every other Vietnamese solar exporter, whatever its own conduct, became liable for duties triggered by others.

One key issue, as highlighted by Iyoha et al. (2024) is that rerouting activity is highly concentrated among Chinese-owned firms and newly established enterprises, with Chinese and Hong Kong-invested firms accounting for more than half of all rerouting activity and post-2018 entrants contributing approximately 92% of the observed increase in firm-level rerouting, rather than incumbent Vietnamese producers. The estimated behavioural response of Chinese-owned firms (0.0459) is more than double that of domestically owned Vietnamese firms (0.0219), implying that country-wide tariff extensions impose the greatest costs on precisely those domestic producers least responsible for the circumvention behaviour that triggered them. This vulnerability is amplified by Vietnam's exceptionally high dependence on the United States' market. Among the ASEAN-5 economies, Vietnam records the highest export concentration toward the United States, with approximately 35.1% of its total exports destined for the United States' market. This level of dependence substantially exceeds that of the Philippines (19.9%), Thailand (22.0%), Malaysia (16.3%), and Indonesia (11.2%), rendering Vietnam disproportionately exposed to any American trade-remedy actions directed at Vietnamese exports (Mercado-Aldaba 2025). Jiang and Xing (2025) also points out that United States' trade measures have generated exceptionally large fluctuations in Vietnam's decoupling indices relative to China, with values reaching as high as 400. Such pronounced volatility reflects instability in Vietnam's economic structure as it participates in global value chains. This also provides evidence that Vietnam faces substantial risks associated with its role as a potential transshipment platform, where economic stability may be directly threatened by the expansion of American trade sanctions and enforcement measures aimed at preventing Chinese goods from entering the United States' market through shadow channels.

At the domestic policy level, Vietnam's institutional framework for countering trade circumvention was formally established under Decision No. 824/QĐ-TTg, signed by the Prime Minister on 4 July 2019, titled "Strengthening State Management of Anti-Circumvention of Trade Remedies and Origin Fraud." This landmark directive provided the legal architecture for a multi-agency oversight mechanism designed to safeguard domestic manufacturing integrity while mitigating external tariff-evasion risks. Central to this institutional arrangement is the

TRAV under the Ministry of Industry and Trade (MOIT). TRAV constructs and updates an early-warning list of Vietnamese export products suspected of being associated with the circumvention of United States' trade remedies on Chinese-origin goods, therefore carrying heightened risks of triggering trade remedy, origin fraud, or anti-circumvention investigations. Products flagged on this alert list are subjected to intensified, multi-tiered surveillance, encompassing mandatory physical verification by Vietnam Customs prior to export clearance, as well as audit protocols by authorized agencies, such as the Vietnam Chamber of Commerce and Industry (VCCI) before issuing non-preferential and preferential C/O. Decision 824 represents one of the most institutionally elaborate unilateral policy responses to trade circumvention risk among ASEAN member states, and it remains fully operative and integral to Vietnam's trade strategy today.

The monitoring list concentrates principally on commodity categories within Harmonized System (HS) Chapters 25, 40, 44, 68, 69, 72, 73, 76, 84, 85, 87, and 94, covering a broad spectrum from raw materials like timber, stone, and base metals to high-value manufactured items including furniture, electronics, and transportation equipment. From a baseline of 34 subheadings at its July 2019 launch, the monitored portfolio broadened considerably, exceeding 60 subheadings between late 2023 and early 2024. Subsequent revisions narrowed this focus, dropping less sensitive items to leave 24 high-priority codes under active review by mid-2026.

2.2 Literature Review

Fisman and Wei (2004) were among the first to systematically examine tariff evasion as a predictable economic response rather than an isolated anomaly. Their landmark contribution in the Journal of Political Economy established that the discrepancy between an exporter's reported trade values and the corresponding import figures recorded by the receiving country responds systematically to the tariff schedule itself. This discrepancy is known as the evasion gap. Using bilateral Hong Kong–China data, Fisman and Wei (2004) showed that a one-percentage-point increase in the tariff rate generated approximately a three-percent increase in this gap. The mechanism operated through both under-invoicing and misclassification into lower-tariffed product codes. This finding has since been replicated across a range of institutional settings, e.g., a global analysis of trade flows between the world's five largest importers and nearly 200 partners (Berger and Nitsch 2011), India (Mishra, Subramanian and Topalova 2008), and US–China trade (Ferrantino, Liu and Wang 2012).

Transshipment-based circumvention differs fundamentally from this under-invoicing mechanism, and requires a different empirical approach to detect. Under-invoicing leaves a visible trace because the exporter and importer are reporting the same bilateral relationship with only the declared value differing between the two sides. Transshipment breaks this correspondence entirely. Once goods are relabelled as originating in a third country, that country's own export declaration to the United States can match the corresponding United States' import record almost perfectly, even though the underlying goods are Chinese. The evasion therefore remains difficult to detect through bilateral trade discrepancies, as the reporting gap that would normally signal under-invoicing may no longer appear. Detecting transshipment therefore requires different empirical strategies. An early literature does so by

looking for trade volumes that outstrip what a transit country's own productive capacity could plausibly generate. Rotunno, Vézina and Wang (2013) show that Chinese textile firms used several sub-Saharan African economies as staging platforms to circumvent Multifibre Agreement quotas, producing apparel export surges from those economies far larger than could be explained by their domestic manufacturing capacity. Liu and Shi (2019) apply a closely related logic to a more directly comparable setting. They show that United States' antidumping duties on Chinese products are systematically followed by rising American imports from third countries in the very same product categories, a pattern more consistent with goods being rerouted through those countries than with genuine new production being established there.

In the context of Trade war between United States and China, transshipment-based circumvention is defined by Benguria et al. (2022) as the use of third-country jurisdictions not subject to United States' tariffs as transshipment platforms, whereby Chinese-origin goods are re-labelled and issued new C/O before being exported to the final destination market without incurring American duties. This mechanism closely resembles the antidumping-evasion strategies documented by Liu and Shi (2019) and Ferrier (2021), in which Chinese exporters strategically re-route shipments through third countries and exploit weaknesses in rules-of-origin enforcement to avoid trade remedies imposed by importing authorities. A rapidly growing literature provides compelling empirical evidence of tariff-induced trade rerouting via third-country conduits. Employing national-level trade flows, Iyoha et al. (2024) quantify that trade deflection accounted for 21.1% of the $52.8 billion increase in third-country exports to the United States between 2018 and 2021. In another investigation, Meltzer and Barron Esper (2025) document how Chinese exporters have leveraged Mexico as a strategic gateway into the United States' market to evade direct tariff exposure. Crucially, their findings remain robust after accounting for price fluctuations, effectively ruling out the possibility that these observed patterns are merely valuation artifacts.

Hayakawa (2022) developed a diagnostic closely related to the empirical design of the present paper. Genuine diversion occurs when a third country's own firms expand output to meet demand redirected from the sanctioned exporter. This carries no particular implication for that country's imports of the same product from the sanctioned exporter itself. Transshipment is different. The goods must first enter the transit country as an import before being re-exported with little or no further processing, so a transshipment-driven rise in exports should be accompanied by a corresponding rise in imports from the same sanctioned country– a joint movement genuine diversion does not predict. Testing this two-margin logic across ASEAN economies, Hayakawa (2022) finds that Vietnam and Cambodia are conspicuous outliers. Both exhibit significant increases along both margins simultaneously, a pattern considerably harder to reconcile with pure diversion alone and is more consistent with the presence of transshipment. This two-margin logic forms the basis of the present paper's empirical design, which operationalizes Hayakawa's (2022) diagnostic directly by estimating the effect of inclusion on the early-warning list separately for Vietnam's exports to the United States and imports from China, rather than relying on either margin alone.

The effectiveness of formal enforcement instruments against evasive trade is a genuinely contested question in the literature. The present paper's findings must be read against a body of evidence that points in two directions at once. On one side, well-designed monitoring and verification mechanisms measurably curb evasive behavior. Yang (2008a) offers the clearest

cross-country demonstration by examining pre-shipment inspection programs, under which nineteen developing countries hired independent firms to verify the value and tariff classification of incoming shipments before they reached customs. Adoption of such programs generated large and sustained increases in duty collection, averaging roughly 2.6 times the programs' operating costs over their first five years. The gains were driven by measurable declines in under-invoicing and misclassification once an independent third party, rather than customs officials alone, verified shipment documentation. Chalendard et al. (2020) maps closely onto Vietnamese Trade Defence early-warning system, examining what happens when specific declarations are flagged as high-risk and given closer scrutiny. Working with Madagascar's port customs administration, the authors first address the non-random assignment of third-party valuation services with an instrumental-variables strategy. Flagged declarations receiving such advice are 21.7 percentage points more likely to have fraud detected, and yield 5.2 percentage points more tax revenue. The authors then use a randomized controlled trial to examine the effect of information alone. Among high-risk declarations that do not receive valuation advice, those randomly assigned detailed risk comments from customs' internal Risk Management Unit still show a 3.1-percentage-point rise in fraud detection and a 1-percentage-point rise in tax yield. This latter finding is particularly relevant to the present study. It suggests that flagging a case as high-risk and informing the relevant official can shift behavior on its own, even without changes in inspection technology, closely related to the mechanism underlying an early-warning list.

On the other side, a body of evidence counsels more caution. Some of it comes from the same research team, studying the same Madagascar port. This evidence centers on what the enforcement literature terms the displacement, or balloon effect. When an intervention makes one form of evasion more costly without removing the underlying incentive to evade, firms may simply shift their activities to other channels that are not covered by the intervention or to strategies that regulators did not anticipate. In this case, the intervention may change how evasion occurs without necessarily reducing the underlying problem. The clearest illustration comes from Chalendard et al. (2023), who return to the same Madagascar port to investigate a structurally different reform: the delegation of inspector-assignment decisions to a third party. The goal was to eliminate the non-random pairing of specific brokers with specific inspectors, which had previously enabled collusive under-assessment. Using deviations from the officially mandated random-assignment rule to detect manipulation, the authors confirm that the reform initially succeeded in disrupting the collusive pairings it targeted. However, it also triggered a new form of manipulationthat the reform had not anticipated, allowing corruption to persist through a different channel. Aggregate revenue losses from the underlying scheme were left largely intact. Javorcik and Narciso (2017) document a structurally identical pattern in a different setting. They study fifteen countries that joined the World Trade Organization (WTO) between 1996 and 2008 and were consequently required, under Article VII of the GATT's Customs Valuation Agreement, to replace customs officials' discretionary use of minimum or reference prices with mandatory acceptance of the exporter's own invoice value. Under-invoicing through the channel targeted by the reform almost disappeared. However, the overall evasion gap across all channels remained statistically unchanged, as firms shifted to other methods of misreporting that were not covered by the reform. An analogous caution emerges from the same author and the same empirical setting examined on the positive side above. In a

companion study of the identical Philippine pre-shipment inspection episode, Yang (2008b) shows that tightening inspection for imports from a targeted subset of trading partners induced systematic switching toward alternative duty-avoidance methods, specifically shipping via duty-exempt export processing zones. This contrasts with the aggregate revenue gains reported in his companion paper.

This tension carries a direct interpretive implication, and it holds regardless of which way this paper's results ultimately turn out. It is worth stating explicitly, in both directions, before turning to the results themselves. Consider first a null result: no detectable effect of a product's inclusion in Trade Remedy early-warning list on the trade flows of listed products. This would not, on the strength of the enforcement-effectiveness literature alone, provide evidence that circumvention through Vietnam was quantitatively negligible to begin with. It would be equally consistent with a less reassuring possibility. Exporters facing heightened scrutiny in listed categories may simply have redirected activity toward adjacent, unlisted product codes– the same adaptive response Javorcik and Narciso's (2017) exporters showed once the reference-price loophole underlying their evasion was closed. Consider the opposite possibility: a detectable reduction in trade flows among listed products. This would not, on its own, settle the question of the early-warning list's effectiveness either. Yang's (2008a, 2008b) two studies of the identical Philippine inspection episode make the point directly. Aggregate effectiveness and channel-level displacement are not mutually exclusive outcomes of the same policy; the two can coexist within it. A decline in the targeted margin, therefore, does not rule out the possibility that evasion has simply shifted to a margin that this study cannot observe.

Despite growing evidence on both the incentive for circumvention through Vietnam and its empirical prevalence, direct causal evaluation of the policy responses remains remarkably sparse. Existing institutional evidence mostly documents the consequences of enforcement action taken by the importing country, while being largely silent on whether a transit country's own, self-administered policy changes exporter behavior in a way detectable in trade data. The single closest comparator is Hayakawa and Sudsawasd (2025), who found no significant effect of Thailand's Ministry of Commerce watchlist established in November 2019, which, like Vietnam's own mechanism, requires additional origin documentation for products identified as high-risk for American-bound circumvention. To our knowledge, this paper provides the second causal evaluation anywhere in the literature of a transit-country-administered anti-circumvention early-warning list in the context of the US–China Trade War, and the first for Vietnam specifically. That gap is worth dwelling on. Vietnam is the economy the broader circumvention literature identifies as the most extensively exposed, and most extensively rerouted-through country in the entire US–China trade conflict (Hayakawa 2022; Alfaro and Chor 2023; Iyoha et al. 2024), not to mention Vietnamese Trade Defence early-warning system is, as discussed, among the most institutionally developed unilateral responses to circumvention risk in the region.

3 Empirical Framework

3.1 Empirical Strategy

3.1.1 Empirical Design

Our empirical strategy is built around two questions. The first concerns the destination market: does placing a product on Vietnam's Trade Remedy early-warning list reduce subsequent export of that product to the United States. The second concerns the input side. Here we examine whether inclusion on Vietnam's Trade Remedy early-warning list leads to a decline in import of the same product from China into Vietnam. These are the margins along which rerouting would have to occur if Chinese goods were being transshipped through Vietnam.

We evaluate the trade effects of the early-warning system using a Poisson Pseudo-Maximum-Likelihood (PPML) estimator following Silva and Tenreyro (2006), with product and calendar-month fixed effects. Two features of the data make this specification particularly appropriate. First, a disaggregated HS6-by-month panel of bilateral trade flows contains a substantial mass of exact zeros (product-months in which no trade occurs). A log-linearized model cannot handle these observations without discarding them or introducing an arbitrary additive constant, either of which biases the resulting estimates. Second, and more fundamentally, trade values are heteroskedastic by nature. Once the errors in a multiplicative gravity relationship are heteroskedastic, Ordinary Least Squares (OLS) on the logged version, by Jensen's inequality, no longer recover consistent estimates of the underlying levels relationship. PPML avoids these problems under much weaker assumptions, which is why it has become the default in the recent trade literature.

We specify two structurally parallel equations, corresponding to the destination and input margins introduced above:

$$Export_{pt} = \exp(\theta^X D_{pt} + X'_{pt}\,\beta^X + u_p + u_t)\,.\,\varepsilon_{pt} \quad (1)$$
$$Import_{pt} = \exp(\theta^M D_{pt} + X'_{pt}\,\beta^M + u_p + u_t)\,.\,\varepsilon_{pt} \quad (2)$$

where $Export_{pt}$ and $Import_{pt}$ denote the value of Vietnam's exports to the United States and imports from China, respectively, of product p in month t; θ^X and θ^M represent the coefficients of central interest throughout this paper, capturing the association between early-warning list status, D_{pt} (our treatment indicator; Section 3.2), and each outcome. Product fixed effects u_p absorb any time-invariant determinant of a product's trade pattern. Calendar-month fixed effects u_t absorb any shock common to all products in a given month. X_{pt} (Section 3.4) further nets out a set of time-varying, product-specific confounds. We cluster standard errors at the product level throughout, allowing for arbitrary serial correlation within a given HS6 product's trade history over nearly eight years of monthly observations. Interpreting θ^X and θ^M requires the two-margin logic set out by Hayakawa (2022). If listing on TRAV's system deters a product's transshipment, both coefficients should be negative. Circumvention inflates exports and imports jointly, so suppressing it should suppress both margins together, not one in isolation.

Equations (1) and (2) are not written in the canonical two-period difference-in-differences notation, but their structure (two way fixed effects plus a treatment indicator that varies across both products and calendar time) puts our design squarely within the generalized difference-in-differences framework now standard in trade-policy evaluation (Goodman-Bacon 2021; Callaway and Sant'Anna 2021). The PPML estimator governs how the conditional mean of trade values is parameterized; it does not alter the underlying comparison–treated products

relative to untreated products, before and after listing–from which θ derives its causal interpretation. Consequently, the interpretation of θ rests on the same identifying assumption that underlies difference-in-differences more broadly, that absent inclusion in the early-warning list, treated and untreated products would have followed parallel trajectories in the outcome of interest. Section 4 examines this assumption in detail and assesses how sensitive our conclusions are to its violation.

3.1.2 Data

Bilateral trade values underlying both equations are drawn from the International Trade Centre's Trade Map database. Our estimation sample is a monthly panel, spanning ninety-three months from January 2018 through September 2025, a considerably longer window than the four-to-six-year horizons typical of earlier trade-war studies. One consequence of this longer horizon is that the panel straddles the 2022 switch from HS 2017 to HS 2022, which reclassified a nontrivial number of product codes. To keep the classification consistent over time, we map all trade values back to their HS 2017 equivalents using the UN Statistics Division's concordance tables[1]. Information of the product on the early-warning lists is obtained from the Trade Remedies Authority of Vietnam. TRAV publishes listed products mostly at the 6-digit HS level and occasionally at the 8-digit level. Where the early-warning list notice specifies a code finer than six digits, we aggregate the trade data to the HS6 level (based on the HS 2017 version). This is because the study period spans two revisions, HS 2017 and HS 2022, while a compatible concordance table between the two versions at the 8-digit level is absent. The resulting dataset combines monthly HS6 products bilateral trade flows with product-level exposure to Trade Remedy early-warning.

3.2 Staggered Treatment

Vietnam's Trade Remedy early-warning system generates treatment that is staggered (distinct products enter the list at distinct calendar dates) and, in contrast to some comparable regimes, not strictly absorbing, since products may subsequently be removed upon review. This combination raises a concern that is now well documented in the difference-in-differences literature. When treatment is coded to revert to zero upon removal, two-way fixed-effects estimators can end up drawing part of their identifying variation from comparisons between currently treated units and units that were treated in the past but are coded as untreated today (Goodman-Bacon 2021; De Chaisemartin and D'Haultfœuille 2020; Sun and Abraham 2021). Such comparisons break down whenever treatment has any effect that outlasts the formal listing period–say, through a lasting shift in customs practice, buyer perception, or firm-level compliance behavior. In that case, previously treated units are no longer a valid counterfactual. We therefore adopt, as our preferred specification, an absorbing "once listed, always listed" coding of D_{pt}, under which treatment status is set to one from a product's first record on the early-warning list date onward, irrespective of subsequent delisting. We do not, however, treat

[1] This procedure follows standard practice for reconciling classification breaks in long-horizon trade panels; see Pierce and Schott (2009) for a detailed treatment of the general concording problem across successive Harmonized System revisions.

this as a purely precautionary convention. In Section 5, we test its consequences directly. We split D_{pt} into two indicators: current listing status, and previously listed but since delisted, and re-estimate equations (1) and (2) with both entered together. We later find that neither indicator differs meaningfully from zero, nor from the other, in either equation. Our conclusions, in other words, do not depend on this coding choice. We report the absorbing specification as our baseline on identification grounds, while presenting the decomposition as direct evidence that this choice is not driving the results we obtain.

A related but distinct concern is that products enter Vietnam's early-warning list at different calendar dates, and even under the absorbing specification adopted here, standard two-way fixed-effects estimation can implicitly draw part of its identifying variation from comparisons between already-treated and not-yet-treated cohorts. If the effect of listing differs across entry cohorts, some of these implicit comparisons can receive negative weight in the resulting estimate of θ, potentially biasing it even when the parallel-trends assumption holds exactly (Goodman-Bacon 2021; Sun and Abraham 2021). We address it directly using the interaction-weighted estimator of Sun and Abraham (2021), extended to the Poisson setting following Wooldridge (2023), which estimates cohort-specific treatment effects separately and aggregates them using weights that are, by construction, non-negative. We report the results alongside our other identification checks in Section 4.

3.3 Identification Strategy

Vietnam's early-warning system is a discretionary policy instrument, which raises two distinct concerns for the causal interpretation of θ. We address them in turn.

3.3.1 A Restricted Comparison Sample

The primary identification challenge arises from the possibility TRAV's selection may itself track United States' tariff policy. Authorities can plausibly reason that products subject to a larger increase in tariffs against China face a stronger incentive to be rerouted through Vietnam, and list accordingly. This same tariff shock independently affects products' two-margin trade outcomes through the diversion channel described above. In that case, a naive comparison between listed and unlisted products that does not account for this would conflate the causal effect of listing with this compositional difference.

We deal with this using the selection-on-observables approach from Hayakawa and Sudsawasd (2025), who evaluate Thailand's analogous regime, applying the same construction to our data [2]. Instead of comparing listed products to the full universe of unlisted ones, we restrict the sample to products that are objectively exposed to trade-war rerouting incentives. For each product p, we compute a tariff margin,

$$Margin_p = \max^{t}_{(\tau_{pt})} - \min^{t}_{(\tau_{pt})}$$

[2] We initially explored an instrumental-variables approach to this problem, drawing on a shift-share construction based on US Section 301 tariff exposure, but it did not survive formal scrutiny. The tariff-based instrument violates the exclusion restriction through a direct trade-diversion channel unrelated to Vietnam's own early-warning list decision.

where τ_{pt} denotes the applied United States' tariff rate on Chinese-origin exports of product p in month t [3]. Both episodes remained targeted at specific product categories. We define our restricted estimation sample as the set of products for whose baseline margin exceeds the sample-average margin across all products ($Margin_p > \overline{Margin_p}$). By construction, every product being compared, whether listed or not, already carries substantial tariff exposure. Therefore, the variable TRAV is hypothesized to select on might not vary systematically between the two groups, but rather be uniformly high throughout the comparison. What distinguishes listed from unlisted products in the restricted sample is therefore not whether they face a meaningful tariff shock, but some finer margin of TRAV's discretion within an already high-exposure pool, a narrower and more defensible source of variation than the one a full-sample comparison would rest on.

Data on τ_{pt} are compiled from the Global Tariff Database United States Trade War Extension (Rodríguez-Clare et al. 2025), which reports national tariff-line and HS6-level (HS 2017) applied tariffs on a monthly basis and builds on the tariff-imputation methodology developed in Teti (2024). We assess the sensitivity of our results to this threshold by re-estimating equations (1)–(2) under alternative multipliers of the sample-average margin: 0.75×, 1.0×, and 1.25× in Section 5.

3.3.2 Parallel-Trends Assumption

A causal reading of θ in equations (1) and (2) requires that, absent placement on the early-warning list, treated products' trade values would have evolved in parallel with those of untreated products. Formally, letting $y_{pt}(0)$ denote the potential outcome product p would exhibit at time t under no treatment, we require:

$$\mathrm{E}\big[y_{pt}(0) - y_{p,t-1}(0)\big|D_{pt} = 1\big] = \mathrm{E}\big[y_{pt}(0) - y_{p,t-1}(0)\big|D_{pt} = 0\big]$$

conditional on the fixed effects and controls specified in X_{pt}. This assumption is neither directly testable (since $y_{pt}(0)$ is, by definition, unobserved for treated products in the post-treatment period) nor plausible to simply presume in a setting where treatment assignment reflects the discretionary judgment of a regulatory authority. We therefore adopt a two-part strategy. First, we check the assumption's observable implication: pre-treatment trajectories shouldn't differ systematically between treated and untreated products. We re-estimate equations (1) and (2) with D_{pt} replaced by a full set of event-time indicators,

$$y_{pt} = \exp\left(\sum_{k=-7,k\#-1}^{7} \delta_k 1\big[t - g_p = k\big] + X'_{pt}\beta + u_p + u_t\right).\varepsilon_{pt}$$

[3] Because Margin is constructed as a within-product difference over time, any time-invariant component of τ, including pre-existing most-favored-nation duties or long-standing antidumping orders unrelated to the trade dispute, cancels out by construction, leaving $Margin_p$ to capture specifically the escalation attributable to trade-war-era tariff actions.

where g_p denotes product p's early-warning list entry date and the indicator for $k = -1$ is omitted as the reference category. Under parallel trends, the estimated $\{\delta_k\}_{k<0}$should be jointly indistinguishable from zero. Second, we plan ahead for the possibility that this test fails for one or both equations. We don't see that as an incidental threat so much as a real feature of evaluating discretionary regulatory listing. A failed pre-trend test isn't treated as fatal to the analysis, and we have a follow-up procedure ready either way. Where the joint pre-trend test rejects, we apply the partial-identification approach of Rambachan and Roth (2023), which doesn't require exact parallel trends, but instead asking how large a violation would need to be to overturn a given conclusion, benchmarked against the size of the violation actually observed pre-treatment. We report the results of both steps for each equation in Section 4.

3.4 Control Variables

We add several time-varying controls to X_{pt}, each aimed at a distinct confound that could otherwise get wrongly credited to the Trade Remedy early-warning list. We control for the rise in the United States' tariff on Chinese exports since the start of the sample, rather than the tariff level itself, so that only the escalation attributable to the trade dispute is captured, net of the pre-existing most-favored-nation baseline [4]. Also, in the export equation, we control for the log of aggregate United States' imports of product p from the rest of the world, net of the Vietnam bilateral flow, capturing evolving American demand conditions unrelated to Vietnam-specific developments. In the import equation, we include the symmetric control: the log of aggregate Chinese exports of product p to the rest of the world, net of the Vietnam bilateral flow, capturing China's global export capacity independent of the Vietnam corridor. Both series come from the International Trade Centre's Trade Map database. We further include the log real effective exchange rate (REER) for Vietnam in the export equation, for China in the import equation together with the untransformed level of the Global Supply Chain Pressure Index (GSCPI) [5]. REER indices for Vietnam and China are drawn from the Bruegel database of real effective exchange rates for 178 economies. GSCPI is published by the Federal Reserve Bank of New York. Both series vary only by calendar month, not by product, so they're mechanically absorbed by the time fixed effects, since u_t already nonparametrically soaks up any shock common to all products in a given month [6].

4 Empirical Results

This section presents our estimation results. Table 1 reports our baseline PPML estimates of equation (1), the export equation, while Table 2 reports the corresponding estimates of equation (2), the import equation. Each table presents three columns: the full product sample; the restricted sample constructed in Section 3.3.1; and, for comparability with conventional gravity-model practice, a log-linear OLS specification estimated on the restricted sample. All three include the full control vector and the two-way fixed effects described in Section 3.

[4] We verified that the resulting series is essentially zero for all products prior to July 2018, consistent with the implementation date of the first Section 301 tranche, supporting the validity of this baseline-differencing approach.
[5] The GSCPI is a standardized index admitting negative values by construction and is therefore entered in levels rather than logarithms.
[6] We retain these variables in the reported specification, notwithstanding their absorption, so that readers can directly verify that no residual product-invariant variation in exchange rates or supply-chain conditions remains unaccounted for.

Table 1 Baseline results: Export equation

	(I)	(II)	(III)
	PPML	PPML	OLS-log
	full sample	restricted sample	restricted sample
List (D_{pt})	−0.0660	−0.0118	−0.0875
	(0.1559)	(0.2069)	(0.2596)
TariffShock	−0.0047**	0.0019	0.0013
	(0.0021)	(0.0014)	(0.0013)
log US demand (excl. VN)	0.0478	0.0000	−0.0208
	(0.0331)	(0.0137)	(0.0171)
log Vietnam REER	absorbed	absorbed	absorbed
GSCPI	absorbed	absorbed	absorbed
Product FE	Yes	Yes	Yes
Time FE	Yes	Yes	Yes
Estimator	Poisson	Poisson	OLS
Products	5,423	4,178	4,245
Observations	504,339	388,554	394,785
Squared Cor.	0.663	0.705	0.866
Pseudo / adjusted R²	0.934	0.946	0.378

Notes: Estimates in columns (I)–(II) are obtained via PPML; column (III) applies OLS to $\log(1 + Export_{pt})$. The dependent variable is the value of Vietnam's exports to the United States. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively. Standard errors, clustered by HS6 product, are reported in parentheses. All columns include product and calendar-month fixed effects and the full control vector described in Section 3.4; $log\ Vietnam\ REER$ and $GSCPI$ are absorbed by the time fixed effects, as discussed in Section 3.4. Columns (II)–(III) restrict the sample to productswith a tariff margin above the sample average of 133.52 percentage points (Section 3.3.1). $Products$ reports the number of fixed effects retained after excluding singleton groups.

Applying the exposure criterion defined in Section 3.3.1 yields a restricted export-side sample of 4,245 HS6 products. After excluding products that enter as singletons or those exhibiting no within-product variation across the sample period and thus contributing no identifying information under fixed-effects estimation, 4,178 products remain, generating 388,554 product-period observations, of which sixty transition into treatment status at some point during the sample. Applying the identical procedure to the import side yields a restricted sample of 3,809 HS6 products, 3,765 of which are retained in estimation (350,145 observations), with the same cohort of sixty products entering treatment.

Consider the export equation. In our specification with the restricted sample, which limits the comparison to the HS6 products whose tariff-margin exposure exceeds the sample average (Table 1), the coefficient on early−warning list inclusion is a mere

−0.012, with a standard error of 0.207 that is nearly eighteen times the point estimate itself. Translated to a proportional effect, this corresponds to a 1.2% reduction in Vietnamese exports associated with placement on the early-warning list. That's not just statistically indistinguishable from zero; it's negligible by any economically meaningful standard. The estimate holds up remarkably well across specifications. It barely moves in the full, unrestricted sample (−0.066, or a 6.6% reduction, still nowhere near conventional significance), and it's similar again under log-linear OLS on the same restricted sample (−0.087, or 8.7%). What makes this estimate persuasive is that three specifications, built to fail in different ways, all arrive at essentially the same place. The full sample adds 1,245 products to our preferred one. By construction, these are exactly the lower-margin products the restricted sample was built to exclude. If this dilution meaningfully biased our estimate, the full-sample coefficient should diverge from the restricted one. Two samples that differ by over a thousand products, but both converge on a coefficient that never exceeds roughly 0.09 in magnitude, and never change sign. A genuine effect large enough to survive one of these choices but not the other would be hard to reconcile with this pattern. A null result robust to both is what we conclude. Similar to OLS design, the choice of estimator did not alter the conclusion.

The coefficient on United States' import from the world net of Vietnam is close to zero throughout all three specifications. This pattern has a straightforward interpretation once the control's composition is made explicit. The control aggregates United States' import flows from every third-country supplier of the corresponding product. Standard demand theory for products differentiated by place of origin decomposes any change in such an aggregate into two components, following Armington (1969). The first component is a genuine expansion in United States' demand for the product. This should move Vietnam's exports in the same direction as those of its competitors. The second component is a reallocation of market share toward or away from Vietnam's competitors. This should move Vietnam's exports in the opposite direction. If both components are of comparable magnitude within our restricted, high-exposure sample, they approximately offset, leaving little of this control's variation to explain the outcome on net, not because United States' demand conditions are irrelevant to Vietnam's exports, but because this particular aggregate conflates two channels that operate in opposite directions.

Table 2 Baseline results: Import equation

	(I) PPML full sample	(II) PPML restricted sample	(III) OLS-log restricted sample
List (D_{pt})	-0.1004 (0.0897)	0.0321 (0.1007)	-0.2267 (0.1825)
TariffShock	0.0006 (0.0008)	0.0012* (0.0006)	-0.0003 (0.0010)
log China supply (excl. VN)	0.5984*** (0.0397)	0.5588*** (0.0425)	0.3445*** (0.0202)
log China REER	absorbed	absorbed	absorbed
GSCPI	absorbed	absorbed	absorbed
Product FE	Yes	Yes	Yes
Time FE	Yes	Yes	Yes
Estimator	Poisson	Poisson	OLS
Products	4,821	3,765	3,809
Observations	448,353	350,145	354,237
Squared Cor.	0.873	0.879	0.852
Pseudo / adj. R^2	0.940	0.925	0.371

Notes: As in Table 1, but with the dependent variable redefined as the value of Vietnam's imports from China. Estimates in columns (I)–(II) are obtained via PPML; column (III) applies OLS to $\log(1 + Import_{pt})$. . ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively. Standard errors, clustered by HS6 product, are reported in parentheses. All columns include product and calendar-month fixed effects and the corresponding control vector; $log\ China\ REER$ and $GSCPI$ are absorbed by the time fixed effects. Columns (II)–(III) restrict the estimation sample to products whose tariff margin exceeds the sample-average margin of 140.06 percentage points (Section 3.3.1). $Products$ reports the number of fixed effects retained after excluding singleton groups.

The import equation likewise yields a null result in our preferred specification. In the restricted sample, the coefficient on early-warning list inclusion is small and statistically indistinguishable from zero (0.032, SE 0.101) (Table 2), corresponding to a proportional effect of more than 3%. The full-sample estimate (column I) yields a coefficient of slightly different sign and magnitude (−0.100, SE 0.090), and since it also falls well short of significance, it doesn't reject the null either, even though this specification lacks the objectively-exposed comparison group constructed in Section 3.3.1. The log-linear OLS is broadly consistent with this null reading (−0.227, SE 0.183).

The United States' tariff hike should move import volumes in the same direction as the tariff itself, since a larger tariff surge against China raises the incentive to route the corresponding input through Vietnam. The full-sample coefficient (0.0006) points the

right way but is indistinguishable from zero (Table 2). Restricting the sample to genuinely exposed products moves it in the same direction further, and to the point of marginal significance. In stark contrast to these modest tariff effects, global supply forces exert a much stronger influence. The control—log of aggregate Chinese exports of the corresponding product to the rest of the world, net of Vietnam, enters both strongly and precisely, at 0.559, far beyond the threshold of conventional significance, revealing that Vietnam's imports of the corresponding Chinese—origin goods are substantially explained by China's own, contemporaneous global export cycle for that product, independent of Vietnam's early—warning list status.

An early-warning list that measurably redirected trade would need to survive this entire battery of specifications to leave a trace behind; however, none shows one.

Having established in Section 3 that causal interpretation of θ rests on the standard parallel-trends assumption, we now assess this assumption directly.

Table 3 Event-study coefficients by months relative to early-warning list entry

Event time	Export (Fig. 3)	Import, unadjusted	Import, cleaned (Fig. 4)
-7	-0.047	0.290	-0.142*
	(0.095)	(0.238)	(0.079)
-6	-0.058	-0.199**	-0.208***
	(0.086)	(0.088)	(0.080)
-5	-0.038	-0.367**	-0.284***
	(0.103)	(0.145)	(0.071)
-4	0.035	-0.191**	-0.163**
	(0.066)	(0.096)	(0.064)
-3	0.053	-0.159*	-0.106
	(0.074)	(0.090)	(0.092)
-2	0.034	-0.080**	-0.144***
	(0.055)	(0.040)	(0.047)
-1		*reference (= 0)*	
0	0.103**	-0.124	-0.037
	(0.051)	(0.102)	(0.046)
1	0.015	-0.115	0.035
	(0.064)	(0.126)	(0.059)
2	0.023	-0.107	0.032
	(0.055)	(0.122)	(0.078)
3	-0.075	0.320*	0.073
	(0.146)	(0.166)	(0.075)
4	-0.049	0.323**	0.035
	(0.132)	(0.159)	(0.073)

5	-0.029	0.364**	-0.015
	(0.142)	(0.178)	(0.077)
6	0.021	0.207	-0.0003
	(0.145)	(0.176)	(0.094)
7	-0.058	0.280*	-0.006
	(0.164)	(0.161)	(0.072)
Joint pre-period Wald (event time –6 to –2)	stat = 1.656 p = 0.140	stat = 2.168 p = 0.055	stat = 3.886 p = 0.0016
+ product-specific trend	—	—	p = 0.0020

Notes: PPML-estimated coefficients on indicators for months relative to a product's early-warning list entry-date, with product-clustered standard errors in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels. Event time −1 is the omitted reference category, fixed at zero by construction. "Export" corresponds to Fig. 1. "Import, unadjusted" is estimated on the restricted import sample before any outlier or cohort adjustment. "Import, cleaned" corresponds to Fig. 2: the same sample after trimming observations above the 99th percentile and dropping the entry cohort found to have an atypical import profile. The joint Wald test evaluates whether the five pre-period coefficients from event time −6 through −2 equal zero; event time −7—an open-ended bin absorbing all months seven or more prior to entry—is excluded from this test. "+ product-specific trend" repeats the test after additionally allowing each product its own linear time trend.

In the export equation, individual pre-treatment coefficients are uniformly small, none exceeding a magnitude of roughly 0.06 in absolute value across the six pre-period months we examine, and they exhibit no discernible trend as event time approaches zero. These figures move essentially at random around zero rather than drifting in either direction. A joint test of the null hypothesis that the five pre-treatment coefficients spanning event time −6 through −2 equal zero fails to reject, even at the loosest conventional threshold ($p = 0.14$, Table 3). This supports the parallel-trends assumption for this margin. However, the import equation exhibits a markedly different pattern. Pre-treatment coefficients are mostly negative, and five of the six pre-period months are individually significant at conventional levels. The coefficient reaches roughly −0.28 five months before listing, which is several times the largest deviation seen anywhere in the export equation, before narrowing again as the entry date approaches. That trajectory looks like transitory mean reversion around the listing decision. The corresponding joint test rejects decisively ($p = 0.002$).

We also examine whether this result is driven by a small number of extreme observations or by a particular entry cohort. Neither appears to be the case. Details of those diagnostics, along with the unadjusted (pre-cleaning) event study, are reported in Section 5. As a further check, we allow each product to follow its own linear time trend. The conclusion barely moves ($p = 0.002$) however.

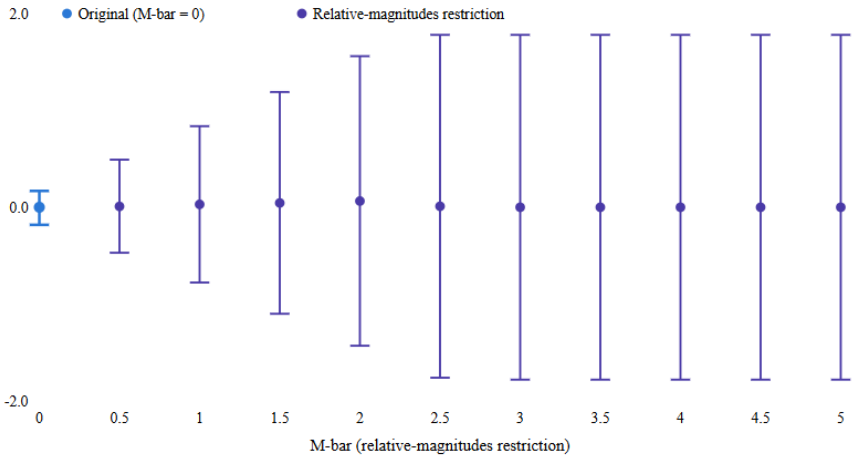
Given this evidence, we are cautious about interpreting the baseline estimate from the import equation as being identified under an exact parallel-trends assumption. Instead, we address the resulting uncertainty using the partial-identification framework of Rambachan and Roth (2023), which characterizes the sensitivity of the estimated treatment effect to a graduated, quantified relaxation of the parallel-trends assumption. We apply the identical procedure to the export equation for internal consistency even though its pre-trend evidence is considerably cleaner, so that we can compare robustness across both margins directly. We report confidence sets for the average post-treatment effect under the relative-magnitudes restriction $\Delta^{RM}\overline{(M)}$, which bounds the maximum permissible post-treatment deviation from parallel trends as a multiple $\overline{(M)}$ of the largest deviation actually observed pre-treatment. This calibrates the plausible scale of unobserved post-treatment bias to the pre-treatment divergence we actually document, rather than to some arbitrary external benchmark [7].

For the export equation, the confidence set for the average post-treatment effect already contains zero at $\overline{M} = 0$ ([−0.181,0.169]). That's consistent with the clean pre−trend evidence documented above, and it continues to contain zero without exception across the full range considered, saturating at approximately [−1.78,1.78] from $\overline{M} = 3$ onward (Table 11, See Appendix). For the import equation, where a genuine pre-trend violation has been documented, the confidence set likewise contains zero at $\overline{M} = 0$ ([−0.097,0.127]) and continues to do so across the entire range we consider, up to $\overline{M} = 5$. At that point, a hypothetical post-treatment violation five times larger than the most extreme pre-treatment deviation we actually observed would be needed to overturn our conclusion. We find no breakdown value within this range for the average post-treatment effect in either equation (Table 12, See Appendix) [8]. Taken together, these results indicate that our conclusion of no detectable early-warning list effect is not an artifact of assuming away the identifying assumption's imperfections. Even for the import equation, where the parallel-trends assumption is not literally satisfied, one would need to posit a degree of hidden bias several times larger than anything actually observed in the pre-treatment data before the evidence could be read as supporting a genuine policy effect. However, the conclusion for the import margin should be read with appropriate caution, as robust to, rather than free of, the identification concern documented above.

[7] Results under the alternative smoothness restriction $\mathrm{SD}\overline{(M)}$, reported in Table 12 (See Appendix), are qualitatively identical.

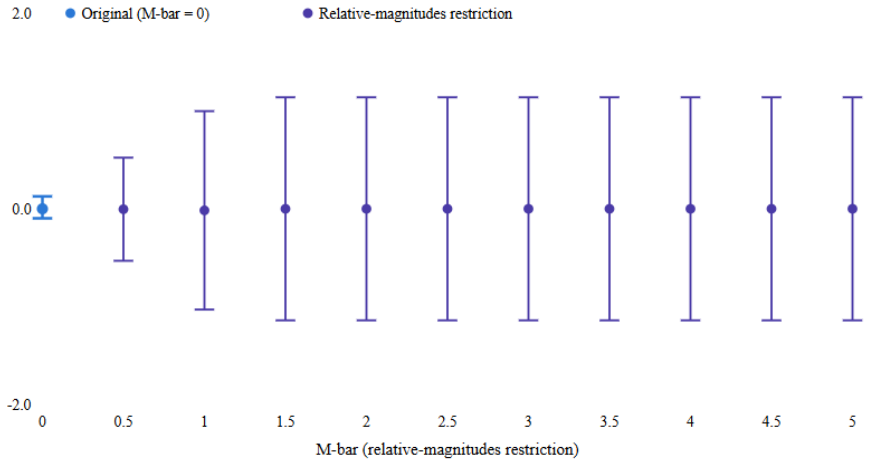
[8] A narrower specification targeting only the coefficient at event time zero, rather than the average across the full post-treatment window, does exclude zero at $\overline{(M)} = 0$ for the export equation ([0.003,0.203]), with breakdown occurring below $\overline{(M)} = 0.05$. We interpret this narrower result as most plausibly attributable to a single marginally significant coefficient among the fourteen leads and lags estimated in the event study, an unsurprising occurrence under repeated testing, rather than as evidence of a robust short-run effect, particularly given that it does not survive averaging across the full post-treatment horizon, which constitutes our preferred and economically relevant object of interest.

Fig. 1 Honest DID sensitivity: export equation



Notes: 95% robust confidence intervals for the average post-treatment effect (event times 0–7), computed under the relative-magnitudes restriction of Rambachan and Roth (2023), plotted as a function of M-bar–the maximum permitted post-treatment parallel-trends violation, expressed as a multiple of the largest pre-treatment deviation observed in the corresponding event study (Table 12, See Appendix). The blue marker at M-bar = 0 reproduces the exact-parallel-trends confidence interval reported in the text; purple markers report the corresponding robust interval at each value of M-bar considered. The dashed horizontal line denotes zero. Both intervals contain zero throughout the full range examined (M-bar = 0 to 5); no breakdown value is identified.

Fig. 2 Honest DID sensitivity: import equation



Notes: Similar to Fig. 1, 95% robust confidence intervals for the average post-treatment effect (event times 0–7), computed under the relative-magnitudes restriction of Rambachan and Roth (2023), plotted as a function of M-bar–the maximum permitted post-treatment parallel-trends violation, expressed as a multiple of the largest pre-treatment deviation observed in the corresponding event study (Table 12, See Appendix). The blue marker at M-bar = 0 reproduces the exact-parallel-trends confidence interval reported in the text; purple markers report the corresponding robust interval at each value of M-bar considered. The dashed horizontal line denotes zero. Both intervals contain zero throughout the full range examined (M-bar = 0 to 5); no breakdown value is identified.

Table 4 Sun-Abraham/Wooldridge estimates versus Standard Two-way Fixed effects

Panel A: Export equation

	Standard TWFE	Extended-TWFE
		(Sun-Abraham/Wooldridge)
List (D_{pt}) / ATT	−0.0118	0.0212
	(0.2069)	(0.1626)
Entry cohorts	10, unrestricted	2, grouped

Panel B: Import equation

	Standard TWFE	Extended-TWFE
		(Sun-Abraham/Wooldridge)
List (D_{pt}) / ATT	0.0321	−0.0580
	(0.1007)	(0.0666)
Entry cohorts	10, unrestricted	2, grouped
Sample	Unadjusted	Cleaned

Notes: The Extended-TWFE column reports the interaction-weighted estimator of Sun and Abraham (2021), extended to the Poisson setting following Wooldridge (2023). Standard errors, clustered by HS6 product, are reported in parentheses. The import-equation comparison is not estimated on identical samples. The standard TWFE estimate uses the unadjusted restricted sample, while the extended-TWFE estimate uses the cleaned sample (winsorized at the 99th percentile with the anomalous entry cohort excluded).

As discussed in Section 3.2, we address negative weighting using the interaction-weighted estimator of Sun and Abraham (2021), extended to the Poisson setting following Wooldridge (2023). This methodology estimates a separate treatment effect for each cohort and relative time and aggregates these into a single average treatment effect on the treated using weights that are non-negative, thus avoiding the specific mechanism through which standard two-way fixed effects can generate the biased comparisons Goodman-Bacon (2021) identifies. A fully saturated specification interacting all ten entry cohorts with relative time is computationally demanding under Poisson estimation, and, in our setting, proved intractable given the panel's size. This is compounded by the limited number of treated products in most individual cohorts, which would leave many cohort-specific effects estimated with very little information in any case. We therefore group entry cohorts into two categories: the single largest cohort, accounting for roughly half of all treated products, and all remaining cohorts, represented by their average entry date. For the export equation, this exercise yields an aggregated average treatment effect of 0.021, against a baseline coefficient of −0.012 . For the import equation, estimated on the same cleaned sample, this exercise yields −0.058, against a baseline coefficient of 0.032 (Table 4). Baseline or extended, export or import, none of the four

coefficients comes close to conventional significance. The 95% confidence interval for the extended estimate also overlaps substantially with that of the baseline estimate in both equations. This pattern is consistent with what we would expect if both estimators were capturing the same underlying null effect. The reduction in standard error under the extended estimator observed in both equations is informative in its own right. If negative weighting were materially distorting the baseline coefficient, correcting for it would be expected to improve precision as well as shift the point estimate, thus the correction would not simply be trading one source of noise for another.

5 Robustness Check

Having established our baseline conclusions, we turn to a further sequence of checks addressing sample construction, treatment coding and the influence of individual products and cohorts on the point estimates themselves.

Table 5 Robustness to the restricted-sample threshold

Panel A: Export equation	0.75× (100.1 pp)	1.0× (133.5 pp, baseline)	1.25× (166.9 pp)
List (D_{pt})	-0.0052	-0.0118	0.1231
	(0.1927)	(0.2069)	(0.1157)
Products	4,442	4,245	2,921
Panel B: Import equation	**0.75× (105.0 pp)**	**1.0× (140.1 pp, baseline)**	**1.25× (175.1 pp)**
List (D_{pt})	0.0588	0.0321	-0.1861*
	(0.1000)	(0.1007)	(0.0956)
Products	3,879	3,809	86

Notes: Each column reports the coefficient (clustered standard error in parentheses) on the early-warning list indicator, D_{pt}, estimated on a restricted sample constructed using the threshold indicated in the column heading, expressed as a multiple of the sample-average tariff margin (Section 3.3.1); the corresponding threshold in percentage points is shown beneath each multiplier. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively. All specifications include the full control vector and two-way fixed effects described in Section 3.

We begin with the restricted-sample threshold, which, as noted in Section 3.3.1, is computed from our data rather than imported from an external benchmark. Re-estimating equation (1) under thresholds set at 0.75, 1.0, and 1.25 times the sample-average tariff margin yields coefficients of −0.005, −0.012, and 0.124, respectively (Table 5). None comes close to conventional significance, and none exceeds a modest fraction of its own standard error. The somewhat larger magnitude at the tightest threshold is where the restricted sample is smallest and the estimate least precise, remaining well within the range one would expect from sampling variation alone rather than signaling any systematic drift in the underlying effect. Importantly, the coefficient doesn't move in one consistent direction as the threshold tightens, and its magnitude doesn't scale predictably with sample size either (If the restricted-sample construction itself were driving the result, we'd expect some kind of pattern here). The three estimates read more naturally as noisy draws around a common null than as points along any systematic trend. The same exercise for the import equation (Table 5) is reassuring at the two looser thresholds: coefficients of 0.059 and 0.032 at 0.75× and 1.0×, both far from significance. But the tightest threshold breaks the pattern. The sample shrinks sharply down to 86 products and the coefficient, −0.186, becomes marginally significant at the 10% level. At 1.25×, the import equation retains fewer than 3% as many products as the export equation does at the same threshold, reflecting a much more concentrated margin distribution on the import side. A sample this small and this far into the tail of the distribution doesn't offer much basis for reliable inference. We regard this result as illustrative of the threshold-robustness exercise's limits at extreme cutoffs rather than as a genuine departure from the null result documented throughout this paper. Note that it does not recur at either of the two less extreme thresholds we examine. In other words, our baseline result doesn't hinge on precisely where the line between exposed and unexposed products happens to be drawn.

Table 6 Robustness to the treatment-timing specification

Panel A: Export equation	Currently listed	Previously listed, since delisted
Coefficient	0.0028	–0.0875
	(0.2077)	(0.2538)
Products / Observations	4,178 / 388,554	
Panel B: Import equation	Currently listed	Previously listed, since delisted
Coefficient	–0.0049	0.0942
	(0.1176)	(0.0838)
Products / Observations	3,738 / 347,634	

Notes: This table replaces the baseline absorbing indicator D_{pt} with two mutually exclusive indicators: one equal to one if the product is listed on the early-warning list in the current month, and one equal to one if the product was listed at any earlier date but is not currently listed. Standard errors, clustered by HS6 product, are reported in parentheses. All other specification details follow Tables 1–2. The import-equation product count (3,738) is modestly lower than in the baseline restricted sample (3,765), reflecting a small number of observations dropped due to missing treatment-timing values under this alternative coding.

We next verify that the treatment-coding decision discussed in Section 3.2 is not itself responsible for the null result. We decompose D_{pt} into two mutually exclusive indicators: current listing and prior listing that has since lapsed, and re-estimate equation (1). This gives coefficients of 0.003 and −0.087 (Table 6), both far smaller than their standard errors and so indistinguishable from zero, or from each other. The corresponding decomposition for the import equation shows the same pattern: two statistically indistinguishable, near-zero coefficients (−0.005 for current listing status, 0.094 for prior-but-lapsed status, neither significant) (Table 6). We regard this as direct evidence that the absorbing coding adopted as our baseline is a matter of identification hygiene, not a determinant of the result. Products currently placed on early-warning list and products that were once listed but later delisted behave statistically the same way in both equations.

Table 7 Leave-one-cohort-out robustness: export equation

Entry cohort (month index)	Products in cohort	Coefficient excluding cohort	p-value
19	29 (48.3%)	–0.0285	0.907
93	7	–0.0118	0.955
31	6	–0.0525	0.807
22	5	–0.0163	0.938

46	4	0.1891	0.160
28	4	–0.0754	0.767
70	2	–0.0196	0.928
39	1	–0.0109	0.958
35	1	–0.1129	0.606
75	1	0.0186	0.936
None excluded (baseline)	60	*–0.0118*	*0.955*

Notes: Each row reports the coefficient on D_{pt} from re-estimating equation (1) after excluding all products first listed in the indicated month, on the restricted sample. Entry cohorts are indexed by month number (1 = January 2018). No coefficient approaches conventional significance. Cohort sizes sum to the 60 treated products in the restricted sample.

Table 8 Robustness to extreme observations

Panel A: Export equation				Baseline	Trim > p99	
List (D_{pt})				–0.0118 (0.2069)	0.0561 (0.1061)	
Panel B: Import equation	Baseline	Trim > p99	Winsorize p99.5	Winsorize p99	Winsorize p98	Winsorize p95
List (D_{pt})	0.0321 (0.1007)	0.1417* (0.0600)	0.0884 (0.0759)	0.0726 (0.0796)	0.0611 (0.0632)	–0.0412 (0.0471)

Notes: Each column reports the coefficient on D_{pt} (clustered standard error in parentheses) under the treatment of the dependent variable's upper tail indicated in the column heading. "Trim > p99" excludes observations above the 99th percentile of the dependent variable; "Winsorize" caps such observations at the indicated percentile without excluding them from the sample. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively. All specifications include the full control vector and two-way fixed effects described in Section 3.

We further assess whether the export-equation estimate is disproportionately shaped by any single treatment cohort, distinct from the specific cohort already implicated in the import equation's pre-trend violation above. A leave-one-cohort-out exercise by sequentially excluding each monthly entry cohort and re-estimating equation (1) yields coefficients ranging from −0.113 to 0.189 across all ten exclusions (Table 7), none statistically significant. This range notably includes the exclusion of the single largest entry cohort, comprising 48.3% of all treated products, for which the resulting coefficient, −0.029, is scarcely distinguishable from the baseline restricted-sample estimate of −0.012. This is direct evidence that our conclusion is not an artifact of any one

cohort, including the numerically dominant one. We run a parallel check for extreme observations (Table 8). In the export equation, trimming observations above the 99th percentile of the dependent variable moves the coefficient from −0.012 to 0.056, and the standard error falls by roughly half, both estimates stay statistically indistinguishable from zero. The tightening of precision upon trimming reflects the well-known sensitivity of PPML's implicit weighting scheme to the right tail of a skewed dependent variable, rather than any change in the conclusion. The import equation is where this matters more, given the anomalous cohort we flagged in the parallel trend analysis. Here we compare outright trimming against winsorization at four thresholds: the 99.5th, 99th, 98th, and 95th percentiles. Trimming alone pushes the coefficient to the edge of conventional significance, a result that does not survive under any of the four winsorized specifications, each of which instead yields a coefficient comfortably indistinguishable from zero. We interpret this contrast as informative rather than as a specification-search exercise. Outright exclusion changes the composition of the estimation sample in a way that's correlated with treatment status, whereas winsorization does not. Winsorization changes an observation's recorded magnitude but not its presence in the panel, and the null conclusion holds across all thresholds we considered. This is also consistent with our earlier finding that the same small cohort behind the pre-trend violation is also what's driving the instability under naive trimming. The results from both diagnostics appear to be driven by the atypical behaviour of this cohort rather than by a genuine effect of early-warning list inclusion.

To corroborate our conventional cluster-robust inference through a procedure that does not rely on asymptotic approximations, we implement a randomization-inference exercise (Fisher 1935; Young 2019). We randomly reassign the observed treatment-entry structure across products within the restricted sample, holding fixed both the number of treated products and the empirical distribution of entry dates, and repeat this procedure 100 times to construct an empirical null distribution for the treatment coefficient. In the export equation, the coefficient we actually observe is smaller in magnitude than ninety-five of the one hundred coefficients this purely random reassignment procedure generates (Fig. 5, See Appendix). It ranks among the smallest handful of outcomes the exercise could plausibly have produced by chance alone. Except for the possibility of an exactly zero coefficient, this provides strong evidence from the randomization test in support of a null effect. The estimated effect is not only consistent with zero, but it's also more muted than what pure noise would typically generate. The

corresponding exercise for the import equation, applied to our preferred, fully controlled specification, gives a similar result. The observed coefficient is smaller in absolute value than 88 of the 100 placebo coefficients generated (Fig. 6, See Appendix). This provides further evidence in support of our main result: early-warning list inclusion carries no detectable association with Chinese input inflows once global supply conditions are properly accounted for.

Table 9 Equivalence testing (TOST) across equivalence bounds

δ	Export	Import
5%	0.4290	0.4341
10%	0.3432	0.2650
15%	0.2681	0.1424
20%	0.2048	0.0678
25%	0.1534	0.0289**
30%	0.1129	0.0111**
35%	0.0817	0.0039**
40%	0.0583	0.0013**
45%	0.0410**	0.0004**
50%	0.0285**	0.0001**
55%	0.0196**	<0.0001**
60%	0.0134**	<0.0001**

Notes: Each cell reports the p−value from a two one−sided equivalence test of the null hypothesis that the true proportional treatment effect lies outside $[-\delta, +\delta]$, against the alternative that it lies within this interval, for the value of δ indicated in the row heading. ** denotes a p−value below 0.05, constituting formal rejection of proportional effects at least as large as δ.

Finally, a statistically insignificant coefficient does not, by itself, establish the absence of an economically meaningful effect. A small or insignificant estimate may also result from limited statistical power. We therefore report two diagnostics that explicitly characterize what our design can reasonably distinguish. A Two One-Sided Test of statistical equivalence (Lakens 2017) allows us to formally reject rather than merely fail to reject the hypothesis that the true proportional effect exceeds a given bound (Table 9). For the export equation, this procedure formally rejects proportional effects larger than ±45%, the point at which the equivalence test first crosses the conventional threshold of confidence. For the import equation, once the global supply control is included, that threshold tightens substantially, to ±25%. In other words, our design is precise enough to state with reasonable confidence that early-warning list inclusion did not move Vietnamese exports to the United States by more than roughly 45% in either direction, and did not move Vietnamese imports from China by more than roughly 25% once China's global export supply is accounted for. We also report the implied Minimum Detectable Effect under conventional power and significance parameters: about 78.5% for the export equation and 32.6% for the import equation. The import figure is

less than half the export one, reflecting a real gain in design precision from the global-supply control. It's a concrete illustration of how the attenuation documented earlier in this section translates into a more informative null rather than just a smaller point estimate. These two benchmarks remain compatible despite their difference in magnitude. The equivalence bound is based on the point estimate we observe, which is already very close to zero. By contrast, the Minimum Detectable Effect is determined entirely by the design of the study and does not depend on the estimated coefficient. It is therefore the more conservative and larger of the two.

6 Discussion and Conclusion

This paper set out to answer a narrow question with a wide shadow: whether the formal inclusion of a product on Vietnam Trade Remedy's anti-circumvention early-warning list effectively affects the volume of Vietnam's exports of that product to the United States or its imports from China? Across a demanding sequence of checks, Vietnam's early-warning system shows no sign of having redirected the flows it was built to influence, moving neither margin by an amount this paper's data can detect.

A null result admits more than one explanation, and this paper's design cannot mechanically choose between them. Hayakawa and Sudsawasd (2025), evaluating Thailand's structurally identical early-warning list, lay out three possible explanations for such a null result. All three carry over directly to the present setting; each implies a different judgment about the policy, and none can be ruled out by the trade-flow evidence alone. The first is that the magnitude of circumvention through the specific products Vietnam has chosen to list was never significant to begin with. Even a fully functioning early-warning list would leave no detectable trace in the trade data, simply because there was little for it to deter. The second is that Chinese-linked exporters found a way to slip through Vietnam's TRAV system itself. They may have adapted their routing, documentation, or product classification around the specific products and procedures the early-warning list monitors, so that circumvention continues largely undiminished while remaining invisible to the trade flows this paper observes. This possibility has a straightforward economic logic. Following Becker's (1968) classic framing of compliance as a cost-benefit calculation, a firm facing TRAV system's added documentation burden weighs that cost against the cost of evading it. Where the tariff differential motivating circumvention in the first place is large relative to the marginal cost of such adaptation, compliance is simply the more expensive option, and the null result observed here is exactly what that calculation predicts. The third is that the initiative simply does not work well in practice. The reasons could be as mundane as insufficient enforcement capacity or manpower within Vietnam's Trade Remedies Authority relative to the scale of the product universe it is tasked with monitoring. This paper evaluates Vietnam, the single economy the broader literature identifies as facing the greatest circumvention exposure of any United States' trading partner. That choice of setting narrows the plausibility of the first explanation considerably, relative to a lower-exposure country. It cannot, however, separate the second explanation from the third.

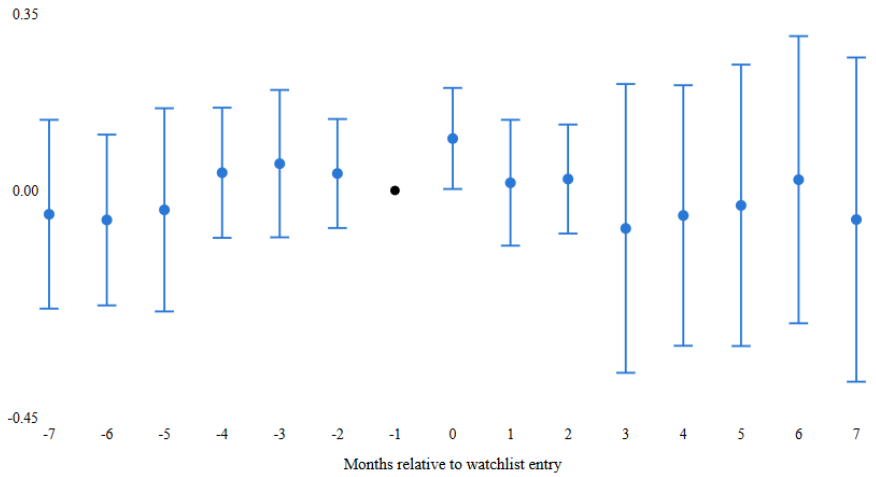
A fourth possibility is worth raising, and it follows less from the empirical results themselves than from the institutional environment around them. Developed in a context far removed from trade remedy law, organizational sociology has a name for the pattern documented here: real institutional elaboration, sustained across multiple review cycles, that nonetheless leaves no

measurable trace in the outcome it is nominally designed to move. Meyer and Rowan (1977) in their account of institutional isomorphism argue that organizations often adopt formal structures to satisfy the expectations of external audiences on whom they depend. These structures can become substantially decoupled from an organization's actual operating activity. That decoupling need not reflect a failure of implementation. It can be the structure working exactly as the incentives that produced it intended. Reading this way, Vietnam's early-warning list looks less like a failed enforcement instrument and more like a textbook case of a compliance structure built for an audience. That audience, in this instance, holds the authority to impose country-wide antidumping and countervailing liability on every exporter in a targeted sector, whether or not a given exporter had any part in the conduct that triggered it. The United States does not need Vietnam's early-warning list to move Vietnamese trade statistics. Instead, they need Vietnam to be able to demonstrate, credibly and upon request, that it has taken meaningful and verifiable steps to address the risk. An early-warning list along these lines may be doing precisely what a small, trade-dependent economy under great-power scrutiny needs it to do: manage diplomatic and legal exposure. That is a different objective, with a different success condition than suppressing rerouted trade volume, and not the one this paper's design was built to evaluate.

Distinguishing among these four readings is beyond what any single trade-flow evaluation can deliver. We leave that task to future research equipped with firm-level enforcement data. The more useful exercise for policymakers is not to guess which is correct, but to ask what each would imply for action, and what would be worth doing regardless. If the second interpretation holds, the appropriate response is a narrower instrument, that is to monitor at the level of the firm and its input sourcing rather than HS6 product codes, especially when Section 2 has revealled that circumvention risk is concentrated among a small set of Chinese-linked and newly established firms. If the third reading holds, the appropriate response is to prioritize scarce inspection capacity on high-risk product–firm combinations identified by the kind of behavioral signature Iyoha et al. (2024) document, rather than spreading a fixed enforcement budget evenly across an expanding product list. If the fourth reading holds, the watchlist functions primarily as a demonstrable-effort signal to United States authorities. Its value would then lie not in trade-volume suppression but in the quality of the documentary record it generates. This criterion is not observable in our data but could be assessed directly by tracking whether listed products fare better than unlisted products in subsequent United States circumvention inquiries.

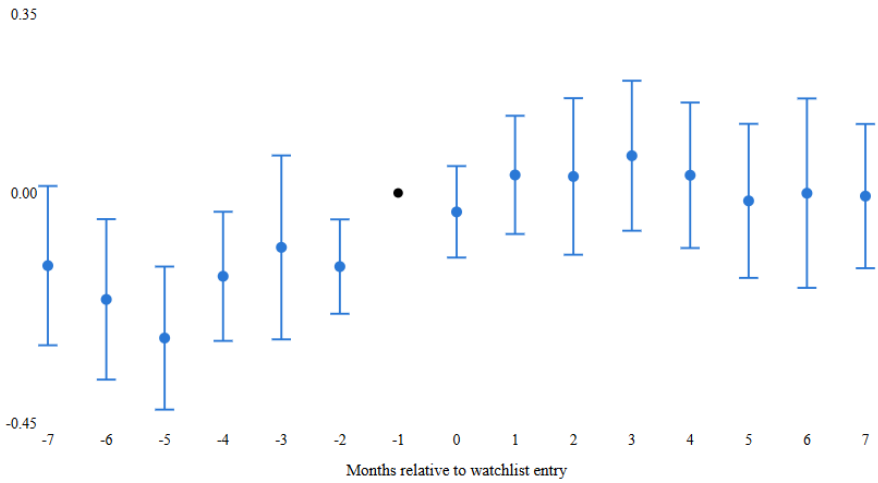
Appendix

Fig. 3 Event-study coefficients, export equation



Notes: PPML-estimated coefficients on indicators for calendar months relative to a product's early-warning list entry date (event time 0 denotes the entry month; the month immediately preceding entry, event time −1, is the omitted reference category, shown as a hollow marker, and is therefore fixed at zero by construction). Vertical bars denote 95% confidence intervals constructed from product-clustered standard errors. Products never subject to the early-warning list are absorbed into the reference group. Specification includes the full control vector and two-way fixed effects described in Section 3.

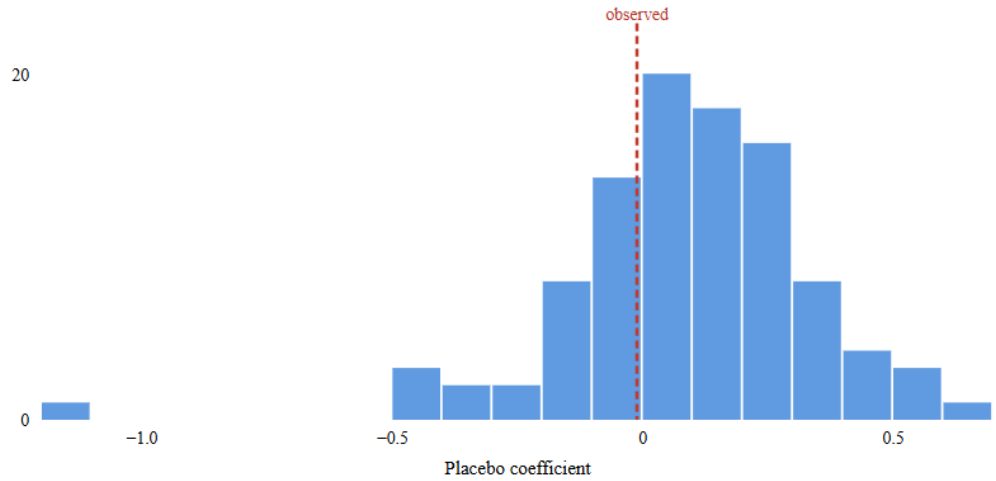
Fig. 4 Event-study coefficients, import equation



Notes: PPML-estimated coefficients on indicators for calendar months relative to a product's early-warning list entry date (event time 0 denotes the entry month; the month immediately preceding entry, event time −1, is the omitted

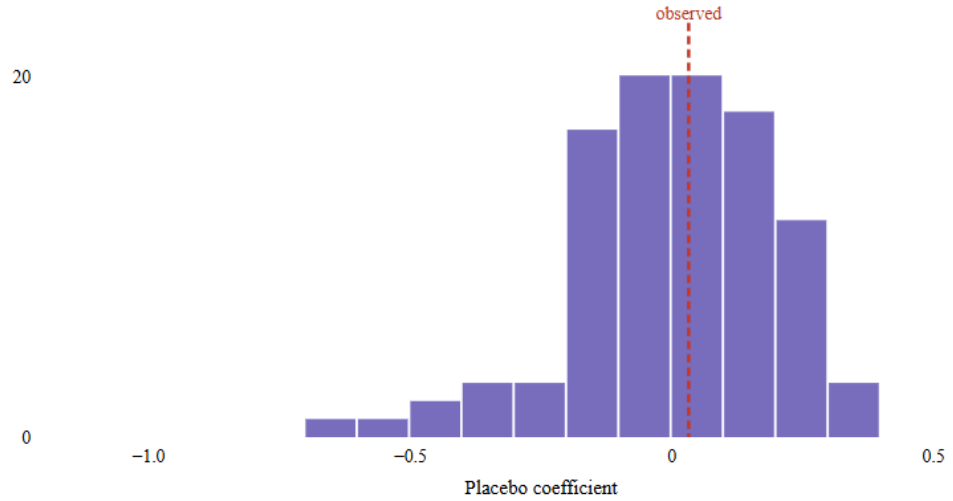
reference category, shown as a hollow marker, and is therefore fixed at zero by construction). Vertical bars denote 95% confidence intervals constructed from product-clustered standard errors. Products never subject to the early-warning list are absorbed into the reference group. Specification includes the full control vector and two-way fixed effects described in Section 3, and is estimated on the import-equation restricted sample after trimming extreme observations at the 99th percentile and excluding the entry cohort identified in Section 4 as exhibiting an atypical import profile.

Fig. 5 Histogram of placebo coefficients under randomization inference, export equation, with the observed coefficient marked by a vertical line



Note: Empirical distribution of the coefficient on D_{pt} obtained under 100 random reassignments of the treatment-entry structure within the restricted sample, holding fixed the number of treated products and the empirical distribution of entry dates, and re-estimating equations on each reassignment. The dashed vertical line marks the coefficient obtained under the true, observed treatment assignment.

Fig. 6 Histogram of placebo coefficients under randomization inference, import equation, with the observed coefficient marked by a vertical line



Note: Empirical distribution of the coefficient on D_{pt} obtained under 100 random reassignments of the treatment-entry structure within the restricted sample, holding fixed the number of treated products and the empirical distribution of entry dates, and re-estimating equations on each reassignment. The dashed vertical line marks the coefficient obtained under the true, observed treatment assignment.

Table 10 Descriptive statistics for the full and restricted estimation samples

Panel A: VN → US (export equation)		
	Full sample	Restricted sample
Number of HS6 products	5,642	4,245
Number of ever-treated products	139	60
Mean export value ($ thousand)	6,087 (SD 74,029)	4,698 (SD 38,005)
Mean TariffShock (pp)	18.3 (SD 19.7)	20.2 (SD 20.1)
Mean tariff margin (pp)	—	133.52
Panel B: CN → VN (import equation)		
	Full sample	Restricted sample
Number of HS6 products	4,878	3,809
Number of ever-treated products	138	60
Mean import value ($ thousand)	2,259 (SD 17,618)	1,868 (SD 10,821)
Mean TariffShock (pp)	19.5 (SD 19.8)	20.5 (SD 20.2)

Mean tariff margin (pp)	—	140.06

Notes: Export and import values are measured in thousands of US dollars. "Ever-treated" denotes HS6 products included on Vietnam's eatly-warning list at least once between January 2018 and September 2025. The count is one lower in the CN→VN full sample because one watch-listed product has no recorded import transactions from China during the period and is therefore absent from that product universe. "Mean tariff margin" equals, by construction, the threshold used to define each panel's restricted sample (Section 3.3.1), and is reported only for that column. Export/Import values and TariffShock are averaged over all product-month observations in the indicated sample; standard deviations reflect panel-wide dispersion rather than a between-product statistic.

Table 11 Honest DID confidence sets under $\Delta^{RM}(\overline{M})$, average post-treatment effect

$\overline{M}$	Export (Fig. 1)	Import (Fig. 2)
0 (original)	[–0.181, 0.169]	[–0.097, 0.127]
0.5	[–0.470, 0.491]	[–0.532, 0.523]
1.0	[–0.777, 0.837]	[–1.03, 0.998]
1.5	[–1.10, 1.19]	[–1.14, 1.14]
2.0	[–1.43, 1.56]	[–1.14, 1.14]
2.5	[–1.76, 1.78]	[–1.14, 1.14]
3.0	[–1.78, 1.78]	[–1.14, 1.14]
3.5	[–1.78, 1.78]	[–1.14, 1.14]
4.0	[–1.78, 1.78]	[–1.14, 1.14]
4.5	[–1.78, 1.78]	[–1.14, 1.14]
5.0	[–1.78, 1.78]	[–1.14, 1.14]

Notes: Each row reports the 95% robust confidence interval for the average post-treatment effect (event times 0–7) under the relative-magnitudes restriction $\Delta^{RM}(\overline{M})$, of Rambachan and Roth (2023), where $\overline{M}$, bounds the maximum permitted post-treatment parallel-trends violation as a multiple of the largest deviation observed in the corresponding pre-period. The $\overline{M} = 0$ row reproduces the exact-parallel-trends confidence interval of Section 4. Both intervals contain zero at every value of $\overline{M}$ considered; no breakdown value is identified. Intervals stabilize once $\overline{M}$ is large enough that the underlying linear program is no longer binding, at $\overline{M} = 3$ for the export equation and $\overline{M} = 1.5$ for the import equation.

Table 12 Honest DID breakdown values

	Δ^{SD} event time 0	Δ^{RM} event time 0	Δ^{RM} average post-period
Export (Fig. 3)	$\overline{M} < 0.05$	$\overline{M} < 0.5$	none up to $\overline{M} = 5$
Import (Fig. 4)	none up to $\overline{M} = 0.3$	none up to $\overline{M} = 5$	none up to $\overline{M} = 5$

Notes: This table reports the breakdown value of the sensitivity parameter $\overline{M}$ under the smoothness restriction Δ^{SD}, $\overline{M}$ under the relative-magnitudes restriction Δ^{RM},, at which the 95% robust confidence interval for the treatment effect ceases to exclude zero. "Event time 0" targets the single coefficient at the month of early-warning

list entry; "average post-period" targets the average effect across event times 0–7, matching the object of interest in the main PPML specification (reported for Δ^{RM} only; see Table 11 for the full sensitivity path). Where no breakdown is identified within the range of the parameter considered, this is indicated explicitly together with the upper bound examined.

Author contribution Hoang Quynh Duong Le is the only author of the paper.
Funding This research received no specific grant from any funding agency in the public, commercial, or not-for-profit sectors.
Data availability The data in this study were obtained from publicly accessible databases. The data details, including data source, is clearly stated in Section 3 of the manuscript for transparency and replicability.
Declaration
The author has no relevant financial or non-financial interests to disclose.
The author declares that this manuscript has not been submitted to any other journal for simultaneous
consideration.
Artificial intelligence tools were not used for data analysis, interpretation, literature review in this study.
Competing interests The author declares no competing interests.
Research involving human participants and/or animals Not applicable.
Informed consent Not applicable.

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