Selection into Tariff Avoidance and the Measured Welfare Cost of Tariffs

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Standard estimates suggest U.S. importers bear the full incidence of tariffs. I show these estimates are biased upwards because low-passthrough exporters (those whose own margins would bear most of the tariff's incidence) select into avoidance behavior. Namely, these exporters avoid tariffs by rerouting exports through countries facing lower tariffs—called transshipping—leaving only high-passthrough firms (which shift the incidence to U.S. importers) directly exporting to the U.S. I construct a measure of transshipment and find that between 30–40 percent of tariffed products were transshipped, reducing the lower bound on estimated domestic incidence to 85 percent. The model identifies a novel enforcement channel: because import costs are tax-deductible, higher corporate tax rates discourage transshipping. For that reason, the 2017 corporate tax cuts *increased* transshipping, costing the U.S. $2–$9 billion in tariff revenue.

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1 Introduction

Standard estimates suggest U.S. importers bore the full incidence of the 2018 China tariffs, implying these tariffs were as costly as top income taxes (Finkelstein and Hendren 2020).[1] But this consensus rests on a problematic assumption: that the firms whose transactions we observe in customs data after a tariff increase are representative of all affected firms. They are not. When tariff avoidance is costly, the firms that exit the direct-shipping channel—through transshipment, relocation, or other strategies—are precisely those that would have borne the highest incidence. This selection bias primarily affects studies based on bilateral trade flows (border designs) rather than those that track specific goods from origin to destination (cohort designs). When low-passthrough firms select into avoidance, they exit the customs sample used in border designs, leaving only high-passthrough "survivors." This mechanically biases measured incidence upward, overstating both the domestic burden and the welfare cost of tariffs.

The bias arises from a simple sorting mechanism. Exporters with low marginal costs charge high markups and have low tariff passthrough, so they must absorb most of the tariff themselves. When costly avoidance options like transshipment become attractive, these high-markup firms have the strongest incentive to pay the fixed cost to reroute through third countries. Higher-cost firms that can pass tariffs through to U.S. buyers continue shipping directly. Standard regressions using customs data therefore average passthrough over a selected sample of survivors, mechanically yielding estimates that are too high. Given that markups are heterogeneous in practice (Gopinath and Itskhoki 2010; De Loecker, Eeckhout, and Unger 2020; Edmond, Midrigan, and Xu 2023), this selection bias is likely empirically important. This mechanism builds on the early intuition of Fisman, Moustakerski, and Wei (2008), who show that indirect trade via Hong Kong rises with tariff rates due to costly evasion; here I generalize that framework to allow heterogeneous markups and selection bias in measured passthrough.

This paper makes three contributions. First, I show that accounting for selection reduces measured aggregate passthrough from 100% to a lower bound of approximately 85%. Since avoidance is highest in intermediate and capital goods and there is essentially none in consumer goods, the correction is almost entirely concentrated in imported business inputs. Second, I document large behavioral responses: a 10 percentage point increase in the tariff rate raises the probability of transshipment by 10-30 percentage points on the extensive margin, and implies an increase in the transshipped share of goods be-

1. See, for example, Amiti, Redding, and Weinstein (2019, 2020), Cavallo et al. (2021), Fajgelbaum et al. (2020), and Flaaen, Hortaçsu, and Tintelnot (2020).

tween 2–11 percentage points on the intensive margin. Third, I identify a novel policy interaction: the 2017 corporate tax cut from 35% to 21% reduced tariff revenue by \$2–\$9 billion through increased avoidance, demonstrating quantitatively significant spillovers between the corporate tax base and trade enforcement.

The selection bias matters because it distorts our understanding of tariff welfare costs. I evaluate these costs using the marginal cost of public funds (MCPF), which measures the welfare cost per dollar of revenue raised. The MCPF combines domestic incidence (who bears the burden) in the numerator with the fiscal externality (how much the tax base shrinks when rates rise) in the denominator. For a nondistortionary lump-sum tax, the MCPF is one. Existing estimates from the 2018 trade war with China place the MCPF between 1.2 and 1.6 (Finkelstein and Hendren 2020; Jaccard 2021), comparable to top income taxes. However, these estimates do not model the underlying selection mechanism, leading to biases in both components. In the numerator, border-based estimates overstate domestic incidence because low-passthrough firms exit the sample. In the denominator, while aggregate elasticity estimates may implicitly capture some of the base erosion from avoidance, this paper is the first to isolate the avoidance elasticity itself. My theoretical and empirical contributions address both margins

I formalize the selection mechanism and derive a portable correction for measured tariff passthrough. Importantly, correcting domestic incidence requires only measuring the *level* of avoidance, which is a sufficient statistic under monotone selection. Characterizing the fiscal externality requires estimating the *elasticity* of avoidance with respect to tariff incentives. I also show theoretically that the domestic corporate tax system has important spillovers into discouraging avoidance. While the public finance literature focuses on explicit enforcement tools like audits, inspections, and penalties (Allingham and Sandmo 1972; Slemrod and Yitzhaki 2002; Bhandari et al. 2024; Slemrod 2019), I show that allowing importer firms to deduct the cost of imported goods is isomorphic to such measures. Deductibility lowers the importer's effective burden of the tariff, making them less sensitive to price increases. This allows exporters to pass through more of the tariff on compliant shipments, raising the profitability of direct shipping relative to transshipment and thus reducing avoidance. Consequently, reforms like the 2017 Tax Cuts and Jobs Act, which reduced corporate taxes but also increased expensing for imported capital goods materially changed the incentive to avoid prior to the 2018 trade war. However, the reform's ultimate effect on avoidance depends on both the level and elasticity of transshipment.

Naturally, it is difficult to identify a behavior which is designed to be undetectable. Following a tradition in the trade and public finance literature that uses anomalies in official trade statistics to infer illicit activity (Fisman and Wei 2004; Deng et al. 2025), I extend the ”trade footprint” screening approach from Freund (2024) to identify upper and lower bounds on the volume of transshipment from China to the United States in bilateral trade data—and hence to identify the correction on incidence. I find that between 30–40% of tariffed product codes exhibit evidence of transshipment, with aggregate rerouting reaching 15% of baseline China-U.S. trade by 2023. To characterize the fiscal externality, I exploit within-product variation in tariff exposure, finding large and robust responses concentrated in business inputs: a 10 percentage point increase in the tariff raises the probability of transshipment by 10–30 percentage points, and increases the volume of transshipment by 2–11 percentage points. This large elasticity indicates that the tariff base erodes substantially in response to rate increases, providing a structural estimate for a key component of the fiscal externality that was previously subsumed within a single, aggregate elasticity. Nevertheless, since transshipment is only one kind of avoidance, the estimates yield a conservative estimate of both the passthrough correction and of the avoidance elasticity.

Combining theory and empirics, I re-evaluate the tariff's welfare cost. The corrected passthrough lowers the MCPF's numerator, while the large avoidance elasticity increases the denominator, with these forces partially offsetting. I find that the corrected MCPF is substantially lower than existing estimates and, in some specifications, cannot be rejected as equal to one—potentially on par with a lump-sum tax. This result complements recent work by Alessandria et al. (2025), who show that tariffs can have low welfare costs when revenue is used to offset distortionary taxes. My mechanism operates through a different channel: lower measured incidence via selection into avoidance, combined with a novel enforcement margin through the corporate tax code.

It is important to note the scope of these findings. Transshipment is one salient channel of tariff avoidance, but firms have access to a broader portfolio of strategies, including misreporting value to fall under de minimis thresholds (Fajgelbaum and Khandelwal 2024), breaking up shipments into smaller parcels, or misclassifying goods (Fisman and Wei 2004). My estimates therefore represent a conservative lower bound on total avoidance. Additionally, the welfare analysis is conducted in partial equilibrium and abstracts from general equilibrium effects on wages, prices, and terms-of-trade effects. In particular, the 85% passthrough I measure reflects the change in import prices paid by U.S. firms, but the ultimate incidence depends on whether these costs are absorbed by domestic firms' profit margins or passed through to consumers (Amiti, Itskhoki, and Konings 2019; Flaaen et al. 2025). These limitations suggest the true correction to measured passthrough could be larger, and the welfare implications more nuanced, than my baseline estimates indicate.

Related Literature. My framework serves to reconcile the findings of both border- and cohort-design studies by showing they measure different populations of firms. Even studies using high-quality BLS microdata that tracks identical goods over time, such as Cavallo et al. (2021) or bilateral administrative trade data from the Census Bureau (Fajgelbaum et al. 2020; Amiti, Redding, and Weinstein 2019, 2020) confirm the puzzle of near-complete passthrough at the border. My selection model explains this finding by showing that these studies correctly measure the passthrough for the selected sample of ”survivors” who continue to ship directly.

The importance of this selection margin is highlighted by a natural experiment in the washing machine market, documented by Flaaen, Hortaçsu, and Tintelnot (2020). They find that country-specific tariffs led to extensive “country-hopping” and minimal price passthrough, but a later global tariff that eliminated this low-cost avoidance option caused passthrough to exceed 100%. This validates my model's core prediction: the cost of avoidance is a key determinant of measured incidence. Critically, Cavallo et al. (2021) also provide direct, firm-level evidence for this avoidance behavior, documenting significant “trade diversion” by major US retailers whose import share from China dropped substantially after the tariffs were imposed. My aggregate findings on transshipment can thus be seen as the economy-wide footprint of the firm-level sourcing adjustments they identify. While the literature is still exploring the second-stage passthrough from retailers to consumers—with Cavallo et al. (2021) finding retailers absorbed the costs while Flaaen, Hortaçsu, and Tintelnot (2020) finds they amplified them—my paper clarifies that the initial cost shock faced by US firms has been systematically overstated due to selection.

Recent work further confirms the empirical prevalence of rerouting using a variety of complementary methodologies, providing a strong foundation for this paper's analysis. Critically, Do et al. (2025) use proprietary bill of lading data to directly observe the physical path of individual shipments, showing that the majority of US containerized imports are transshipped via a concentrated set of hub countries. This establishes that large-scale rerouting is a structural feature of modern global logistics, not just a niche response to tariffs. Other studies infer this behavior using customs data. Deng et al. (2025), for example, apply the classic trade discrepancies methodology of Fisman and Wei (2004) and find that Regional Trade Agreements (RTAs) systematically create incentives for circumvention . In a case study highly relevant to this paper, they estimate that $5.5 billion of the 2018 surge in U.S. imports from Mexico was illicit transshipment from other countries seeking to avoid U.S. tariffs. From the perspective of a key hub country, Iyoha et al. (2025) also document substantial trade diversion in Vietnamese customs records. My empirical approach builds on Freund (2024), who first developed a screening methodology for customs data and also finds substantial diversion.

While this literature confirms that rerouting is a widespread and rational response to trade policy incentives, my contribution is to provide the theoretical framework linking this observed avoidance to passthrough bias in border designs (Amiti, Redding, and Weinstein 2019, 2020; Fajgelbaum et al. 2020), estimate the elasticity needed for policy counterfactuals, and identify the corporate tax channel as an implicit enforcement tool.[2] My estimates, constructed using publicly available bilateral trade flows with conservative screening procedures, are broadly consistent with these independent findings across methodologies.

Roadmap. Section 2 develops a partial equilibrium model of avoidance and passthrough. Section 3 describes the data and methodology used to measure transshipment, which Section 4 uses to present the main empirical results, including the magnitude of avoidance and the elasticity estimates. Section 5 uses these estimates to re-evaluate the tariff's welfare cost and conduct the TCJA counterfactual. Section 6 concludes.

2 A Model of Selection into Avoidance

This section presents the economic logic behind the paper's three main results: (i) costly tariff avoidance induces low-passthrough firms to exit the direct-shipping channel, (ii) this selection biases standard estimates of domestic incidence upward, and (iii) spillovers with the domestic tax system can discipline avoidance. Section 2.1 sets up the model and Section 2.2 characterizes the firm's routing decision and derives the passthrough bias. Section 2.3 shows how this bias manifests in standard regressions. Section 2.4 analyzes the corporate tax enforcement channel, and Section 2.5 shows how to map the selection discussion into parameters relevant for welfare.

2.1 Environment

Consider a static partial equilibrium setting with monopolistic competition over differentiated varieties indexed by $i \in \mathcal{J}$. Each exporter sets a price p_i and sells to a Home market that imposes a uniform ad valorem tariff $\tau^d \geq 0$ on direct shipments from the targeted

2. The bias affects studies that rely on bilateral trade flows classified by recorded country of origin at the border. When firms reroute through third countries, they exit the direct-shipping sample. This affects Amiti, Redding, and Weinstein (2019, 2020) and Fajgelbaum et al. (2020) and the unit-value components of Cavallo et al. (2021). Studies with cohort designs that track specific goods from true origin to destination avoid this bias (Flaaen, Hortaçsu, and Tintelnot 2020; Cavallo et al. 2021).

origin. The delivered price paid in Home is $\tilde{p}_i^d = (1 + \tau^d)p_i$. The firm may pay a fixed cost $F > 0$ to avoid the tariff. However, this entails its own cost. Firms which choose to avoid face an expected cost $\tau^a \in (0, \tau^d)$ per unit shipped. This reflects the expected value of enforcement: the probability of detection multiplied by penalties (fines, confiscation, legal costs). The key parameter is the wedge $\tau^d - \tau^a$: a larger wedge makes avoidance more attractive.[3]

The fixed cost F captures setup expenses for avoidance (e.g., establishing transshipment routes, contracting with intermediaries, relabeling). While this cost likely varies across products (a possibility explored in Appendix A.5), it is best interpreted as a sunk entry cost. This interpretation implies that the decision to reroute trade could exhibit significant hysteresis. Once the cost is paid and the network is built, firms may not revert to direct shipping even if tariffs are later removed. Furthermore, F may not be static; it could decline endogenously over time as firms learn optimal routing strategies and intermediaries specialize in avoidance services. If so, the long-run avoidance elasticity could be larger than implied by a model that treats F as fixed, suggesting that tariffs may erode the tax base more rapidly over the long term.

Domestic Importers. Domestic importers submit demand for each variety based on its own delivered price $\tilde{p}_i$ and on a price aggregator $\mathbf{P}$ parameterized by some set of parameters Θ. Formally, the demand for variety i takes the form

$$q_i = D_i(\tilde{p}_i \mid \mathbf{P}), \quad \mathbf{P} \equiv \mathbf{P}(\mathbf{P}_{-i}; \Theta),$$

$\mathbf{P}$ is exogenous at the firm level. This corresponds to a standard monopolistic competition setup in which each firm is small relative to the market and takes the price distribution of rivals as given. Denoting the elasticity of demand as $\varepsilon_i > 1$, I assume that demand is weakly Kimball (1995) with superelasticity

$$\kappa_i(\tilde{p}_i \mid \mathbf{P}) \equiv -\frac{\partial \ln \varepsilon_i(\tilde{p}_i \mid \mathbf{P})}{\partial \ln \tilde{p}_i} \leq 0$$

When $\kappa < 0$, elasticity rises with price, making high-markup firms face flatter residual demand. This generates heterogeneous passthrough, which is key to the selection mechanism. Under CES ($\kappa = 0$), all firms have identical passthrough regardless of cost, eliminating selection. Additional weak regularity conditions are imposed in Appendix

3. As a particular example, the August 2025 Executive Order 14326 called for additional taxes on any foreign exporters engaging in transshipping.

A.1.

Foreign Exporters. Each firm $i \in \mathcal{J}$ has a constant marginal cost m_i. Exporter marginal costs m_i are independently drawn from a continuous distribution with support $[\underline{m}, \bar{m}] \subset (0, \infty)$ and density $g(m) > 0$. The distribution is independent of policy and of the aggregator $\mathbf{P}$, and has no atoms.

Each firm must choose whether to pay a uniform fixed cost F to avoid the tariff via transshipment or absorb some part of the tariff and ship directly to the Home nation. The distribution of marginal costs $G(\cdot)$ determines the mass of firms on each side of the routing cutoff. Products with cost distributions concentrated at low m (high-markup goods) will exhibit more avoidance than products with high-cost distributions. Given $r \in \{d, a\}$, the firm solves

$$\pi_i(m_i; r) = \max_{p_i \geq m_i} \underbrace{(p_i - m_i) D_i(\tilde{p}_i^r \mid \mathbf{P})}_{\equiv \Phi_i(p_i; m_i, \tilde{p}_i^r)} - \mathbb{1}\{r = a\} F, \quad \tilde{p}_i^r = (1 + \tau^r) p_i.$$

Appendix A.2 establishes that, for any routing choice, the optimal price follows from a Lerner condition under which firms with higher marginal costs charge higher prices, sell lower quantities, and have lower markups. Under CES demand, elasticity is constant and there is no variation in markups or passthrough across firms. Under Kimball-type demand, however, elasticity rises with the delivered price, so there is heterogeneity in markups and hence passthrough.

2.2 Selection into Avoidance and Heterogeneous Passthrough

The model features two key elements that interact to generate selection bias: firms have heterogeneous marginal costs, and under Kimball demand, tariff passthrough rises with marginal cost. This means that low-cost firms, which operate with higher markups, have low passthrough. A low passthrough rate means these high-markup firms must absorb a larger share of the tariff themselves, cutting directly into their profit margins. This "price effect" hurts their operating profits more severely than the simple "quantity effect" (lost sales volume) faced by firms that can fully pass on the tariff. Therefore, it is precisely these low-cost, high-markup firms—the ones who would otherwise have to absorb the tariff—that have the strongest incentive to pay the fixed cost to avoid it altogether. Higher-cost firms, which can pass on most of the tariff, find it more profitable to continue shipping directly. This selection process is the key mechanism driving the paper's results.

We now formally characterize how firms sort between direct shipment and avoidance.

The only asymmetry across channels is that the tariff enters the delivered price in the direct channel. Given our primitives, the direct and avoidance pricing problems are well-behaved, so we can study avoidance by comparing their optimized values as functions of marginal cost m and the tariff.

For any marginal cost m, define the optimized value in a channel r with delivered-price multiplier $1+\tau^r$:

$$V(m, 1+\tau^r) \equiv \max_{p \geq m} (p-m) D((1+\tau^r)p), \quad \pi^d(m) = V(m, 1+\tau^d), \quad \pi^a(m) = V(m, 1+\tau^a) - F,$$

and the avoid–direct difference

$$H(m; \tau^d, \tau^a) \equiv \pi^a(m) - \pi^d(m) = V(m, 1+\tau^a) - V(m, 1+\tau^d) - F.$$

Proposition 1 (Cutoff routing and wedge comparative statics)**.** *Under D1–D4 (see A.1), there exists a unique cutoff $\widehat{m}(\tau^d, \tau^a) \in [\underline{m}, \bar{m}]$ such that*

$$m < \widehat{m}(\tau^d, \tau^a) \Rightarrow \textit{Avoid}, \quad m \geq \widehat{m}(\tau^d, \tau^a) \Rightarrow \textit{Direct}.$$

If the cutoff is interior, then

$$\frac{\partial \widehat{m}}{\partial \tau^d} > 0 \quad \textit{and} \quad \frac{\partial \widehat{m}}{\partial \tau^a} < 0.$$

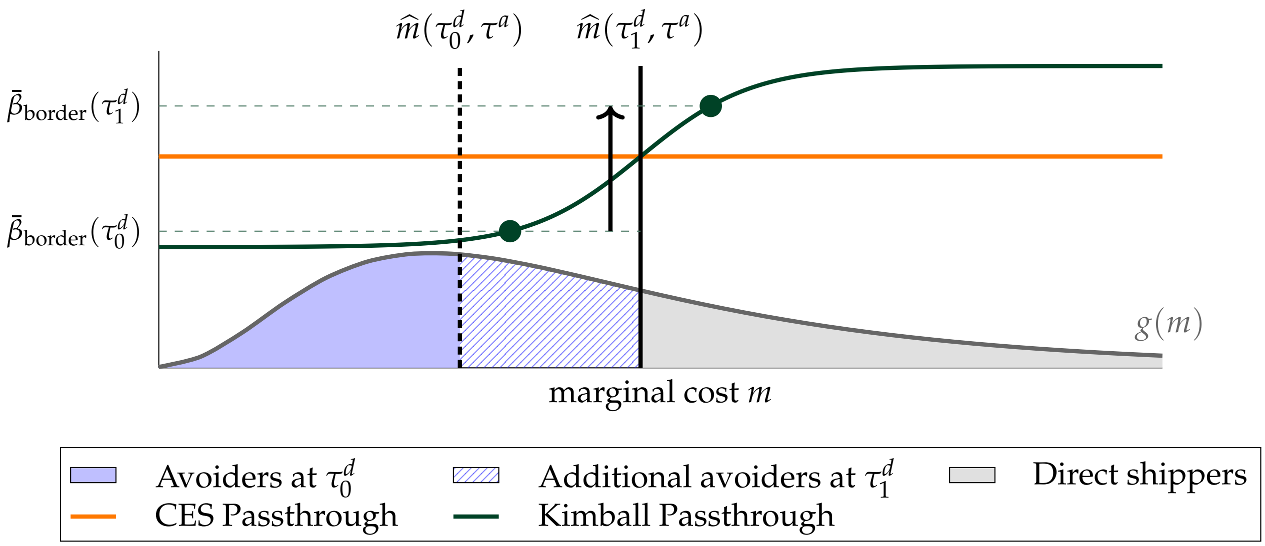
Proof: See Appendix A.3.

Let $\theta(\tau^d, \tau^a) \equiv \Pr(m < \widehat{m}(\tau^d, \tau^a))$. By the signs in Proposition 1, $\partial\theta/\partial\tau^d > 0$ and $\partial\theta/\partial\tau^a < 0$: compressing the direct–avoidance wedge reduces avoidance. The threshold $\widehat{m}(\tau^d, \tau^a)$ partitions the cohort into two routing groups: firms with $m < \widehat{m}(\tau^d, \tau^a)$ optimally pay F and choose the avoidance channel, while firms with $m \geq \widehat{m}(\tau^d, \tau^a)$ continue to ship direct. The reason is straightforward. As a firm's marginal cost m rises, its per-unit margin $(p^* - m)$ shrinks, equilibrium quantity falls, and overall operating profits decline in both channels.

The decline, however, is not symmetric. The direct channel's value, $V(m, 1+\tau^d)$, is diminished more severely than the avoidance channel's value, $V(m, 1+\tau^a)$, because the direct wedge is larger ($\tau^d > \tau^a$). For low-cost, high-volume firms, the absolute profit loss from facing the higher direct wedge is substantial, making it worthwhile to pay the fixed cost F to access the lower-wedge avoidance channel. For higher-cost firms, volumes and margins are smaller, so the incremental gain from avoiding the higher wedge is not large enough to justify the fixed cost. This monotone ordering in m generates the unique cutoff. As Proposition 1 shows, widening the gap between the wedges—by increasing τ^d

or decreasing τ^a—shifts the cutoff $\widehat{m}$ to the right, increasing the share of firms that choose to avoid.

Figure 1: Passthrough $\beta(m)$ under CES and Kimball Demand



Note: The distribution $g(m)$ shows the density of marginal costs. Under CES (flat orange line), passthrough is constant. Under Kimball (upward-sloping green line), passthrough increases with cost. The avoidance cutoff $\widehat{m}(\tau^d, \tau^a)$ partitions the distribution: low-cost firms (blue shaded area) avoid, while high-cost firms ship directly. A tariff increase shifts the cutoff rightward (dashed to solid line), causing additional low-passthrough firms to exit, mechanically raising average passthrough among survivors. This upward selection bias is visualized on the y-axis, where the average passthrough of the survivor group rises from $\bar{\beta}_{\text{border}}(\tau_0^d)$ to $\bar{\beta}_{\text{border}}(\tau_1^d)$.

Since a wider wedge gap pushes low-cost firms into the avoidance channel, the average characteristics of the remaining firms that ship direct are altered. To see why this biases measured passthrough, I now characterize how passthrough varies with marginal cost.

Proposition 2 (Passthrough heterogeneity)**.** *Holding* **P** *fixed, the passthrough of tariffs onto the delivered price of variety i is*

$$\beta_i \equiv \frac{d \ln \tilde{p}_i^d}{d\tau^d} = \frac{1}{1+\tau^d} \cdot \frac{\varepsilon(\tilde{p}_i^d) - 1}{\varepsilon(\tilde{p}_i^d) - 1 - \kappa(\tilde{p}_i^d)}, \tag{1}$$

where $\tilde{p}_i^d = (1+\tau^d)p_i^*(m_i; d)$. When $\kappa = 0$ (CES), passthrough is homogeneous across firms. When $\kappa < 0$ (Kimball), passthrough is monotonically increasing in marginal cost: $\partial\beta_i/\partial m_i > 0$.
Proof: See Appendix A.4.

Equation (1) decomposes passthrough into a mechanical factor $1/(1+\tau^d)$ and a curvature factor $(\varepsilon-1)/(\varepsilon-1-\kappa)$. Under CES ($\kappa = 0$), elasticity is constant and all firms have identical passthrough. Under Kimball demand ($\kappa < 0$), elasticity rises with the delivered price, so low-cost firms (who charge high prices) face high demand elasticity and must compress markups when tariffs rise, resulting in low passthrough. From Proposition 1, when τ^d increases, the avoidance cutoff shifts right and low-cost, low-passthrough firms exit the direct-shipping sample. This mechanically raises average passthrough among survivors, generating the bias formalized in the next section.

Figure 1 illustrates the mechanism. We take a distribution of marginal costs and overlay two lines: a flat tangerine line corresponding to homogeneous markups under CES demand, and an upward-sloping green line corresponding to heterogeneous markups under Kimball demand. At any given wedge gap, survivors are the high-m types with (weakly) higher passthrough. As the tariff τ^d rises, selection tilts the survivor set toward these high-β types, even though each individual firm's passthrough, β_i, can fall mechanically with τ^d. This selection has a first-order effect on measured passthrough.

The magnitude of this behavioral response is a key parameter for welfare analysis, captured by the avoidance elasticity, ψ. We define this as the elasticity of the avoidance share θ with respect to the gross tariff wedge factor, $(1+\tau^d)/(1+\tau^a)$:

$$\psi \;\equiv\; \frac{\partial \ln \theta}{\partial \Delta} \;>\; 0, \quad \Delta \equiv \ln \frac{1+\tau^d}{1+\tau^a} \tag{2}$$

This theoretical parameter, which in the model reflects the extensive margin of firms switching into avoidance, is the counterpart to the empirical elasticity estimated in Section 4. The empirical measure captures changes along both the intensive (volume) and extensive (participation) margins of transshipment, and it serves as a crucial input for quantifying the fiscal externality when we re-evaluate the tariff's welfare cost in Section 5.

2.3 Selection in Passthrough Regressions

Propositions 1 and 2 establish that (i) low-cost firms select into avoidance and (ii) low-cost firms have low passthrough. Combined, these results imply that standard passthrough regressions are biased upward. This section formalizes the bias and derives a correction.

Many influential studies estimate passthrough using "border designs," which rely on customs unit values or aggregate import price indices constructed from direct shipments. The central issue with this approach is that the data-generating process itself is subject to the selection mechanism. By construction, these datasets only capture transactions from firms that continue to ship directly after the tariff change. The firms that select into avoidance are unobserved and mechanically drop out of the estimating sample. This creates a gap between the estimand from the regression and the true cohort-level parameter of interest.

To formalize the resulting bias, let $S_i \in \{0,1\}$ be the survivor indicator in the post-period ($S_i = 1$ if i ships direct, 0 if i avoids), and let β denote passthrough. Consider a regression run on data on imported goods from the targeted country:

$$\Delta^{\text{chg}} \ln \tilde{p}_i = \alpha + \beta \, \Delta^{\text{chg}} \tau_i^d + \varepsilon_i, \tag{3}$$

where $\tilde{p}_i = (1+\tau^d)p_i$ is the delivered price and $\Delta^{\text{chg}}\tau^d$ is the policy change.[4] Since $\Delta^{\text{chg}} \ln \tilde{p}_i = \beta_i \, \Delta^{\text{chg}} \tau^d$ for a direct shipper i, the OLS coefficient in (3) run on survivors identifies the survivor average:

$$\widehat{\beta}_{\text{border}} \;=\; \mathbb{E}[\beta_i \mid S_i = 1].$$

Our cohort object of interest is $\beta_{\text{cohort}} \equiv \mathbb{E}[\beta_i]$, the average direct-channel passthrough for the pre-tariff cohort. The following identity makes the selection bias transparent.

Proposition 3 (Border vs. cohort bias)**.** *Let* $\Pr(S_i = 1)$ *be the survivor share. Then*

$$\beta_{border} \;\equiv\; \mathbb{E}[\beta_i \mid S_i = 1] \;=\; \underbrace{\mathbb{E}[\beta_i]}_{\beta_{cohort}} + \frac{\operatorname{Cov}(\beta_i, S_i)}{\Pr(S_i = 1)}. \tag{4}$$

Proof. $\mathbb{E}[\beta_i \mid S_i = 1] = \mathbb{E}[\beta_i S_i] / \mathbb{E}[S_i] = \big(\mathbb{E}[\beta_i]\mathbb{E}[S_i] + \operatorname{Cov}(\beta_i, S_i)\big) / \Pr(S_i = 1)$. □

The selection term $\frac{\operatorname{Cov}(\beta_i, S_i)}{\Pr(S_i=1)}$ is positive whenever low-passthrough firms are more likely to avoid (as in Proposition 1 under Kimball demand). This means border estimates $\widehat{\beta}_{\text{border}}$ systematically overstate the true cohort average β_{cohort}. Importantly, the bias is zero under CES demand ($\kappa = 0$) because all firms have identical passthrough, so $\operatorname{Cov}(\beta_i, S_i) = 0$. This clarifies why cohort-design studies like Flaaen, Hortaçsu, and Tintelnot (2020) find near-complete passthrough for washing machines: durable goods may have high fixed costs of transshipment, pushing most firms above the avoidance cutoff.

When does selection create bias? The selection term in (4) applies whenever the regression outcome is observed only for direct shippers or aggregated from their transactions. Under Kimball demand and Proposition 1, both β_i and S_i increase in marginal cost

4. In practice, most empirical work relies on much richer specifications, with variation across units in a number of dimensions. That is irrelevant for the broader point about selection.

m, generating positive covariance. Low-cost firms charge high markups and have low passthrough. They must absorb tariff increases to maintain market share. These firms are most likely to pay the fixed cost to avoid tariffs. When they exit the direct-shipping sample, average passthrough among survivors rises, biasing border estimates upward.

Two benchmarks clarify when bias matters. Under CES ($\kappa \equiv 0$), all firms have identical passthrough $\beta_i = (1+\tau^d)^{-1}$, so $\text{Cov}(\beta_i, S_i) = 0$ and $\beta_{\text{border}} = \beta_{\text{cohort}}$. Under Kimball demand, passthrough increases with cost. Low-cost, low-passthrough firms avoid, creating $\text{Cov}(\beta_i, S_i) > 0$ and upward bias. Given heterogeneous markups in practice (Edmond, Midrigan, and Xu 2023), border-based regressions likely overstate the burden on domestic consumers.

Border-level regressions using customs unit values or aggregate price indices estimate β_{border} by construction, since transshippers exit when tariffs induce origin switching. This is the estimand in Amiti, Redding, and Weinstein (2019), Fajgelbaum et al. (2020), and the unit-value components of Cavallo et al. (2021).

When is there no selection bias? When firm–product transactions are followed across routing with the cohort fixed at the micro level, there is no selection bias. This requires observing the same exporter–importer unit before and after the tariff whether it ships direct or avoids. Parts of Cavallo et al. (2021) and Flaaen, Hortaçsu, and Tintelnot (2020) implement this and therefore suffer no bias. The findings from these studies provide a real-world validation of this paper's core mechanism.

For instance, in their analysis of washing machine tariffs, Flaaen, Hortaçsu, and Tintelnot (2020) study two different tariff regimes. For the 2018 global tariff, which made avoidance prohibitively expensive, they find near-complete passthrough, with prices rising by more than 100% . This is consistent with my model's prediction for a scenario with a high cost of avoidance. However, for earlier country-specific tariffs, they document extensive "country-hopping" where firms used relocation as a low-cost avoidance strategy, resulting in minimal or even negative passthrough. The washing machine case therefore serves as a compelling natural experiment for my model: when avoidance is costly, passthrough is high; when it is cheap, passthrough is low. This points to a key heterogeneity—the cost of avoidance—which we will analyze later in the empirics.

A portable correction. The magnitude of the selection bias can be bounded directly. By the law of total expectation,

$$\beta_{\text{cohort}} = (1-\theta)\mathbb{E}[\beta_i | S_i = 1] + \theta \mathbb{E}[\beta_i | S_i = 0].$$

Using $\beta_{\text{border}} \approx \mathbb{E}[\beta_i | S_i = 1]$ and non-negative passthrough,

$$\beta_{\text{cohort}} \in [(1-\theta)\beta_{\text{border}}, \beta_{\text{border}}].$$

This bounds the bias using only the avoidance share θ, which we measure in Section 4.[5] The correction shows that correcting measured incidence requires quantifying how much avoidance occurs. But the welfare analysis also depends on understanding what drives avoidance in the first place because the tariff base shrinks as firms respond to incentives. We now show that domestic tax policy provides a powerful, and previously unrecognized, lever for controlling this response.

2.4 Corporate Tax as Enforcement

When U.S. firms import intermediate or capital goods, they deduct the tariff-inclusive cost from taxable income. This deductibility compresses the effective wedge between direct and avoided shipments, making avoidance less attractive. This reveals a novel enforcement margin: the corporate tax code acts as an implicit penalty on avoidance, with higher corporate tax rates τ^c or greater deductibility z reducing the incentive to transship. I formalize this channel and show it provides a quantitatively significant enforcement mechanism.

Tax Deductibility and the Avoidance Wedge. Suppose the importer faces corporate tax rate $\tau^c \in [0, 1)$ with partial deductibility $z \in [0, 1]$ of tariff-inclusive input costs, where z is the net present value of a dollar of deductions. For intermediate inputs, $z = 1$; for capital expensed over time, $z < 1$. After-tax unit costs in each channel are

$$C_d = (1-\tau^c z)(1+\tau^d)p, \qquad C_a = (1-\tau^c z)p,$$

yielding a cost gap

$$\Delta C_{\text{ded}} \;=\; (1-\tau^c z)\,\tau^d\,p.$$

In the general two-wedge model, the gap is $\Delta C_{\text{wedge}} = (\tau^d - \tau^a)p$. Equating these yields a simple isomorphism.

5. Formally, this bound is the sufficient-statistics analogue of the share function $F((1-\gamma)\tau V - C)$ in Fisman, Moustakerski, and Wei (2008). In my model, θ plays the role of their aggregate evasion share, but the key innovation is that θ also determines the sample-selection bias in measured passthrough and thus enters welfare analysis through the MCPF.

Corollary 1 (Deductibility–Wedge Isomorphism)**.** *When imported inputs are deductible, the routing problem is behaviorally equivalent to a two-wedge problem with effective avoidance wedge*

$$\tau^a = z\tau^c \, \tau^d.$$

With no deductibility, $\tau^a = 0$.

The isomorphism implies that corporate tax policy and trade enforcement are intrinsically linked. A higher corporate tax rate τ^c or greater expensing z effectively raises the avoidance wedge τ^a, discouraging transshipment. Conversely, cutting corporate taxes—as in the 2017 TCJA—lowers implicit enforcement, increasing avoidance. Section 5 quantifies this effect, showing that the TCJA rate cut from 35% to 21% reduced tariff revenue by \$2–\$9 billion through increased transshipment.

Combining Corollary 1 with Proposition 1 turns domestic tax policy into clean comparative statics:

$$\frac{d\widehat{m}}{d\tau^c} < 0 \quad \text{and} \quad \frac{d\widehat{m}}{dz} < 0.$$

Higher corporate tax rates or greater deductibility reduce avoidance. The cutoff $\widehat{m}$ shifts left, the survivor set expands, and the marginal firm is less inclined to avoid because deductibility effectively loads a share $z\tau^c$ of the direct tariff into the avoidance channel. Thus, domestic tax policy spills over into the enforcement of international tax policy.

2.5 Taking Stock: The Marginal Cost of Public Funds

The theoretical results clarify how selection into avoidance affects the welfare cost of tariffs. As established in the introduction, the MCPF combines domestic incidence (numerator) and the fiscal externality from behavioral responses (denominator). I now show how the model's parameters map into this formula.

Numerator: Domestic incidence. Hendren (2016) shows that the welfare effect of a tax is the willingness to pay to avoid it. Applying the envelope theorem, Finkelstein and Hendren (2020) discuss this is simply the domestic incidence, or passthrough, of a tariff. Proposition 3 shows that the true cohort passthrough is bounded below by $(1-\theta)\beta_{\text{border}}$ and above by β_{border}. This provides a direct, portable correction for the numerator of the MCPF.

Denominator: Fiscal externality. The total fiscal externality, η_{total}, is the elasticity of the tax base with respect to the tariff. It has two components: the externality from standard behavioral responses like substitution, which we can call η_{other}, and the novel externality from selection into avoidance, η_{avoid}. The total externality is the sum: $\eta_{\text{total}} = \eta_{\text{other}} + \eta_{\text{avoid}}$. The avoidance externality, η_{avoid}, depends on how rapidly the tariff base (the share of firms shipping directly, $1-\theta$) shrinks as the effective wedge $\Delta = \ln\frac{1+\tau^d}{1+\tau^a}$ increases. This is a function of the avoidance elasticity, $\psi \equiv \frac{\partial\theta}{\partial\Delta}\frac{1}{\theta}$, which this paper estimates.[6]

Combining these components, welfare bounds can be written as:

$$\text{MCPF} \in \left[\frac{\beta_{\text{border}}(1-\theta)}{1+\eta_{\text{total}}}, \frac{\beta_{\text{border}}}{1+\eta_{\text{total}}}\right] = \left[\frac{\beta_{\text{border}}(1-\theta)}{1+\eta_{\text{other}} - \frac{\theta}{1-\theta}\psi}, \frac{\beta_{\text{border}}}{1+\eta_{\text{other}} - \frac{\theta}{1-\theta}\psi}\right], \tag{5}$$

where η_{other} captures all other fiscal externalities (e.g., substitution to domestic goods), which is taken as given from other studies. A larger ψ increases the MCPF through the denominator. When firms are highly responsive to the effective wedge, the base $1-\theta$ erodes rapidly, so raising rates generates less revenue per unit of welfare cost—that is, the standard Laffer-curve logic.

This expression clarifies what must be measured. To correct the numerator, we must estimate the avoidance share θ. To correct the denominator, we need the avoidance elasticity ψ. Section 5 combines these estimates to re-evaluate the MCPF.

3 A Methodology for Identifying Transshipment

The theoretical framework established two empirical tasks required to evaluate the tariff's true welfare cost: measuring the level of avoidance θ to correct the bias in the MCPF's numerator, and estimating the elasticity of avoidance to analyze the denominator's fiscal externality. This section and the next are dedicated to these two tasks, focusing on transshipment as a key, measurable channel of avoidance. We begin by developing a methodology to identify the level of transshipment in the data.

I use the 2018 tariffs applied on China by the United States to identify transshipment by extending the methodology of Freund (2024) to construct a bounded measure of the share of goods originating from China but transshipped through third countries to the United States over the period 2012-2023.

6. The fiscal externality from avoidance, η_{avoid}, is the elasticity of the tax base (the survivor share $1-\theta$) with respect to the effective wedge:

$$\eta_{\text{avoid}} \equiv \frac{\partial(1-\theta)}{\partial\Delta}\frac{1}{1-\theta} = -\frac{\partial\theta}{\partial\Delta}\frac{1}{1-\theta} = -\frac{\theta}{1-\theta}\left(\frac{\partial\theta}{\partial\Delta}\frac{1}{\theta}\right) = -\frac{\theta}{1-\theta}\psi.$$

3.1 Data

I use annual data on bilateral trade flows from CEPII's BACI HS-12 vintage from 2012-2023 at the 6-digit product (HS6) level. For each year y, country pair (i, j), and HS6 code k, we observe the customs value of shipments $v_{i\to j,k,t}$. Our goal is to discern when goods originate in China and go to another country j on the first leg, and then arrive in the United States as the destination in the second leg. We restrict the intermediate countries i on the second leg to a list of 36 potential transshipment hubs. The hubs are selected for geographic plausibility and the mutual importance for them in trade between the US and China before the trade war. I deflate the value of shipments to 2017 dollars using the end-use import price index for all commodities from the Bureau of Labor Statistics.

While the model in Section 2 characterizes firm-level routing decisions, the empirical analysis operates at the HS6 product level where trade flows aggregate across potentially many exporters. Appendix B shows how the firm-level selection mechanism manifests in observable product-level statistics. The key result is that product-level transshipment shares θ_k reflect the mass of firms below the routing cutoff, and the compositional effects predicted by Propositions 1-2 (exiters have lower costs, lower passthrough, and lower prices) aggregate to observable correlations between θ_k and product-level quantities and unit values. This justifies using HS6-level variation to test the model's predictions.

The tariff wedge is specified as the log ratio of the gross cost factor for direct shipments versus the effective cost factor for avoided shipments. This formulation directly operationalizes the theory, which models the firm's routing decision as a choice between two effective price multipliers, $(1 + \tau^d)$ for direct shipping and $(1 + \tau^a)$ for avoidance, making their ratio the key incentive to transship:

$$\Delta_{k,t} \equiv \log \frac{1 + \tau^d_{k,t}}{1 + \tau^a_{k,t}} = \log \frac{1 + \tau^d_{k,t}}{1 + \tau^d_{k,t} \times \tau^c_t z_{k,t}}.$$

To construct this wedge, we need data on its two components: the direct tariff wedge, τ^d, and the avoidance wedge, $\tau^a = z\tau^c\tau^d$, which Corollary 1 shows is a function of domestic tax policy. I rely on Amiti, Redding, and Weinstein (2020) for a panel of monthly HS10-by-product China-specific tariff rates to construct the direct wedge, $1 + \tau^d_{k,t}$ up until 2017. I aggregate these to the annual, HS6-product level by computing a weighted average, using 2017 import shares as weights. From 2018-2023, I use the Global Tariff Database from Teti (2025), which has bilateral HS6 tariffs. As the model shows, however, the direct tariff is only one piece of the puzzle; the avoidance wedge, τ^a, requires inputs from domestic tax policy and varies by the good's end use.

I use the UN's classification by broad economic categories (BEC) to determine whether each import is a consumption, intermediate, or capital good. For consumption and intermediate goods, which are fully expensed ($z_k = 1$), Corollary 1 implies the avoidance wedge is simply $\tau_t^d \times \tau_t^c$.[7] The Internal Revenue Service places most capital goods into one of several tax lives according to the Modified Accelerated Cost Recovery System (MACRS). Let $z_{k,t}$ denote the net present value of statutory depreciation deductions for an imported capital good k in year t.[8] For these goods, the avoidance wedge is $\tau_{k,t}^a = z_{k,t}\tau_t^c\tau_{k,t}^d$. I map each capital good into its corresponding IRS tax life and calculate $z_{k,t}$ accordingly. Figure C.1 plots the interquartile range of the resulting tariff wedge, $\Delta_{k,t}$, for each end-use category.

To handle goods which enter multiple end-use categories, I stack them on top of each other and weight all resulting outcomes according to the BEC's weights.

3.2 Methodology

Measuring transshipment is a non-trivial task, as international trade avoidance is inherently difficult to detect. Rather than relying on nonexistent audit data, I infer the degree of transshipment by extending a methodology from Freund (2024). The goal of this approach is not to capture the full, pre-existing level of structural transshipment, but rather to identify the change in rerouting behavior specifically induced by the 2018 tariffs. The plausibility of such a large-scale shift is supported by recent work from Do et al. (2025), who use bill-of-lading data to show that the majority of US imports are already transshipped through a vast, pre-existing network of global hubs. My methodology therefore aims to measure how firms leveraged this existing infrastructure for tariff avoidance.

The core idea is to identify a specific "trade footprint" of tariff avoidance by setting rigorous screens on bilateral trade data. To be flagged as transshipment, a trade flow must simultaneously satisfy five conditions that, together, paint a compelling picture of rerouting. Intuitively, we are looking for products where (1) the U.S. tariff on China increased; (2) China's market share in the U.S. subsequently fell while a specific hub country's share rose; (3) China's global competitiveness in that product remained stable; and (4) the new

7. The effective rate may differ from statutory due to firm-level tax planning, so the wedge may not perfectly capture heterogeneous incentives. Similarly, not all firms face the corporate tax rate because they are passthrough entities like partnerships or S-corporations electing into passthrough status.

8. Suppose a firm discounts the future at rate r, an asset has a tax life of T years, and a share d_s of it can be deducted in year $s = 0, \ldots, T$. The present value of one dollar of deductions is $z = \sum_{s=0}^{T} \left(\frac{1}{1+r}\right)^s d_s$. With bonus depreciation, firms can deduct an extra fraction b upon purchase, so the NPV of deductions becomes $z_{\text{bonus}} = b + (1-b)z$. With 100% bonus depreciation, as was the case after the TCJA, $b = 1$.

trade flows are quantitatively large enough for the China-to-hub shipments to plausibly supply the hub-to-U.S. shipments.

My approach makes three key improvements to the baseline methodology in Freund (2024). I (i) measure deviations relative to pre-period trends rather than in levels, (ii) impose a within-product cap to prevent double-counting across hubs, and (iii) calibrate quantitative thresholds on placebo products to control the false positive rate.

Preliminaries. The screening methodology relies on comparing observed trade patterns to counterfactual predictions based on pre-tariff trends. To generate these predictions, I first define the two potential legs of diversion for each HS6 product k and hub i in year t as:

- First leg (China $\rightarrow$ hub): $m_{k,i,t} \equiv v_{\text{CHN}\rightarrow i,k,t}$
- Second leg (hub $\rightarrow$ United States): $x_{k,i,t} \equiv v_{i\rightarrow \text{USA},k,t}$.

To disentangle unusual growth from underlying trends, I estimate product- and partner-specific trends in the pre-tariff period (2012-2017) and use them to form counterfactuals. This analysis is restricted to a pre-defined set of potential transshipment hubs (listed in the Appendix) and China; all other trading partners are excluded.

Since the screening criteria (detailed below) involve both market shares and trade flow levels, I estimate pre-period trends for both types of variables. For each HS6 product k and relevant partner i (China or a hub):

- Market Share Trends: Trends for market shares (specifically, China's and the hub's share of U.S. imports, and their respective shares of exports to the Rest of World) are estimated using a logistic regression of the share on a centered year index. These yield predicted shares import shares later used in U.S. Reallocation and ROW Specificity screens below.

- Trade Level Trends: I also compute trends for the absolute value of the trade legs ($m_{k,i,t}$ and $x_{k,i,t}$) using a log-linear regression of the value on the same centered year index. These yield predicted levels ($\widehat{m}_{k,i,t}$ and $\widehat{x}_{k,i,t}$).

The predictions from the level trends allow us to compute the "surprise" growth relative to the counterfactual trend for each leg:

$$\tilde{m}_{k,i,t} \equiv \max\{m_{k,i,t} - \widehat{m}_{k,i,t}, 0\} \quad \text{and} \quad \tilde{x}_{k,i,t} \equiv \max\{x_{k,i,t} - \widehat{x}_{k,i,t}, 0\}.$$

These surprise components are used in the growth-based quantitative consistency screen

Screening Criteria. With these preliminaries, I impose five screens to detect transshipment.

1. **Tariff Exposure.** The product must have been exposed to U.S. tariff increases on China in 2018 or 2019.

2. **U.S. Reallocation.** China's share of U.S. imports for the product must fall below its pre-trend while the hub's share rises above its pre-trend.

3. **Rest-of-World Specificity.** The growth in China's export share to the rest of of the world above its pre-trend must be at least as large as the hub's growth above its pre-trend.

4. **Tariff Feasibility.** In order to qualify as transshipment, the exporter must have a reason for it to be rerouted. Therefore, the rerouting country's tariff rate on that specific good must be lower than the tariff applied by the U.S. on China. This ensures the calibration relies only on economically plausible channels.

5. **Quantitative Consistency.** Using non-treated products as a calibration target, I set a final *low* (conservative) and a *high* (liberal) condition to ensure the China-to-hub flow is large enough to supply the hub-to-U.S. flow.

   (a) **Low (growth-based):** The above-trend increase in China's exports to the hub must be sufficiently proportional to the above-trend increase in the hub's exports to the U.S.: $\tilde{m}_{k,i,t} \geq \tau^* \max(\tilde{x}_{k,i,t}, x_{\text{hinge},\Delta})$. The flagged quantity is the bottleneck, $\min(\tilde{m}_{k,i,t}, \tilde{x}_{k,i,t})$.

   (b) **High (levels-based):** The trade legs must be of a minimum size and proportional in levels: $\min(m_{k,i,t}, x_{k,i,t}) \geq x_{\text{hinge,levels}}$ and $m_{k,i,t} \geq \phi^* x_{k,i,t}$. The flagged quantity is the bottleneck, $\min(m_{k,i,t}, x_{k,i,t})$.

   The thresholds τ^* and ϕ^* are chosen from a pre-defined grid via a calibration procedure designed to limit the false positive rate. Specifically, we select the smallest values in their respective grids (prioritizing τ^*, then ϕ^*) such that the implied transshipment share for a placebo group of never-treated products does not exceed 1% in *any* single pre-tariff year. This calibration yields baseline values of $\tau^* = 0$ and $\phi^* = 0.325$. The hinge constants ($x_{\text{hinge},\Delta}$, $x_{\text{hinge,levels}}$) and the share tolerance (ϵ) are set to small, fixed values. The levels-based rule (5b) is more liberal because it can flag pre-existing trade routes, while the growth-based rule (5a) only captures newly formed or sharply expanding routes. I use these two rules to compute bounds on transshipment, denoted θ_s, for $s \in \{\text{Low, High}\}$.

After flagging all qualifying (k, i, t) triplets, the final product-level measure θ_s is constructed using a precise order of operations. For each product k and year t, the flagged nominal values are summed across all passing hubs. After deflating to 2017 dollars, this total is capped so it cannot exceed the nominal value of direct China-to-US imports for product k in 2017. As a final step, any single-year spikes during the post-2018 period are set equal to zero to reduce noise from temporary fluctuations; flagged transshipment must occur for at least two years to count. Summary statistics are in Table C.3.

Scope and Interpretation. It is important to emphasize the scope of this measurement exercise. The screening methodology identifies *transshipment*—the physical rerouting of goods through third countries—as one channel within a broader portfolio of tariff avoidance strategies. Firms facing tariffs can respond through multiple margins: misreporting value to qualify for de minimis treatment (Fajgelbaum and Khandelwal 2024), breaking shipments into smaller parcels, misclassifying products into lower-tariff categories (Fisman and Wei 2004), quality downgrading, or permanently relocating production. Each strategy involves different fixed and variable costs, and firms may substitute across them in response to enforcement intensity or relative prices.

My estimates represent a conservative lower bound on total avoidance for two reasons. First, transshipment is only one channel within a broader portfolio: firms can also misreport values to qualify for de minimis treatment (Fajgelbaum and Khandelwal 2024), misclassify goods into lower-tariff categories (Fisman and Wei 2004), downgrade quality, or permanently relocate production. If firms substitute toward these margins when transshipment becomes costly, the measured elasticity of transshipment understates the true elasticity of total avoidance.[9] Second, the screens deliberately minimize false positives. The growth-based rule (Screen 4a) flags only newly formed or sharply expanding routes, missing pre-existing infrastructure that scales up gradually.

4 Measured Transshipment

The theoretical framework established the two key parameters needed to evaluate the welfare cost of tariffs: the level of avoidance θ and the avoidance elasticity. This section

9. The welfare implications depend critically on substitution elasticities across avoidance margins. If channels are highly substitutable, single-margin enforcement policies (e.g., stricter transshipment audits) will prove largely ineffective at restoring the tariff base.

provides empirical estimates for both. While the model in Section 2 is characterized at the firm level, its predictions about selection and passthrough aggregate to the observable product-level trade flows used in the analysis that follows (see Appendix B for a detailed discussion).

4.1 Identification: Event Study Evidence

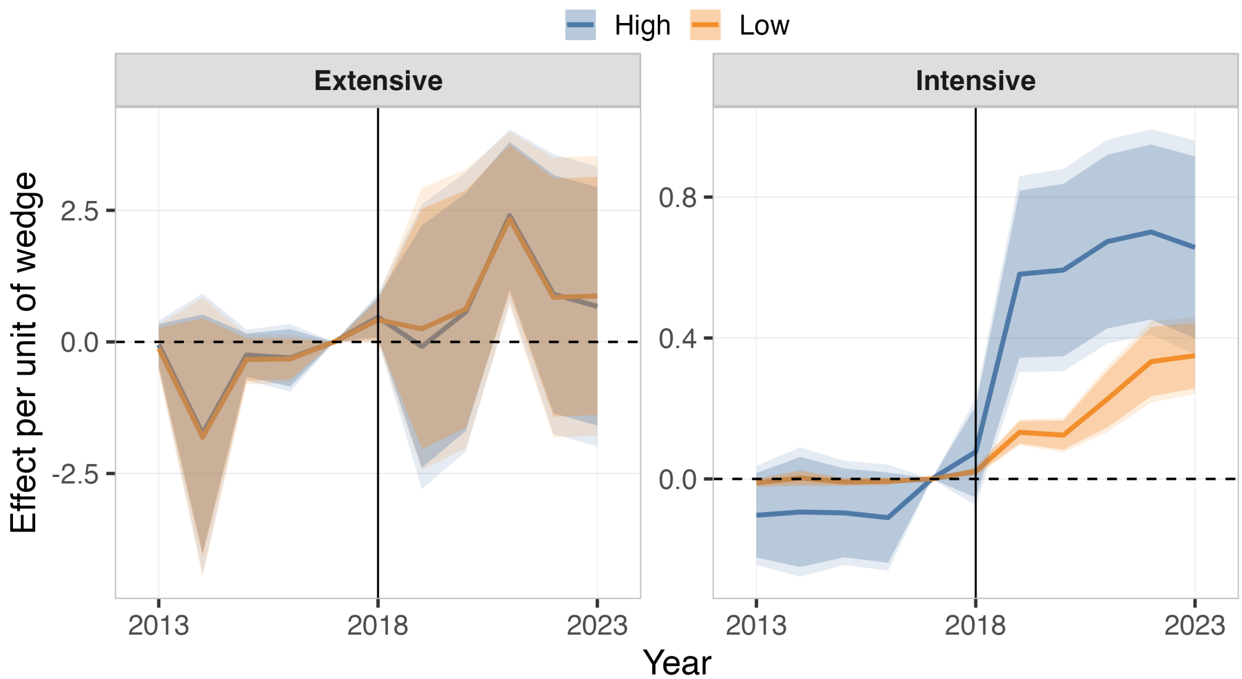
Before presenting the magnitude of transshipment, I first establish that the relationship between tariff exposure and avoidance is causal. Figure 2 plots coefficients from an event study that interacts annual event time with each product's fixed exposure to the tariff wedge. For each HS6 code k, I define a time-invariant exposure measure as the product's effective wedge in 2019, $\Delta_{k,2019}$.[10] The specification is

$$f(\theta_{k,t}) = \alpha_k + \delta_{t\times e(k)} + \sum_{r\neq -1} \beta_r \, \mathbf{1}\{\text{rel}_t = r\} \times \Delta_{k,2019} + \varepsilon_{k,t}, \tag{6}$$

where $\text{rel}_t \in \{-5, \ldots, -1, 0, 1, \ldots, 5\}$ denotes years relative to 2018 (with $\text{rel} = -1$, i.e. 2017, as the omitted reference), α_k are HS6 fixed effects, and $\delta_{t\times e(k)}$ are year$\times$end-use fixed effects. The outcome $f(\theta_{k,t})$ is either the inverse hyperbolic sine of the transshipment share or an indicator for positive transshipment. Observations are weighted by 2017 China $\rightarrow$ US import shares and standard errors are clustered by HS6 code. I run the regressions separately for both the high and low measures of transshipment.

10. I anchor the exposure variable at its 2019 level so that coefficients measure the dynamic response per unit of the long-run tariff wedge rather than per unit of the time-varying contemporaneous exposure. This makes the event-study coefficients interpretable as the timing and magnitude of adjustment among more-exposed products relative to less-exposed ones. This is robust to anchoring at different periods (Figure S1). I choose 2019 because it is the first year of full tariff implementation; the tariffs were partial during 2018.

Figure 2: Transshipment Event Study Around 2018 Tariffs



Note: Coefficients from (6) with HS6 and year×end–use fixed effects, weighted by 2017 exposure and clustered by HS6. The y–axis reports effects per unit of exposure for the intensive margin (right) and extensive margin (left) along with both 90% and 95% confidence intervals. The orange lines are for the conservative screen, while blue is for liberal. Pre–policy leads are flat; effects begin in 2018 and persist.

Each coefficient β_r measures the change in transshipment in calendar year $2018 + r$ relative to 2017 per unit of the product's exposure to the tariff wedge. The results show visually flat and statistically insignificant pre-trends from 2013 through 2017 for all four outcomes. Block-permutation tests (Table D.2) largely confirm the absence of confounding pre-period trends. For three of the four measures, including both extensive margin outcomes and the liberal intensive margin, we find no statistically significant difference in pre-period transshipment levels between treated and placebo products (all Holm-adjusted p-values > 0.2). The conservative intensive margin ($sinh^{-1}(\theta_{low})$) shows a small but statistically significant positive gap (P(Holm) = 0.005). As the visually flat pre-trends in Figure 3 suggest, the post-2018 surge represents a clear structural break rather than a continuation of pre-existing divergent trends. A battery of pretrend tests in the Online Appendix support that result.

Beginning in 2018, both margins respond sharply and positively. The intensive margin rises immediately after 2018 and remains elevated through 2023, while the extensive margin shows a discrete jump in 2018-2019 that persists in subsequent years. The discrete post-2018 break coincides precisely with the implementation of Section 301 tariffs, and the persistence of effects through 2023 indicates that transshipment represents a stable behavioral response rather than a temporary adjustment. This persistence is consistent with the hysteresis discussed in Section 2; once firms pay the sunk costs to establish these new avoidance networks, the channels remain in place, leading to a permanent shift in trade patterns. Moreover, the stability of post-2018 coefficients through the Covid-19 period suggests the estimated effects reflect tariff-induced avoidance rather than pandemic-related supply chain disruptions.

Dynamics. Figure S3 shows cumulative local projections for both the extensive and intensive margin. Transshipment responds quickly to tariff shocks. On impact, both the extensive and intensive margins of diversion rise sharply, peaking within one to two years after implementation of the 2018 tariffs. The effect then stabilizes or partially attenuates over subsequent years, indicating that most rerouting occurred during the initial reorganization of supply chains rather than through continuous adjustment. The lower-bound (growth-based) specification produces effects that persist for four to five years, while the upper-bound (levels-based) measure captures short-lived surges in trade volumes that fade by year two. Together, these patterns suggest that the estimated elasticities in the main specification below reflect genuine, medium-run restructuring of trade flows rather than mechanical co-movement in shipments. The dynamics therefore strengthen the causal interpretation: transshipment is a gradual but durable process of adjustment to tariff policy, rather than a transient statistical artifact.

4.2 The Magnitude of Transshipment

Figure 3 plots the aggregate quantity of transshipment. Prior to the 2018 tariffs, our measure shows that the identified share of transshipment is low. Both the conservative (low) and liberal (high) bands indicate that around 1-2 percent of the volume of shipments came to the US from China via transshipment, with only around 10-15 percent of HS6 codes showing any evidence of rerouting. Following the onset of tariffs, both numbers spiked. Under the levels-based liberal screen, transshipment reached approximately 15 percent of the 2017 baseline China-US trade volume by 2023, while the conservative growth-based screen identifies about one-third as much. Critically, both screens find that nearly 40 percent of HS6 codes exhibit some positive transshipment by 2023. The placebo series in the left panel of Figure 3—constructed by applying the same screens to HS6 products that were never subject to Section 301 tariff increases—remains near zero throughout this period, confirming that the methodology does not spuriously generate transshipment where no tariff-induced rerouting exists.

These magnitudes are economically large and consistent with independent evidence using different methodologies. Freund (2024) does not display an aggregate series, but the trends are broadly consistent. Iyoha et al. (2025) document similar trade diversion in Vietnamese customs records. My estimates, constructed using publicly available bilateral trade flows with conservative screening procedures, provide a lower bound on total avoidance since transshipment is only one channel within a broader portfolio of strategies including undervaluation, misclassification, and permanent relocation.

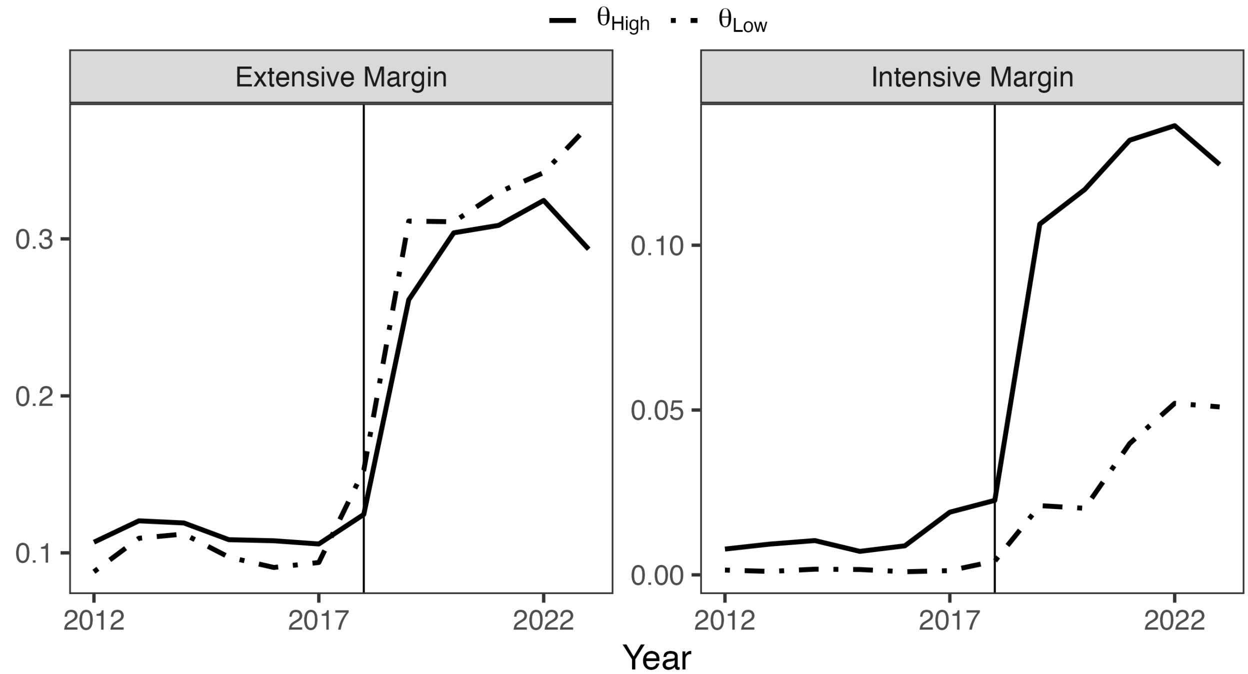
The aggregate patterns mask substantial heterogeneity by end-use. Appendix Figure C.2 shows that transshipment is heavily concentrated in capital and intermediate goods, with far lower magnitudes for consumption goods. This heterogeneity is consistent with the model's prediction that avoidance responds more strongly when the effective wedge is larger and fixed costs are lower, which are both conditions that favor business inputs over final consumer goods.

Products with higher transshipment exhibit significantly larger declines in direct import volumes relative to their pre-2018 trends (columns 1–2). This quantity effect indicates that transshipment substitutes nearly one-for-one for direct trade. More subtly, higher θ is also associated with lower observed China FOB unit values within the same HS6 and year (columns 3–4). This negative price-diversion correlation confirms that avoidance reshapes the composition of observed trade flows, though the precise mechanism is ambiguous without firm-level panel data.

Three interpretations are consistent with the model and data. First, low-marginal-cost, high-markup firms—which Proposition 1 predicts are most likely to avoid—may also charge high prices (due to quality premia), so their exit mechanically lowers average unit values. Second, surviving firms may engage in quality downgrading to preserve market share under tariff pressure. Third, transshippers may preferentially reroute higher-value varieties to reduce detection risk (higher-value goods generate larger savings from avoiding tariffs). Distinguishing these mechanisms requires firm-product panel data tracking the same varieties across channels and is left for future work. For this paper's purposes, all three interpretations support the central claim: selection into avoidance biases measured passthrough upward in border designs, and the magnitude of this bias can be bounded using the sufficient statistic θ.[11]

11. Distinguishing these mechanisms requires firm-product panel data tracking varieties across channels. If quality downgrading is significant, the welfare cost includes not just reduced trade volumes but also quality losses borne by consumers. That cost is not captured in the sufficient statistics framework.

Figure 3: Aggregate Transshipment on the Intensive and Extensive Margins



Notes: The left panel plots the share of HS6 codes which exhibit positive transshipment (extensive margin) according to the screening conditions. The low and high bands come from the low (growth-based) and high (level-based) screening conditions. The right panel does the same for the intensive margin.

Selection's Footprint in Border Data

The model predicts that transshipment should reshape observable trade flows through two channels: low-passthrough firms exit the direct-shipping sample (extensive margin) and the composition of surviving exporters shifts toward higher-cost, higher-passthrough firms (intensive margin). Table 1 provides direct evidence for both mechanisms by regressing deviations from pre-2018 trends in direct China→US trade on the measured transshipment intensity.

Table 1: Regression of Deviations in Direct China–US Imports on Diversion Intensity

	$\Delta^{\text{trend}}(\text{CN}\rightarrow\text{US})_{k,t}$			
	Quantities		Prices	
	(1)	(2)	(3)	(4)
$\sinh^{-1}(\theta_{\text{low}})$	-0.952***		-0.945**	
	(0.306)		(0.386)	
$\sinh^{-1}(\theta_{\text{high}})$		-0.732***		-0.448***
		(0.142)		(0.139)
R^2	0.46	0.47	0.34	0.34
Observations	53,485	53,485	53,134	53,134
HS6 & Year×Use FE	✓	✓	✓	✓
Weighted	✓	✓	✓	✓

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports fixed effects regressions of $\Delta^{\text{trend}}_{\text{CN}\rightarrow\text{US}}$, the deviation of HS6-level direct China→US import values from their pre-2012–2017 linear trend (in log units), on the inverse hyperbolic sine of the diversion measure θ. The dependent variable is constructed by fitting HS6-specific pre-trends in the value of shipments $\log(1 + v_{k,t})$ using BACI bilateral trade data and taking the residuals from these fitted trends for 2012–2023. All specifications include HS6 and year×use fixed effects, with standard errors clustered at the HS6 level. Columns (1)–(2) correspond to alternative specifications around the two diversion bounds. I use the same procedure to analyze FOB prices from Chinese exporters (Columns (3)–(4)).

Together, the quantity and price patterns in Table 1 provide the empirical signature of selection predicted by Propositions 1 and 2. Products experiencing greater diversion show both scale reductions (fewer direct shipments) and compositional shifts (lower average unit values), confirming that the firms exiting through transshipment differ systematically from those that continue to ship directly.

4.3 From θ to Passsthrough: A Portable Correction

The transshipment shares measured previously provide a direct correction for the bias in border-based passthrough estimates. Proposition 3 showed that when low-passthrough firms select into avoidance, border designs overstate the true cohort passthrough:

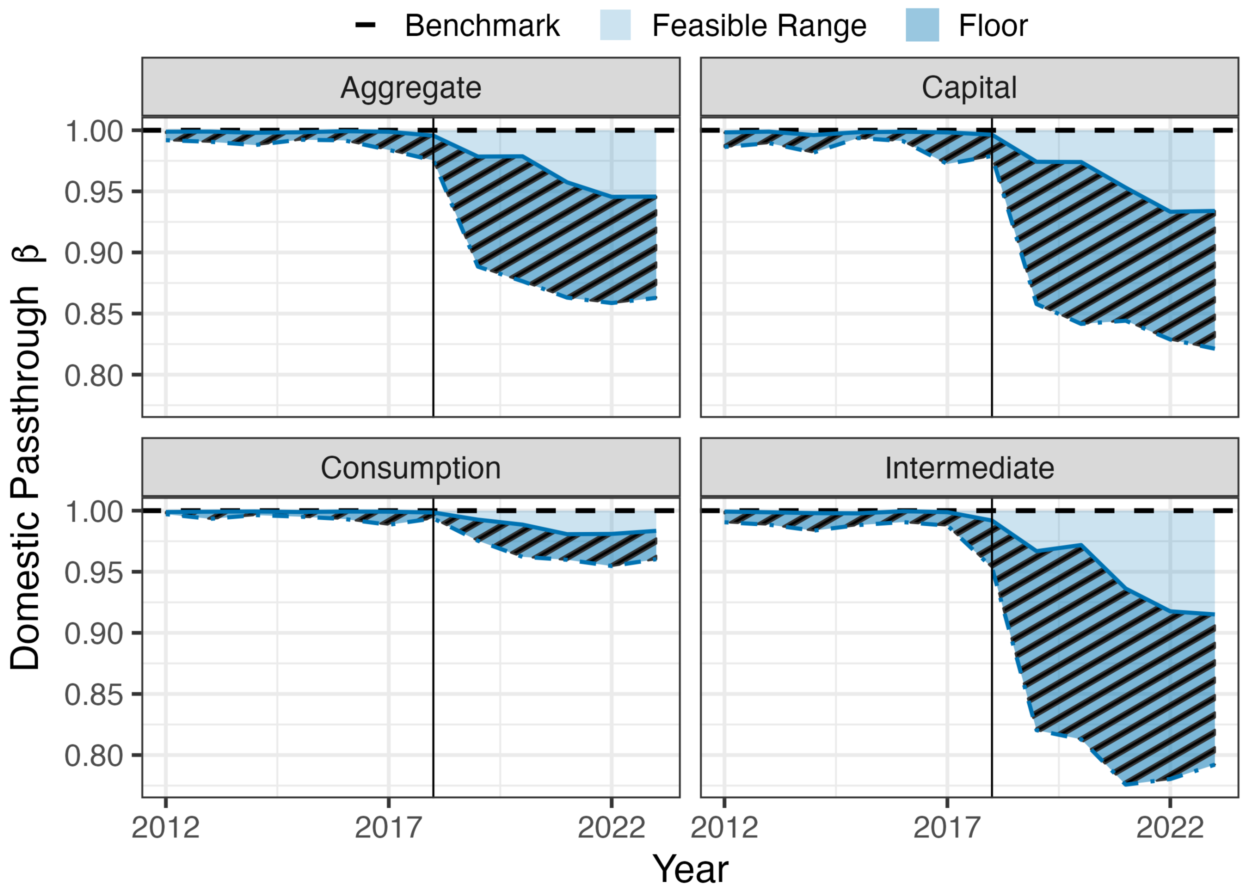
$$\beta_{\text{cohort}} \in \left[(1-\theta_{\text{high}})\beta_{\text{border}},\ \beta_{\text{border}}\right].$$

This bound requires only the avoidance share θ—a sufficient statistic under monotone selection—and does not require estimating a structural selection model.

Figure 4 applies this correction to existing border estimates of tariff passthrough. A large body of work finds that U.S. importers bore the full incidence of the 2018 China tariffs, with passthrough coefficients indistinguishable from one (Amiti, Redding, and Weinstein 2019, 2020; Cavallo et al. 2021; Fajgelbaum and Khandelwal 2024). Using $\beta_{\text{border}} = 1$ as the benchmark and the transshipment shares from above, the corrected passthrough is about 0.85 in the post-2019 period. This pattern is consistent with Flaaen, Hortaçsu, and Tintelnot (2020), who find near-complete passthrough for washing machines after global tariffs eliminated low-cost avoidance options.

The correction varies substantially by end-use. For consumption goods, where transshipment is minimal, corrected passthrough remains near unity—exactly consistent with the cohort-design findings of Flaaen, Hortaçsu, and Tintelnot (2020) and Cavallo et al. (2021). For capital and intermediate goods, where transshipment is concentrated, corrected passthrough falls to approximately 0.80. The shaded regions show the feasible range given the conservative (θ_{low}) and liberal (θ_{high}) transshipment bounds, with the hatched band marking the conservative floor. This convergence between the corrected border estimates and cohort-design findings validates both approaches. Cohort designs correctly measure passthrough for direct shippers by following the same firms before and after the tariff. Border designs capture the selected sample of survivors. The correction shows that once we account for who exits through avoidance, the two approaches yield consistent estimates of true incidence. The remaining wedge between 0.80 and 1.0 for business inputs suggests that even after correcting for transshipment, some burden falls on foreign exporters, either because they absorb part of the tariff directly or because transshipment itself is only one channel within a broader avoidance portfolio.

Figure 4: Corrected Passthrough Coefficient by End Use



Note: Shaded regions show corrected domestic passthrough relative to the border benchmark. The light blue area indicates the full feasible range implied by the model, $\beta_{\text{cohort}} \in [(1-\theta_{\text{high}})\beta_{\text{border}}, \beta_{\text{border}}]$. The hatched band marks the conservative floor using our two transshipment screens, $[(1-\theta_{\text{high}})\beta_{\text{border}}, (1-\theta_{\text{low}})\beta_{\text{border}}]$. The dashed line shows the border benchmark $\beta_{\text{border}} = 1$.

4.4 The Tariff Elasticity of Transshipment

Correcting measured incidence requires only the level of avoidance θ. But evaluating the welfare cost of tariffs through the marginal cost of public funds also requires characterizing the fiscal externality, i.e., how much the tariff base erodes as rates rise. This section estimates the elasticity of avoidance with respect to the effective wedge using within-HS6 variation in tariff exposure.

The baseline specification is

$$f(\theta_{k,t}) = \alpha_k + \delta_{t\times e(k)} + \psi \cdot \Delta_{k,t} + \varepsilon_{k,t}, \tag{7}$$

where $\Delta_{k,t} \equiv \log[(1+\tau^d_{k,t})/(1+\tau^a_{k,t})]$ is the effective tariff wedge, α_k are HS6 fixed effects,

$\delta_{t\times e(k)}$ are year×end-use fixed effects, and standard errors are clustered by HS6. The outcome $f(\theta_{k,t})$ is either $\sinh^{-1}(\theta_{k,t})$ (intensive margin) or $\mathbb{1}(\theta_{k,t} > 0)$ (extensive margin). Observations are weighted by 2017 China→US import shares.

The year×end-use fixed effects are motivated by the heterogeneous fixed-cost extension in Appendix A.5, which shows that the avoidance elasticity ψ varies with product characteristics that affect the cost of transshipment. By allowing consumption, capital, and intermediate goods to follow different aggregate time paths, these fixed effects absorb common shocks within each category while preserving the identifying variation from differential exposure to the wedge within end-use groups. Table S12 in the appendix confirm this intuition, showing that products with pre-existing hub networks, lower bulkiness, or electronics content exhibit larger wedge elasticities along the intensive margin, although that is not true along the extensive margin (Table S13).

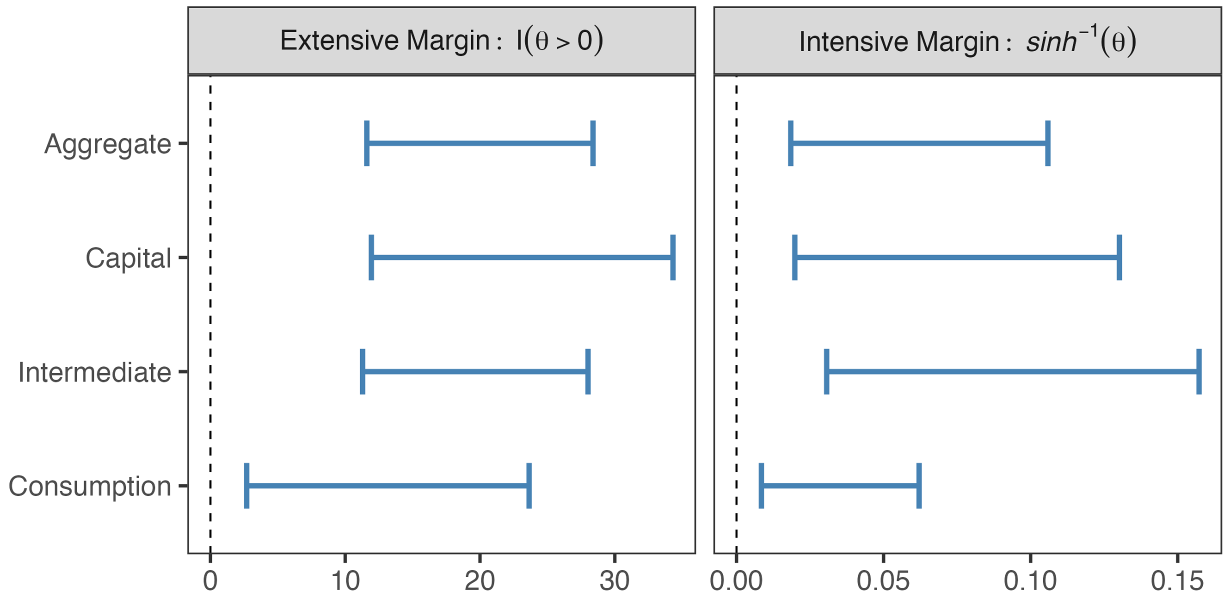
Identification comes from within-product changes in the effective wedge induced by the 2018-2019 tariff increases. The product fixed effect α_k absorbs time-invariant product characteristics, while the year-end-use fixed effect absorbs common shocks within categories. Under the assumption that, conditional on these fixed effects, no unobserved HS6-by-year shock both moves transshipment and the wedge except the tariff itself (a parallel-trends condition), the coefficient ψ identifies the causal elasticity. Section 4.1 established that this parallel-trends assumption holds: pre-2018 event-study coefficients are flat and jointly insignificant.

Because the conservative and liberal transshipment measures provide bounds rather than point estimates, I report joint Imbens and Manski (2004) confidence sets that are guaranteed to contain the true effect under either specification.[12] Figure 5 shows these bounds scaled to represent the impact of a 10 percentage point increase in the tariff wedge.

A 10 percentage point increase in the effective tariff wedge raises the probability that a product exhibits positive transshipment by approximately 25-35 percentage points (extensive margin). On the intensive margin, it increases the transshipment share, transformed by the inverse hyperbolic sine, by 0.05 to 0.15. These effects are largest for capital and intermediate goods and smallest for consumption goods, consistent with the heterogeneity in transshipment levels documented earlier.

12. The bounds takes the form $\left[\widehat{\beta}_{\text{Low}} - c^\star\,\text{se}(\widehat{\beta}_{\text{Low}}),\ \widehat{\beta}_{\text{High}} + c^\star\,\text{se}(\widehat{\beta}_{\text{High}})\right]$, where $c^\star$ is an adjusted critical value that depends on the estimated correlation $\rho \in (-1, 1)$ between the two slopes. The constant $c^\star$ is obtained by integrating the bivariate normal distribution to achieve joint coverage of $1-\alpha$.

Figure 5: The Tariff Elasticity of Transshipment



Note: This figure plots the estimated elasticity of transshipment with respect to the effective tariff wedge, scaled to show the impact of a 10 percentage point increase. The left panel shows the effect on the probability of any transshipment occurring (extensive margin, in percentage points), while the right panel shows the effect on the volume of transshipment (intensive margin, in $\sinh^{-1}(\theta)$ units). The horizontal bars are 95% joint confidence sets from the baseline specification in equation (7), constructed using the method of Imbens and Manski (2004) to account for the upper and lower bound estimates of transshipment.

The large elasticities indicate that the tariff base erodes substantially in response to rate increases. This has direct implications for the fiscal externality term in the marginal cost of public funds. When avoidance is highly elastic, raising tariff rates generates less revenue per unit of welfare cost, increasing the MCPF. Section 5 combines these elasticity estimates with the level corrections from the previous subsection to re-evaluate the overall welfare cost.

4.5 Robustness and Falsification

The estimated elasticities survive two sharp falsification tests that directly address potential confounds. First, a natural concern is that these elasticities reflect global trade dynamics rather than U.S.-specific tariff avoidance. If the observed relationship between the U.S. effective wedge $\Delta_{k,t}$ and measured transshipment were driven by broader shifts in China's export patterns such as capacity constraints, changing comparative advantage, or global supply chain restructuring, we should observe similar effects when applying the transshipment screens to other destinations. Table D.1 shows this is not the case.

For both the EU-27+UK and Canada, the coefficients on the U.S. tariff wedge are small and generally statistically indistinguishable from zero across all four outcomes. While a few coefficients are marginally significant, they show no consistent pattern and are orders of magnitude smaller than the main estimates for the U.S., confirming that the wedge-transshipment relationship is specific to the U.S. market where the tariffs were imposed. This rules out the concern that our results reflect coincidental timing between the U.S. tariff implementation and unrelated changes in China's global export networks.

Second, I directly test whether the network structure underlying transshipment matters. For each year, I randomly reassign hub→US shipments across HS6 codes within HS4 product groups, breaking the directional China→hub→US link while preserving each product's total China exports, each hub's total US-bound shipments, the exposure wedge, and all fixed effects. This "broken network" placebo preserves any mechanical correlation between trade volumes and the wedge but eliminates the transshipment channel. Figure D.1 shows that across 5,000 such permutations, the resulting coefficients center far below the true estimates, with the observed elasticities lying in the extreme right tail of the null distribution. This confirms that only the intact two-leg network structure generates the large responses observed in the data.

Additional robustness checks in the Supplemental Appendix show the results are insensitive to pre-trend adjustment (Table S7), long-difference specifications (Table S8), HS4-specific trends (Table S9), entropy balancing (Table S11), and alternative wedge definitions excluding the deductibility channel (Table S4). Leave-one-out analysis excluding individual hubs one at a time (Table S14) confirms that no single country drives the results, though CanadaL, Japan, and Mexico are the most influential transshipment routes.

5 Re-evaluating the Welfare and Revenue Effects of Tariffs

Given the parameters estimated in the previous section, I now re-evaluate the marginal cost of public funds for tariffs. The MCPF provides a welfare-relevant metric for comparing tax instruments by measuring the total welfare cost to society of raising one additional dollar of government revenue. A natural benchmark is a hypothetical lump-sum tax, which does not distort economic behavior. For such a tax, the welfare cost of raising one dollar is exactly one dollar, yielding an MCPF of one. Real-world taxes like tariffs, by contrast, distort behavior and generate deadweight losses. The MCPF captures these costs through two channels, represented in the numerator and denominator of the following expression:[13]

$$\text{MCPF} = \frac{\beta}{1+\eta}. \tag{8}$$

The numerator, β, represents the *domestic incidence* of the tariff—the share of the tariff burden borne by domestic consumers and firms rather than foreign exporters. When $\beta = 1$, domestic agents bear the full incidence; when $\beta = 0$, the tariff is effectively a transfer from foreign producers. The denominator, $1 + \eta$, captures the *fiscal externality* from behavioral responses. The elasticity $\eta \leq 0$ reflects how the tax base shrinks as economic agents change their behavior to reduce their tax burden. A larger behavioral response (more negative η) means the government collects less revenue per unit of welfare cost, raising the MCPF.

Existing estimates of the tariff MCPF following the 2018 U.S.-China trade war place it between 1.2 and 1.6 (Finkelstein and Hendren 2020; Jaccard 2021), suggesting tariffs are more costly than lump-sum taxation but comparable to top income taxes. However, these estimates do not fully account for selection into tariff avoidance. In the numerator, border-based estimates overstate domestic incidence because low-passthrough firms exit the sample. In the denominator, the fiscal externality η in prior studies aggregates across multiple behavioral margins—substitution to domestic goods, quality downgrading, and avoidance—but does not explicitly decompose the avoidance component or account for how selection biases its measurement.

The net effect on the MCPF is *a priori* ambiguous: lower true incidence reduces the numerator, but a larger avoidance elasticity (once properly measured) may increase or decrease the denominator depending on how it compares to the implicit avoidance response embedded in prior estimates. Which effect dominates is an empirical question that this section addresses.

Three caveats qualify the forthcoming welfare analysis. First, the framework is partial equilibrium, abstracting from general equilibrium effects on wages, the terms of trade, and sectoral reallocation.[14] Second, the resource costs of transshipment—additional shipping, relabeling, intermediary fees—represent genuine deadweight losses not captured in the sufficient-statistics approach. Third, because transshipment is only one avoidance channel, the 85% passthrough correction likely overstates the true burden, making the welfare cost estimates conservative upper bounds.

13. As derived in Appendix A.6. Hendren (2016) and Finkelstein and Hendren (2020) provide a thorough overview of the marginal *value* of public funds. Because I focus on taxes rather than transfers, I refer to it as the marginal *cost* for clarity.

14. In particular, I do not model how foreign exporters adjust supply in response to U.S. tariffs, nor do I account for potential terms-of-trade improvements if the U.S. has market power.

5.1 Correcting Measured Domestic Incidence

Our empirical findings allow us to directly correct existing estimates of the tariff's welfare cost. As shown in Section 2.3, the true domestic incidence can be approximated by scaling the biased border estimate by $(1-\theta)$. The literature finds $\beta_{\text{border}} \approx 1.0$ for the 2018 tariffs. Applying our aggregate estimate for the level of avoidance ($\theta_{\text{high}} \approx 0.15$), the corrected scaling factor is approximately 0.85. This implies a new, lower range for the MCPF of roughly 1.0–1.6, depending on the magnitude of the fiscal externality in the denominator. This is a notable result: after correcting for selection bias in the numerator alone, we cannot reject that the welfare cost of the 2018 tariffs approaches that of a distortion-free lump-sum tax.

However, the MCPF also depends on the fiscal externality in the denominator: how rapidly the tax base erodes when rates rise. In reality, the total fiscal externality aggregates across multiple margins: substitution to domestic goods ($\eta_{\text{substitution}}$), quality adjustments ($\eta_{\text{quality}}$), and avoidance ($\eta_{\text{avoid}}$), such that $\eta_{\text{total}} = \eta_{\text{substitution}} + \eta_{\text{quality}} + \eta_{\text{avoid}}$. Prior estimates do not decompose these components or explicitly model the avoidance elasticity. Section 4.4 implies that the elasticity of avoidance is high. These two forces—lower incidence (pushes MCPF down) versus high avoidance elasticity (pushes MCPF up)—work in opposite directions. To isolate the welfare cost attributable specifically to the avoidance mechanism, I construct an "avoidance-only" MCPF in the next subsection.

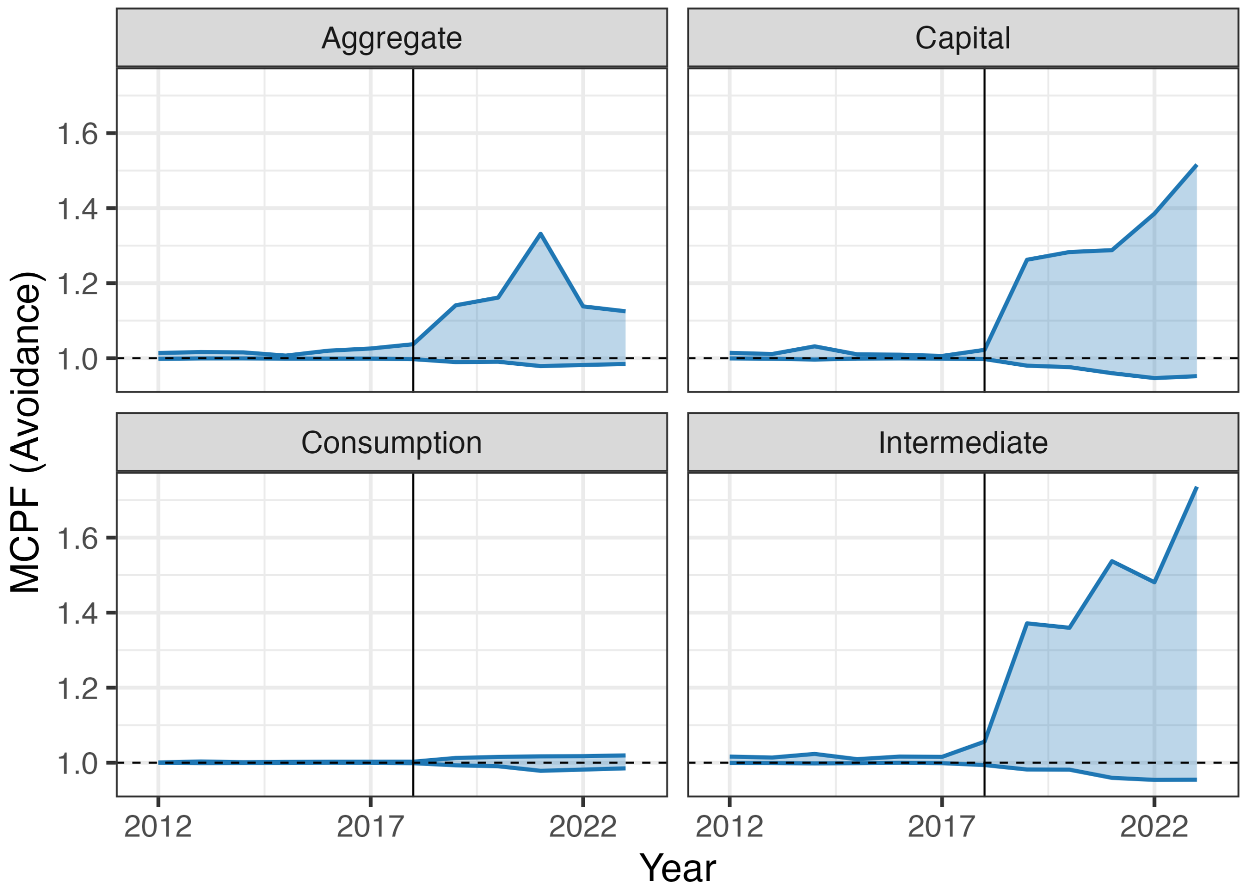
5.2 The Avoidance-Based MCPF

To isolate the welfare cost of avoidance, I construct an MCPF using only the avoidance component of the fiscal externality. This exercise asks: holding fixed the other behavioral responses (substitution, quality adjustment), what is the incremental contribution of avoidance to the tariff's welfare cost? I use the avoidance level θ to correct the incidence term and the avoidance elasticity ψ to construct the avoidance-specific fiscal externality, $\eta_{\text{avoid}} \approx -\frac{\theta}{1-\theta}\psi$. The avoidance-based MCPF is then

$$\text{MCPF}_{\text{avoid}} \in \left[\frac{(1-\theta)\beta_{\text{border}}}{1-\frac{\theta}{1-\theta}\psi}, \frac{\beta_{\text{border}}}{1-\frac{\theta}{1-\theta}\psi} \right]. \tag{9}$$

Using the Imbens–Manski bounds on ψ from Section 4.4, I compute a 95% confidence set for this avoidance-based MCPF, propagating the uncertainty from the elasticity estimates. Figure 6 plots the resulting time series. Before the 2018 trade war, with little avoidance, the MCPF for all goods is approximately one. After 2018, the story diverges sharply by end-use. For consumption goods, where avoidance is minimal (Section 4.2), the MCPF remains near one throughout the post-tariff period. The corrected incidence is close to unity (Figure 4), and the fiscal externality from avoidance is negligible, leaving the welfare cost indistinguishable from a lump-sum tax.

Figure 6: The Marginal Cost of Public Funds by End Use



Note: Shaded regions show the avoidance-based marginal cost of public funds evaluated in each year given the corresponding regressions. Uncertainty is propagated via the Imbens–Manski bands. The dashed horizontal line marks MCPF $= 1$, the benchmark for a lump-sum tax.

For capital and intermediate goods, however, the picture is starkly different. The high elasticity of avoidance documented in Section 4.4—with 10 percentage point wedge increases raising transshipment probability by 25–35 percentage points—generates a substantial fiscal externality. This pushes the upper bound of the avoidance-based MCPF to around 1.5 by 2023. While the lower bound of our estimates remains near one, the wide confidence interval indicates that we cannot rule out a substantial welfare cost for these goods, driven entirely by the avoidance margin. The mechanism is straightforward: when the tariff base erodes rapidly in response to rate increases (large negative η_{avoid}), each dollar of revenue requires greater welfare sacrifice. Combined with the lower corrected incidence for business inputs ($\beta \approx 0.80$ from Figure 4), the net effect leaves the MCPF for capital and intermediate goods meaningfully above one.

This concentration of the welfare cost in production-related goods is consistent with the findings in Alessandria et al. (2025). In their dynamic general equilibrium model, tariffs are especially distortionary for investment because capital goods have a high import share and tariffs distort the intertemporal margin. My paper provides a complementary mechanism: these are precisely the goods where the fiscal externality from avoidance is largest. Both papers thus underscore the critical interaction between trade and fiscal policy.

Alessandria et al. (2025) focus on how tariff revenue is spent—using it to offset distortionary taxes lowers the net welfare cost. My mechanism highlights how the domestic tax system itself alters the incentive to *pay* the tariff in the first place. Through deductibility, the corporate tax code acts as an implicit enforcement penalty: higher corporate tax rates compress the effective wedge between direct and avoided shipments, discouraging transshipment. This interaction was fundamentally altered by the 2017 Tax Cuts and Jobs Act (TCJA), which cut the corporate tax rate from 35% to 21% just before the 2018 tariffs were imposed. The next section quantifies the revenue consequences of this policy change.

5.3 The Tax Cuts and Jobs Act's Impact on Tariff Revenue

Recall that the tariff wedge on good k is

$$\Delta_k = \log(1 + \tau_k^d) - \log\left(1 + \tau_k^d \times \underbrace{\tau^c \times z_k}_{\text{Domestic Taxes}}\right),$$

where τ_k^d is the tariff rate on good k, τ^c is the business income tax rate, and z_k is the present value of depreciation deductions. The theoretical monotonicity results and the empirical work together show that when either of z_k or τ^c go up, then avoidance declines and tariff revenue rises. Just prior to the 2018 trade war, the 2017 Tax Cuts and Jobs Act increased z_k for all imported capital goods through full expensing (100% bonus depreciation), but the corporate tax rate τ^c fell from 35% to 21%. In principle, both should have effects on avoidance and revenue. Given that, we construct two counterfactuals to isolate the different effects of the 2018 tax cuts on avoidance behavior.

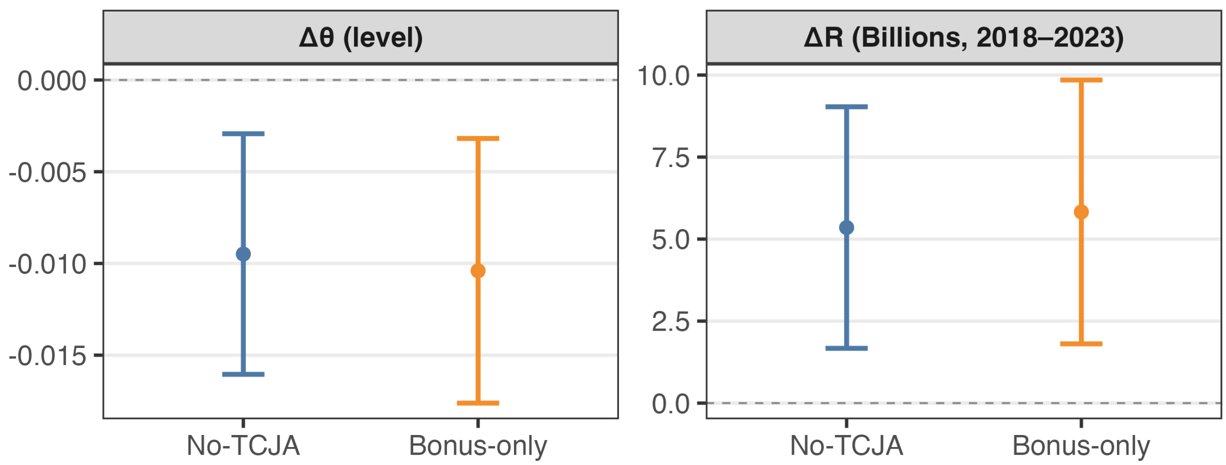
- In the first counterfactual, I consider what would have happened to avoidance and revenue had TCJA not been passed. Under this counterfactual, τ^c remains at 35%

and bonus depreciation follows its statutory path from 40% in 2018 to 30% in 2019, and 0% from 2020 onward.

- Under a second counterfactual, I examine the effect of full bonus depreciation for equipment, but maintaining the $\tau^c = 0.35$.

In each scenario, I compute the change in estimated avoidance and the resulting increase in tariff revenue given the larger tax base and compare it to the observed avoidance and revenue given TCJA's passage. Figure 7 plots the predicted change in the aggregate avoidance rate (top panel) and cumulative tariff revenue (bottom panel) over 2018–2023 under three counterfactual tax regimes, with Imbens–Manski bounds reflecting uncertainty in the intensive margin elasticity ψ.

Figure 7: Counterfactual Impact of the TCJA on Avoidance and Tariff Revenue



Note: This figure plots the estimated change in the aggregate transshipment share ($\Delta\theta$) and cumulative tariff revenue (ΔR) over 2018–2023 under two counterfactual scenarios relative to the observed outcome under the TCJA. "No-TCJA" assumes the corporate tax rate remained at 35% and bonus depreciation followed its pre-TCJA phase-out schedule. "Bonus-only" assumes a 35% corporate tax rate but with 100% bonus depreciation. The points represent the central estimates, and the vertical bars show 95% joint confidence sets constructed using the method of Imbens and Manski (2004).

Without the reform ("No-TCJA") the aggregate avoidance share would have been lower, implying that firms would have shifted 0.3-1.6 percentage points less of imported value through transshipment channels. Cumulatively, tariff revenue would have been \$2–\$9 billion higher, reflecting both a higher effective tariff base and reduced avoidance behavior. Since the first scenario entails a lower z_k for capital goods than under TCJA, I now layer in if we made $z_k = 1$ across all goods but held fixed τ^c. This marginally raises revenue and lowers avoidance compared to the first scenario. The effect is small because the reform only affects imported equipment, which had a high expensing rate relative to the reform anyway.

This demonstrates a meaningful and quantitatively significant interaction between the corporate tax base and tariff revenue. Domestic tax policy serves as a powerful, if unintentional, trade enforcement tool. The magnitude—$2 to $9 billion in foregone revenue—is economically substantial and suggests that policymakers evaluating trade enforcement strategies should account for spillovers from the domestic tax system. While the public finance literature emphasizes explicit enforcement tools like audits and penalties, the deductibility of imported inputs provides an implicit enforcement margin that operates automatically through the tax code. Indeed, spillovers from domestic tax policy may lessen the need for explicit penalties on transshipment and other forms of avoidance.

6 Conclusion

This paper shows that tariff avoidance generates economically significant selection bias in standard estimates of tariff incidence. When costly avoidance is possible, low-cost, low-passthrough firms exit the direct-shipping channel through transshipment, mechanically biasing border-based passthrough estimates upward. Measuring transshipment in bilateral trade flows, I find nearly 40% of tariffed products exhibit evidence of rerouting by 2023. Accounting for this avoidance implies measured aggregate passthrough lies in the range 85–100%, with the correction concentrated in intermediate and capital goods where avoidance is highest.

The paper also identifies a novel enforcement mechanism: the corporate tax code. Because firms deduct tariff-inclusive import costs, higher corporate tax rates discourage avoidance by compressing the effective wedge between direct and avoided shipments. The 2017 corporate tax cut from 35% to 21% reduced tariff revenue by an estimated $2–$9 billion through increased transshipment, revealing quantitatively significant spillovers between tax and trade policy. Important caveats qualify these findings. Transshipment is only one avoidance channel; firms can also misreport values, misclassify goods, downgrade quality, or permanently relocate production. My estimates therefore represent a conservative lower bound on total avoidance. The welfare analysis abstracts from general equilibrium effects on wages, prices, and terms of trade. To the extent firms substitute toward unmeasured avoidance margins, the true incidence correction could be larger.

The selection framework applies broadly to any enforcement regime where high-cost agents can exit observed transactions; value-added taxes, capital controls, and financial sanctions all create similar selection problems. The sufficient-statistic approach developed here provides a portable methodology for bounding bias in these settings. Future work should measure the full portfolio of avoidance strategies and estimate substitution elasticities across margins. If stricter transshipment enforcement merely displaces evasion toward misclassification, single-margin policies will prove ineffective. Understanding these substitution patterns is essential for optimal enforcement design and for evaluating how features of the domestic tax system shape incentives to avoid trade taxes. As policymakers evaluate tariffs for both revenue generation and industrial policy, recognizing that enforcement extends beyond traditional customs measures into the broader fiscal architecture is critical for informed decision-making. Moreover, the potential for hysteresis suggests that even temporary trade policies can have permanent and costly effects on the fiscal architecture.

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A Theoretical Appendix

A.1 Assumptions about Demand

I impose several regularity conditions on demand:

Assumption 1 (Demand)**.** *The demand for each* variety *i depends on its own delivered price $\tilde{p}_i$ and on a price aggregator $\mathbf{P}$ summarizing rivals. Fixing $\mathbf{P}$ as parametric at the variety level, the primitives satisfy:*

D1. ***Downward sloping own demand and outward shifts in rivals:*** $\dfrac{\partial D_i}{\partial \tilde{p}_i} < 0$ *and* $\dfrac{\partial D_i}{\partial \mathbf{P}} > 0$.

D2. ***Smoothness:*** $D_i(\cdot \mid \mathbf{P}) \in C^2$ *in own price.*

D3. ***Elastic demand:*** $\varepsilon_i(\tilde{p}_i \mid \mathbf{P}) \equiv -\dfrac{\tilde{p}_i}{D_i}\dfrac{\partial D_i}{\partial \tilde{p}_i} > 1$.

D4. ***Profit concavity / local stability:*** *For either channel $c \in \{D, T\}$ with delivered price $\tilde{p}_i^c$,*

$$\varepsilon(\tilde{p}_i^c \mid \mathbf{P}) \;-\; 1 \;-\; \kappa(\tilde{p}_i^c \mid \mathbf{P}) \;>\; 0.$$

D5. ***Kimball curvature:*** $\kappa_i(\tilde{p}_i \mid \mathbf{P}) \equiv -\dfrac{\partial \ln \varepsilon_i(\tilde{p}_i \mid \mathbf{P})}{\partial \ln \tilde{p}_i} \leq 0$ *(equals 0 under CES). We also impose two extra conditions:*

(a) $\dfrac{\partial \kappa(\tilde{p}_i \mid \mathbf{P})}{\partial \ln \tilde{p}_i} \leq 0.$

(b) $\kappa'(\tilde{p}) \equiv \partial \kappa(\tilde{p}) / \partial \ln \tilde{p} \geq -\frac{\varepsilon(\tilde{p})}{\varepsilon(\tilde{p})-1}\,\kappa(\tilde{p})^2.$

Conditions **D1–D4** are standard and ensure well-behaved monopolistic competition with elastic demand and unique profit-maximizing prices. Condition **D5** introduces Kimball-type demand, where $\kappa \leq 0$ means the demand elasticity ε is weakly increasing in price. Intuitively, consumers become more price-sensitive as prices rise, making high-priced (high-markup) goods face flatter residual demand curves. This is the source of heterogeneous passthrough: when a tariff raises delivered prices, high-markup firms face larger elasticity increases and therefore compress their markups more, leading to lower passthrough. The technical conditions **D5(a)–(b)** ensure that this passthrough is monotone in marginal cost, which is critical for the sorting result in Proposition 1. Without these curvature restrictions, passthrough could be non-monotone, and the selection mechanism would be more complex. Empirically, **D5** is a weak condition: the literature consistently finds that markups vary across firms (Gopinath and Itskhoki 2010; Amiti, Itskhoki, and Konings 2019; De Loecker, Eeckhout, and Unger 2020; Edmond, Midrigan, and Xu 2023), and Kimball demand is a tractable way to microfound this heterogeneity.

A.2 Lerner Condition

Lemma 1 (Lerner condition and monotone comparative statics)**.** *For either channel $r \in \{d, a\}$ and corresponding wedges τ^r, under **D1–D4**, any $p_i^*(m_i; r)$ is interior and satisfies*

$$\frac{p_i^* - m_i}{p_i^*} = \frac{1}{\varepsilon_i(\tilde{p}_i^r \mid \mathbf{P})}, \qquad \tilde{p}_i^r = (1 + \tau^r) p_i^*.$$

Moreover: (i) p_i^* strictly increases in m_i; (ii) p_i^* is weakly decreasing in the wedge τ^r (constant under CES); and (iii) q_i^* strictly decreases in m_i and τ^r.

Proof. Interior and FOC. By **D2–D4**, $\Phi_i(\cdot)$ is strictly concave in p_i; the unique maximizer is interior and characterized by the FOC. Let the delivered price be

$$\tilde{p}_i \equiv \tilde{p}_i(p_i, \tau^r) = \begin{cases} (1 + \tau^a) p_i & \text{(avoid channel)} \\ (1 + \tau^d)\, p_i & \text{(direct channel).} \end{cases}$$

Then $\partial \tilde{p}_i / \partial p_i \in \{1 + \tau^a,\ 1 + \tau^d\}$, and the FOC is

$$0 = \frac{\partial \Phi_i}{\partial p_i} = D_i(\tilde{p}_i) + (p_i - m_i)\, D_i'(\tilde{p}_i)\, \frac{\partial \tilde{p}_i}{\partial p_i}.$$

With $\varepsilon_i(\tilde{p}_i) \equiv -\, \tilde{p}_i D_i'(\tilde{p}_i) / D_i(\tilde{p}_i)$,

$$\frac{p_i^* - m_i}{p_i^*} = \frac{\tilde{p}_i}{(\partial \tilde{p}_i / \partial p_i)\, p_i^*} \cdot \frac{-D_i(\tilde{p}_i)}{\tilde{p}_i D_i'(\tilde{p}_i)} = \frac{1}{\varepsilon_i(\tilde{p}_i)}.$$

This is the Lerner condition.

Monotonicity in m_i. Define

$$g(p, m; \tau^r) \equiv D(\tilde{p}) + (p - m)\, D'(\tilde{p})\, \frac{\partial \tilde{p}}{\partial p}, \qquad \tilde{p} = \tilde{p}(p, \tau^r).$$

At the optimum, $g(p^*, m; \tau^r) = 0$. Partials:

$$\frac{\partial g}{\partial m} = -\, D'(\tilde{p})\, \frac{\partial \tilde{p}}{\partial p} > 0 \quad (\mathbf{D1}), \qquad \frac{\partial g}{\partial p} = 2\, D'(\tilde{p})\, \frac{\partial \tilde{p}}{\partial p} + (p - m)\, D''(\tilde{p}) \Big(\frac{\partial \tilde{p}}{\partial p}\Big)^2.$$

Strict concavity of profits (**D4**) implies $\partial g / \partial p < 0$ at p^*. By the implicit function theorem, $\partial p^* / \partial m = -(\partial g / \partial m) / (\partial g / \partial p) > 0$.

Monotonicity in τ^r. For the direct channel, $\tilde{p} = (1+\tau^r)p$. Write

$$G(p,\tau^r) \;\equiv\; \frac{p-m}{p} - \frac{1}{\varepsilon((1+\tau^r)p)} \;=\; 0.$$

Then

$$\frac{\partial G}{\partial \tau^r} = \frac{\varepsilon'((1+\tau^r)p)}{\varepsilon((1+\tau^r)p)^2}\, p \;\geq 0, \qquad \frac{\partial G}{\partial p} = \frac{m}{p^2} + \frac{\varepsilon'((1+\tau^r)p)}{\varepsilon((1+\tau^r)p)^2}\,(1+\tau^r).$$

Under Kimball demand, $\varepsilon'(\cdot) \geq 0$, so $\partial G/\partial \tau^r \geq 0$. Profit concavity (D4, equivalently $\varepsilon - 1 - \kappa > 0$ at the optimum) implies $\partial G/\partial p > 0$. Hence, by the IFT,

$$\frac{\partial p^*}{\partial \tau^r} = -\frac{\partial G/\partial \tau^r}{\partial G/\partial p} \;\leq\; 0,$$

with equality under CES ($\varepsilon' \equiv 0$).

Quantities. In either channel $\tilde{p} = (1+\tau^r)p^*$ and

$$\frac{d\big((1+\tau^r)p^*\big)}{d\tau^r} = p^* + (1+\tau^r)\frac{dp^*}{d\tau^r} = p^*\left(1 + \frac{\kappa(\tilde{p})}{\varepsilon(\tilde{p}) - 1 - \kappa(\tilde{p})}\right) = p^*\frac{\varepsilon(\tilde{p}) - 1}{\varepsilon(\tilde{p}) - 1 - \kappa(\tilde{p})} \;>\; 0,$$

since $\varepsilon(\tilde{p}) > 1$ and the denominator is positive by D4. With $D'(\cdot) < 0$, $q_i^* = D(\tilde{p})$ strictly falls in τ^r; similarly, q_i^* strictly falls in m because p^* rises in m and demand slopes down. This proves (i)–(iii). □

Lemma 1 establishes two key results. First, it confirms the standard Lerner condition: firms set their markup inversely to the elasticity of demand they face. This makes all comparative statics run through how a given wedge, τ^r, shifts the delivered price and how elasticity varies with that price. Second, the lemma establishes the monotone comparative statics that are essential for the paper's sorting mechanism.

Part (i) shows that a higher marginal cost pushes a firm's optimal pre-wedge price upward, providing the clean ordering in m necessary to derive a unique sorting cutoff. Part (ii) highlights that any wedge acts as a demand shifter. Finally, Part (iii) records the quantity implications: a higher marginal cost or a higher wedge τ^r reduces the quantity sold. This decline in profitability within a channel is what ultimately drives selection when wedges differ, a mechanism we formalize next.

A.3 Proof of Proposition 1

Proof. By **D2–D4**, the maximizer in $V(m, 1+\tau^r)$ is interior and unique; envelope arguments apply. The primitive $(p-m)D((1+\tau^r)p)$ has increasing differences in $(m, 1+\tau^r)$

because $\partial^2[(p-m)D((1+\tau^r)p)]/\partial m\,\partial(1+\tau^r) = -pD'((1+\tau^r)p) > 0$ by **D1**. Hence V inherits increasing differences in $(m, 1+\tau^r)$ by Topkis. Fix (τ^d, τ^a). Then

$$\frac{\partial H}{\partial m} = V_m(m, 1+\tau^a) - V_m(m, 1+\tau^d) < 0$$

since $1+\tau^a < 1+\tau^d$ and V has increasing differences; continuity of V yields a unique threshold $\hat{m}$ solving $H(\hat{m}; \tau^d, \tau^a) = 0$. For the comparative statics, differentiate the indifference condition:

$$\frac{d\hat{m}}{d\tau^d} = -\frac{\partial H/\partial \tau^d}{\partial H/\partial m}, \qquad \frac{d\hat{m}}{d\tau^a} = -\frac{\partial H/\partial \tau^a}{\partial H/\partial m}.$$

By the envelope theorem at the direct and avoidance optima p_d^* and p_a^*,

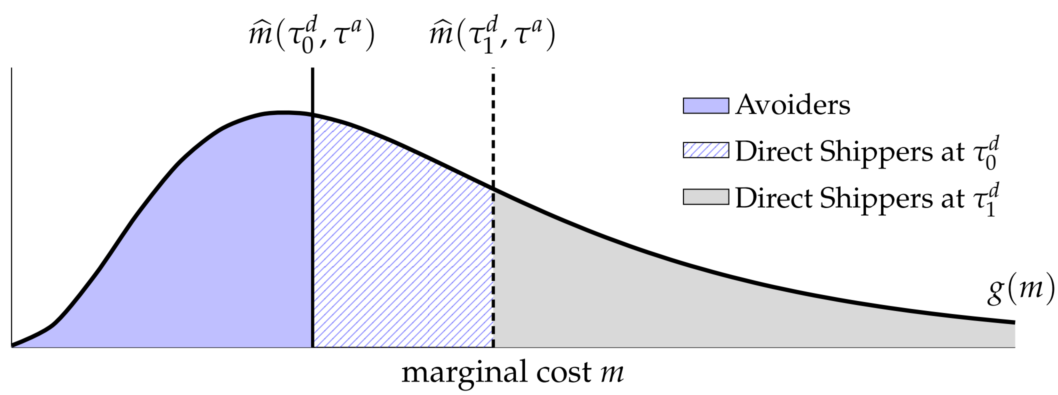
$$\frac{\partial V(m, 1+\tau^r)}{\partial(1+\tau^r)} = (p^* - m)\, p^*\, D'\big((1+\tau^r)p^*\big) \le 0,$$

so $\partial V(m, 1+\tau^d)/\partial \tau^d \le 0$ and $\partial V(m, 1+\tau^a)/\partial \tau^a \le 0$. Hence

$$\frac{\partial H}{\partial \tau^d} = -\frac{\partial V(m, 1+\tau^d)}{\partial \tau^d} \ \ge\ 0, \qquad \frac{\partial H}{\partial \tau^a} = \frac{\partial V(m, 1+\tau^a)}{\partial \tau^a} \ \le\ 0.$$

Since $\partial H/\partial m < 0$ at an interior cutoff, the stated signs follow. $\square$

Figure A.1: An increase in tariffs raises the cutoff



Note: Distribution of marginal costs and wedge-induced selection. The cutoff $\widehat{m}(\tau^d, \tau^a)$ partitions the cohort: firms with $m < \widehat{m}$ avoid tariffs, while $m \ge \widehat{m}$ ship direct. A wider wedge gap (e.g., a higher τ^d or a lower τ^a shifts the cutoff right, increasing the avoidance share θ and shrinking the survivor set.

A.4 Proof of Proposition 2

Proof. Write the first-order condition for a firm in the direct channel as $G(p, \tau^d) \equiv (p - m)/p - 1/\varepsilon((1+\tau^d)p) = 0$. Totally differentiate and use

$$\frac{\partial G}{\partial \tau^d} = \frac{\varepsilon'((1+\tau^d)p)}{\varepsilon((1+\tau^d)p)^2}\, p, \qquad \frac{\partial G}{\partial p} = \frac{m}{p^2} + \frac{\varepsilon'((1+\tau^d)p)}{\varepsilon((1+\tau^d)p)^2}\,(1+\tau^d).$$

Using $\kappa(u) = -u\,\varepsilon'(u)/\varepsilon(u)$ and the Lerner condition $m = p^*(\varepsilon - 1)/\varepsilon$,

$$\frac{dp^*}{d\tau^d} = -\frac{\partial G/\partial \tau^d}{\partial G/\partial p} = \frac{\kappa(\tilde{p}^d)}{1+\tau^d} \cdot \frac{p^*}{\varepsilon(\tilde{p}^d) - 1 - \kappa(\tilde{p}^d)}.$$

Finally, the passthrough elasticity is

$$\beta_i = \frac{d \ln\big((1+\tau^d)p^*\big)}{d\tau^d} = \frac{1}{1+\tau^d} + \frac{1}{p^*}\frac{dp^*}{d\tau^d} = \frac{1}{1+\tau^d} \cdot \frac{\varepsilon(\tilde{p}^d) - 1}{\varepsilon(\tilde{p}^d) - 1 - \kappa(\tilde{p}^d)}.$$

Under Assumption **D4**, strict profit concavity at the optimum is equivalent to $\varepsilon - 1 - \kappa > 0$, which ensures the denominator is positive. Moreover, under Assumption **D5(b)**, $\partial\beta/\partial \ln \tilde{p} \geq 0$. Since Lemma 1 implies $\tilde{p}$ increases in m, it follows that $\partial\beta/\partial m \geq 0$. □

A.5 Heterogeneous Fixed Costs of Avoidance

This appendix extends the baseline model by allowing the fixed cost of avoidance to vary across firms or products. We show that the selection logic and the border–cohort bias results of Propositions 1–3 remain valid under heterogeneity, and we clarify how cross-category differences in fixed costs rationalize the patterns in Section 4.

Corollary 2 (Heterogeneous fixed costs of avoidance)**.** *Let Assumptions **D1–D5** hold. For each unit i, let the fixed cost of avoidance F_i be drawn from a continuous distribution G with support $[F_{\min}, F_{\max}]$ and density $g > 0$, independent of the unit's marginal cost m_i. For $r \in \{d, a\}$, write*

$$V(m, F; 1+\tau^r) \equiv \max_{p \geq m}\,(p - m)\,D\big((1+\tau^r)p \,\big|\, P\big) \;-\; 1\{r = a\}\,F,$$

and define the avoidance advantage

$$H(m, F; \tau^d, \tau^a) \equiv V(m, F; 1+\tau^a) - V(m, F; 1+\tau^d).$$

Then:

(i) ***Cutoff and comparative statics.*** *For every $F \in [F_{\min}, F_{\max}]$ there exists a unique cutoff $m_b(F; \tau^d, \tau^a)$ solving $H(m_b(F; \tau^d, \tau^a), F; \tau^d, \tau^a) = 0$ such that*

$$m < m_b(F; \tau^d, \tau^a) \Rightarrow \textit{Avoid}, \qquad m \geq m_b(F; \tau^d, \tau^a) \Rightarrow \textit{Direct}.$$

Moreover,

$$\frac{\partial m_b}{\partial \tau^d} > 0, \qquad \frac{\partial m_b}{\partial \tau^a} < 0, \qquad \frac{\partial m_b}{\partial F} < 0.$$

(ii) ***Monotone selection and border bias.*** *Let $S_i = 1\{m_i \geq m_b(F_i; \tau^d, \tau^a)\}$ denote survival in the direct (border) sample. Under Kimball curvature (**D5**), direct-channel passthrough is increasing in marginal cost: $\beta_i = \beta(m_i)$ with $\beta'(m) \geq 0$. Because S_i is weakly increasing in m_i for any realization of F_i, it follows that*

$$\operatorname{Cov}(\beta_i, S_i) \;\geq\; 0,$$

so that the border estimate exceeds the cohort average:

$$\beta_{\,border} \;=\; \mathbb{E}\big[\beta_i \mid S_i = 1\big] \;\geq\; \mathbb{E}[\beta_i] \;=\; \beta_{\,cohort}.$$

(iii) ***Incidence bound unchanged.*** *With $\theta \equiv \Pr\big(m < m_b(F; \tau^d, \tau^a)\big)$ denoting the avoidance share, the cohort passthrough remains bounded by*

$$\beta_{\,cohort} \;\in\; \big[(1-\theta)\,\beta_{\,border},\; \beta_{\,border}\big].$$

(iv) ***Cross-category heterogeneity.*** *If F is deterministic by end-use category k, all conclusions hold* within *k:*

$$\beta_{\,border,k} \geq \beta_{\,cohort,k}, \qquad \beta_{\,cohort,k} \in \big[(1-\theta_k)\,\beta_{\,border,k},\; \beta_{\,border,k}\big],$$

where $\theta_k \equiv \Pr\big(m < m_b(F_k; \tau^d, \tau^a)\big)$. Lower F_k raises m_b and thus increases θ_k, providing a structural rationale for larger avoidance among capital/intermediate goods even when their statutory wedge Δ_k is smaller (e.g., because of deductibility).

Proof. Proposition 1 implies that $H(m, F; \tau^d, \tau^a)$ is continuous and strictly decreasing in m, with $\partial H / \partial \tau^d > 0$ and $\partial H / \partial \tau^a < 0$. For any F, define $m_b(F; \tau^d, \tau^a)$ by the indifference condition $H(m_b, F; \tau^d, \tau^a) = 0$. Uniqueness and the signs of the partial derivatives follow from the Implicit Function Theorem:

$$\frac{\partial m_b}{\partial \tau^d} = -\frac{\partial H/\partial \tau^d}{\partial H/\partial m} > 0, \quad \frac{\partial m_b}{\partial \tau^a} = -\frac{\partial H/\partial \tau^a}{\partial H/\partial m} < 0,$$

$$\frac{\partial m_b}{\partial F} = \frac{1}{\partial H/\partial m} < 0, \quad \text{since } \frac{\partial H}{\partial F} = -1 \text{ and } \frac{\partial H}{\partial m} < 0.$$

Monotonicity of $S_i = 1\{m_i \geq m_b(F_i; \cdot)\}$ in m_i and of $\beta(m)$ in m (Proposition 2 under Kimball demand implies $\mathrm{Cov}(\beta_i, S_i) \geq 0$ by Chebyshev's association inequality; the border–cohort ordering and identity of Proposition 3 then deliver $\beta_{\text{border}} \geq \beta_{\text{cohort}}$. The bound $\beta_{\text{cohort}} \in [(1-\theta)\beta_{\text{border}}, \beta_{\text{border}}]$ follows from the law of total expectation and does not depend on whether F is degenerate. Category-specific statements are immediate by conditioning on k. □

Remarks. Independence of F and m is sufficient, not necessary. The results continue to hold whenever the survival probability is weakly increasing in m after integrating over F, e.g., if $F \mid m$ shifts in first-order stochastic dominance toward larger F as m increases (a positive association that makes survival more likely at higher m since $\partial m_b/\partial F < 0$). A sufficiently strong negative association could, in principle, overturn monotone selection.

The heterogeneity in fixed costs can be interpreted as *endogenous* in reduced form. For example, let $F_i = F_0(m_i) - \phi\,\theta$ with $\phi \geq 0$, where θ is the aggregate avoidance share (network effects or thicker intermediation reduce setup costs). The cutoff solves

$$H(m_b, F; \tau_d, \tau_a) = 0 \quad \text{where } F = F_0(m_b) - \phi\,\theta,$$

and the aggregate share is the fixed point

$$\theta \;=\; T(\theta) \;\equiv\; \Pr\big(m < m_b(F_0(m) - \phi\theta; \tau^d, \tau^a)\big).$$

Since $\partial m_b/\partial F < 0$, we have $\partial m_b/\partial \theta = -\phi\,\partial m_b/\partial F > 0$, so T is monotone; a fixed point exists by Tarski's theorem. Uniqueness holds under a mild slope/contraction condition (e.g., $\sup_\theta |T'(\theta)| < 1$, which obtains for sufficiently small ϕ or under standard log-concavity/MLR shape restrictions). Under these conditions, the comparative statics $\partial m_b/\partial \tau^d > 0$ and $\partial m_b/\partial \tau^a < 0$ carry through, and the border–cohort ordering $\beta_{\text{border}} \geq \beta_{\text{cohort}}$ is unchanged. Moreover, $\phi > 0$ steepens T and makes $\theta(\tau^d)$ more concave (stronger diffusion/plateau), reinforcing the dynamics in Section 4.

A.6 MCPF Derivation

Suppose a planner has access to a vector of linear tax instruments $\boldsymbol{\tau}$ (with individual elements τ_i). Each tax instrument has a corresponding base $B_i(\cdot)$. For simplicity, assume no spillovers across instruments, so B_i depends only on its own instrument. The government chooses instruments to maximize a well-behaved welfare function $W(\boldsymbol{\tau})$ subject to a revenue constraint:

$$\max_{\boldsymbol{\tau}} W(\boldsymbol{\tau}) \quad \text{subject to} \quad G = R(\boldsymbol{\tau}) \equiv \sum_{i=1}^{N} \tau_i \, B_i(\tau_i). \tag{SPP}$$

Denote λ as the shadow value of public funds:

$$\lambda \equiv -\frac{\partial W/\partial \tau_i}{\partial R/\partial \tau_i} \quad \iff \quad \frac{\partial W/\partial \tau_i}{\partial R/\partial \tau_i} = \frac{\partial W/\partial \tau_j}{\partial R/\partial \tau_j} \;\; \forall\, i, j.$$

With no spillovers,

$$\frac{\partial R}{\partial \tau_i} = B_i(\tau_i) + \tau_i \frac{\partial B_i}{\partial \tau_i} = B_i(\tau_i) \underbrace{\left(1 + \tau_i \frac{(\partial B_i/\partial \tau_i)}{B_i(\tau_i)}\right)}_{1+\eta_i}, \qquad \eta_i \equiv \frac{\partial \ln B_i}{\partial \ln \tau_i}.$$

Under quasi-linear preferences and in partial equilibrium, the marginal welfare cost of increasing each instrument equals the domestic willingness to pay to avoid it, i.e.

$$-\frac{\partial W}{\partial \tau_i} \;=\; \beta_i \, B_i(\tau_i),$$

where β_i is the (cohort) domestic incidence per unit increase in instrument i. Hence

$$\text{MCPF}_{\tau_i} = \frac{-\,(\partial W/\partial \tau_i)}{\partial R/\partial \tau_i} = \frac{\beta_i \, B_i(\tau_i)}{B_i(\tau_i)\,(1+\eta_i)} = \frac{\beta_i}{1+\eta_i}.$$

Specialization to the wedge. In our application the policy instrument is the effective wedge $\Delta \equiv \ln\frac{1+\tau_d}{1+\tau_a}$. Identifying $i = \Delta$ and using $\beta_\Delta = \beta_{\text{cohort}} = (1-\theta)\beta_{\text{border}}$ and $\eta_\Delta = \eta_{\text{other}} - \frac{\theta}{1-\theta}\psi$, the expression above delivers the MCPF used in the main text.

B From Firm-Level Selection to Product-Level Observables

The model developed in Section 2 characterizes firm-level choices: each exporter i with marginal cost m_i optimally chooses whether to ship directly or avoid. However, the empirical analysis I do in Section 4 operates at the HS6 product level, where trade flows aggregate across potentially many firms. This appendix bridges the firm-level theory and product-level empirics by showing how individual selection decisions manifest in observable product-level statistics.

Consider an HS6 product code k as defining a market populated by a measure-one continuum of monopolistically competitive firms, each drawing marginal cost m_i from the distribution $F_m(\cdot)$ with density $f_m(\cdot)$. Before the tariff increase, all firms ship directly. After the tariff increase, the routing cutoff $\widehat{m}(\tau^d, \tau^a)$ from Proposition 1 partitions this continuum: firms with $m_i < \widehat{m}$ select into avoidance, while firms with $m_i \geq \widehat{m}$ continue direct shipping. The product-level avoidance share θ_k is simply the mass of firms below the cutoff:

$$\theta_k = \Pr(m_i < \widehat{m}) = F_m(\widehat{m}(\tau_k^d, \tau_k^a)). \tag{A.1}$$

This aggregation has three immediate implications for product-level observables. First, direct trade volumes fall not only because of standard substitution effects but also because the extensive margin of firms exits the direct channel. For product k, observed direct imports are

$$\text{Imports}_k^{\text{direct}} = \int_{\widehat{m}}^{\overline{m}} q_i^*(m_i; \tau_k^d)\, dF_m(m_i), \tag{A.2}$$

which mechanically declines as $\widehat{m}$ rises. This is the quantity effect documented in Table 1, Column (1)-(2): products with higher θ_k exhibit larger declines in direct trade flows relative to pre-tariff trends.

Second, and more subtly, the composition of surviving direct exporters shifts toward higher-cost, higher-passthrough firms. Since price is increasing in marginal cost (Lemma 1), the average unit value of direct shipments is

$$\overline{p}_k^{\text{direct}} = \frac{\int_{\widehat{m}}^{\overline{m}} p_i^*(m_i; \tau_k^d) \cdot q_i^*(m_i; \tau_k^d)\, dF_m(m_i)}{\int_{\widehat{m}}^{\overline{m}} q_i^*(m_i; \tau_k^d)\, dF_m(m_i)}. \tag{A.3}$$

As the cutoff $\widehat{m}$ rises with tariff exposure, low-cost firms with low prices exit, mechanically raising the weighted-average unit value in the denominator's integral. However, under Kimball demand (D5), *passthrough* is also increasing in marginal cost (Proposition 2), so the exiters are precisely the low-passthrough, high-markup firms. This creates a composition effect that works in the *opposite* direction: the remaining direct shippers are higher-cost but also better able to pass through cost increases.

The net effect on observed unit values depends on which force dominates. Proposition 1 implies that exiters are low-m_i firms, which under standard pricing (p_i increasing in m_i)

have *lower* prices. Their exit should *raise* average unit values. However, Table 1, Columns (3)-(4) documents the opposite: products with higher θ_k exhibit *lower* observed unit values for direct shipments. This negative price-diversion correlation is consistent with the theoretical prediction that compositional shifts toward higher-cost survivors are overwhelmed by other adjustments. One interpretation is that the exiters were high-*quality* varieties (which may have low marginal cost but high prices due to quality premia), so their departure lowers average unit values even as average marginal costs rise. Alternatively, remaining firms may engage in quality downgrading to preserve market share, a margin not modeled here. Distinguishing between these mechanisms is an important avenue for future work, but the key point for this paper is that both the quantity decline and the price pattern are consistent with selection driving compositional change in the direct-shipping sample.[15]

Third, the product-level passthrough regression in Equation 3 averages over the selected survivor distribution. Discretizing for exposition, suppose product k consists of N_k firms in the pre-tariff period. Post-tariff, $(1-\theta_k)N_k$ firms ship directly, each with passthrough $\beta_i(m_i)$. The border-design regression estimates

$$\widehat{\beta}_{k,\text{border}} = \frac{1}{(1-\theta_k)N_k} \sum_{i \in \text{Survivors}} \beta_i(m_i) = \mathbb{E}[\beta_i \mid m_i \geq \widehat{m}], \tag{A.4}$$

which is the survivor-weighted average passthrough. Under Kimball demand, β_i is increasing in m_i (Proposition 2), so $\mathbb{E}[\beta_i \mid m_i \geq \widehat{m}] > \mathbb{E}[\beta_i]$, confirming that the border estimate exceeds the true cohort passthrough. The product-level correction in Proposition 3 therefore applies: the bias equals $\text{Cov}(\beta_i, S_i) / \Pr(S_i = 1)$, where now $S_i = \mathbb{1}\{m_i \geq \widehat{m}\}$ is the firm-level survival indicator.

In summary, the product-level avoidance share θ_k aggregates firm-level routing decisions, and observable trade statistics—quantities, unit values, and passthrough estimates—reflect the composition of firms that select into each channel. The empirical strategy in Section 3 exploits this link by measuring θ_k at the HS6 level and using within-product variation in tariff exposure to identify the avoidance elasticity.

C Empirical Results

15. An additional consideration is that transshippers may re-label higher-value goods to evade detection, which would also contribute to the negative correlation between θ_k and observed unit values in the direct channel.

C.1 Data Construction

Countries

I include nearly 40 different countries labeled by their three-digit country code in Table C.1. In the BACI data, Taiwan is recorded as "Other Asia" and has country code S19. Restricting the set of potential transshipment partners is justified by several key considerations that enhance the analysis's focus and reliability. Primarily, it concentrates the search on countries possessing the geographic location, port infrastructure, and established trade relationships with both China and the US necessary to serve as economically viable intermediaries, excluding implausible routes. This focus improves computational tractability and leverages more reliable trade data often found with significant trading partners. Furthermore, by excluding economically or logistically unlikely countries, the restriction minimizes the risk of spurious correlations and false positives, ensuring that the identified "trade footprints" more likely reflect genuine rerouting responses to tariffs rather than noise in global trade data.

Table C.1: Potential Transshipment Hubs (ISO-3 codes; includes one BACI aggregate)

ARE	BHR	CAN	CHL	DEU	DOM
EGY	HKG	IDN	IND	ISR	JOR
JPN	KAZ	KHM	LAO	LKA	MAC
MAR	MEX	MYS	NLD	NPL	OMN
PAK	PAN	PHL	POL	RUS	SAU
SGP	THA	TUR	VNM	ZAF	TWN

Table C.2 lists fixed parameters and calibrated thresholds. The calibrated values $\tau^\star$ (growth rule) and $\phi^\star$ (levels rule) are selected separately via placebo discipline in the 2012–2017 pre-period.

We estimate pre-period trends (2012–2017) with time centered at 2014.5. For each HS6 k and partner i:

1. **Share Trends (logit).** For China/hub shares in the U.S. and in ROW,

$$\text{logit}\big(s_{k,i,t}\big) = \alpha_{k,i} + \beta_{k,i}\,(t - 2014.5) + \varepsilon_{k,i,t}, \quad \widehat{s}_{k,i,t} = \text{invlogit}\big(\widehat{\alpha}_{k,i} + \widehat{\beta}_{k,i}(t - 2014.5)\big).$$

   Fallback: if estimation is unreliable (sparse series), use the pre-period mean $\bar{s}_{k,i,\text{pre}}$.

Table C.2: Baseline Screening Parameters

Parameter	Value	Description
ϵ	10^{-6}	Share tolerance for the U.S. reallocation test and ROW test (Screens 2–3).
γ	1.0	ROW specificity multiplier: China's ROW gain must be at least γ times the hub's gain (Screen 3).
$x_{\text{hinge},\Delta}$	0	Hinge for $\tilde{x}$ in the growth rule (thousands of *current* USD).
$x_{\text{hinge,levels}}$	2000	Minimum size for $\min(m, x)$ in the levels rule (thousands of *current* USD).
$\tau^\star$	0	Growth-rule proportionality threshold (Screen 5a): $\tilde{m} \geq \tau^\star \max(\tilde{x}, x_{\text{hinge},\Delta})$.
$\phi^\star$	0.325	Levels-rule proportionality threshold (Screen 5b): $m \geq \phi^\star\, x$ (with the size hinge).
Centering Year	2014.5	Midpoint of the 2012–2017 pre-period used to center time in trend regressions.
Deflator Base	2017	Values are scaled to 2017 USD when forming θ.

Unit note: Hinges apply to nominal leg values in year t; deflation occurs after aggregation and within-HS6 capping).

2. **Leg Levels (log-linear).** For $v \in \{m_{k,i,t}, x_{k,i,t}\}$,

$$\log\big(\max(v, 1)\big) = a_{k,i} + g_{k,i}\,(t - 2014.5) + \nu_{k,i,t}, \quad \widehat{v}_{k,i,t} = \exp(\widehat{a}_{k,i} + \widehat{g}_{k,i}(t - 2014.5)).$$

   Fallback: use $\bar{v}_{k,i,\text{pre}}$.

Define "surprises" (growth above trend) as $\tilde{m}_{k,i,t} = \max\{m_{k,i,t} - \widehat{m}_{k,i,t}, 0\}$ and $\tilde{x}_{k,i,t} = \max\{x_{k,i,t} - \widehat{x}_{k,i,t}, 0\}$. ROW is world not including the U.S.

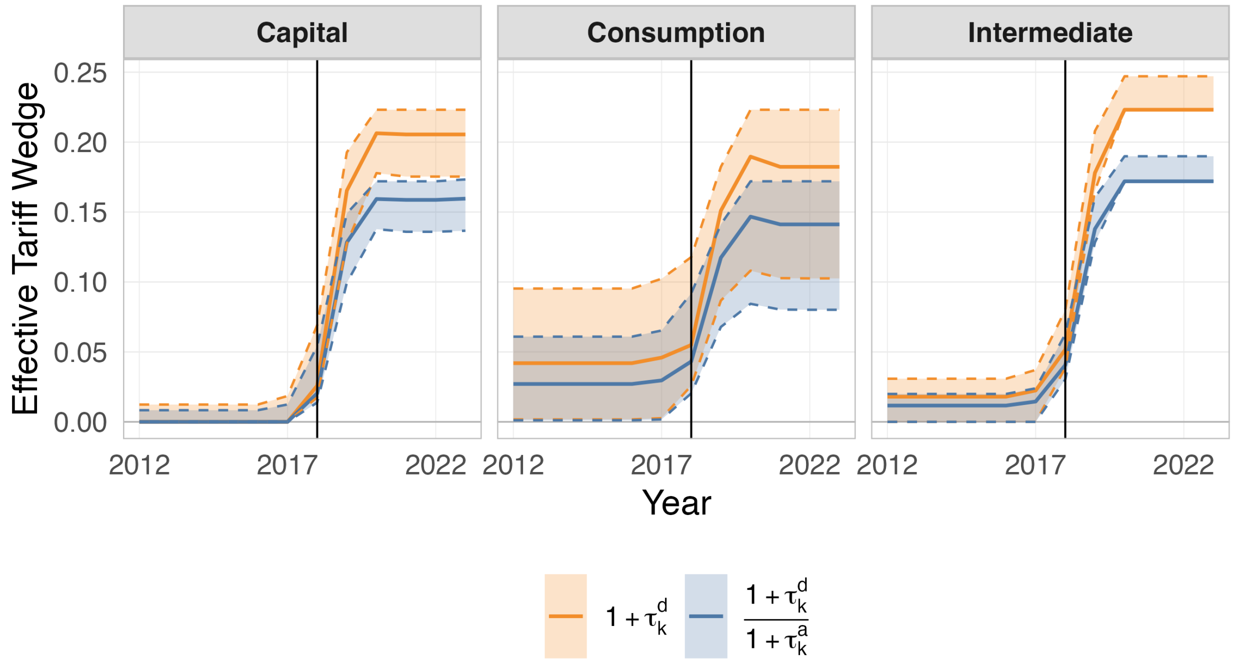
C.2 Results

Table C.3: Summary Statistics

	Pre		Post			N		
Variable	Mean	SD	Mean	SD	Post–Pre	$HS6_{\text{Pre}}$	$HS6_{\text{Post}}$	$HS6_{\cap}$
$\Delta_{k,t}$	0.025	0.066	0.121	0.062	0.096	4246	4246	4246
θ_{low}	0.014	0.092	0.045	0.148	0.031	4246	4246	4246
θ_{high}	0.037	0.160	0.079	0.212	0.042	4246	4246	4246
Share$(\theta_{\text{low}} > 0)$	0.097	0.296	0.295	0.456	0.197	4246	4246	4246
Share$(\theta_{\text{high}} > 0)$	0.110	0.313	0.263	0.440	0.153	4246	4246	4246
Consumption share (2017)	0.203	0.384	0.203	0.385	0.000	4246	4246	4246
Capital share (2017)	0.151	0.346	0.149	0.348	-0.002	4246	4246	4246
Intermediate share (2017)	0.646	0.462	0.648	0.463	0.003	4246	4246	4246

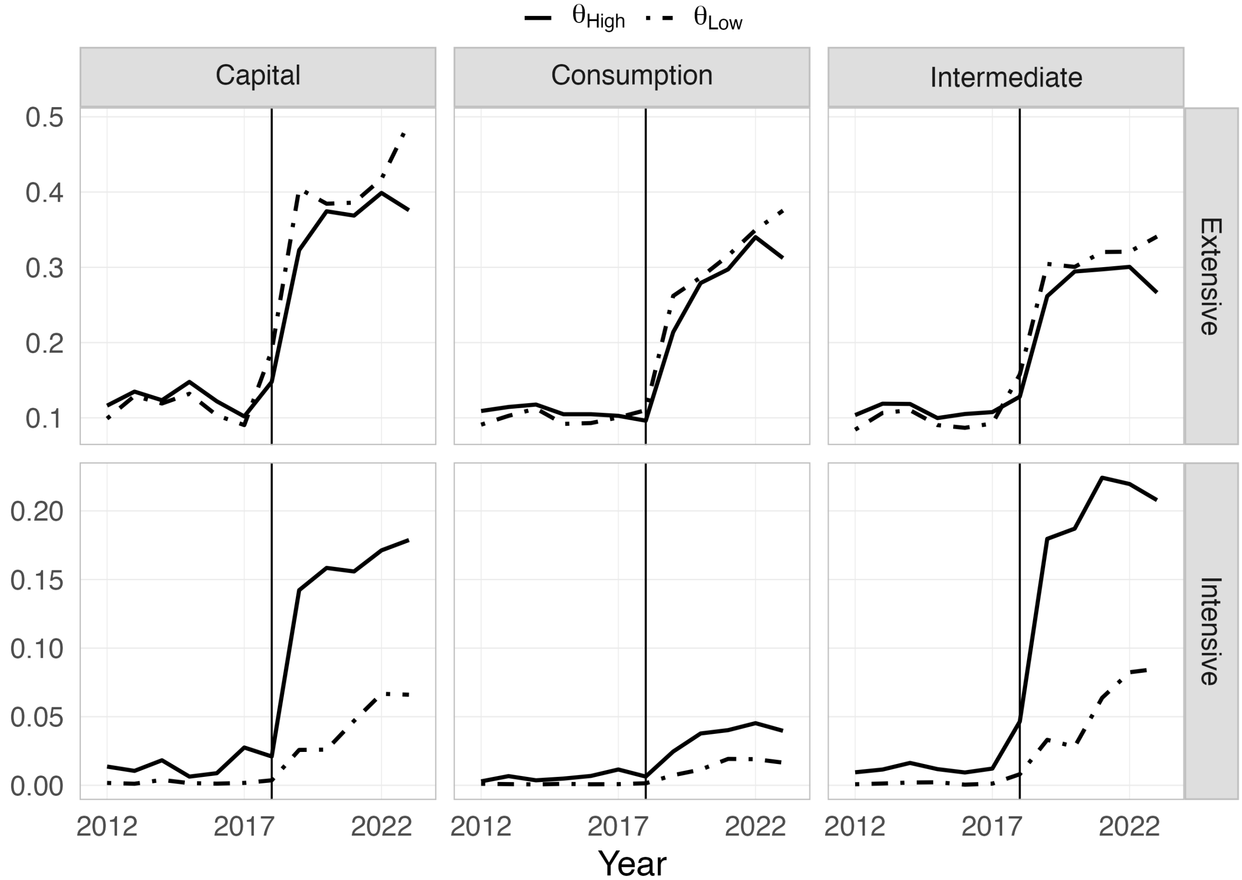
Notes: Variable construction is described in the main text.

Figure C.1: Interquartile Range of the Tariff Wedge by End Use



Note: Each panel displays the interquartile range of effective tariff rates for imported goods categorized by end use. The orange line plots the uncorrected effective tariff rate $1 + \tau^d$, while the blue line plots the tariff rate corrected for import deductibility: $(1 + \tau^d)/(1 + \tau^a)$. Since intermediates are fully tax-deductible, $t_T = \tau_t^c \times t_{D,t}$. Prior to the 2017 Tax Cuts and Jobs Act, $\tau_t^c = 0.35$; after TCJA $\tau_t^c = 0.21$. Capital goods are expensed over time. Denoting the present-value of such deductions as z_t, the effective tariff wedge for capital goods is $t_T = z\tau^c \times \tau^d$. After TCJA, $z = 1$. Prior to TCJA, z was slightly less than one for most imported capital goods. I obtain z by mapping the imported capital good HS6 codes into the corresponding IRS tax lives and calculating the present value of deductions with a discount rate of 0.06.

Figure C.2: Transshipment by End Use Category



Note: The top panels plot the share of HS6 codes within each end use category which have detected transshipment. The bottom panels display a weighted average transshipment share by end use. Each HS6 code is weighted by its share of 2017 imports from China.

D Robustness

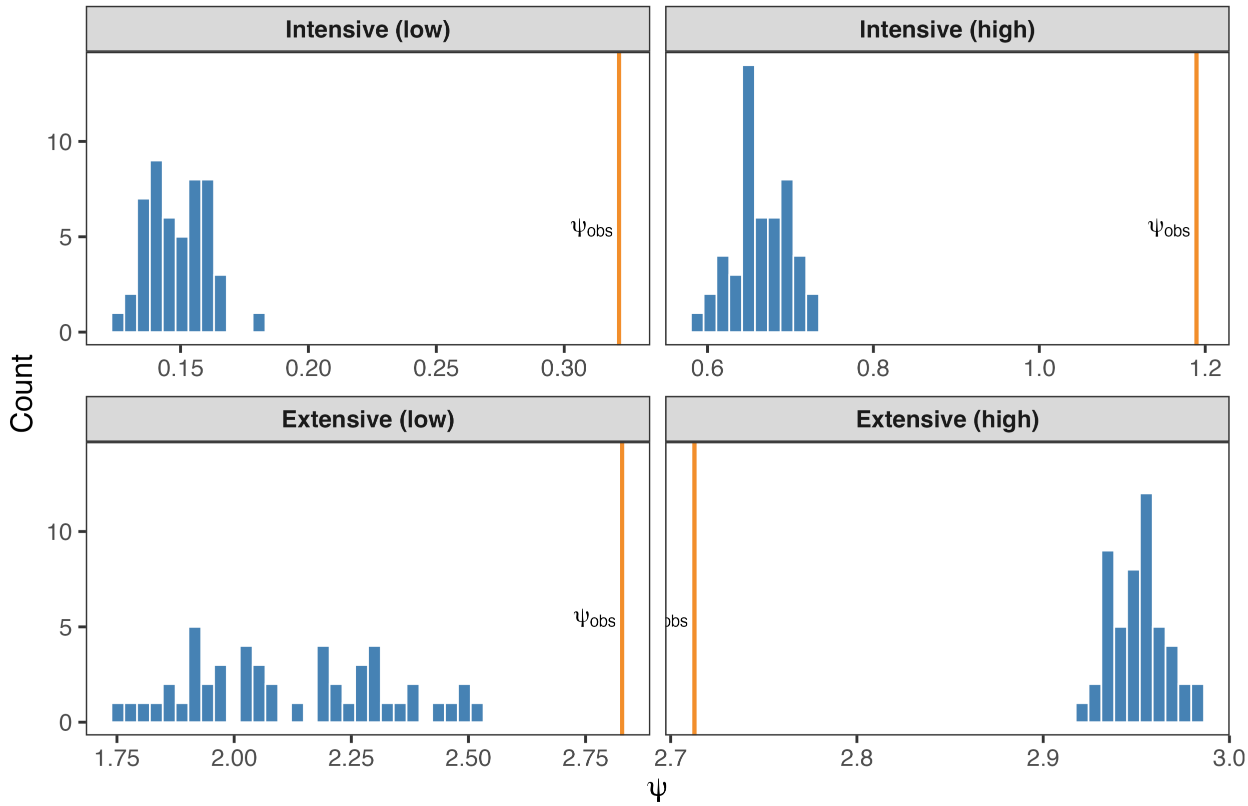
Table D.1: EU-27+UK and Canada Destination Placebo Results

EU-27+UK	$\sinh^{-1}(\theta_{\text{low}})$ (1)	(2)	$\sinh^{-1}(\theta_{\text{high}})$ (3)	(4)	$\mathbb{1}(\theta_{\text{low}} > 0)$ (5)	(6)	$\mathbb{1}(\theta_{\text{high}} > 0)$ (7)	(8)
$\Delta_{k,t}$	0.018*	0.021	0.030	0.034	0.110	0.134	0.047	0.106
	(0.010)	(0.014)	(0.020)	(0.026)	(0.158)	(0.121)	(0.158)	(0.104)
$\Delta_{k,t} \times \mathbb{1}(t \geq 2018)$		-0.003		-0.004		-0.027		-0.067
		(0.006)		(0.011)		(0.131)		(0.141)
Observations	54,564	54,564	54,564	54,564	54,564	54,564	54,564	54,564
R^2	0.29	0.29	0.32	0.32	0.33	0.33	0.37	0.37
Canada	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\Delta_{k,t}$	0.059***	0.053*	0.058**	0.012	0.242	0.095	0.162	0.057
	(0.018)	(0.027)	(0.024)	(0.040)	(0.186)	(0.215)	(0.187)	(0.202)
$\Delta_{k,t} \times \mathbb{1}(t \geq 2018)$		0.006		0.053		0.170		0.121
		(0.017)		(0.035)		(0.323)		(0.318)
Observations	52,716	52,716	52,716	52,716	52,716	52,716	52,716	52,716
R^2	0.19	0.19	0.23	0.23	0.29	0.29	0.31	0.31
HS6 & Year×Use FE	✓	✓	✓	✓	✓	✓	✓	✓
Weighted	✓	✓	✓	✓	✓	✓	✓	✓

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: Entries report two–way fixed–effects regressions of placebo diversion measures on the U.S. tariff wedge, where the wedge is the same as in the main specification and is constructed from statutory U.S. HS6 tariffs and the tax–shield (see text). Outcomes are diversion measures computed with (i) the EU–27+UK treated as a single destination (left block) and (ii) Canada as the destination (right block). For each destination and year, diversion is built from HS6 flows using the same screening steps as in the main analysis but with the destination replaced by EU–27+UK or Canada. For comparability with the U.S. baseline, the exposure gate uses the U.S. HS6 tariff panel and diversion is capped within HS6 by the 2017 China→U.S. base so that $\sum_i \theta_{k,t} \leq 1$. Columns (1)–(2) and (5)–(6) use inverse–hyperbolic–sine outcomes $\sinh^{-1}(\theta_{\text{low}})$ and $\sinh^{-1}(\theta_{\text{high}})$; columns (3)–(4) and (7)–(8) use indicators $\mathbb{1}\{\theta_{\text{low}} > 0\}$ and $\mathbb{1}\{\theta_{\text{high}} > 0\}$. All specifications include HS6 and year×use fixed effects with standard errors clustered by HS6. The bottom panel repeats the exercise for Canada.

Figure D.1: Permutation Null Distributions of β under a Broken China–Hub–US Network Mapping



Note: This figure displays the permutation-based placebo distributions for the estimated elasticity of diversion with respect to the tax wedge, β, under a broken network mapping. For each replication, the pairing between China→hub and hub→US shipment legs is randomly deranged within HS4–year product groups while keeping the marginal leg volumes, pre-trend screens, and denominator structure fixed. Each permutation thus preserves the observed scale and composition of trade at the HS4 level but eliminates the structural correspondence between the two legs of the network. The distributions shown in black correspond to the resulting estimates of β from 5,000 such random permutations, re-estimated separately for intensive and extensive outcomes. The vertical orange lines indicate the coefficients obtained under the intact network mapping. The top panels use the inverse-hyperbolic-sine transformations of the diversion measures $\sinh^{-1}(\theta_{\text{low}})$ and $\sinh^{-1}(\theta_{\text{high}})$, while the bottom panels use binary indicators for positive diversion $1\{\theta_{\text{low}} > 0\}$ and $1\{\theta_{\text{high}} > 0\}$. All specifications include HS6 and year fixed effects and are weighted by 2017 China–US import shares.

Table D.2: Permutation Tests for Pre-Period Balance

Outcome	Observed Gap	P (left)	P (right)	P (two-sided)	P (Holm)
$\sinh^{-1}(\theta_{\text{high}})$	0.006	0.960	0.040	0.079	0.238
$\sinh^{-1}(\theta_{\text{low}})$	0.001	0.999	0.001	0.001	0.005
$\mathbb{1}\{\theta_{\text{high}} > 0\}$	-0.007	0.096	0.904	0.193	0.386
$\mathbb{1}\{\theta_{\text{low}} > 0\}$	0.001	0.132	0.868	0.263	0.386

Notes: Each row reports the observed weighted mean difference (treated minus placebo) in the specified outcome during the pre-period (2012–2017), along with permutation-based p-values. For each outcome, 5,000 block permutations were performed by reassigning treated HS6 codes within HS4 product groups while preserving the overall number of treated observations. For each permutation draw, the treated–placebo gap in the weighted mean outcome (weighted by 2017 China–US import shares) was recalculated. The empirical p-values are computed as the fraction of permuted gaps that are as or more extreme than the observed gap, with Holm–Bonferroni adjustments applied for multiple outcomes. All outcomes are defined at the HS6–year level and correspond to the conservative and liberal diversion measures in the main text, expressed as either inverse hyperbolic sine or binary indicators.

Supplemental Appendix

Table S1: Transshipment Response to Tariffs

	Intensive Margin				Extensive Margin			
	$\sinh^{-1}(\theta_{\text{low}})$		$\sinh^{-1}(\theta_{\text{high}})$		$\mathbb{1}(\theta_{\text{low}} > 0)$		$\mathbb{1}(\theta_{\text{high}} > 0)$	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\Delta_{k,t}$	0.276***		0.867***		2.13***		2.08***	
	(0.045)		(0.135)		(0.533)		(0.528)	
$\Delta_{k,t}$ × Cap		0.264***		0.897***		2.05***		1.99***
		(0.064)		(0.211)		(0.671)		(0.632)
$\Delta_{k,t}$ × Con		0.158***		0.389***		1.36*		1.23
		(0.045)		(0.132)		(0.815)		(0.833)
$\Delta_{k,t}$ × Int		0.502***		1.60***		3.60***		3.71***
		(0.085)		(0.182)		(0.405)		(0.474)
R^2	0.40	0.41	0.49	0.50	0.52	0.52	0.52	0.52
Observations	54,912	54,912	54,912	54,912	54,912	54,912	54,912	54,912
HS6 & Year×Use FE	✓	✓	✓	✓	✓	✓	✓	✓
Weighted	✓	✓	✓	✓	✓	✓	✓	✓

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the coefficients for regressions of (7). Each HS6–year observation is fractionally allocated across end-uses according to its UN-BEC shares. We estimate on the stacked HS6–year–end-use panel with HS6 and year × end-use fixed effects; weights are pre-period end-use share multiplied by the HS6 code's share of imports from China to the United States, and standard errors are clustered by HS6.

Table S2: Transshipment Response to Tariffs (Clustered by HS4 × year)

	Intensive Margin				Extensive Margin			
	$\sinh^{-1}(\theta_{\text{low}})$		$\sinh^{-1}(\theta_{\text{high}})$		$\mathbb{1}(\theta_{\text{low}} > 0)$		$\mathbb{1}(\theta_{\text{high}} > 0)$	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\Delta_{k,t}$	0.276***		0.867***		2.13***		2.08***	
	(0.065)		(0.144)		(0.576)		(0.589)	
$\Delta_{k,t}$ × Cap		0.264***		0.897***		2.05**		1.99**
		(0.077)		(0.223)		(0.702)		(0.676)
$\Delta_{k,t}$ × Con		0.158***		0.389**		1.36		1.23
		(0.048)		(0.135)		(0.766)		(0.815)
$\Delta_{k,t}$ × Int		0.502***		1.60***		3.60***		3.71***
		(0.125)		(0.183)		(0.502)		(0.532)
R^2	0.40	0.41	0.49	0.50	0.52	0.52	0.52	0.52
Observations	54,912	54,912	54,912	54,912	54,912	54,912	54,912	54,912
HS6 & Year×Use FE	✓	✓	✓	✓	✓	✓	✓	✓
Weighted	✓	✓	✓	✓	✓	✓	✓	✓

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the coefficients for regressions of (7). Each HS6–year observation is fractionally allocated across end-uses according to its UN-BEC shares. We estimate on the stacked HS6–year–end-use panel with HS6 and year × end-use fixed effects; weights are pre-period end-use share multiplied by the HS6 code's share of imports from China to the United States, and standard errors are clustered by HS4 and year.

Table S3: Transshipment Response to Tariffs (unweighted)

	Intensive Margin				Extensive Margin			
	$\sinh^{-1}(\theta_{\text{low}})$		$\sinh^{-1}(\theta_{\text{high}})$		$\mathbb{1}(\theta_{\text{low}} > 0)$		$\mathbb{1}(\theta_{\text{high}} > 0)$	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\Delta_{k,t}$	0.131***		0.202***		0.612***		0.529***	
	(0.019)		(0.030)		(0.071)		(0.067)	
$\Delta_{k,t}$ × Cap		0.125***		0.145*		0.404***		0.295**
		(0.038)		(0.078)		(0.126)		(0.120)
$\Delta_{k,t}$ × Con		0.044*		0.092***		0.408***		0.347***
		(0.025)		(0.028)		(0.097)		(0.092)
$\Delta_{k,t}$ × Int		0.186***		0.295***		0.828***		0.742***
		(0.031)		(0.048)		(0.119)		(0.112)
R^2	0.23	0.23	0.27	0.27	0.31	0.31	0.32	0.32
Observations	54,912	54,912	54,912	54,912	54,912	54,912	54,912	54,912
HS6 & Year×Use FE	✓	✓	✓	✓	✓	✓	✓	✓
Weighted	✓	✓	✓	✓	✓	✓	✓	✓

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the coefficients for regressions of (7). Each HS6–year observation is fractionally allocated across end-uses according to its UN-BEC shares. We estimate on the stacked HS6–year–end-use panel with HS6 and year × end-use fixed effects; weights are pre-period end-use share (since we are stacking by end-use, this is the same as unweighted), and standard errors are clustered by HS6.

Table S4: Transshipment Response to Tariffs (No Deductibility)

	$\sinh^{-1}(\theta_{\text{low}})$		$\sinh^{-1}(\theta_{\text{high}})$	
	(1)	(2)	(3)	(4)
$1+\tau^{d}_{k,t}$	0.128^{***}		0.430^{***}	
	(0.039)		(0.121)	
$1+\tau^{d}_{k,t} \times$ Cap		0.113^{**}		0.394^{**}
		(0.054)		(0.175)
$1+\tau^{d}_{k,t} \times$ Con		0.094^{***}		0.226^{***}
		(0.029)		(0.081)
$1+\tau^{d}_{k,t} \times$ Int		0.257^{***}		0.981^{***}
		(0.048)		(0.137)
R^2	0.39	0.40	0.48	0.49
Observations	54,912	54,912	54,912	54,912
HS6 & Year×Use FE	✓	✓	✓	✓
Weighted	✓	✓	✓	✓

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table repeats the intensive margin exercise from Table S1, but uses the tariff wedge gross of the deductibility penalty as a regressor. Each HS6–year observation is fractionally allocated across end-uses according to its UN-BEC shares. We estimate on the stacked HS6–year–end-use panel with HS6 and year × end-use fixed effects; weights are pre-period end-use share multiplied by the HS6 code's share of imports from China to the United States, and standard errors are clustered by HS6.

Table S5: Transshipment Response to Tariffs (with HS4×Year clustering and HS6, year, and HS2×Year Fixed Effects)

	Intensive Margin				Extensive Margin			
	$\sinh^{-1}(\theta_{\text{low}})$		$\sinh^{-1}(\theta_{\text{high}})$		$\mathbb{1}(\theta_{\text{low}} > 0)$		$\mathbb{1}(\theta_{\text{high}} > 0)$	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\Delta_{k,t}$	0.383***		1.26***		2.48***		2.42***	
	(0.087)		(0.166)		(0.673)		(0.689)	
$\Delta_{k,t}$ × Cap		0.327***		1.09***		2.57**		2.52**
		(0.079)		(0.196)		(0.890)		(0.913)
$\Delta_{k,t}$ × Con		0.266***		0.742***		1.80**		1.78**
		(0.069)		(0.177)		(0.631)		(0.663)
$\Delta_{k,t}$ × Int		0.458***		1.53***		2.56***		2.46***
		(0.107)		(0.199)		(0.577)		(0.596)
R^2	0.42	0.43	0.52	0.53	0.58	0.58	0.58	0.58
Observations	54,912	54,912	54,912	54,912	54,912	54,912	54,912	54,912
HS6 & Year & Year×HS2 FE	✓	✓	✓	✓	✓	✓	✓	✓
Weighted	✓	✓	✓	✓	✓	✓	✓	✓

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the coefficients for regressions of (7). Each HS6–year observation is fractionally allocated across end-uses according to its UN-BEC shares. We estimate on the stacked HS6–year–end-use panel with HS6, year, and year × HS2 fixed effects; weights are pre-period end-use share (since we are stacking by end-use, this is the same as unweighted), and standard errors are clustered by HS4 and year.

Table S6: Pre-Period Test

	Intensive Margin		Extensive Margin	
	$\sinh^{-1}(\theta_{\text{low}})$	$\sinh^{-1}(\theta_{\text{high}})$	$\mathbb{1}(\theta_{\text{low}} > 0)$	$\mathbb{1}(\theta_{\text{high}} > 0)$
	(1)	(2)	(3)	(4)
$\Delta_{k,t}$	0.281***	0.882***	2.07***	1.98***
	(0.040)	(0.124)	(0.576)	(0.580)
$\Delta_{k,t}$ × I(year¡2018)	-0.052	-0.142	0.556	0.864
	(0.070)	(0.205)	(0.957)	(0.943)
R^2	0.40	0.49	0.52	0.52
Observations	54,912	54,912	54,912	54,912
HS6 & Year×Use FE	✓	✓	✓	✓
Weighted	✓	✓	✓	✓

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: Each column repeats the main regression specification for both intensive and extensive margins, but controls for the pre-period with an indicator variable for pre-tariff years. Each HS6–year observation is fractionally allocated across end-uses according to its UN-BEC shares. We estimate on the stacked HS6–year–end-use panel with HS6 and year × end-use fixed effects; weights are pre-period end-use share multiplied by the HS6 code's share of imports from China to the United States, and standard errors are clustered by HS6.

Table S7: Slope-adjusted difference-in-differences

	$\sinh^{-1}(\tilde{\theta}_{\text{low}})$	$\sinh^{-1}(\tilde{\theta}_{\text{high}})$	$\mathbb{1}(\tilde{\theta}_{\text{low}} > 0)$	$\mathbb{1}(\tilde{\theta}_{\text{high}} > 0)$
	(1)	(2)	(3)	(4)
$\Delta_{k,t}$	0.264***	0.766***	2.08***	2.09***
	(0.047)	(0.177)	(0.692)	(0.703)
R^2	0.40	0.47	0.52	0.51
Observations	54,912	54,912	54,912	54,912
Weighted	✓	✓	✓	✓
HS6 & Year×Use FE	✓	✓	✓	✓

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: Each outcome is first residualized by its HS6-specific linear trend estimated over 2012–2017, removing product-level pre-slopes before estimation. We then re-estimate the baseline specification with HS6 and year×end-use fixed effects, weighted by 2017 China→US import shares multiplied by pre-period end-use share and clustered by HS6. Coefficients therefore capture the effect of the tariff wedge on deviations from each product's pre-trend.

Table S8: Long-differenced Specification

	$\sinh^{-1}(\theta_{\text{Low}})$	$\sinh^{-1}(\theta_{\text{High}})$	$\mathbb{1}(\theta_{\text{low}} > 0)$	$\mathbb{1}(\theta_{\text{high}} > 0)$	$\mathbb{1}(\theta^{\text{Act}}_{\text{low}} > 0)$	$\mathbb{1}(\theta^{\text{Act}}_{\text{high}} > 0)$
	(1)	(2)	(3)	(4)	(5)	(6)
$\Delta_{k,\text{Post}} - \Delta_{k,\text{Pre}}$	0.224***	0.684***	1.48***	1.50***	1.53**	1.48**
	(0.050)	(0.135)	(0.474)	(0.474)	(0.607)	(0.591)
Observations	4,246	4,246	4,246	4,246	2,640	2,513
R^2	0.18	0.24	0.12	0.11	0.16	0.15
Weighted	✓	✓	✓	✓	✓	✓
Use FE	✓	✓	✓	✓	✓	✓

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: For each HS6 we compute pre (2012–2017) and post (2018–2023) means of the outcome and the effective wedge, form long differences, and regress the long-differenced outcomes on the long-differenced wedge with end-use fixed effects. All regressions are weighted by 2017 import shares and standard errors are clustered by HS6. Columns (5)-(6) estimate an "entry" of the extensive margin in which we restrict the sample to HS6 codes with zero evidence of pre-period transshipment.

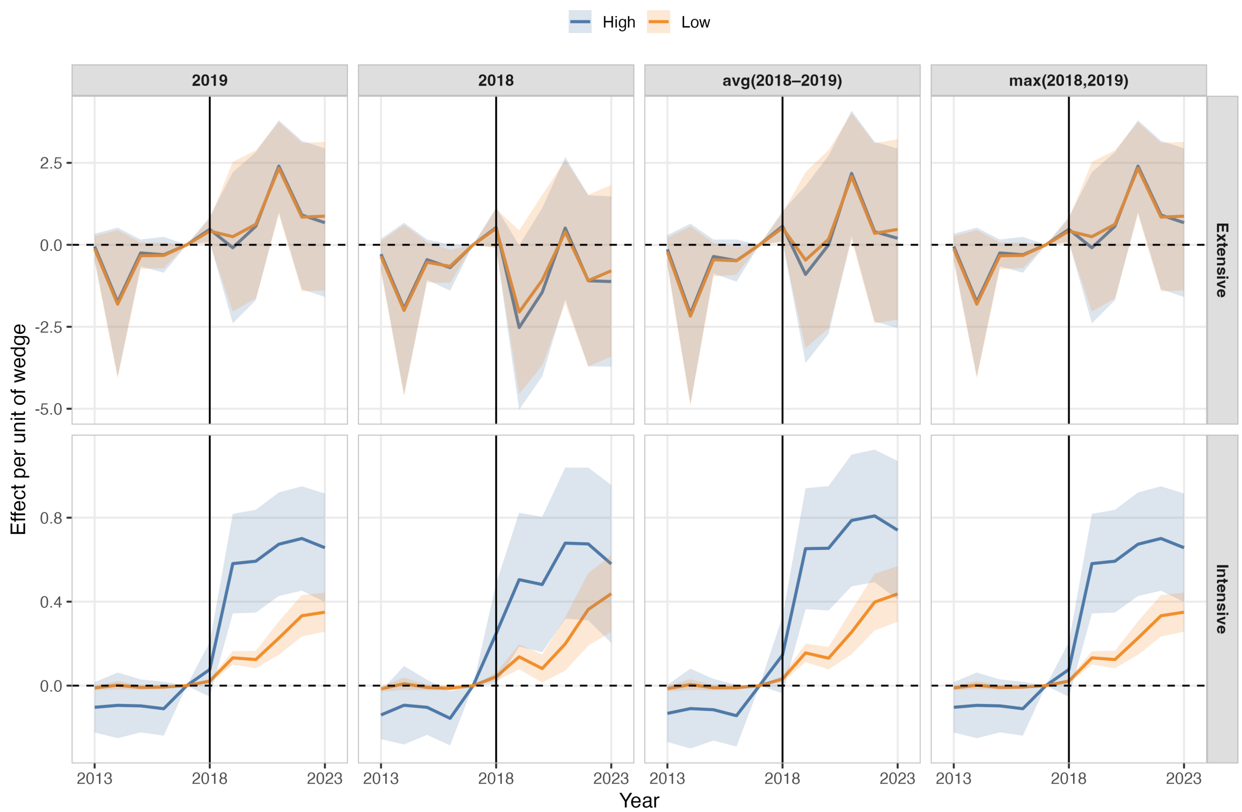
Table S9: Baseline Specification with HS4 Trends

	$\sinh^{-1}(\theta_{\text{low}})$	$\sinh^{-1}(\theta_{\text{high}})$	$\mathbb{1}(\theta_{\text{low}} > 0)$	$\mathbb{1}(\theta_{\text{high}} > 0)$
	(1)	(2)	(3)	(4)
$\Delta_{k,t}$	0.183***	0.813***	2.85***	2.76***
	(0.030)	(0.132)	(0.819)	(0.862)
R^2	0.54	0.61	0.59	0.59
Observations	54,912	54,912	54,912	54,912
HS6 & Year×Use FE	✓	✓	✓	✓
Weighted	✓	✓	✓	✓
HS4 Trend	✓	✓	✓	✓

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table repeats the baseline specification but adds linear trends for HS4 codes. Each HS6–year observation is fractionally allocated across end-uses according to its UN-BEC shares. We estimate on the stacked HS6–year–end-use panel with HS6 and year × end-use fixed effects; weights are pre-period end-use share multiplied by the HS6 code's share of imports from China to the United States, and standard errors are clustered by HS6.

Figure S1: Robustness to Anchoring Differently



Note: This figure estimates the event–study under four alternative definitions of the exposure anchor used to scale coefficients: (i) the 2019 effective wedge (baseline), (ii) the 2018 wedge, (iii) the average of the 2018–2019 wedges, and (iv) the maximum of the two. The dynamic profiles are nearly identical across anchors, with flat pre–trends through 2017, a sharp rise in 2018–2019, and persistent positive coefficients thereafter. The only exception is the *extensive* margin when exposure is anchored to 2018, where the coefficients appear attenuated or slightly negative immediately after 2018.

This discrepancy is mechanical. The 2018 wedge captures only the partial–year effect of tariff exposure, since most Section 301 tariffs were announced in the spring and implemented between July and September 2018. The exposure variable therefore understates the full shock in that year, while the dependent variable—the share of HS6 products exhibiting positive transshipment—responds to the cumulative policy change by the end of the year. In other words, the denominator (the wedge) is too small relative to the behavioral response, biasing the per–unit effect downward. When exposure is defined using the 2019 wedge, or alternatively using the average or maximum of the 2018–2019 wedges to account for the phase–in of tariffs, the extensive–margin coefficients align closely with the baseline pattern. The intensive–margin results are virtually unaffected across all anchors. Overall, the event–study evidence is robust to how the exposure anchor is defined, and the muted 2018–anchor response is fully consistent with the mid–year timing of tariff implementation and the partial measurement of exposure in that year.

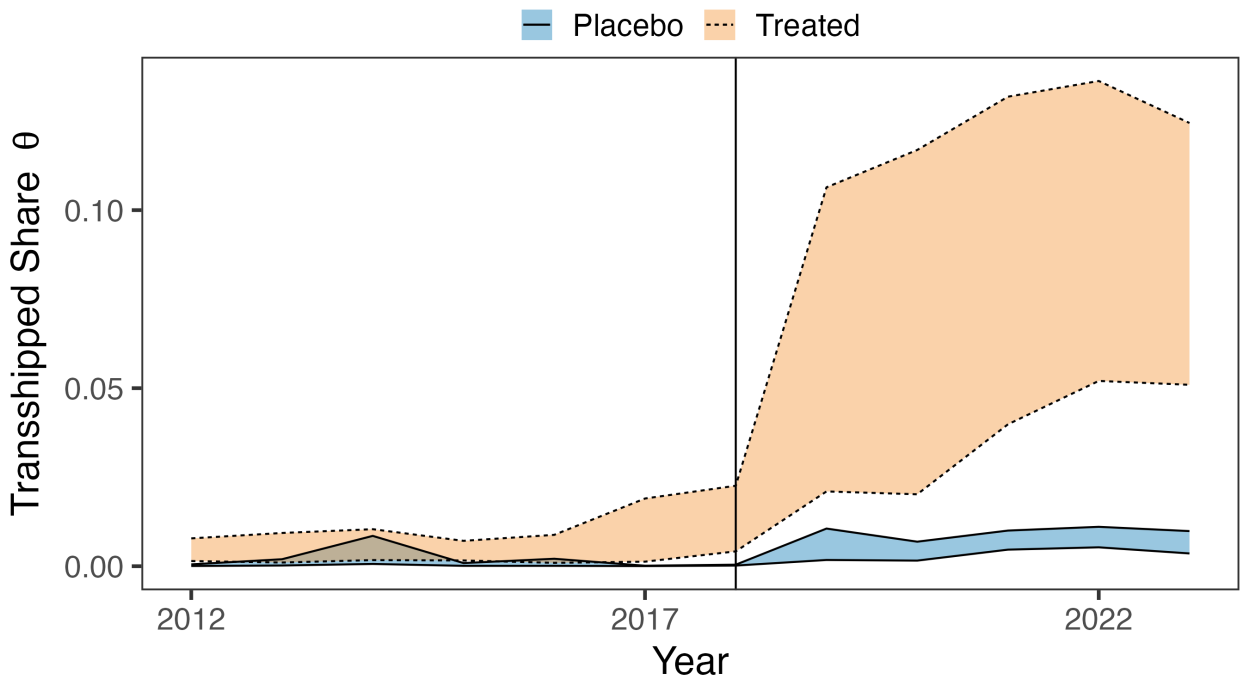
Table S10: Transshipment Response to Tariffs (2016-2019)

	Intensive Margin				Extensive Margin			
	$\sinh^{-1}(\theta_{\text{low}})$		$\sinh^{-1}(\theta_{\text{high}})$		$\mathbb{1}(\theta_{\text{low}} > 0)$		$\mathbb{1}(\theta_{\text{high}} > 0)$	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\Delta_{k,t}$	0.168***		0.854***		1.63		1.40	
	(0.028)		(0.165)		(1.08)		(1.10)	
$\Delta_{k,t}$ × Cap		0.157***		0.822***		1.32		1.15
		(0.043)		(0.260)		(1.31)		(1.32)
$\Delta_{k,t}$ × Con		0.094***		0.264**		0.669		0.302
		(0.024)		(0.125)		(1.41)		(1.45)
$\Delta_{k,t}$ × Int		0.311***		1.85***		3.87***		3.68***
		(0.046)		(0.245)		(0.480)		(0.490)
R^2	0.35	0.36	0.44	0.45	0.49	0.49	0.47	0.47
Observations	22,880	22,880	22,880	22,880	22,880	22,880	22,880	22,880
HS6 & Year×Use FE	✓	✓	✓	✓	✓	✓	✓	✓
Weighted	✓	✓	✓	✓	✓	✓	✓	✓

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the coefficients for regressions of (7). Each HS6–year observation is fractionally allocated across end-uses according to its UN-BEC shares. We estimate on the stacked HS6–year–end-use panel with HS6 and year × end-use fixed effects; weights are pre-period end-use share multiplied by the HS6 code's share of imports from China to the United States, and standard errors are clustered by HS6.

Figure S2: Aggregate Transshipment Comparing Treated to Untreated Codes



Notes: This shows the intensive margin of transshipment for both treated and untreated HS6 codes following the construction as described in the text.

Table S11: The Entropy-Weighted Effect of Tariffs on Transshipment

	Intensive Margin		Extensive Margin	
	$\sinh^{-1}(\theta_{\text{low}})$	$\sinh^{-1}(\theta_{\text{high}})$	$\mathbb{1}(\theta_{\text{low}} > 0)$	$\mathbb{1}(\theta_{\text{high}} > 0)$
	(1)	(2)	(3)	(4)
$\Delta_{k,t}$	0.275***	0.862***	2.12***	2.06***
	(0.045)	(0.135)	(0.532)	(0.527)
Observations	54,912	54,912	54,912	54,912
R^2	0.40	0.49	0.52	0.52
HS6 & Year×Use FE	✓	✓	✓	✓
Weighted	✓	✓	✓	✓

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: We re-weight HS6 units via entropy balancing so that treated (top quartile of pre-period wedge) and control have identical pre-period moments of diversion (winsorized and standardized). The panel regressions are re-estimated with weights = 2017 exposure × EB weights × pre-period end-use share, HS6 and year×end-use fixed effects, and HS4-clustered SEs. Coefficients are per unit of the wedge. Estimates are effectively unchanged, indicating the results are not driven by pre-period imbalances.

Table S12: Heterogeneous Fixed Costs Along the Intensive Margin

	$\sinh^{-1}(\theta_{low})$			$\sinh^{-1}(\theta_{high})$		
	(1)	(2)	(3)	(4)	(5)	(6)
$\Delta_{k,t}$	0.266***	0.266***	0.208***	0.803***	0.802***	0.614***
	(0.044)	(0.043)	(0.035)	(0.134)	(0.132)	(0.101)
$\Delta_{k,t}$ × Pre-2017 Hub Intensity	0.059			0.374**		
	(0.052)			(0.150)		
$\Delta_{k,t}$ × Bulkiness		-0.045			-0.304**	
		(0.030)			(0.151)	
$\Delta_{k,t}$ × Electronics			0.089***			0.331***
			(0.028)			(0.083)
R^2	0.40	0.40	0.41	0.50	0.49	0.51
Observations	54,912	54,912	54,912	54,912	54,912	54,912
Weighted	✓	✓	✓	✓	✓	✓
HS6 & Year×Use FE	✓	✓	✓	✓	✓	✓

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: Each column reports regressions of $\sinh^{-1}(\theta_{k,t})$ on the effective wedge and its interaction with a pre-2017 product characteristic. Hub intensity is the standardized share of China's pre-2017 exports of product k routed through third-country hubs. "Bulkiness" is kilograms per U.S. dollar of import value. "Electronics" is an indicator for HS2 codes 84, 85, or 90. HS6 and year×use fixed effects are included; observations are weighted by 2017 China→US exposure multiplied by pre-period end-use share; standard errors are clustered by HS6. Coefficients are per unit of the wedge (log gross factor). The interaction terms measure how the wedge effect varies across products with different pre-existing hub exposure, bulkiness, or electronics content.

Table S13: Heterogeneous Fixed Costs Along the Extensive Margin

	$\mathbb{1}(\theta_{\text{low}} > 0)$			$\mathbb{1}(\theta_{\text{high}} > 0)$		
	(1)	(2)	(3)	(4)	(5)	(6)
$\Delta_{k,t}$	2.07***	2.14***	1.92***	1.99***	2.04***	1.83***
	(0.533)	(0.522)	(0.541)	(0.530)	(0.518)	(0.539)
$\Delta_{k,t}$ × Pre-2017 Hub Intensity	0.345			0.493		
	(0.376)			(0.405)		
$\Delta_{k,t}$ × Bulkiness		0.043			-0.188	
		(0.260)			(0.296)	
$\Delta_{k,t}$ × Electronics			0.280			0.317
			(0.219)			(0.238)
R^2	0.52	0.52	0.52	0.52	0.52	0.52
Observations	54,912	54,912	54,912	54,912	54,912	54,912
Weighted	✓	✓	✓	✓	✓	✓
HS6 & Year×Use FE	✓	✓	✓	✓	✓	✓

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

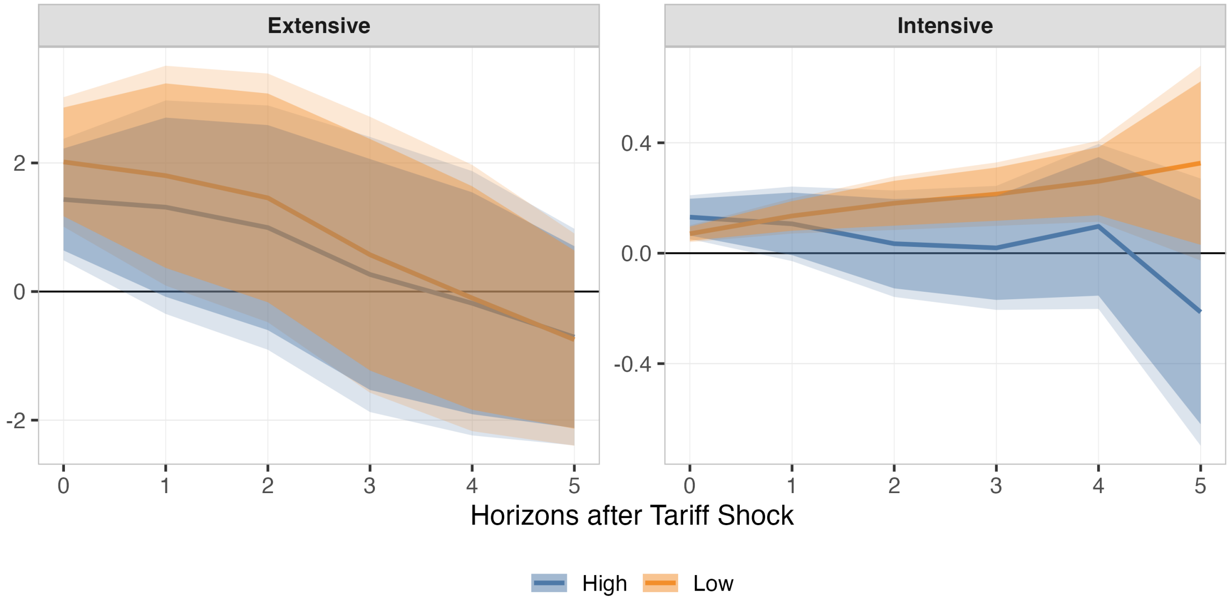
Notes: The procedure is the same as in Table S12, except we now have the extensive margin as the dependent variable.

Table S14: Percent difference between baseline and leave-one-out: $100 \times \frac{\beta_{LOO}-\beta}{\beta}$

Country Code	Extensive (High)	Extensive (Low)	Intensive (High)	Intensive (Low)
CAN	-17.741	-17.074	17.887	19.343
JPN	-3.000	-4.686	30.124	29.223
MEX	4.916	4.173	31.988	25.131
DEU	-8.019	-7.489	23.321	13.826
SGP	-17.408	-16.486	2.176	3.188
TWN	-10.480	-11.643	3.447	5.798
MYS	-1.852	-2.032	3.420	9.883
VNM	-45.038	-44.523	0.136	4.061
HKG	-13.705	-12.496	0.476	1.888
IDN	-1.840	-1.933	1.845	5.406
IND	-0.433	0.745	7.272	8.685
THA	-1.719	-0.346	3.393	7.414
KHM	-1.019	1.706	1.225	6.550
POL	-1.388	-0.611	2.198	2.402
TUR	1.561	1.202	1.371	1.862

Notes: For each regression specification, I leave out one hub and recompute the coefficient. After that, I compute the percent difference between the leave-one-out estimate and the baseline regression. For example, the leave-one-out coefficient when excluding Canada from the sample is 17 percent smaller than the baseline intensive margin coefficient from Column 1 of Table S1. The table shows the most influential fifteen hubs.

Figure S3: Local Projections of Transshipment from Tariffs



Note: This figure estimates a local projection of the form

$$f(\theta_{k,t+h}) - f(\theta_{k,t-1}) = \alpha_i + \delta_{e(u)} + \psi_{t+h}\Delta_{k,t} + \varepsilon_{k,t}.$$

I do this for both the extensive margin, using the linear probability model from the main text, as well as an intensive margin with the inverse hyperbolic sine transformation. Standard errors are clustered by HS6 code.