

# The Decline of Multilateralism and Foreign Lobbying: Evidence from the WTO Appellate Body Crisis

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## Abstract

How do firms adapt to the decline of multilateralism? I argue that the delegalization of international institutions increases the returns to domestic political lobbying for firms exposed to foreign trade barriers. Focusing on WTO adjudication, I argue that as this system becomes less reliable, the trajectory of contested trade barriers becomes less institutionally anchored and their administration less constrained by external review. These changes make lobbying a more valuable means of managing political risk and seeking firm-specific relief. I test this argument using the WTO Appellate Body crisis and a novel firm-level measure of exposure to U.S. antidumping and countervailing duties, constructed from 3.9 million bill-of-lading records and the timing of U.S. trade-remedy actions. Panel event-study estimates show that the effect of trade-remedy exposure on lobbying rises sharply in late 2017 and persists across the Trump and Biden administrations, indicating a durable shift in firm strategy. The response is concentrated among firms from active WTO litigants and those importing products previously contested at the WTO, for whom the loss of adjudication was most consequential. My findings show that the erosion of multilateral institutions does not eliminate foreign influence over trade policy, but rather redirects it toward the more opaque arena of domestic lobbying.

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# Introduction

The paralysis of the World Trade Organization (WTO)’s Appellate Body (AB) marks one of the most consequential ruptures in the postwar trade order. In December 2019, the AB lost the formal quorum to issue binding final rulings, following two years of U.S. refusal to approve new appointments. More than six years on, this impasse persists, with no viable multilateral solution in sight. Given the WTO’s central role in facilitating trade liberalization (Goldstein, Rivers, and Tomz, 2007), even among countries with divergent domestic politics (Carnegie, 2014), this incapacitation has opened a major gap in global trade governance.

The crisis raises a broader question: where does trade politics go when multilateral adjudication loses force? Trade barriers have not disappeared with the AB’s paralysis. If anything, governments have increasingly relied on unilateral and discretionary trade measures. The firms harmed by these barriers still seek relief, but the channels available to them have changed. Under a functioning WTO, firms could push governments to enforce treaty commitments through litigation, supplying the information, resources, and political support needed to overcome the constraints that often limit state action (Brutger, 2024; Davis, 2012). As access to multilateral adjudication weakens, however, firms must pursue their interests through other means.

This paper examines one such adaptation: direct lobbying by foreign firms in the country imposing trade barriers. I argue that the *delegalization* of multilateral trade adjudication—specifically the erosion of delegated legal authority in the international trade regime (Abbott et al., 2000)—raises the expected return to lobbying for firms exposed to foreign trade barriers, through two channels. As appellate review erodes, the future path of a contested barrier becomes less predictable, which increases the value of lobbying as a means of man-

aging political risk (Buzard, Canen, and Saiegh, 2026; Hassan et al., 2019). And as external discipline over administrative decisions weakens, officials gain latitude to treat similar firms differently, which raises what lobbying can accomplish. Firms that might otherwise have relied on rule-based adjudication therefore have stronger incentives to seek relief through domestic political channels. The core implication is that firms' lobbying responses to trade barriers should grow as multilateral adjudication becomes less effective.

I test this argument using the weakening of the WTO dispute settlement system as an empirical case. I develop a novel measure of firm-level exposure to the U.S. trade remedy process that combines within-firm variation in the product-country composition of imports, drawn from bill-of-lading records, with the administrative timing of trade remedy actions. I link this measure to U.S. lobbying data covering the full set of foreign-affiliated lobbying clients, including foreign-owned firms and their domestic subsidiaries. Using a panel event-study design that exploits within-firm variation in exposure over time, I find that firms' propensity to respond to U.S. antidumping and countervailing duties (AD/CVD) through lobbying rises sharply beginning in late 2017, as the blocking of appointments first signaled the impending breakdown of multilateral adjudication. This effect persists into the Biden administration, suggesting a durable shift in firm strategy rather than an administration-specific response. I also show that this pattern is not driven by firm exposure to trade war-related shocks, and that the increase in political activity is concentrated among firms from active WTO litigants and those importing products previously contested at the WTO, for whom the loss of adjudication was most consequential.

The paper makes several contributions. First, it provides new evidence on the political consequences of declining multilateralism. While many scholars have warned about the

erosion of the rule-based trade order, there are surprisingly few empirical research on its downstream effects. This paper shows that the weakening of WTO dispute settlement shifts firm behavior toward domestic political channels: when multilateral adjudication becomes less effective, foreign firms increasingly seek relief through direct lobbying inside the target state.

This finding points to a broader paradox in sovereignty-based critiques of international institutions. International organizations (IOs), including the WTO, are often criticized for infringing on state sovereignty by subjecting domestic policy choices to external legal review. Yet weakening international adjudication does not necessarily insulate domestic politics from foreign influence. Instead, it may redirect foreign influence from formal, legal, and publicly observable arenas into more discretionary forms of domestic political engagement. In this sense, the decline of multilateral institutions may reduce one form of sovereignty cost while increasing another: less rule-based, less transparent, and more politically mediated foreign influence within domestic policymaking.

Second, it advances research on firms as actors in international economic governance by showing how firms adapt when multilateral enforcement weakens. Existing work shows that firms help activate WTO litigation by providing governments with political support, information, legal expertise, and financial resources (Davis, 2012; Brutger, 2024). I extend this research by examining what firms do when the institutional channel through which they once supported state enforcement becomes less effective. Firms do not withdraw from politics, but rather redirect their efforts toward the domestic political institutions of the state imposing the barrier.

Finally, the paper introduces a new empirical approach for measuring firm-level expo-

sure to trade barriers. Existing studies often rely on aggregate indicators of trade policy uncertainty (Baker, Bloom, and Davis, 2016; Caldara et al., 2020), or measure exposure at the country, industry, or product level. Recent work have developed firm-level measures of political risk or tariff exposure using textual references in corporate filings (Hassan et al., 2019; Hu, Wang, and Han, 2025). While these approaches are valuable for capturing perceived or disclosed exposure, they are less suited to identify the concrete trade-policy shocks that affect firms through their product portfolios, supply chains, and ownership structures. By combining bill-of-lading records with information on corporate ownership and the timing of trade remedy actions, I construct a continuous, time-varying measure of exposure that captures how intensely individual firms are affected by specific barriers, at the level at which they encounter them. This approach makes it possible to study firm political behavior in response to trade policy shocks with greater precision than aggregate or text-based measures allow.

The remainder of the paper proceeds as follows. The next two sections discuss foreign firms' political responses to trade barriers and develop a theory of how delegalization increases the returns to lobbying. The empirical section analyzes the lobbying behavior of foreign firms exposed to U.S. trade remedy actions in the context of the WTO Appellate Body crisis. I conclude by discussing the implications of these findings for our understanding of the erosion of the international economic order.

# Foreign Firms and the Politics of Trade Barriers

When foreign governments impose trade barriers, affected firms can respond through several channels. They may adjust economically by diverting exports to alternative markets (Bown and Crowley, 2007; Handley, 2014), reorganizing supply chains (Grossman, Helpman, and Redding, 2024; Handley, Kamal, and Monarch, 2024), relocating production (Alfaro and Charlton, 2009; Antràs and Yeaple, 2014), or absorbing the resulting costs. They may also pursue political remedies through their home governments, pressing for diplomatic intervention or recourse to international legal mechanisms such as the WTO dispute settlement system (Davis, 2012; Davis and Shirato, 2007; Brutger, 2024). A third option is to engage policymakers directly in the country imposing the restriction through lobbying. These responses are not mutually exclusive, and firms often pursue several at once. This paper focuses on the structural conditions that make one response more attractive: directly lobbying policymakers in the country imposing the trade barrier.

Large multinational firms are particularly likely to pursue their interests through direct political action. Firms within the same industry often have different trade preferences, giving those with sufficiently large and concentrated stakes incentives to lobby individually rather than rely on broader industry coalitions (Kim, 2017; Osgood, 2017). Multinational corporations have especially strong incentives to do so: their scale reduces the relative cost of political engagement, while their operations across multiple jurisdictions increase the value of influencing government policy (Kim and Milner, 2019).

Lobbying gives firms access to policymakers with discretion over policy design and implementation. Rather than simply purchasing favorable outcomes, firms can use lobbying to provide information, expertise, and political support that make their preferences more use-

ful or persuasive to policymakers (Hall and Deardorff, 2006). In the executive branch, firms can seek to influence how policies are designed and implemented through administrative processes (Yackee and Yackee, 2006). Firms can also make their interests politically salient by connecting policy choices to domestic jobs, production, and other constituency interests. Firms disproportionately locate employment in electorally competitive districts, consistent with using economic presence to build political ties (Bisbee and You, 2024), while those embedded in domestic production networks are better positioned to obtain favorable trade policy because their interests implicate a broader set of domestic actors (Betz and Hummel, 2025). Lobbying can thus be mutually valuable: firms obtain access to the policy process, while policymakers receive information and support relevant to their own objectives.

Foreign ownership shapes the routes through which firms build this political influence. Foreign MNCs may command substantial economic resources while lacking some of the political relationships and domestic constituencies that locally rooted firms accumulate over time. Domestic subsidiaries provide one route into the political system: Lee (2024a) finds that foreign-owned subsidiaries lobby more frequently and spend more on lobbying than comparable U.S. multinationals, often on issues that benefit their foreign parents. Domestic economic ties can also create local allies with incentives to defend foreign investors (Johns and Wellhausen, 2016).

Foreign firms can also purchase political access that they have not built internally. External lobbyists provide relationships with policymakers that firms cannot readily reproduce on their own, and political connections command a substantial premium in the lobbying market (Bertrand, Bombardini, and Trebbi, 2014). This service can be particularly valuable for foreign clients whose political legitimacy or familiarity is less established. Consistent

with this logic, Kim and Siegel (2024) find that lobbyists charge foreign firms from autocratic countries a substantial fee premium, reflecting the additional costs of representing and legitimating these clients.

Political access can in turn produce narrow benefits in trade policy. Lobbying and political connections shape antidumping decisions, tariff suspensions, tariff exclusions, and other administrative trade outcomes (Drope and Hansen, 2004; Eldes, Lee, and Osgood, 2025; Ludema, Mayda, and Mishra, 2018; Fotak et al., 2025). These remedies can be especially attractive to individual firms because they are targeted and can sometimes generate benefits concentrated on particular products or companies. By contrast, interstate bargaining and WTO dispute settlement generally operate at a broader level and extend the benefits of concessions beyond a single firm.

In sum, existing research shows that multinational firms have strong incentives to engage politically, that foreign firms can build or purchase domestic political access, and that such access can yield favorable trade-policy outcomes. Yet lobbying does not occur in an institutional vacuum. Firms facing trade barriers may also pursue relief through diplomatic or legal channels, so the returns to domestic political engagement should depend in part on the institutional environment surrounding those alternatives. This relationship has received little attention because domestic lobbying and international dispute settlement are typically studied as separate arenas of firm political activity. The next section examines one such institutional change—the weakening of WTO dispute settlement—and argues that it raised the returns to domestic lobbying for firms exposed to contested trade barriers.

# Delegalization and the Returns to Lobbying

## The Delegalization of the Trade Regime

Foreign firms' willingness to incur the costs of domestic political engagement depends on what lobbying can plausibly deliver. I argue that the delegalization of multilateral trade adjudication increases the returns to lobbying through two related mechanisms. First, it makes the future path of contested trade barriers less tightly anchored to external adjudication and more dependent on domestic political and administrative decisions, increasing the value of political engagement over those decisions. Second, it weakens external constraints over the administration of those barriers, increasing the scope for lobbying to shape firm-specific treatment.

The argument builds on the concept of legalization, a form of institutionalization characterized by obligation, precision, and delegation (Abbott et al., 2000; Goldstein et al., 2000). Delegation, in particular, grants third parties authority to interpret and apply rules, resolve disputes, and potentially make further rules (Abbott et al., 2000). International trade has historically been among the most legalized domains of world politics—the WTO dispute settlement system represented an especially high degree of delegation, combining compulsory jurisdiction, quasi-automatic adoption of rulings, and a standing Appellate Body whose decisions the losing party could not unilaterally block (Abbott and Snidal, 2000).

Two features of the legalization framework bear directly on my theoretical argument. First, delegation is a matter of degree rather than a dichotomy; institutions occupy positions along a continuum (Abbott et al., 2000). Second, movement along that continuum can occur in either direction. Although legalization scholarship has overwhelmingly examined

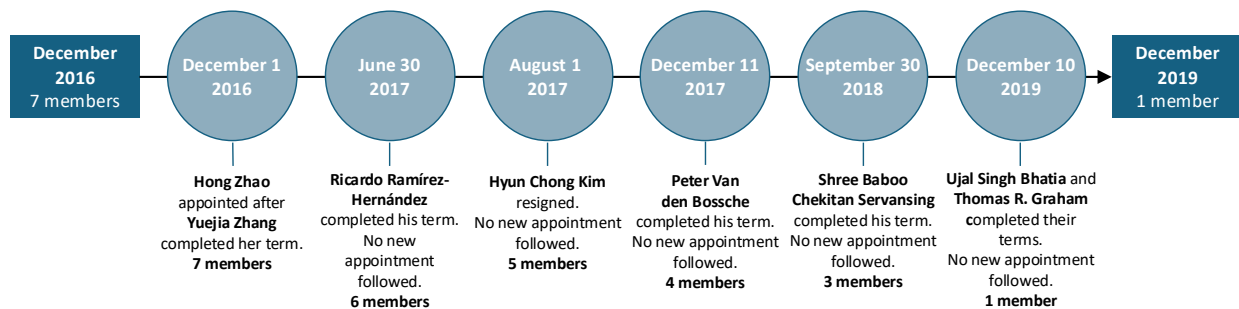


Figure 1: Timeline of WTO AB member composition, December 2016 to December 2019. Adapted from Payosova, Hufbauer, and Schott (2018).

movement toward greater legalization, institutions can also move in reverse as previously delegated authority is weakened or withdrawn (Abebe and Ginsburg, 2019). I use *delegalization* to describe this reverse movement: a reduction in obligation, precision, or delegation within an existing institutional order.<sup>1</sup>

The paralysis of the WTO Appellate Body provides an unusually clear instance of such movement. Beginning in 2017, the United States refused to consent to the appointment of new Appellate Body members. Membership fell from seven in December 2016 to four in December 2017 and to three, the minimum required to hear an appeal, in December 2018. In December 2019, the Body lost its quorum, and by November 2020 it had no sitting members (Figure 1). This was not simply an administrative lapse, but a form of deliberate court curbing in which a member state exploited the institution’s appointment rules to weaken authority it had previously delegated (Pollack, 2023; Zaccaria, 2022).

<sup>1</sup>Adjacent literatures describe related phenomena from different analytical perspectives. Whereas *delegalization* concerns changes in an institution’s degree of legalization, Abebe and Ginsburg (2019) use *dejudicialization* to describe the removal of particular areas of law from external judicial review, situating it within the literature on backlash against international courts (Madsen, Cebulak, and Wiebusch, 2018; Alter, Gathii, and Helfer, 2016). Pollack (2023), in turn, uses *de facto de-delegation* to describe efforts by principals to reduce the discretion of delegated bodies from within an existing delegation contract.

The consequences were immediate. Under WTO procedure, an appealed panel report cannot be adopted until the appeal is resolved; without a functioning Appellate Body, a losing party can appeal into the void and suspend a ruling indefinitely. This has now become standard practice rather than an edge case: a majority of panel rulings are now appealed into the void, while new dispute filings have fallen to roughly a third of their pre-crisis level (Hopewell, 2025; Pollack, 2023). In response, WTO members created the Multi-Party Interim Appeal Arbitration Arrangement (MPIA), particularly to preserve their ability to challenge trade partners' measures (Pelc, 2025). Yet the arrangement binds only its signatories, leaving nonparticipants—including the United States—free to continue appealing into the void. The result is a substantial weakening of one of the WTO's central institutional functions: the provision of binding dispute settlement.

To be sure, WTO adjudication never provided frictionless enforcement. Adjudication has always been slow, governments varied in their willingness to bring disputes, and compliance was neither automatic nor complete (Bown, 2005; Busch, Reinhardt, and Shaffer, 2009; Chaudoin, 2014; Davis and Bermeo, 2009; Davis, 2012; Guzman and Simmons, 2005). The relevant comparison is not between delegated adjudication and frictionless enforcement, but between the presence of the mechanism and its absence.

## **Delegalization and the Returns to Lobbying**

The preceding discussion establishes how delegalization changes the institutional environment surrounding contested trade barriers. I now turn to how those changes alter firms' incentives to lobby.

## **Policy uncertainty**

The first mechanism concerns uncertainty about a barrier’s future path. Firms’ investment and market-entry decisions depend not only on current trade policy but also on expectations about how that policy may change (Handley and Limão, 2015, 2017). A firm subject to a trade barrier must therefore assess how long the measure will remain in force, whether it will be revised, and whether an external process may ultimately compel its modification.

Delegated adjudication helped structure these expectations. WTO dispute settlement did not guarantee relief, but it placed contested measures within a formal and public legal process involving third-party adjudication and the prospect of compliance (Davis, 2012). Firms therefore had some basis for judging whether a measure was likely to persist, be altered, or eventually be removed. The erosion of appellate review has weakened that institutional anchor. Where a legal challenge once offered some prospect of a conclusive external resolution, the future of a contested measure is now less tightly tied to adjudication and more dependent on subsequent domestic political and administrative choices. This matters most for barriers that a firm’s home government might otherwise have contested through the WTO.

Under these conditions, lobbying becomes more valuable as a means of limiting exposure to an uncertain policy path. Firms may lobby to preserve favorable policies, prevent the escalation or extension of existing restrictions, or encourage their revision or removal. Recent work similarly conceptualizes lobbying as a costly investment through which firms can actively manage political risk (Buzard, Canen, and Saiegh, 2026; Hassan et al., 2019). As the future path of the measure becomes less institutionally anchored, firms have stronger incentives to use political engagement to shape the domestic decisions that increasingly determine whether the measure is maintained, revised, or removed.

## **Policymaker discretion**

The second mechanism concerns policymaker discretion. Trade barriers are rarely self-executing; their incidence depends on a series of administrative judgments, including which products fall within their scope, how ambiguous provisions are interpreted, what evidence is considered sufficient, and which firms qualify for exclusions (Betz and Hummel, 2025; Blonigen, 2006; Eldes, Lee, and Osgood, 2025; Grandy and Hiatt, 2020). These decisions leave room for variation across firms even when the underlying level of protection is unchanged.

Delegated adjudication constrains that room by requiring governments to defend their measures and administrative practices under common external rules (Alter, Hafner-Burton, and Helfer, 2019). The prospect of external review raises the cost of sustaining interpretations or practices that cannot be reconciled with those rules, limiting the range of administrative choices governments can defend when contested by other members. When external review weakens, officials have greater latitude to make such distinctions. This does not imply that they become more favorable to foreign firms; greater discretion can produce harsher treatment as well as accommodation. It does, however, create greater scope for political engagement to influence the treatment a particular firm receives. When officials have more room to interpret, adjust, or selectively apply a measure, lobbying has greater potential to affect the firm-specific outcome.

The key distinction is therefore between the severity of a trade barrier and the political malleability of its application. A more protectionist policy makes the barrier more costly. Delegalization increases the scope for firm-specific political input to matter in the administration of that barrier. The former increases the stakes; the latter increases what lobbying can plausibly accomplish.

## Implications

The two mechanisms point in the same direction. As adjudication becomes less reliable, lobbying becomes more valuable as a means of managing political risk and pursuing firm-specific relief. These changes should affect both whether firms lobby and how intensively they do so. Firms already engaged in political activity have stronger incentives to devote additional effort to it. Firms that previously found lobbying too costly may begin to participate once the potential benefits increase.

The argument does not require firms to abandon other responses. Economic adjustment and efforts to mobilize the home government may continue alongside direct lobbying. The claim is narrower: lobbying becomes more attractive when an external institution that previously structured the future path and constrained the administration of contested trade barriers becomes less effective.

**Hypothesis 1** *The delegalization of multilateral trade adjudication increases the extent to which foreign firms respond to trade barriers through direct lobbying.*

The argument carries a scope condition. Delegalization should matter most where WTO adjudication had previously been a meaningful part of the policy environment. Governments litigate selectively, and many trade barriers are never challenged (Bown, 2005; Davis, 2012). Firms therefore differ in whether dispute settlement plausibly shaped expectations about the measures they faced or constrained the way those measures were administered.

The effects should be strongest among firms whose home governments historically contested similar measures through the WTO. For those firms, the paralysis of appellate review weakened a previously relevant source of both policy predictability and external discipline.

Where a firm’s barriers were unlikely to be challenged, delegalization removed comparatively little, and the incentives described above should be correspondingly weaker. The empirical analysis exploits this variation in the prior relevance of WTO adjudication.

## **Empirical Strategy: U.S. Trade Remedies, Foreign Lobbying, and the WTO Crisis**

To evaluate the impact of delegalization on the relationship between trade barriers and lobbying, I focus on the case of the WTO Appellate Body crisis. The goal of the empirical analysis is to examine the impact of the WTO crisis on foreign firms’ political activities in the United States. To do so, I assess the how the relationship between trade remedy processes and foreign lobbying in the United States has evolved over time.

Based on maritime shipment data of U.S. imports and administrative data of AD/CVD petitions filed by U.S. domestic industries, I build a novel firm-specific, continuous measure of exposure to U.S. AD/CVD actions, constructed as a Bartik-style shift-share variable that exploits within-firm variation in product-country import composition. I connect this with lobbying data from LobbyView and firm-level data from Orbis to create a firm-quarter panel of exposure to AD/CVD duties and lobbying, which I then use to estimate the differentiated effects of exposure on lobbying over time.

### **Sample**

My sample of firms is the population of foreign importers to the U.S. that have ever lobbied the U.S. federal government. A firm must satisfy three criteria for inclusion: it must be

(1) foreign, (2) importing to the U.S., and (3) lobbying under the Lobbying Disclosure Act (LDA).

I define foreign firms as firms headquartered in non-U.S. countries with U.S. subsidiaries, or U.S.-headquartered firms with foreign global ultimate owners (GUOs). Under this definition, both Toyota Motor Corporation (headquartered in Japan) and Toyota Motor North America, Inc. (headquartered in the United States) are treated as foreign-affiliated. This convention captures lobbying activities by domestic subsidiaries on behalf of their foreign parents (Lee, 2024b), which would otherwise be misclassified as purely domestic.

To identify U.S. importing firms, I use Panjiva (S&P Global Market Intelligence), which records U.S. import shipments at the bill-of-lading level, including the foreign supplier, HS product codes, country of origin, and shipment counts. For lobbying data, I use LobbyView (Kim, 2018), which compiles quarterly lobbying disclosure reports filed under the LDA<sup>2</sup>.

Constructing the sample requires matching firms across three databases. I begin by matching LobbyView’s 54,341 unique lobbying clients to the 216,097 foreign firms in Orbis, using a combination of direct BvD identifier joins (where LobbyView supplies them) and Jaro–Winkler fuzzy name matching with first-letter blocking for the remainder. All fuzzy matches below a conservative distance threshold are manually reviewed. Each matched U.S. subsidiary is then linked to its foreign GUO through the Orbis ownership hierarchy, using a 50.01 percent direct-or-indirect ownership threshold. This step yields 1,755 foreign-affiliated

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<sup>2</sup>Two U.S. statutes govern lobbying by foreign clients: the LDA and the Foreign Agents Registration Act (FARA). The 1995 LDA added an exemption to FARA for agents of foreign persons and organizations who register under the LDA, leaving FARA as the framework for agents of foreign governments and political parties (22 U.S.C. § 613(h)). The exemption does not apply where a foreign government or political party is the principal beneficiary of the activities. As a result, FARA filings are dominated by foreign governments and state-owned enterprises, while foreign commercial entities appear under the LDA. I therefore focus on LDA filings.

lobbying clients that roll up to 999 unique GUOs headquartered in more than 40 countries.

I then match each of the GUOs to its corresponding shipment records in Panjiva. This step is non-trivial because Panjiva indexes shipments at the shipper (consignor) level, and a single GUO typically corresponds to many distinct shippers—foreign manufacturing subsidiaries, trading arms, regional affiliates—whose names rarely align verbatim with Orbis legal names. I therefore identify each GUO at the parent level, exploiting Panjiva’s internal corporate hierarchy to aggregate all subsidiary shipments. For each GUO, I select the best-matching Panjiva parent using a scoring algorithm combining name similarity, stock ticker agreement, and shipment volume, then manually review low-confidence matches. The resulting sample consists of 448 firms from 41 countries, exporting 4,525 unique HS-6 products across 98 HS-2 sectors from 146 unique shipment origins.

The sample conditions on two forms of prior capacity: a U.S. corporate presence, and at least one lobbying report filed under the LDA. The estimates therefore describe the response among foreign firms already positioned to act politically in the United States. This has several implications. First, because the sample includes only firms with demonstrated lobbying capacity, the estimated per-firm effect is likely larger than the average effect among all foreign firms exposed to U.S. trade barriers, some of which may be less able to respond through lobbying. At the same time, firms outside the sample may still exert indirect influence through trade associations or domestic partners, although whether they do so is outside the scope of this paper. If such indirect responses also increase as adjudication weakens, the aggregate redirection of foreign influence toward domestic politics could be larger than the direct lobbying response captured here. Finally, conditioning on prior lobbying capacity highlights a potential distributive consequence of institutional erosion: firms already equipped to engage

in U.S. politics are better positioned to obtain the firm-specific relief that lobbying, unlike adjudication, can deliver.

## **Firm-Specific AD/CVD Exposure**

My key explanatory variable is a continuous, firm-specific measure of exposure to U.S. AD/CVD actions, constructed as a Bartik-style shift-share variable that exploits within-firm variation in product-country import composition:

$$Exposure_{gt} = \sum_k \sum_c (share_{gkc} \times duty_{kct})$$

where  $g$  indexes GUOs,  $k$  indexes HS six-digit product codes,  $c$  indexes origin countries, and  $t$  indexes quarters. The share component  $share_{gkc}$  is the fraction of GUO  $g$ 's pre-period U.S.-bound shipments accounted for by product  $k$  originating from country  $c$ , computed from bill-of-lading records. The shock component  $duty_{kct}$  is a binary indicator for an active AD/CVD duty on product  $k$  from country  $c$  in quarter  $t$ , computed from U.S. AD/CVD petitions records. The double sum therefore captures how much of the GUO's pre-period U.S. trade portfolio is subject to country-product specific trade remedies in a given quarter.

## **Product-country shares**

Shares are fixed in a pre-period window from 2010 to 2017, after the Global Financial Crisis and before the onset of the 2018 trade war. This ensures that the product-country mix reflects each firm's structural production and sourcing decisions rather than endogenous responses to the tariff shocks. The pre-2018 cutoff also avoids contamination from the Secion 232

and 301 tariffs, which substantially altered U.S. trade patterns beginning in 2018 and were themselves a potential driver of lobbying activity.

Importantly, I construct shares over the joint product-country distribution rather than products alone, so that  $share_{gkc}$  records not only *what* a firm ships to the U.S. but *where* it sources each product from. Because trade remedies apply to specific product-country pairs, this joint distribution is what determines a firm’s true exposure: a firm’s shipments of a given product may be heavily exposed if sourced from one origin and untouched if sourced from another. Resolving sourcing at the country level therefore captures the full geographic footprint of each firm’s import portfolio and recovers exposure that a product-only measure would miss. Figure 2 illustrates this point; the average firm sources around 54% of its pre-period shipments from outside its home country, and 53% of firms source a majority from third countries.

### **AD/CVD duties**

Trade remedy actions are determined at the country-product level. The U.S. process begins when a domestic industry files a petition with the U.S. International Trade Commission (USITC) and the Department of Commerce, requesting duties on foreign goods alleged to be dumped or to benefit from foreign government subsidies. The two agencies then determine, respectively, whether the domestic industry is materially injured and whether dumping or subsidization is in fact occurring. If both make affirmative preliminary determinations, provisional duties are imposed for the first time. After a more comprehensive review, a final determination establishes whether these duties are revoked or remain in place until later rescinded.

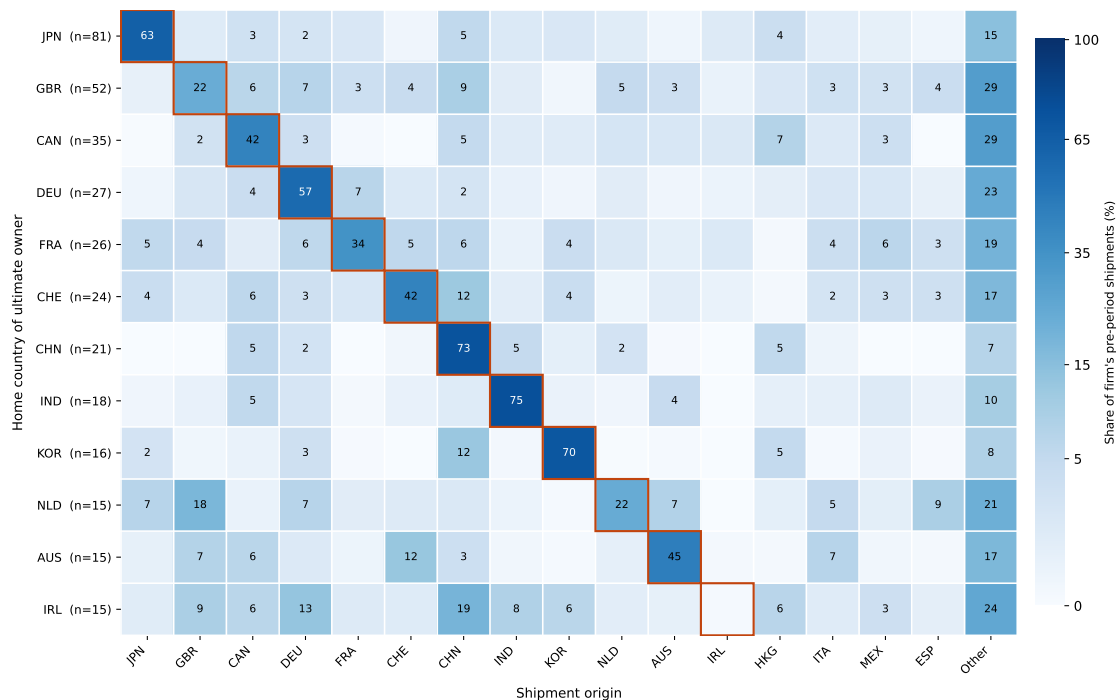


Figure 2: Sourcing of U.S.-bound shipments by home country of the ultimate owner, 2010–2017. Rows are the twelve home countries with the most firms in the sample, with firm counts in parentheses. Columns are the corresponding twelve origins, the four remaining origins accounting for the most shipment activity, and all other origins collected in *Other*. Cells report the average firm’s percentage of pre-period shipments from each origin, so rows sum to 100. Outlined cells are shipments originating in the firm’s own home country; off-diagonal mass is exposure that a measure built on firm nationality would misassign.

Bown et al. (2020) compiles, for each petition, the dates and decisions of preliminary and final determination, as well as the HS8 or 10 products that are affected by the measure. I use the dates of these rulings to identify, for each petition, the interval over which a duty was in force. A petition that received an affirmative preliminary determination but failed to obtain an affirmative final determination, for instance, generates a duty spanning only the period between the preliminary and final rulings. I code  $duty_{kct} = 1$  for each product-country pair and quarter falling within such an interval, and zero otherwise.

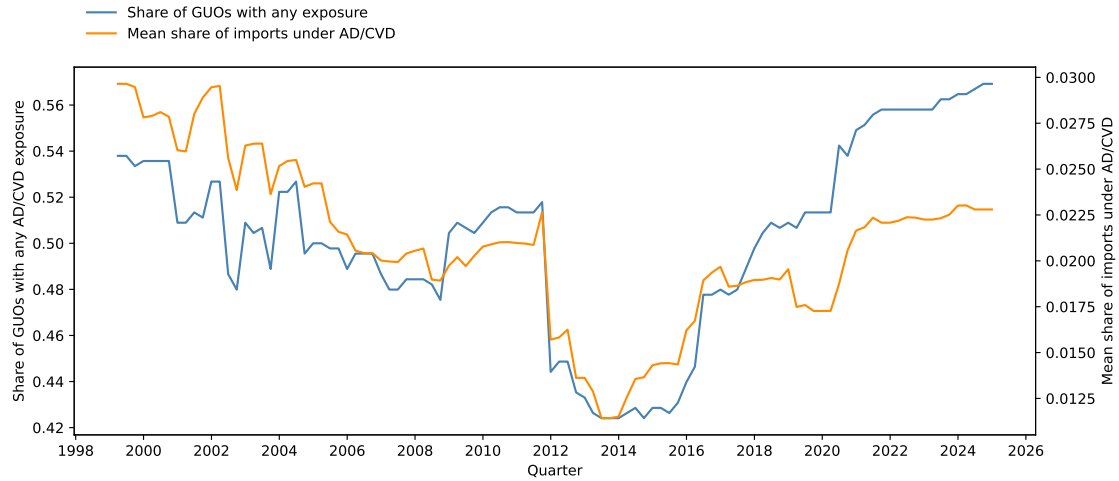


Figure 3: AD/CVD exposure of foreign firms over time. The blue line (left axis) shows the share of GUOs in the sample with any active AD/CVD exposure in each quarter. The orange line (right axis) shows the mean Bartik exposure across GUOs.

Figure 3 shows the evolution of AD/CVD exposure among the 448 foreign firms in the sample. The blue line, plotted on the left axis, reports the share of firms with any active AD/CVD exposure in each quarter. This share fluctuates between 42% and 58% over the panel. The orange line, plotted on the right axis, reports mean Bartik exposure across firms, capturing the average share of firms' import portfolios subject to active AD/CVD orders. Both measures decline noticeably between 2012 and 2016 before returning to earlier levels after 2017. The post-2017 increase is steeper for the share of exposed firms than for mean exposure, suggesting that the increase reflects the entry of newly targeted firms rather than deeper exposure among firms already affected by AD/CVD activity.

## Foreign Lobbying

I examine two measures of lobbying activity, both derived from Kim (2018). The first is the logarithm of total quarterly lobbying expenditure,  $\log(1 + \text{LobbyExpenditure}_{gt})$ , estimated via OLS. The second is a binary indicator  $\text{LobbyBin}_{gt}$  equal to one if the GUO’s subsidiaries filed any lobbying report in quarter  $t$ , estimated via a linear probability model. When a lobbying report spans multiple quarters, the reported expenditure is apportioned equally across the covered quarters.

Table 1: Sample characteristics by exposure status. A firm is classified as ever exposed if it was under an active AD/CVD order in any quarter of the panel.

|                                            | Ever exposed | Never exposed |
|--------------------------------------------|--------------|---------------|
| Firms                                      | 306          | 142           |
| Share of quarters with any lobbying        | 0.45         | 0.41          |
| Reports per quarter, conditional on filing | 3.6          | 2.8           |
| Median expenditure, conditional on filing  | \$60,000     | \$50,000      |

Table 1 describes the sample by exposure status. Firms ever placed under an AD/CVD order lobby at similar rates to those never placed under one, filing in 45 percent of quarters against 41 percent, though they file more reports and spend more when they do. The control group is therefore not politically dormant, and level differences of this kind are absorbed by the firm fixed effects in the estimation below.

LobbyView also records the issue areas each report covers and the government entities named on it, which allows a closer look at what these firms lobby about and whom they approach. Trade is central to the exposed group’s agenda but not exclusive, accounting for nine percent of their issue mentions against five percent among never-exposed firms and ranking second in both groups behind taxation (Figure 4). As for lobbying targets, nearly

all firms in both groups lobby Congress, while the executive bodies they add distinguish them (Figure 5). Exposed firms are substantially more likely to lobby Commerce, USTR, and Customs and Border Protection, the bodies that determine dumping margins, set trade policy, and collect the resulting duties. Firms that my measure identifies as exposed thus disproportionately engage the agencies that administer the orders it codes, which provides reassurance for a measure built from shipment records and petition dates rather than from any observed firm behavior.

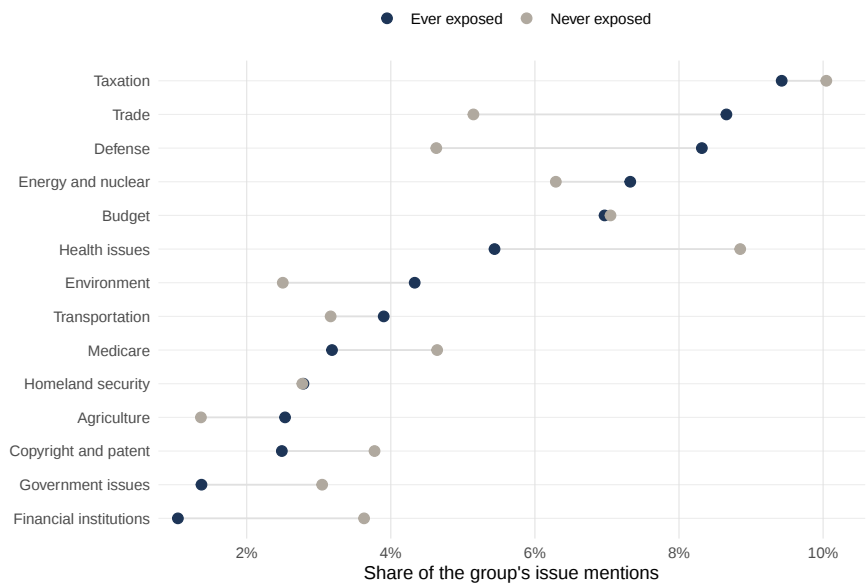


Figure 4: Composition of exposed and unexposed group’s lobbying agenda, share of issue mentions across all reports.

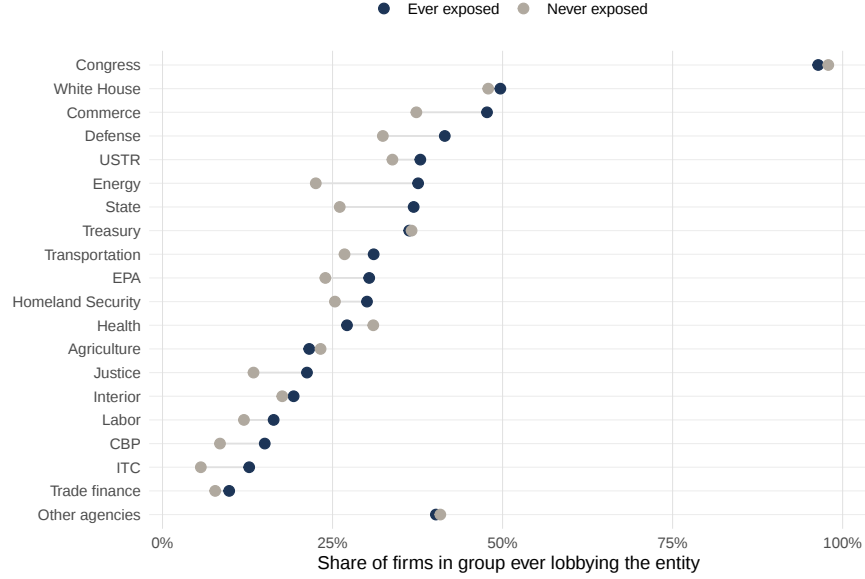


Figure 5: Share of firms in each group that ever named a given part of government on a lobbying report, across all issue areas. Sub-agencies are rolled up to their parent department; *Other* collects roughly ninety independent agencies and commissions.

## Identification Strategy

My empirical strategy is a panel event study that exploits within-firm variation in AD/CVD exposure over time. Because the exposure measure combines each firm's pre-determined product-country import composition with the subsequent timing of duty actions, it generates variation in how heavily a given firm is affected by trade remedies from quarter to quarter.

I estimate

$$\log(1 + LobbyExpenditure_{gt}) = \alpha_g + \delta_t + \sum_{\tau \neq 2016Q4} \beta_{\tau} \cdot Exposure_{gt-1} + \gamma \mathbf{X}_{gt-1} + \varepsilon_{gt}$$

where  $\alpha_g$  are GUO fixed effects,  $\delta_t$  are quarter fixed effects, and the  $\beta_{\tau}$  trace the relationship between exposure and lobbying in each quarter relative to the omitted reference period. The

GUO fixed effects absorb all time-invariant differences across firms—size, sector, home country, and baseline propensity to lobby—so that the estimates are not driven by the fact that some firms simply lobby more than others. The quarter fixed effects absorb shocks common to all firms in a given period, such as the electoral and legislative calendar, macroeconomic conditions, and aggregate shifts in trade politics. Robust standard errors are clustered by GUO and year.

I control directly for each firm’s exposure to the Section 301 and Section 232 tariffs so that the estimated coefficients reflect the response to AD/CVD trade remedies rather than the contemporaneous trade-war measures that affected many of the same firms beginning in 2018. Firm-level exposure to these tariffs are constructed in the same way as the main AD/CVD exposure variable, utilizing the firm’s product-country mix and the specific product-country pairs the tariffs target at a given point in time. This also demonstrates the broader applicability of my method in measuring firm-level exposure to trade policies that target specific product-country pairs.

The identifying assumption is a parallel-trends assumption: absent differences in their AD/CVD exposure, more- and less-exposed firms would have followed common lobbying trajectories, conditional on the firm and quarter fixed effects and the trade-war controls. Two features of the research design lend credibility to this assumption. First, the import shares are fixed in a pre-period window (2010–2017) that ends before both the trade war and the WTO crisis. Because these shares are pre-determined, a firm’s exposure cannot reflect strategic adjustment of its sourcing in anticipation of trade-remedy actions, and the measure is not contaminated by post-treatment changes in firm behavior. Second, AD/CVD actions are initiated by U.S. domestic industries and adjudicated by the USITC and the Department

of Commerce on statutory injury and dumping criteria. The foreign firms in my sample are the targets of these actions, not their initiators, so the timing of the duties that drive a firm’s exposure is plausibly exogenous to that firm’s own lobbying decisions. Appendix F further probes the exogeneity of both the predetermined import shares and duty shocks through alternative constructions and tests of potential endogeneity channels.

Although this assumption is not directly testable, the pre-period coefficients ( $\tau$  before the reference quarter) provide evidence on whether the data are consistent with it. If exposed and unexposed firms were already diverging in their lobbying behavior before exposure became consequential, such divergence would likely be reflected in systematic deviations from zero in the pre-period estimates. Flat and statistically insignificant pre-period coefficients, on the other hand, would support the plausibility of the parallel-trends assumption and alleviate the concern that the post-period divergence reflects a continuation of pre-existing differential trends. Appendix E supplements these diagnostics with formal sensitivity analyses for violations of parallel trends and specifications absorbing sector-specific quarterly shocks.

## Results

### Baseline Effects

Figures 6 and 7 present the baseline event-study estimates for the full sample, using as outcomes an indicator for any lobbying activity and the log of lobbying expenditure, respectively. Two features stand out. First, the pre-period coefficients are uniformly small and statistically indistinguishable from zero throughout 2008–2016. Exposed and unexposed firms exhibit no detectable divergence in either the incidence or level of lobbying in the years

### Foreign-firm lobbying response to AD/CVD exposure: Participation

Coefficients relative to 2016 Q4; 95% CI. Dashed lines mark WTO Appellate Body membership step-downs.

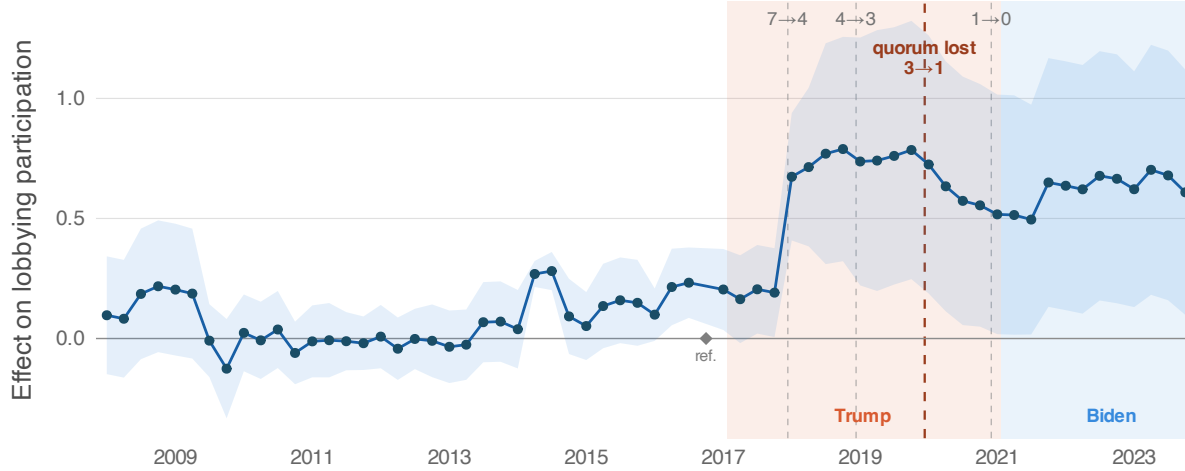


Figure 6: Quarterly event study estimates of the effect of AD/CVD exposure on the probability of lobbying participation. Coefficients are estimated relative to 2016 Q4. All specifications include firm and quarter fixed effects and control for firm-level Section 232 and Section 301 tariff exposure. Standard errors are two-way clustered by firm and quarter. Shaded bands distinguish the Trump (2017–2021) and Biden (2021–) administrations; dashed vertical lines mark successive contractions of the WTO Appellate Body as terms expired without replacement: 7 to 4 members (December 2017), 4 to 3 (December 2018), loss of quorum at 3 to 1 (December 2019), and the last member’s departure (November 2020).

before the trade-policy environment shifts.

Second, a sharp increase in lobbying among exposed firms emerges in the first quarter of 2018 and is sustained through the end of the sample in 2023. In the participation specification (Figure 6), the exposed–unexposed gap is small and statistically indistinguishable from zero through 2017 (roughly 2 percentage points at 2017Q4), then jumps sharply to approximately 8.5 percentage points at 2018Q1. The effect neither reverts nor decays after the initial shock: the participation gap remains in the range of 6 to 8 percentage points through 2023. The expenditure specification (Figure 7) traces the same dynamic on the intensive margin. The

### Foreign-firm lobbying response to AD/CVD exposure: Expenditure

Coefficients relative to 2016 Q4; 95% CI. Dashed lines mark WTO Appellate Body membership step-downs.

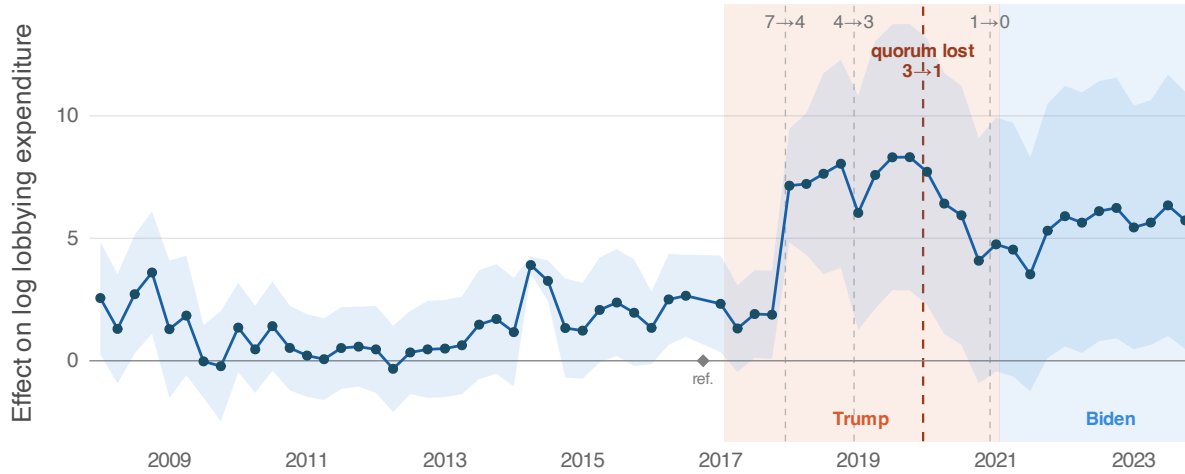


Figure 7: Quarterly event study estimates of the effect of AD/CVD exposure on lobbying expenditure. The same specification and plotting conventions apply.

exposed–unexposed gap, statistically flat through 2017, jumps at 2018Q1 and remains large and significant across the following years, with estimates similarly stable through 2023.

The timing and persistence of the break are both informative. The response does not coincide with the Trump inauguration in early 2017, but appears at 2018Q1 and is sustained thereafter. Critically, the elevated lobbying does not subside with the change in administration: the effect persists undiminished into the Biden presidency, well after the tariff escalation of 2017–2019 had given way to a different trade-policy posture. This continuity is difficult to reconcile with an account in which firms are responding to the particular tariff agenda of the Trump administration, which would predict attenuation once that agenda lapsed. It is more consistent with the argument advanced here: firms lobby more because the institutional channel for contesting trade barriers became permanently less reliable, and

the Appellate Body’s incapacitation, unlike any single administration’s trade policy, did not reverse. The persistence of the effect across administrations thus reflects the durability of the institutional breakdown rather than the persistence of any particular tariff regime.

The response is not evenly distributed across the sample. Figures 8–9 splits firms at the median of mean revenue over 2015–2017, measured before treatment. The post-2018 increase is concentrated among larger firms, with little response below the median. Even within a sample of multinational firms with a U.S. presence and prior lobbying activity, the capacity to respond through domestic political channels appears to scale with firm resources. This suggests that access to domestic political channels is unevenly distributed. Because gains secured through lobbying are typically more firm-specific than those produced by a WTO dispute ruling, the pattern also points to a distributive consequence of the institutional shift.

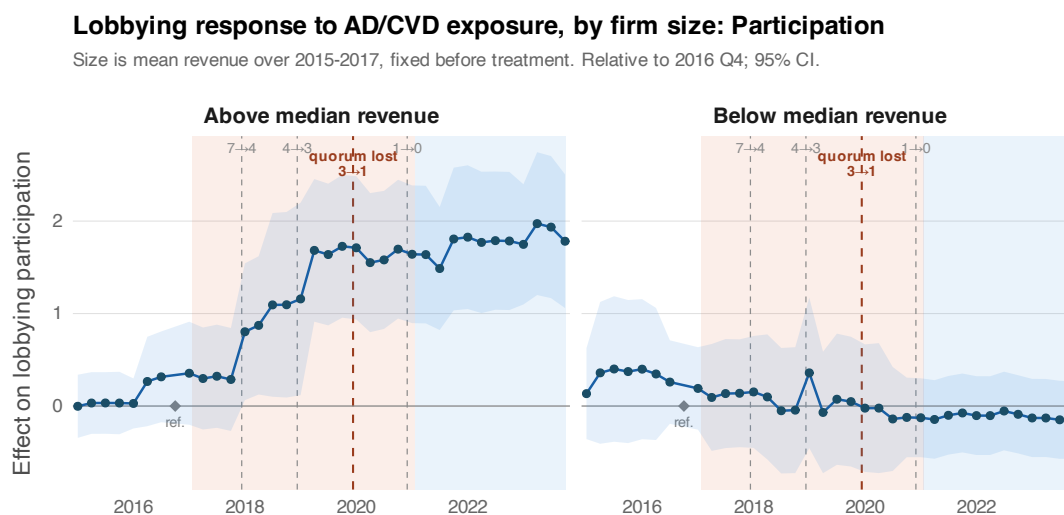


Figure 8: Lobbying participation by firm size. Firms are split at the median of mean revenue (\$5.9 billion) over 2015–2017. Revenue is available for 396 of the 448 firms; Relative to 2016Q4; 95% confidence intervals.

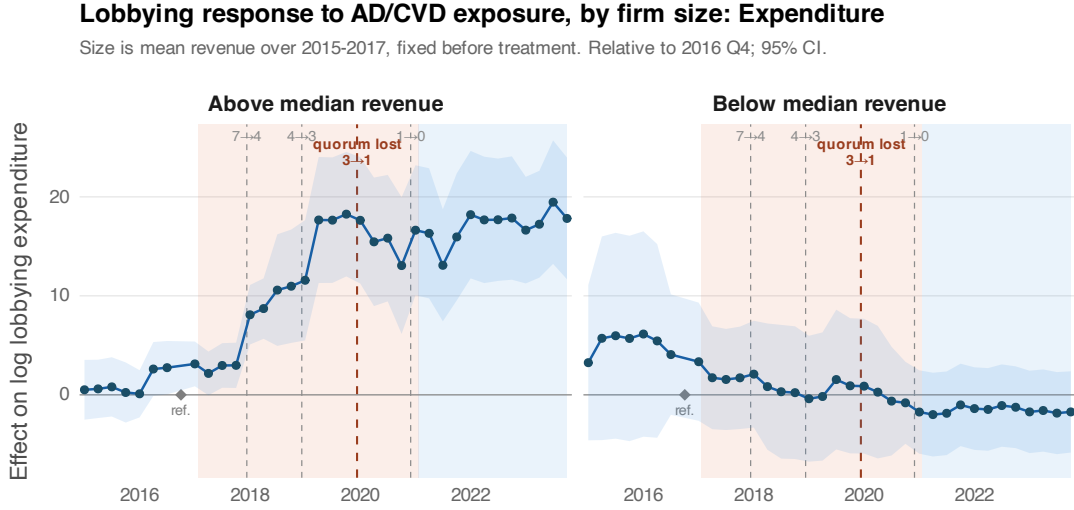


Figure 9: Lobbying expenditure by firm size. As in Figure 8, with  $\log(1 + \text{expenditure})$  as the outcome. Relative to 2016Q4; 95% confidence intervals.

## Robustness Checks

I run a series of additional checks for robustness. Appendix B shows the results do not depend on functional form, holding under a fixed-effects logit and Poisson pseudo-maximum likelihood, across alternative clustering schemes, and that the response is concentrated in trade lobbying. Because exposure is a continuous, non-absorbing dose, two-way fixed effects identifies a weighted average of dose-response effects whose weights can be non-convex under heterogeneity (Callaway, Goodman-Bacon, and Sant’Anna, 2024); Appendix C fixes each firm’s dose in the pre-period and estimates effects by dose group. The response is absent among the lowest-dose firms and positive among the rest, with no group responding negatively, establishing that the continuous specification aggregates effects of a common sign.

Appendix D addresses treatment timing and anticipation. The results are similar when exposure is lagged by one or two quarters and when the omitted quarter is moved to 2017Q4,

while a placebo break in the pre-period produces no comparable response. Separating exposure during the investigation window from exposure to duties in force likewise shows no response to petition initiation but leaves the duty effect unchanged. Appendix E finds no evidence of differential pre-trends, and sensitivity analysis following Rambachan and Roth (2023) shows that the 2018Q1 effect remains significant even when post-treatment violations are allowed to be as large as the largest pre-treatment deviation. The estimates are also unchanged with sector-by-quarter fixed effects that account for sector-level shocks.

Appendix F probes both components of the shift-share exposure measure. Reconstructing exposure using only petitions initiated before 2018, testing whether firms' prior lobbying predicts subsequent petitions, and controlling for leave-one-out measures of home-country protectionism all leave the estimates stable. The results are likewise robust to alternative windows for fixing import shares and to excluding the products that contribute most to aggregate exposure.

Appendix G removes potentially contaminated control firms in industries containing exposed firms, while Appendix H excludes the firms with the highest lobbying or exposure. Neither exercise materially changes the results. Appendix I decomposes the exposure by Chinese and non-Chinese origins and finds the response concentrated among non-Chinese origins, weighing against the trade war as an alternative explanation.

## **Heterogeneity by WTO Relevance**

### **Country-Level Dispute Activity**

To assess whether the pattern of treatment effects is consistent with the argument, I examine heterogeneity by home-country WTO dispute activity. The logic is as follows: the firms that

stand to lose the most from the WTO's incapacitation are those whose home governments are active and capable users of the dispute settlement mechanism, and it is precisely these firms that should turn most sharply to domestic lobbying as the multilateral venue disappears. Firms whose governments rarely litigate at the WTO are less affected by its decline, and should display a weaker response. To test this, I divide the sample at the median of home-country WTO dispute activity and re-estimate the event study separately for firms from high- and low-WTO-user countries<sup>3</sup>.

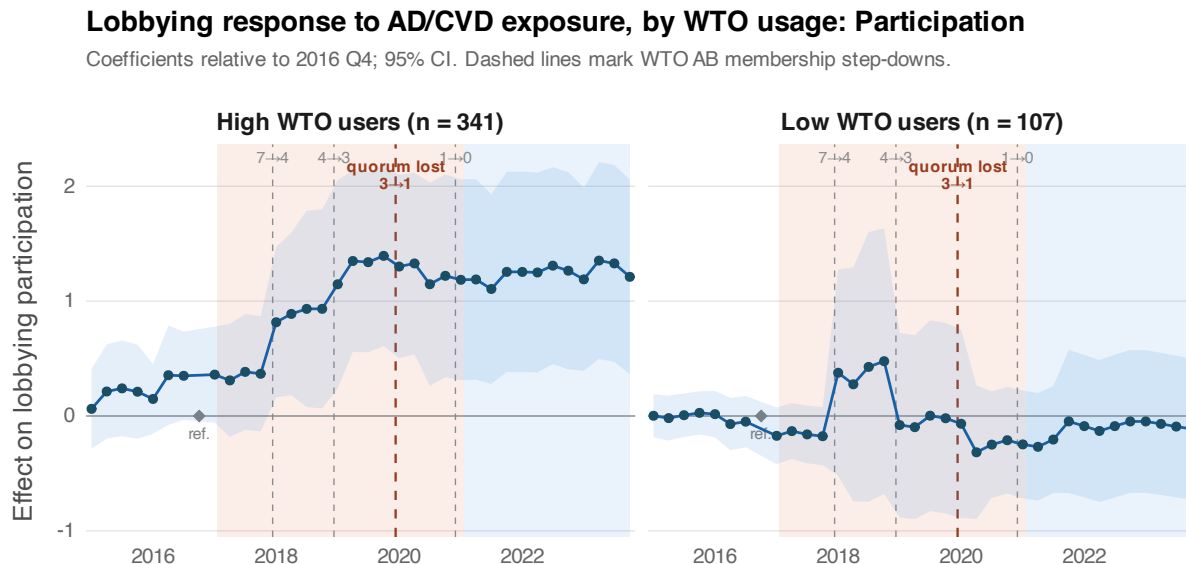


Figure 10: Quarterly event study estimates of the effect of AD/CVD exposure on the probability of any lobbying activity, by WTO dispute activity. Firms are classified as high WTO users if their home country filed WTO disputes more than the sample median number of times during the pre-period. The same specification and plotting conventions apply.

<sup>3</sup>A possible concern is that firms from high-WTO-use countries are simply larger, so that the split captures firm size rather than the prior relevance of WTO adjudication. The two are correlated but not collinear: 56 percent of firms from high-WTO-use countries are above median revenue, against 31 percent of firms from low-WTO-use countries. Nearly half of the firms from high-WTO-use countries are below median size, so the split is not a proxy for size.

### Lobbying response to AD/CVD exposure, by WTO usage: Expenditure

Coefficients relative to 2016 Q4; 95% CI. Dashed lines mark WTO AB membership step-downs.

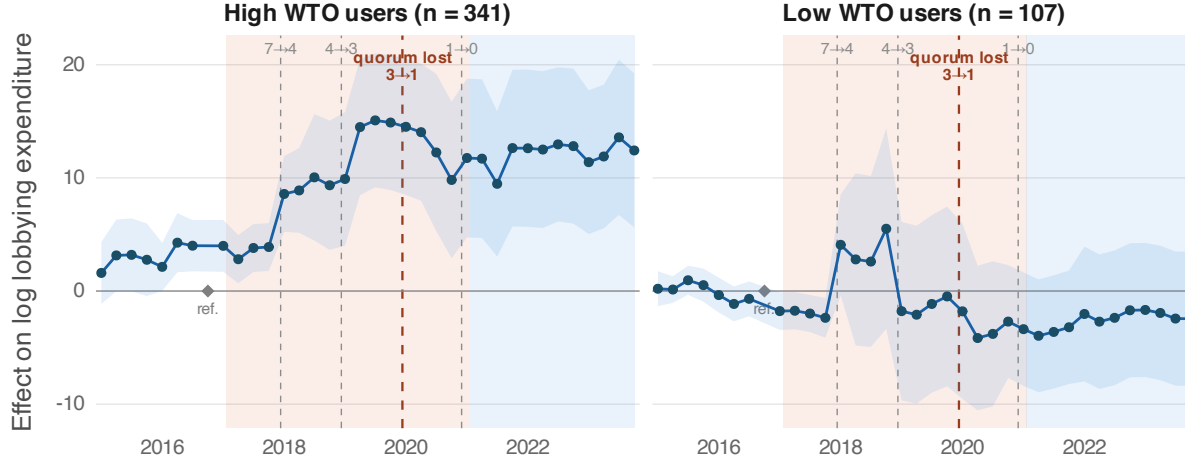


Figure 11: Quarterly event study estimates of the effect of AD/CVD exposure on lobbying expenditure, by WTO dispute activity. Firms are classified as high WTO users if their home country filed WTO disputes more than the sample median number of times during the pre-period. The same specification and plotting conventions apply.

Figures 10 and 11 report the split-sample estimates for expenditure and participation. The contrast between the two groups is pronounced. The post-2018 increase in lobbying is concentrated almost entirely among firms from high-WTO-use countries. For these firms, the exposed–unexposed gap is statistically indistinguishable from zero throughout the pre-period and then rises sharply from 2018 onward, reaching its largest and most precisely estimated values in 2019, as the Appellate Body crossed the threshold of paralysis. In the participation specification (Figure 10), a firm from a high-WTO-user country at one standard deviation of exposure is approximately 10 to 17 percentage points more likely to lobby in a given quarter than an otherwise-identical unexposed firm during the period spanning the Appellate Body’s collapse.

Low-WTO-user firms, by contrast, show no comparable response. Their post-period expenditure coefficients, though occasionally large in point estimate, are imprecisely estimated and statistically indistinguishable from zero throughout, and their participation coefficients remain near zero across the entire sample. The increase in trade-remedy exposure these firms faced did not translate into a detectable shift toward domestic lobbying.

### **Product-level Litigation History**

The country-level split establishes that the response is concentrated among firms whose governments are active users of the dispute settlement system. A sharper implication of the argument is that what should matter is not merely whether a firm's home government litigates at the WTO in general, but whether it has litigated on the specific products the firm trades. A firm gains little from its government's WTO activity in unrelated sectors; the relevant protection is a demonstrated willingness and ability to challenge U.S. trade remedies on the firm's own products.

To capture this, I construct a measure of *litigated-product exposure*: the share of a firm's pre-period imports in products its home government has a history of challenging at the WTO. I first identify all WTO disputes in which a foreign country has challenged a U.S. trade remedy, that is, disputes naming the United States as respondent and invoking the Antidumping or Subsidies and Countervailing Measures agreements<sup>4</sup>. For each such dispute, I trace the challenged measure to the underlying U.S. antidumping or countervailing duty order and, through it, to the specific products the order covers, identified at the HS six-digit level. This yields, for each complainant country, the set of products on which that country

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<sup>4</sup>I include only disputes challenging specific U.S. antidumping or countervailing duties, excluding those that challenge U.S. subsidy programs, administrative practices, or laws.

## Lobbying response by litigated-product exposure

Each line: exposure effect for firms above a percentile threshold of litigated-product exposure; relative to 2016 Q4

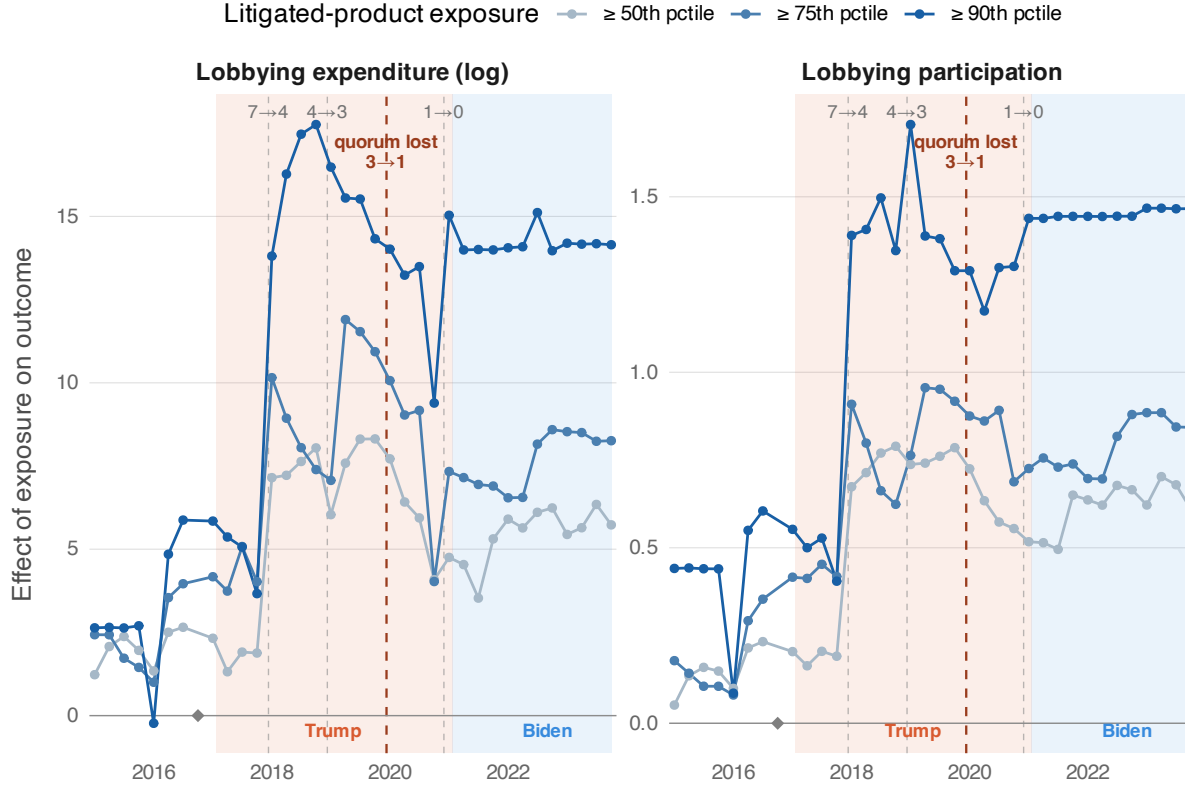


Figure 12: Quarterly event-study estimates of the effect of AD/CVD exposure on lobbying, by litigated-product exposure. Litigated-product exposure is the share of a firm's pre-period imports in products its home government challenged at the WTO. Each line re-estimates the event study on firms above a given percentile of this measure ( $\geq 50$ th, 75th, and 90th). The left panel uses log lobbying expenditure; the right panel uses an indicator for any lobbying participation. Coefficients are relative to 2016 Q4. Specification and plotting conventions follow those described above.

has contested U.S. trade remedies at the WTO.

I then map this record onto the firms in my sample. For each firm, I compute the share of its pre-period imports accounted for by products that its own home government has challenged at the WTO, matching on the firm's home country and its imported products. A

firm scores high on this measure if a large fraction of its import portfolio consists of products its government has a demonstrated record of contesting; it scores zero if its government has never challenged a U.S. trade remedy on any product the firm imports.

With this data, I re-estimate the event study on firms above successively higher thresholds of this measure, so that each threshold isolates firms more heavily concentrated in litigated products. Figure 12 reports these estimates. The response scales with litigated-product exposure. At the 50th percentile, the post-2018 increase is present but modest; as the threshold rises to the 75th and 90th percentiles, the effect grows substantially larger and more sharply defined, in both the expenditure and participation specifications. Firms in the top decile of litigated-product exposure drive the strongest response.

## Conclusion

The GATT/WTO was the defining institution of the postwar trade order. What happens to trade politics in the absence of this central venue? This paper shows that the weakening of multilateral adjudication draws affected firms more deeply into domestic politics. Using a novel firm-level measure of exposure to U.S. AD/CVD duties constructed from bill-of-lading records, I find that exposure to trade remedies produces substantially more lobbying after the onset of the Appellate Body crisis than before. The response is strongest among firms from countries that historically made greater use of WTO dispute settlement and among firms trading products previously contested at the WTO—those for whom multilateral adjudication had been most relevant. These patterns support the argument that as external legal constraints weaken, the returns to domestic political influence rise.

My findings provide empirical evidence on the downstream political consequences of declining multilateralism, a development often lamented but rarely traced to concrete changes in actor behavior. The erosion of multilateral adjudication does not extinguish foreign influence over trade policy; it relocates that influence from a salient, rule-bound, and publicly observable forum to the more discretionary and less visible arena of domestic lobbying. This relocation complicates sovereignty-based critiques of international institutions. Contrary to the view that institutions primarily enable external constraints on domestic policymaking, multilateral adjudication may also serve to channel foreign influence into institutionalized, legal, and more transparent forms.

The paper advances a growing literature on firms as actors in international economic governance. Existing work has shown that firms are the engine behind WTO litigation, supplying the information, expertise, and pressure that move governments to challenge trade barriers on their behalf (Davis, 2012; Brutger, 2024). Where this work shows how firms engage a functioning system, I examine how they adapt as it erodes, increasing engagement in domestic politics. Studying firms as multilateral enforcement weakens thus offers a distinct window into how private actors navigate a changing institutional order.

In doing so, the paper bridges two literatures that have developed in isolation. Scholars of WTO dispute escalation study how firms pursue trade grievances through international legal institutions and when governments choose to escalate them (Davis, 2012; Brutger, 2024); scholars of business-government relations study how firms seek political influence in domestic arenas (Kim, 2017; Kim and Milner, 2019; Lee, 2024*b*; You, 2023). Where each illuminates one venue, I examine how the effectiveness of the international venue shapes firms' incentives to engage in domestic politics. This builds on work on forum shopping,

which recognizes that actors choose strategically among venues, but has largely studied selection across legal forums, between courts and across levels of a stable institutional order (Alter and Meunier, 2009; Busch, 2007; Davis, 2009; Eckhardt and De Bievre, 2015). I extend the logic across a different divide—the legal and the political—and show that firms’ choices across that divide is governed by the strength of the international institutions structuring their choices.

Finally, the paper offers a novel approach to study firm-level exposure to trade policy shocks with greater precision. By combining bill-of-lading records, corporate ownership data, and the timing of trade remedy actions, the analysis measures exposure *at the level at which firms actually encounter trade barriers*: through their product portfolios, sourcing patterns, and ownership structures. The same framework extends readily to other policies and other dimensions of firm exposure, as the paper demonstrates in applying it both to Section 232 and 301 tariffs and to firms’ historical exposure to WTO litigation over the products they trade.

The argument also has distributive implications that the present analysis only begins to probe. Because lobbying-based relief is exclusive in a way that most-favored-nation dispute rulings are not, and because larger firms tend to lobby more effectively, the shift from multilateral to domestic channels may widen the gap between firms that can secure firm-specific relief and those that cannot. Characterizing which firms capture the returns to lobbying as institutional constraints weaken, and whether the concessions they obtain come at the expense of competitors, is a natural next step.

The broader implication extends beyond the WTO. Wherever delegated adjudication has structured expectations about a contested measure and constrained how it is administered,

its erosion can make outcomes less dependent on external legal review and increase the importance of the officials who apply the measure. Investment arbitration provides a close parallel: denunciations of the ICSID Convention and the termination of intra-EU bilateral investment treaties have narrowed access to external adjudication for affected investors. Regional human rights regimes facing weakened judicial authority present a similar structure, as do other enforcement regimes in which the capacity to compel compliance declines without a corresponding change in formal obligations. In each case, weakening external review may increase the returns to political influence outside the international institution, particularly for actors with the resources and access to exercise it. The scope condition is the one this paper's design exploits: external review must previously have meaningfully structured the policy environment, and affected actors must have access to domestic political channels through which they can seek influence.

Several further questions follow from this work. The analysis identifies a shift toward lobbying but does not establish what that lobbying obtains; linking firm-level political activity to concrete trade-policy outcomes, such as tariff exclusions, favorable administrative determinations, or the duration of duties, would clarify whether the strategy succeeds and would speak to the welfare consequences of the institutional shift. Future research could also examine other alternative venues through which firms seek relief when multilateral adjudication weakens. Domestic lobbying is one such venue, but foreign firms may also turn to domestic courts, administrative procedures, investment arbitration, or bilateral diplomatic channels. Studying how firms choose among these alternatives would deepen our understanding of how trade politics is reorganized after the breakdown of multilateral dispute settlement.

As the rule-based trade order frays, the central analytical task for political scientists shifts

from explaining when and how states litigate their disputes within international institutions to explaining how firms, governments, and other actors pursue their interests once those institutions can no longer structure outcomes. This paper takes a first step by identifying one such adaptation and the conditions that produce it, but the larger transformation it points to—the migration of trade politics from multilateral fora into domestic and discretionary arenas—is only beginning to be understood. Mapping where that influence goes, who captures it, and what it means for the openness and accountability of the trading system will be among the defining questions for the political economy of trade in the coming decade.

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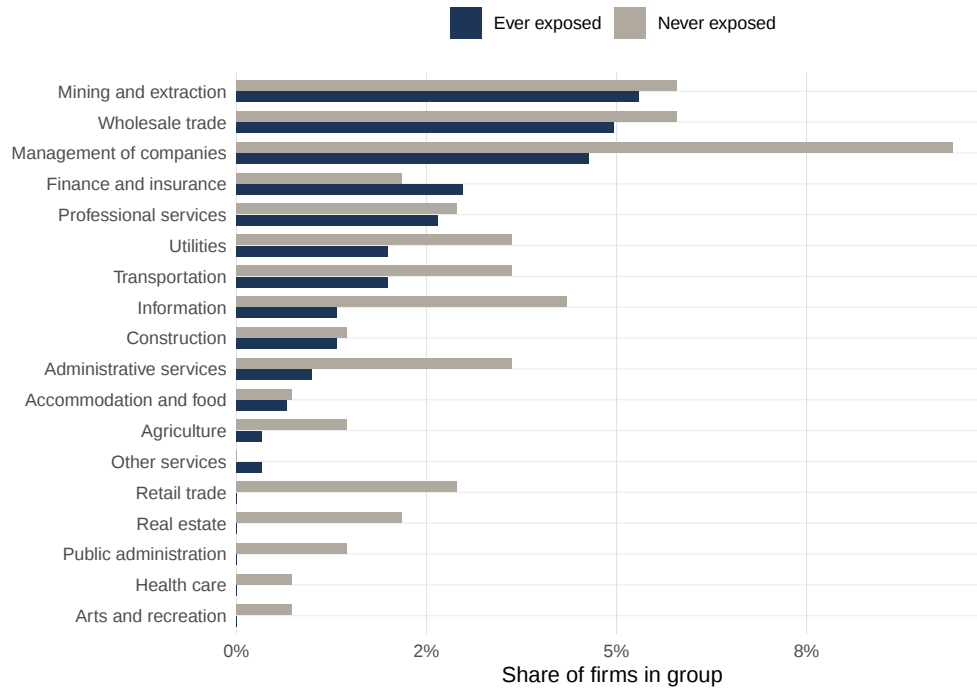
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# APPENDICES

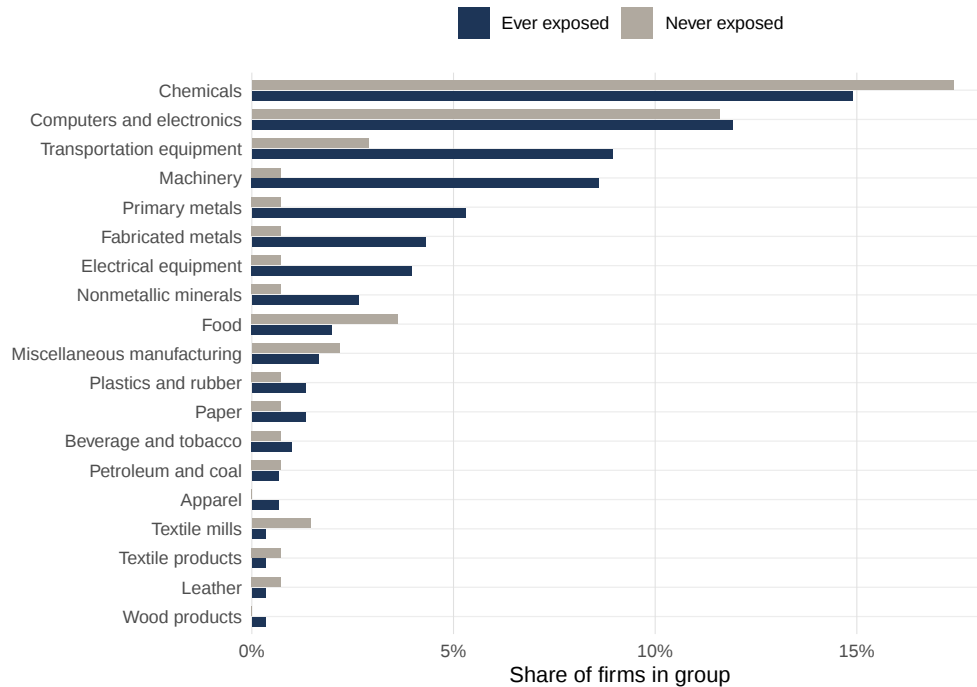
## A Data Construction

Table A1: Summary statistics. Firm-quarter observations. Exposure is the share of a firm's fixed pre-period import value in product-origin cells under an active AD/CVD order. The measure is right-skewed, so a small number of heavily exposed firms account for a large share of the identifying variation; Appendix H reports leave-out estimates.

|                                | Mean    | SD       | p90     | Max         | N     |
|--------------------------------|---------|----------|---------|-------------|-------|
| Lobbying participation         | 0.440   | 0.496    | 1.000   | 1.000       | 44800 |
| Lobbying expenditure (\$1,000) | 116.366 | 1694.743 | 251.164 | 350 095.000 | 44800 |
| AD/CVD exposure                | 0.021   | 0.089    | 0.028   | 0.938       | 44800 |
| Section 301 exposure           | 0.016   | 0.099    | 0.000   | 1.000       | 44800 |
| Section 232 exposure           | 0.007   | 0.064    | 0.000   | 1.000       | 44800 |



(a) Non-manufacturing sectors (NAICS-2)



(b) Within manufacturing (NAICS-3)

Figure A1: Industry composition, by whether the firm was ever placed under an active AD/CVD order. Both panels report shares of all firms in the group, so the subsector shares in panel (b) sum to each group's manufacturing share. Manufacturing accounts for 71 percent of ever-exposed and 47 percent of never-exposed firms and is omitted from panel (a).

## B Alternative Specifications

Unless stated otherwise, every specification includes firm and quarter fixed effects and controls for Section 301 and Section 232 exposure, with standard errors two-way clustered by firm and quarter. Event-study coefficients are relative to 2016Q4. Robustness checks in this and following sections are reported as event studies where the concern bears on the timing of the response or the behavior of the pre-period, and as pooled post-period coefficients where the concern is whether the estimated magnitude is sensitive to a change in sample or measurement.

Table B1: Pooled post-period effect of AD/CVD exposure. Coefficients are relative to the average of all pre-treatment quarters.

| Dependent Variables:               | Lobbying participation | Log lobbying expenditure |
|------------------------------------|------------------------|--------------------------|
| Model:                             | Participation          | Expenditure              |
|                                    | (1)                    | (2)                      |
| <i>Variables</i>                   |                        |                          |
| AD/CVD exposure                    | 0.099<br>(0.084)       | 1.62*<br>(0.921)         |
| Section 301 exposure               | -0.047<br>(0.076)      | -0.423<br>(0.804)        |
| Section 232 exposure               | -0.031<br>(0.162)      | 0.121<br>(1.65)          |
| AD/CVD exposure $\times$ Post-2018 | 0.559*<br>(0.324)      | 4.59<br>(3.38)           |
| <i>Fixed-effects</i>               |                        |                          |
| Firm                               | Yes                    | Yes                      |
| Quarter                            | Yes                    | Yes                      |
| <i>Fit statistics</i>              |                        |                          |
| Observations                       | 44,352                 | 44,352                   |
| R <sup>2</sup>                     | 0.524                  | 0.611                    |

*Clustered (Firm & Quarter) standard-errors in parentheses*

*Signif. Codes: \*\*\*: 0.01, \*\*: 0.05, \*: 0.1*

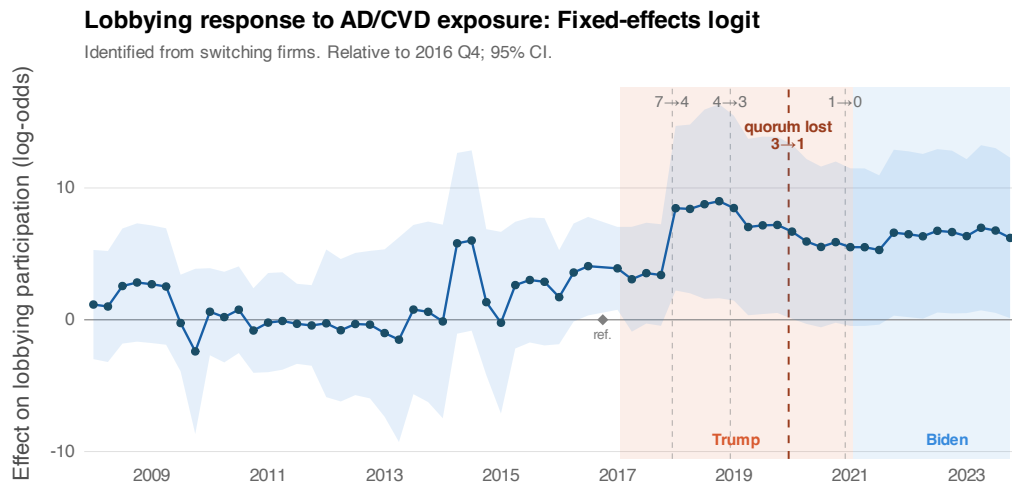


Figure B1: Lobbying participation and AD/CVD exposure, fixed-effects logit. Coefficients are log-odds, identified from firms that switch participation status. Subject to the incidental parameters problem, so the specification is reported for sign and significance rather than magnitude. Relative to 2016Q4; 95% confidence intervals.

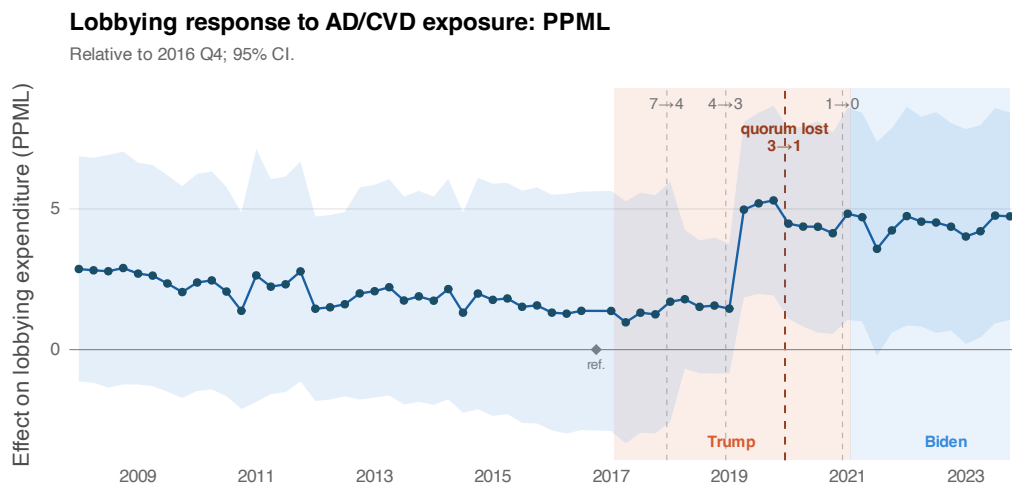


Figure B2: Lobbying expenditure and AD/CVD exposure, Poisson pseudo-maximum likelihood. PPML estimated on expenditure in levels. Relative to 2016Q4; 95% confidence intervals.

Table B2: Alternative clustering schemes. Pooled post-period coefficient under firm, firm-and-quarter, and firm-and-year clustering.

| Dependent Variables:               | Lobbying participation |                   |                   | Log lobbying expenditure |                   |                   |
|------------------------------------|------------------------|-------------------|-------------------|--------------------------|-------------------|-------------------|
| Model:                             | Part.: firm            | Part.: firm+qtr   | Part.: firm+year  | Exp.: firm               | Exp.: firm+qtr    | Exp.: firm+year   |
|                                    | (1)                    | (2)               | (3)               | (4)                      | (5)               | (6)               |
| <i>Variables</i>                   |                        |                   |                   |                          |                   |                   |
| AD/CVD exposure                    | 0.099<br>(0.085)       | 0.099<br>(0.084)  | 0.099<br>(0.082)  | 1.62*<br>(0.931)         | 1.62*<br>(0.921)  | 1.62*<br>(0.902)  |
| Section 301 exposure               | -0.047<br>(0.076)      | -0.047<br>(0.076) | -0.047<br>(0.074) | -0.423<br>(0.811)        | -0.423<br>(0.804) | -0.423<br>(0.784) |
| Section 232 exposure               | -0.031<br>(0.165)      | -0.031<br>(0.162) | -0.031<br>(0.155) | 0.121<br>(1.69)          | 0.121<br>(1.65)   | 0.121<br>(1.59)   |
| AD/CVD exposure $\times$ Post-2018 | 0.559*<br>(0.329)      | 0.559*<br>(0.324) | 0.559*<br>(0.309) | 4.59<br>(3.43)           | 4.59<br>(3.38)    | 4.59<br>(3.23)    |
| <i>Fixed-effects</i>               |                        |                   |                   |                          |                   |                   |
| Firm                               | Yes                    | Yes               | Yes               | Yes                      | Yes               | Yes               |
| Quarter                            | Yes                    | Yes               | Yes               | Yes                      | Yes               | Yes               |
| <i>Fit statistics</i>              |                        |                   |                   |                          |                   |                   |
| Observations                       | 44,352                 | 44,352            | 44,352            | 44,352                   | 44,352            | 44,352            |
| R <sup>2</sup>                     | 0.524                  | 0.524             | 0.524             | 0.611                    | 0.611             | 0.611             |

*Signif. Codes: \*\*\*: 0.01, \*\*: 0.05, \*: 0.1*

Table B3: With and without trade-war controls. Section 301 and 232 exposure controls added and removed, both outcomes.

| Dependent Variables:               | Lobbying participation |                      | Log lobbying expenditure |                     |
|------------------------------------|------------------------|----------------------|--------------------------|---------------------|
|                                    | Part.: no controls     | Part.: with controls | Exp.: no controls        | Exp.: with controls |
| Model:                             | (1)                    | (2)                  | (3)                      | (4)                 |
| <i>Variables</i>                   |                        |                      |                          |                     |
| AD/CVD exposure                    | 0.104<br>(0.082)       | 0.099<br>(0.084)     | 1.57*<br>(0.916)         | 1.62*<br>(0.921)    |
| AD/CVD exposure $\times$ Post-2018 | 0.516*<br>(0.263)      | 0.559*<br>(0.324)    | 4.63<br>(2.80)           | 4.59<br>(3.38)      |
| Section 301 exposure               |                        | -0.047<br>(0.076)    |                          | -0.423<br>(0.804)   |
| Section 232 exposure               |                        | -0.031<br>(0.162)    |                          | 0.121<br>(1.65)     |
| <i>Fixed-effects</i>               |                        |                      |                          |                     |
| Firm                               | Yes                    | Yes                  | Yes                      | Yes                 |
| Quarter                            | Yes                    | Yes                  | Yes                      | Yes                 |
| <i>Fit statistics</i>              |                        |                      |                          |                     |
| Observations                       | 44,352                 | 44,352               | 44,352                   | 44,352              |
| R <sup>2</sup>                     | 0.524                  | 0.524                | 0.611                    | 0.611               |

*Clustered (Firm & Quarter) standard-errors in parentheses*

*Signif. Codes: \*\*\*: 0.01, \*\*: 0.05, \*: 0.1*

# Trade vs. non-trade lobbying

Effect of AD/CVD exposure on lobbying decomposed by issue; relative to 2016 Q4

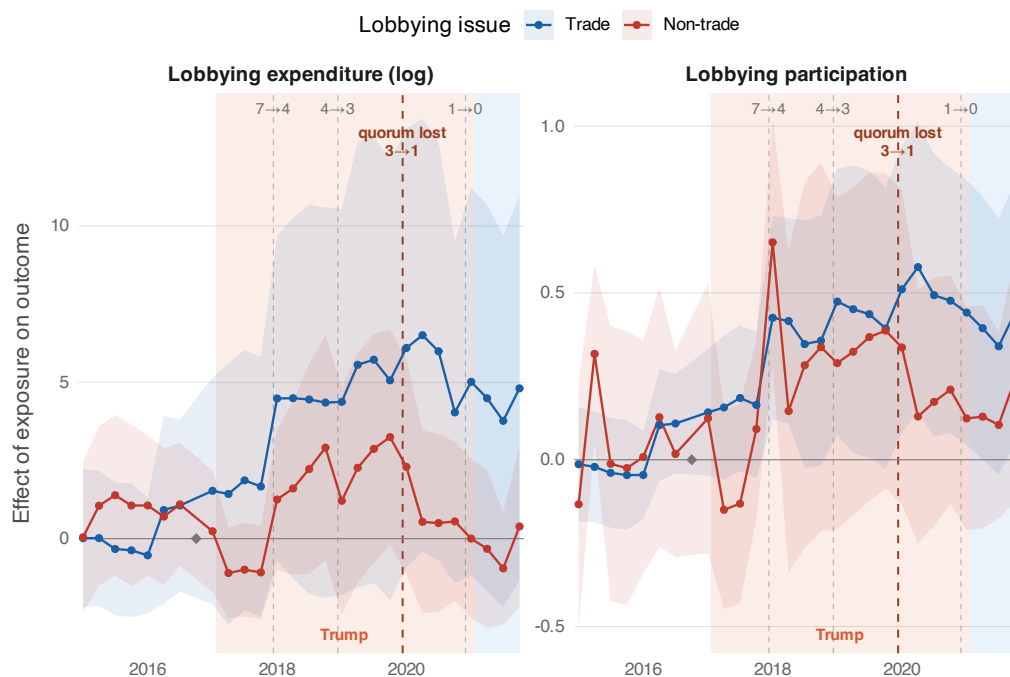


Figure B3: Lobbying response to AD/CVD exposure, decomposed by issue. Trade reports carry the TRD issue code; non-trade reports are all others. The two series partition the main outcome. Estimation ends in 2021Q4, after which no reports carry the TRD issue code. Base rates differ substantially: roughly 12.8 percent of firm-quarters contain a trade report against 39.6 percent for non-trade, so a given coefficient represents a much larger proportional change on the trade series. Relative to 2016Q4; 95% confidence intervals.

## C Continuous Treatment

Two-way fixed effects with a continuous, non-absorbing dose identifies a weighted average of dose-response effects whose weights can be non-convex under heterogeneity in the response to dose. Following Callaway, Goodman-Bacon, and Sant’Anna (2024), I estimate effects for a saturated set of dose groups against an untreated comparison group, which requires no functional form on the dose-response relationship. Exposure here is severely right-skewed, so coarsening the treatment to an indicator for any positive exposure would assign firms with negligible dose to the treated group. I instead fix each firm’s dose at its 2015–2017 average, which holds group assignment constant over time, and estimate the event study for terciles of the positive-dose distribution against firms with no pre-period exposure.

Table C1: Dose-group composition. Firms are assigned to terciles of the positive pre-period dose distribution, or to a zero-dose comparison group, using average exposure over 2015Q1–2017Q4. Base rates are pre-2018 means.

| Dose group | Firms | Mean dose | Median dose | Participation rate | Log expenditure |
|------------|-------|-----------|-------------|--------------------|-----------------|
| None       | 223   | 0.0000    | 0.0000      | 0.4156             | 4.0992          |
| Low        | 75    | 0.0001    | 0.0001      | 0.4879             | 4.8885          |
| Mid        | 75    | 0.0018    | 0.0014      | 0.4986             | 5.0914          |
| High       | 75    | 0.1022    | 0.0302      | 0.3096             | 2.9017          |

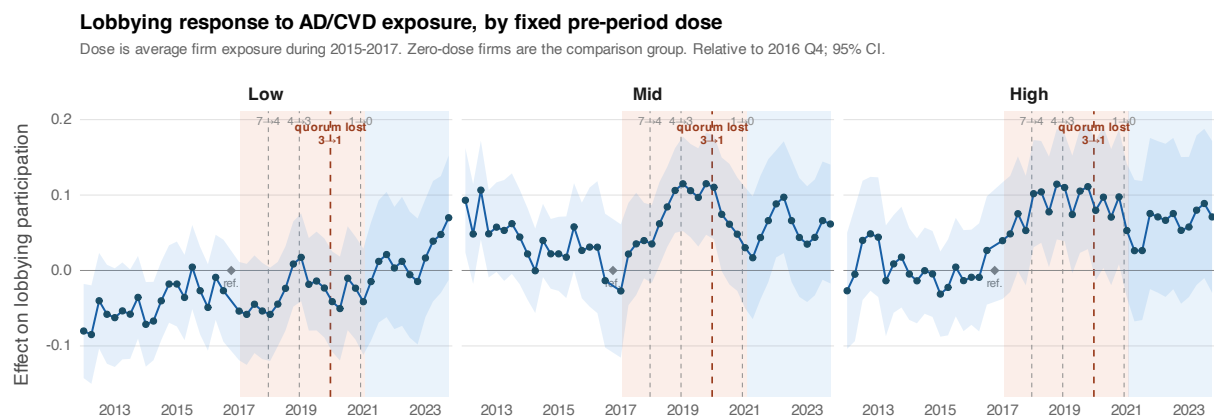


Figure C1: Lobbying participation by fixed pre-period dose. Dose is each firm’s average AD/CVD exposure over 2015Q1–2017Q4, held constant over the panel. Panels show terciles of the positive-dose distribution against firms with zero pre-period dose. Coefficients are level effects relative to zero dose; comparisons across positive-dose groups identify causal responses to additional dose only under strong parallel trends and are not interpreted as such. Relative to 2016Q4; 95% confidence intervals.

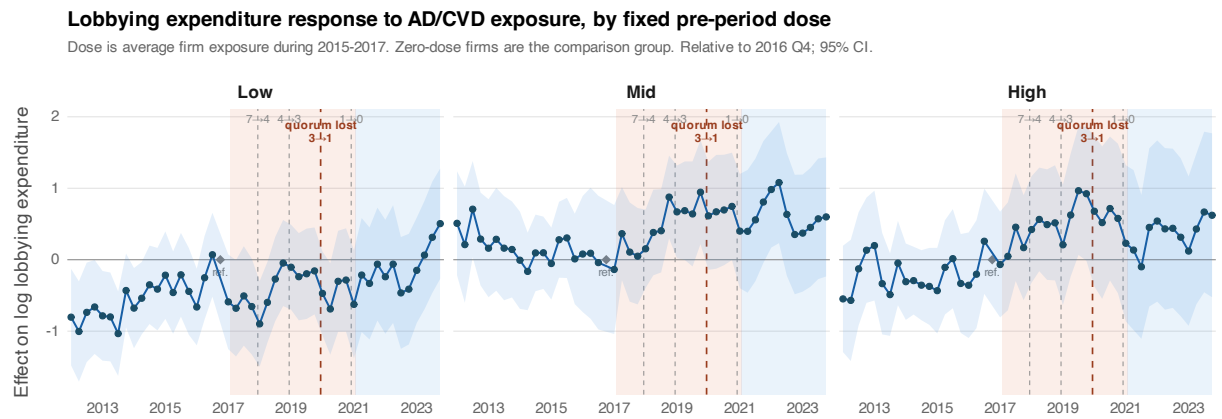


Figure C2: Lobbying expenditure by fixed pre-period dose. As in Figure C1, with  $\log(1 + \text{expenditure})$  as the outcome. Relative to 2016Q4; 95% confidence intervals.

## D Timing and Anticipation

A possible concern is that the treatment is mistimed. Duties are anticipated well before they take effect: a petition is public at filing, so firms may respond to the prospect of a barrier rather than to its imposition. If they do, quarters coded untreated are in fact treated, which attenuates the estimate and contaminates the immediate pre-period. To assess this, I first vary the lag with which exposure enters, comparing contemporaneous, one-quarter, and two-quarter specifications (Figure D1). I then re-estimate with 2017Q4 as the omitted quarter rather than 2016Q4, confirming that the break is not an artifact of the reference choice (Figure D2), and run a placebo test on the pre-2018 sample with a fake treatment date at 2013Q4 (Figure D3).

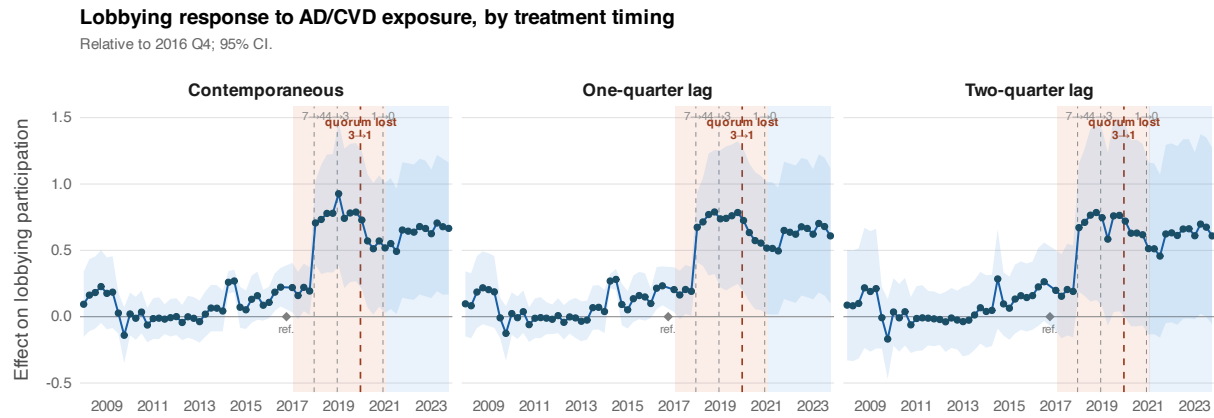


Figure D1: Lobbying participation by treatment timing. Exposure entered contemporaneously and at one- and two-quarter lags, with Section 301 and 232 controls lagged to match in each panel. Relative to 2016Q4; 95% confidence intervals.

The sharpest test uses a second exposure measure covering the investigation window only, from petition initiation to duty imposition (Table D1). For any given case this window is disjoint in time from the duty spell apart from the transition quarter, so the two measures can enter jointly: the window coefficient captures the response to the allegation, the duty coefficient captures the response to the barrier. Entered together, the response to duties in force is unchanged and the investigation-window coefficient is centered on zero.

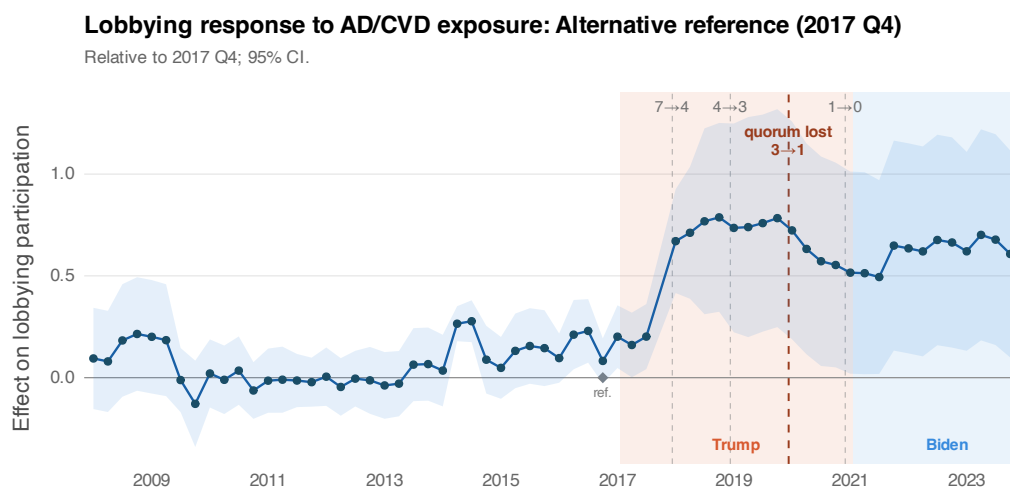


Figure D2: Lobbying participation, alternative reference quarter. As in the main specification, with 2017Q4 as the omitted quarter rather than 2016Q4. Relative to 2017Q4; 95% confidence intervals.

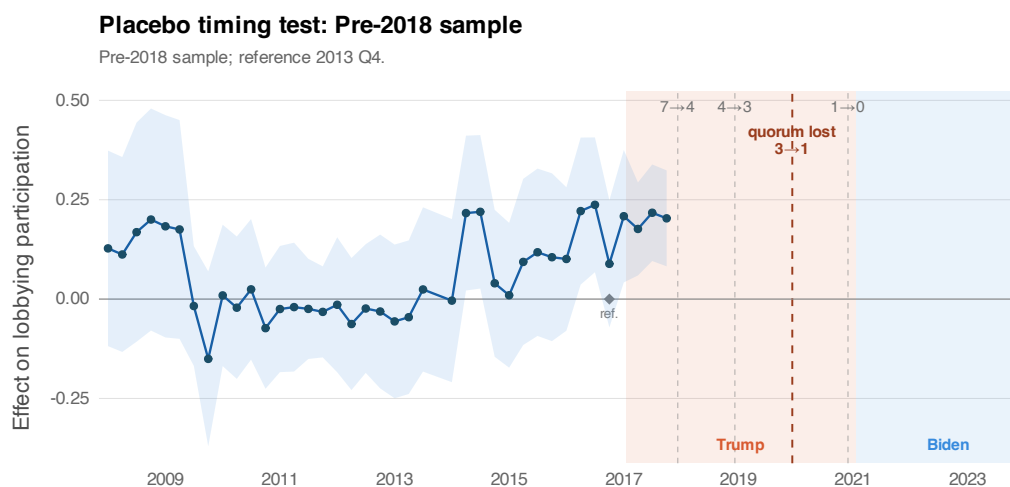


Figure D3: Placebo timing test. Estimation restricted to quarters through 2017Q4 with 2013Q4 as the reference. Relative to 2013Q4; 95% confidence intervals.

Table D1: Investigation-window exposure. The investigation window covers petition initiation to duty imposition; the window coefficient is the response to the allegation, the duty coefficient the response to the barrier.

| Dependent Variables:                    | Lobbying participation |                    |                   | Log lobbying expenditure |                   |
|-----------------------------------------|------------------------|--------------------|-------------------|--------------------------|-------------------|
|                                         | Part.: duty only       | Part.: window only | Part.: both       | Exp.: duty only          | Exp.: both        |
| Model:                                  | (1)                    | (2)                | (3)               | (4)                      | (5)               |
| <i>Variables</i>                        |                        |                    |                   |                          |                   |
| AD/CVD exposure                         | 0.099<br>(0.084)       |                    | 0.095<br>(0.085)  | 1.62*<br>(0.921)         | 1.60*<br>(0.932)  |
| Section 301 exposure                    | -0.047<br>(0.076)      | -0.009<br>(0.081)  | -0.046<br>(0.076) | -0.423<br>(0.804)        | -0.421<br>(0.804) |
| Section 232 exposure                    | -0.031<br>(0.162)      | 0.212<br>(0.149)   | -0.031<br>(0.162) | 0.121<br>(1.65)          | 0.117<br>(1.65)   |
| AD/CVD exposure $\times$ Post-2018      | 0.559*<br>(0.324)      |                    | 0.565*<br>(0.323) | 4.59<br>(3.38)           | 4.61<br>(3.36)    |
| Investigation window                    |                        | 0.089<br>(0.106)   | 0.110<br>(0.106)  |                          | 0.324<br>(1.07)   |
| Investigation window $\times$ Post-2018 |                        | 0.132<br>(0.417)   | -0.080<br>(0.343) |                          | -0.605<br>(3.48)  |
| <i>Fixed-effects</i>                    |                        |                    |                   |                          |                   |
| Firm                                    | Yes                    | Yes                | Yes               | Yes                      | Yes               |
| Quarter                                 | Yes                    | Yes                | Yes               | Yes                      | Yes               |
| <i>Fit statistics</i>                   |                        |                    |                   |                          |                   |
| Observations                            | 44,352                 | 44,352             | 44,352            | 44,352                   | 44,352            |
| R <sup>2</sup>                          | 0.524                  | 0.522              | 0.524             | 0.611                    | 0.611             |

*Clustered (Firm & Quarter) standard-errors in parentheses*

*Signif. Codes: \*\*\*: 0.01, \*\*: 0.05, \*: 0.1*

## E Parallel Trends Violations

A joint test of whether all pre-treatment event-study coefficients equal zero fails to reject the null for both outcomes in the main specifications shown in Figures 6–7 ( $F = 0.47$ ,  $p = 0.998$  for participation;  $F = 0.90$ ,  $p = 0.644$  for expenditure). I additionally report the sensitivity of the results to parallel trends violations following Rambachan and Roth (2023), which bounds the post-treatment violation rather than testing for its absence.

Results in Figure E1 show that the estimated jump at 2018Q1 remains significantly positive under post-treatment violations of parallel trends as large as the largest period-to-period deviation observed before 2018 ( $\bar{M} = 1$ ). The result therefore requires no assumption that differential trends are smaller after treatment than before.

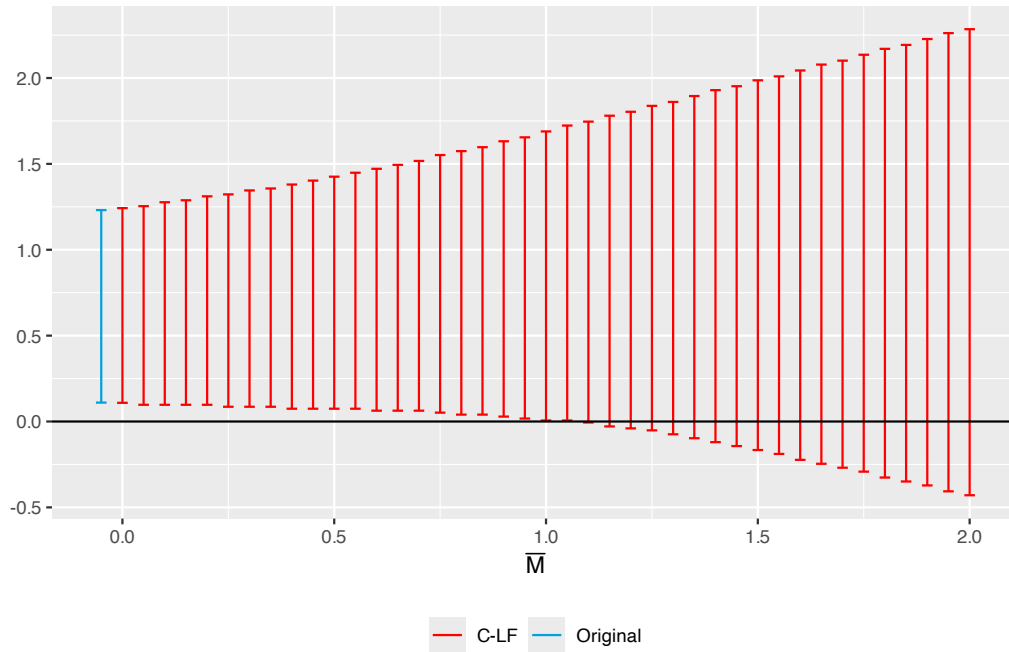


Figure E1: Sensitivity to parallel-trends violations, relative magnitudes. Robust confidence sets following Rambachan and Roth (2023) for the 2018Q1 coefficient calculated with **HonestDiD**.  $\bar{M}$  bounds the post-treatment violation as a multiple of the largest period-to-period violation in the pre-period;  $\bar{M} = 1$  permits a post-treatment violation no larger than what is already visible before 2018. The target is the single quarter in which the break occurs. Reference quarter is set to 2017Q4, the last untreated quarter, as **HonestDiD** assumes.

I additionally test whether the response reflects shocks common to firms in the same sector rather than the AD/CVD orders themselves. AD/CVD petitions cluster in particular industries, so exposed firms are disproportionately concentrated in sectors that may have faced other contemporaneous pressures. Table E1 allows every two-digit NAICS sector its own quarterly time path, so the effect is identified only from firms in the same sector at the same moment. The estimated effect is essentially unchanged under sector-by-quarter fixed effects for both participation and expenditure, indicating that the response is not driven by trends common to firms in the same sector.

Table E1: Sector-by-quarter fixed effects

| Dependent Variables:               | Lobbying participation |                     | Log lobbying expenditure |                    |
|------------------------------------|------------------------|---------------------|--------------------------|--------------------|
| Model:                             | Part.: baseline        | Part.: sector x qtr | Exp.: baseline           | Exp.: sector x qtr |
|                                    | (1)                    | (2)                 | (3)                      | (4)                |
| <i>Variables</i>                   |                        |                     |                          |                    |
| AD/CVD exposure                    | 0.099<br>(0.084)       | 0.125<br>(0.095)    | 1.62*<br>(0.921)         | 1.64<br>(1.01)     |
| Section 301 exposure               | -0.047<br>(0.076)      | -0.047<br>(0.081)   | -0.423<br>(0.804)        | -0.579<br>(0.831)  |
| Section 232 exposure               | -0.031<br>(0.162)      | -0.057<br>(0.182)   | 0.121<br>(1.65)          | -0.220<br>(1.84)   |
| AD/CVD exposure $\times$ Post-2018 | 0.559*<br>(0.324)      | 0.603*<br>(0.320)   | 4.59<br>(3.38)           | 5.36<br>(3.31)     |
| <i>Fixed-effects</i>               |                        |                     |                          |                    |
| Firm                               | Yes                    | Yes                 | Yes                      | Yes                |
| Quarter                            | Yes                    |                     | Yes                      |                    |
| Quarter-Sector                     |                        | Yes                 |                          | Yes                |
| <i>Fit statistics</i>              |                        |                     |                          |                    |
| Observations                       | 44,352                 | 43,560              | 44,352                   | 43,560             |
| R <sup>2</sup>                     | 0.524                  | 0.553               | 0.611                    | 0.635              |

*Clustered (Firm & Quarter) standard-errors in parentheses*

*Signif. Codes: \*\*\*: 0.01, \*\*: 0.05, \*: 0.1*

## F Shock and Share Exogeneity

Neither component of the shift-share measure is exogenous by construction. On the shock side, AD/CVD duties are not assigned by nature: they follow petitions filed by US domestic industries that choose which products and countries to target. If petitioning itself responded to the post-2018 environment, or petitioners target foreign firms that are politically active, the treatment is correlated with lobbying propensity by construction. A related possibility is that firms are responding to their home country’s own trade-distorting conduct, of which US petitions are the visible symptom, rather than to the duty on their products. On the share side, the import baskets that weight the shocks are predetermined but not exogenous: firms concentrated in particular products could have been on different lobbying trajectories for reasons correlated with which products later drew orders.

To address the shock side, I first rebuild exposure using only orders whose petitions were initiated before 2018, so that post-period treatment cannot reflect petitioning during the Appellate Body crisis (Table F1). I then test the reverse-causality channel directly, asking whether a firm’s own lobbying predicts subsequent petitions against products in its import basket (Table F2). Finally, I construct a leave-one-out measure of US countervailing-duty petitions against each firm’s origin countries, computed only on products the firm does not ship, and enter it alongside own-product exposure. Because CVD petitions allege home-government subsidization directly, this proxies the home country’s trade-distorting conduct while remaining orthogonal to the firm’s own treatment (Table F3). The estimated response to own-product exposure is stable across all three tests.

To address the share side, I vary the window over which shares are fixed and re-estimate (Tables F4–F5), and re-estimate after dropping the products that contribute most to aggregate exposure (Tables F6–F7). The estimate is stable across every variant, with the loss of identifying variation showing up in wider confidence intervals rather than smaller point estimates.

Table F1: Exposure from pre-2018 order initiations. Exposure rebuilt using only orders whose petitions were initiated on or before 31 December 2017, so post-period treatment cannot reflect petitions filed after Appellate Body paralysis began.

| Dependent Variables:               | Lobbying participation   |                        | Log lobbying expenditure |                       |
|------------------------------------|--------------------------|------------------------|--------------------------|-----------------------|
| Model:                             | Part.: all orders<br>(1) | Part.: pre-2018<br>(2) | Exp.: all orders<br>(3)  | Exp.: pre-2018<br>(4) |
| <i>Variables</i>                   |                          |                        |                          |                       |
| AD/CVD exposure                    | 0.099<br>(0.084)         | 0.119<br>(0.083)       | 1.62*<br>(0.921)         | 1.84**<br>(0.907)     |
| Section 301 exposure               | -0.047<br>(0.076)        | -0.047<br>(0.074)      | -0.423<br>(0.804)        | -0.444<br>(0.783)     |
| Section 232 exposure               | -0.031<br>(0.162)        | -0.117<br>(0.184)      | 0.121<br>(1.65)          | -0.818<br>(1.82)      |
| AD/CVD exposure $\times$ Post-2018 | 0.559*<br>(0.324)        | 0.758*<br>(0.400)      | 4.59<br>(3.38)           | 6.77<br>(4.08)        |
| <i>Fixed-effects</i>               |                          |                        |                          |                       |
| Firm                               | Yes                      | Yes                    | Yes                      | Yes                   |
| Quarter                            | Yes                      | Yes                    | Yes                      | Yes                   |
| <i>Fit statistics</i>              |                          |                        |                          |                       |
| Observations                       | 44,352                   | 44,352                 | 44,352                   | 44,352                |
| R <sup>2</sup>                     | 0.524                    | 0.524                  | 0.611                    | 0.611                 |

*Clustered (Firm & Quarter) standard-errors in parentheses*

*Signif. Codes: \*\*\*: 0.01, \*\*: 0.05, \*: 0.1*

Table F2: Does own lobbying predict subsequent petitions? The outcome is an indicator for any new AD/CVD petition initiated that quarter against a product in the firm's fixed pre-period basket.

| Dependent Variable:                                    | New petition on own products      |
|--------------------------------------------------------|-----------------------------------|
|                                                        | New petition against own products |
| Model:                                                 | (1)                               |
| <i>Variables</i>                                       |                                   |
| Lobbying, $t - 1$                                      | 0.002<br>(0.005)                  |
| Lobbying, $t - 2$                                      | -0.003<br>(0.006)                 |
| Lobbying, $t - 3$                                      | 0.007<br>(0.007)                  |
| Lobbying, $t - 4$                                      | 0.002<br>(0.006)                  |
| <i>Fixed-effects</i>                                   |                                   |
| Firm                                                   | Yes                               |
| Quarter                                                | Yes                               |
| <i>Fit statistics</i>                                  |                                   |
| Observations                                           | 43,008                            |
| R <sup>2</sup>                                         | 0.257                             |
| <i>Clustered (Firm) standard-errors in parentheses</i> |                                   |
| <i>Signif. Codes: ***: 0.01, **: 0.05, *: 0.1</i>      |                                   |

Table F3: Home-country protectionism. The control is US countervailing-duty petitions against the firm's origin countries on products the firm does not ship, weighted by the firm's share in each origin. CVD petitions allege home-government subsidization directly, which makes this the sharper indicator; the all-petition version is reported alongside.

| Dependent Variables:               | Lobbying participation |                     |                     | Log lobbying expenditure |                    |
|------------------------------------|------------------------|---------------------|---------------------|--------------------------|--------------------|
| Model:                             | Part.: baseline<br>(1) | Part.: + CVD<br>(2) | Part.: + all<br>(3) | Exp.: baseline<br>(4)    | Exp.: + CVD<br>(5) |
| <i>Variables</i>                   |                        |                     |                     |                          |                    |
| AD/CVD exposure                    | 0.099<br>(0.084)       | 0.099<br>(0.084)    | 0.098<br>(0.084)    | 1.62*<br>(0.921)         | 1.63*<br>(0.921)   |
| Section 301 exposure               | -0.047<br>(0.076)      | -0.048<br>(0.074)   | -0.046<br>(0.076)   | -0.423<br>(0.804)        | -0.397<br>(0.790)  |
| Section 232 exposure               | -0.031<br>(0.162)      | -0.031<br>(0.162)   | -0.031<br>(0.162)   | 0.121<br>(1.65)          | 0.115<br>(1.66)    |
| AD/CVD exposure $\times$ Post-2018 | 0.559*<br>(0.324)      | 0.559*<br>(0.324)   | 0.559*<br>(0.324)   | 4.59<br>(3.38)           | 4.60<br>(3.38)     |
| Home-country CVD petitions         |                        | 0.002<br>(0.009)    |                     |                          | -0.058<br>(0.096)  |
| Home-country petitions             |                        |                     | 0.002<br>(0.003)    |                          |                    |
| <i>Fixed-effects</i>               |                        |                     |                     |                          |                    |
| Firm                               | Yes                    | Yes                 | Yes                 | Yes                      | Yes                |
| Quarter                            | Yes                    | Yes                 | Yes                 | Yes                      | Yes                |
| <i>Fit statistics</i>              |                        |                     |                     |                          |                    |
| Observations                       | 44,352                 | 44,352              | 44,352              | 44,352                   | 44,352             |
| R <sup>2</sup>                     | 0.524                  | 0.524               | 0.524               | 0.611                    | 0.611              |

*Clustered (Firm & Quarter) standard-errors in parentheses*

*Signif. Codes: \*\*\*: 0.01, \*\*: 0.05, \*: 0.1*

Table F4: Alternative share-fixing windows, participation. Bartik import shares fixed over the stated pre-period window rather than the baseline 2010–2017. Shorter windows use fewer shipments per firm and produce noisier shares.

| Dependent Variable:                | Lobbying participation |                    |                   |                    |
|------------------------------------|------------------------|--------------------|-------------------|--------------------|
|                                    | 2010-2017              | 2010-2013          | 2010-2015         | 2013-2017          |
| Model:                             | (1)                    | (2)                | (3)               | (4)                |
| <i>Variables</i>                   |                        |                    |                   |                    |
| AD/CVD exposure                    | 0.099<br>(0.084)       | 0.146<br>(0.090)   | 0.131<br>(0.080)  | 0.044<br>(0.094)   |
| Section 301 exposure               | -0.047<br>(0.076)      | -0.076<br>(0.076)  | -0.054<br>(0.079) | -0.037<br>(0.082)  |
| Section 232 exposure               | -0.031<br>(0.162)      | -0.068<br>(0.136)  | -0.030<br>(0.148) | -0.072<br>(0.172)  |
| AD/CVD exposure $\times$ Post-2018 | 0.559*<br>(0.324)      | 0.661**<br>(0.311) | 0.566*<br>(0.303) | 0.644**<br>(0.321) |
| <i>Fixed-effects</i>               |                        |                    |                   |                    |
| Firm                               | Yes                    | Yes                | Yes               | Yes                |
| Quarter                            | Yes                    | Yes                | Yes               | Yes                |
| <i>Fit statistics</i>              |                        |                    |                   |                    |
| Observations                       | 44,352                 | 42,273             | 43,362            | 43,560             |
| R <sup>2</sup>                     | 0.524                  | 0.527              | 0.522             | 0.525              |

*Clustered (Firm & Quarter) standard-errors in parentheses*

*Signif. Codes: \*\*\*: 0.01, \*\*: 0.05, \*: 0.1*

Table F5: Alternative share-fixing windows, expenditure. As in Table F4, with log lobbying expenditure as the outcome.

| Dependent Variable:<br>Model:      | Log lobbying expenditure |                   |                   |                   |
|------------------------------------|--------------------------|-------------------|-------------------|-------------------|
|                                    | 2010-2017<br>(1)         | 2010-2013<br>(2)  | 2010-2015<br>(3)  | 2013-2017<br>(4)  |
| <i>Variables</i>                   |                          |                   |                   |                   |
| AD/CVD exposure                    | 1.62*<br>(0.921)         | 1.61<br>(1.08)    | 1.73*<br>(0.919)  | 0.914<br>(0.963)  |
| Section 301 exposure               | -0.423<br>(0.804)        | -0.697<br>(0.819) | -0.471<br>(0.839) | -0.361<br>(0.852) |
| Section 232 exposure               | 0.121<br>(1.65)          | -0.499<br>(1.43)  | -0.028<br>(1.54)  | -0.372<br>(1.73)  |
| AD/CVD exposure $\times$ Post-2018 | 4.59<br>(3.38)           | 6.12*<br>(3.29)   | 5.01<br>(3.24)    | 5.59*<br>(3.29)   |
| <i>Fixed-effects</i>               |                          |                   |                   |                   |
| Firm                               | Yes                      | Yes               | Yes               | Yes               |
| Quarter                            | Yes                      | Yes               | Yes               | Yes               |
| <i>Fit statistics</i>              |                          |                   |                   |                   |
| Observations                       | 44,352                   | 42,273            | 43,362            | 43,560            |
| R <sup>2</sup>                     | 0.611                    | 0.612             | 0.608             | 0.613             |

*Clustered (Firm & Quarter) standard-errors in parentheses*

*Signif. Codes: \*\*\*: 0.01, \*\*: 0.05, \*: 0.1*

Table F6: Top-product leave-out, participation. Products contributing most to aggregate AD/CVD exposure are excluded from the exposure measure. Ranking is by share-weighted exposure contribution.

| Dependent Variable:<br>Model:      | Lobbying participation |                   |                   |                   |
|------------------------------------|------------------------|-------------------|-------------------|-------------------|
|                                    | All products<br>(1)    | Drop top 1<br>(2) | Drop top 3<br>(3) | Drop top 5<br>(4) |
| <i>Variables</i>                   |                        |                   |                   |                   |
| AD/CVD exposure                    | 0.099<br>(0.084)       | 0.055<br>(0.084)  | 0.049<br>(0.089)  | 0.005<br>(0.087)  |
| Section 301 exposure               | -0.047<br>(0.076)      | -0.043<br>(0.075) | -0.034<br>(0.076) | -0.046<br>(0.074) |
| Section 232 exposure               | -0.031<br>(0.162)      | -0.055<br>(0.164) | 0.043<br>(0.167)  | 0.052<br>(0.152)  |
| AD/CVD exposure $\times$ Post-2018 | 0.559*<br>(0.324)      | 0.615*<br>(0.331) | 0.450<br>(0.355)  | 0.724<br>(0.443)  |
| <i>Fixed-effects</i>               |                        |                   |                   |                   |
| Firm                               | Yes                    | Yes               | Yes               | Yes               |
| Quarter                            | Yes                    | Yes               | Yes               | Yes               |
| <i>Fit statistics</i>              |                        |                   |                   |                   |
| Observations                       | 44,352                 | 44,352            | 44,352            | 44,352            |
| R <sup>2</sup>                     | 0.524                  | 0.524             | 0.523             | 0.523             |

*Clustered (Firm & Quarter) standard-errors in parentheses*

*Signif. Codes: \*\*\*: 0.01, \*\*: 0.05, \*: 0.1*

Table F7: Top-product leave-out, expenditure. As in Table F6, with log lobbying expenditure as the outcome.

| Dependent Variable:<br>Model:      | Log lobbying expenditure |                   |                   |                   |
|------------------------------------|--------------------------|-------------------|-------------------|-------------------|
|                                    | All products<br>(1)      | Drop top 1<br>(2) | Drop top 3<br>(3) | Drop top 5<br>(4) |
| <i>Variables</i>                   |                          |                   |                   |                   |
| AD/CVD exposure                    | 1.62*<br>(0.921)         | 1.30<br>(0.991)   | 0.838<br>(1.08)   | 0.416<br>(1.13)   |
| Section 301 exposure               | -0.423<br>(0.804)        | -0.387<br>(0.794) | -0.302<br>(0.801) | -0.429<br>(0.788) |
| Section 232 exposure               | 0.121<br>(1.65)          | -0.105<br>(1.67)  | 0.675<br>(1.76)   | 0.638<br>(1.61)   |
| AD/CVD exposure $\times$ Post-2018 | 4.59<br>(3.38)           | 5.11<br>(3.46)    | 3.76<br>(3.73)    | 6.57<br>(4.77)    |
| <i>Fixed-effects</i>               |                          |                   |                   |                   |
| Firm                               | Yes                      | Yes               | Yes               | Yes               |
| Quarter                            | Yes                      | Yes               | Yes               | Yes               |
| <i>Fit statistics</i>              |                          |                   |                   |                   |
| Observations                       | 44,352                   | 44,352            | 44,352            | 44,352            |
| R <sup>2</sup>                     | 0.611                    | 0.611             | 0.610             | 0.611             |

*Clustered (Firm & Quarter) standard-errors in parentheses*

*Signif. Codes: \*\*\*: 0.01, \*\*: 0.05, \*: 0.1*

## G Spillovers

A possible concern is that unexposed firms are also affected by orders placed on their competitors, which would violate SUTVA and contaminate the comparison group. Trade lobbying is often collective, so a firm placed under order may be represented through an industry association or a coordinated effort that also covers unexposed firms in the same sector. Unexposed firms may also respond to competitors being tariffed, whether by lobbying to preserve the advantage or by anticipating orders of their own. To assess this, I exclude never-exposed firms in NAICS-4 industries containing exposed firms, leaving a comparison group drawn only from industries where no order landed (Table G1). The estimate is stable for both outcomes.

Table G1: Spillover-purged control group. Never-exposed firms in NAICS-4 industries containing exposed firms are excluded, leaving a comparison group drawn from industries where no order landed.

| Dependent Variables:               | Lobbying participation |                   | Log lobbying expenditure |                   |
|------------------------------------|------------------------|-------------------|--------------------------|-------------------|
| Model:                             | Part.: full            | Part.: purged     | Exp.: full               | Exp.: purged      |
|                                    | (1)                    | (2)               | (3)                      | (4)               |
| <i>Variables</i>                   |                        |                   |                          |                   |
| AD/CVD exposure                    | 0.099<br>(0.084)       | 0.104<br>(0.084)  | 1.62*<br>(0.921)         | 1.58*<br>(0.924)  |
| Section 301 exposure               | -0.047<br>(0.076)      | -0.052<br>(0.084) | -0.423<br>(0.804)        | -0.428<br>(0.884) |
| Section 232 exposure               | -0.031<br>(0.162)      | -0.026<br>(0.164) | 0.121<br>(1.65)          | 0.031<br>(1.68)   |
| AD/CVD exposure $\times$ Post-2018 | 0.559*<br>(0.324)      | 0.533<br>(0.326)  | 4.59<br>(3.38)           | 4.56<br>(3.42)    |
| <i>Fixed-effects</i>               |                        |                   |                          |                   |
| Firm                               | Yes                    | Yes               | Yes                      | Yes               |
| Quarter                            | Yes                    | Yes               | Yes                      | Yes               |
| <i>Fit statistics</i>              |                        |                   |                          |                   |
| Observations                       | 44,352                 | 33,363            | 44,352                   | 33,363            |
| R <sup>2</sup>                     | 0.524                  | 0.546             | 0.611                    | 0.638             |

*Clustered (Firm & Quarter) standard-errors in parentheses*

*Signif. Codes: \*\*\*: 0.01, \*\*: 0.05, \*: 0.1*

## H Sample Composition

Exposure is heavily concentrated: ten of the 448 firms account for roughly 40 percent of aggregate post-2018 exposure, so a handful of observations carry much of the identifying variation. To assess this, I re-estimate excluding the five firms with the highest total lobbying expenditure and, separately, the five with the highest mean exposure (Tables H1–H2). The estimate is stable in both cases, with the loss of identifying variation showing up in wider confidence intervals rather than smaller point estimates.

Table H1: Influential firms, participation. The five firms with the highest total lobbying expenditure, and separately the five with the highest mean exposure, are excluded.

| Dependent Variable:<br>Model:      | Lobbying participation |                             |                            |
|------------------------------------|------------------------|-----------------------------|----------------------------|
|                                    | Full sample<br>(1)     | Excl. top-5 spenders<br>(2) | Excl. top-5 exposed<br>(3) |
| <i>Variables</i>                   |                        |                             |                            |
| AD/CVD exposure                    | 0.099<br>(0.084)       | 0.100<br>(0.084)            | 0.052<br>(0.088)           |
| Section 301 exposure               | -0.047<br>(0.076)      | -0.048<br>(0.076)           | -0.043<br>(0.075)          |
| Section 232 exposure               | -0.031<br>(0.162)      | -0.032<br>(0.162)           | -0.052<br>(0.195)          |
| AD/CVD exposure $\times$ Post-2018 | 0.559*<br>(0.324)      | 0.558*<br>(0.324)           | 0.537<br>(0.414)           |
| <i>Fixed-effects</i>               |                        |                             |                            |
| Firm                               | Yes                    | Yes                         | Yes                        |
| Quarter                            | Yes                    | Yes                         | Yes                        |
| <i>Fit statistics</i>              |                        |                             |                            |
| Observations                       | 44,352                 | 43,857                      | 43,857                     |
| R <sup>2</sup>                     | 0.524                  | 0.517                       | 0.522                      |

*Clustered (Firm & Quarter) standard-errors in parentheses*

*Signif. Codes: \*\*\*: 0.01, \*\*: 0.05, \*: 0.1*

Table H2: Influential firms, expenditure. As in Table H1, with log lobbying expenditure as the outcome.

| Dependent Variable:<br>Model:      | Log lobbying expenditure |                             |                            |
|------------------------------------|--------------------------|-----------------------------|----------------------------|
|                                    | Full sample<br>(1)       | Excl. top-5 spenders<br>(2) | Excl. top-5 exposed<br>(3) |
| <i>Variables</i>                   |                          |                             |                            |
| AD/CVD exposure                    | 1.62*<br>(0.921)         | 1.62*<br>(0.921)            | 1.21<br>(1.07)             |
| Section 301 exposure               | -0.423<br>(0.804)        | -0.434<br>(0.805)           | -0.330<br>(0.806)          |
| Section 232 exposure               | 0.121<br>(1.65)          | 0.111<br>(1.65)             | 0.182<br>(1.97)            |
| AD/CVD exposure $\times$ Post-2018 | 4.59<br>(3.38)           | 4.57<br>(3.38)              | 3.42<br>(4.20)             |
| <i>Fixed-effects</i>               |                          |                             |                            |
| Firm                               | Yes                      | Yes                         | Yes                        |
| Quarter                            | Yes                      | Yes                         | Yes                        |
| <i>Fit statistics</i>              |                          |                             |                            |
| Observations                       | 44,352                   | 43,857                      | 43,857                     |
| R <sup>2</sup>                     | 0.611                    | 0.598                       | 0.609                      |

*Clustered (Firm & Quarter) standard-errors in parentheses*

*Signif. Codes: \*\*\*: 0.01, \*\*: 0.05, \*: 0.1*

# I Heterogeneity by Origin

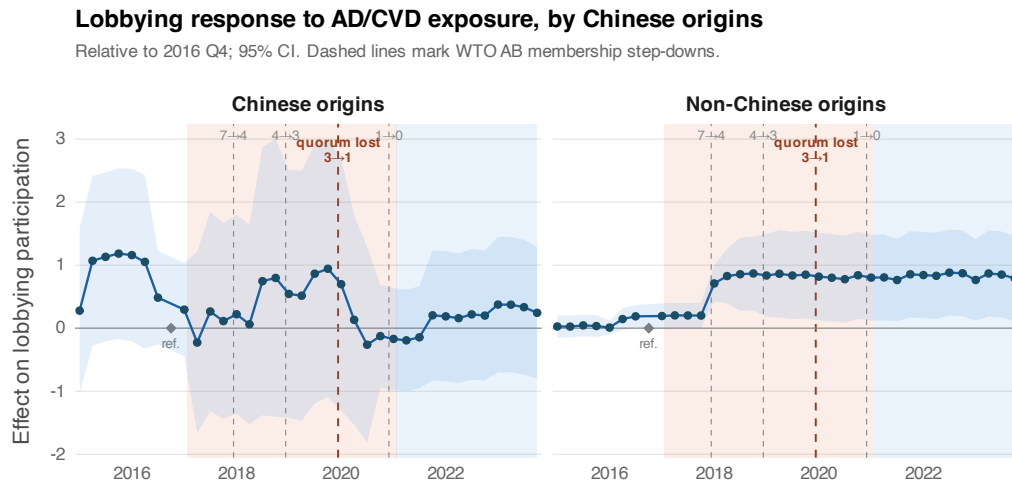


Figure I1: Lobbying participation by Chinese and non-Chinese origins. Exposure decomposed by shipment origin. Relative to 2016Q4; 95% confidence intervals.

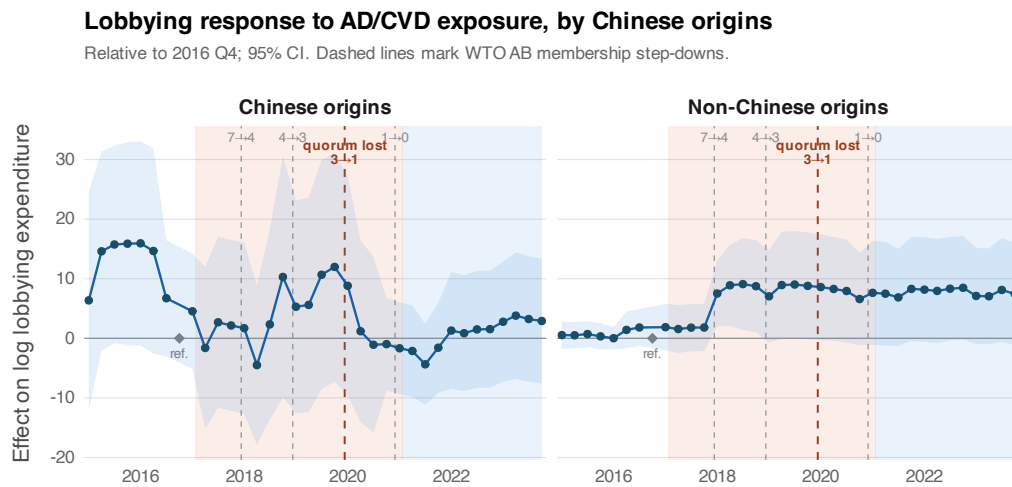


Figure I2: Lobbying expenditure by Chinese and non-Chinese origins. As in Figure I1, with  $\log(1 + \text{expenditure})$  as the outcome. Relative to 2016Q4; 95% confidence intervals.