Pre-Trends and Trade Effects of Temporary Trade Barriers*

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Abstract

This paper studies trade patterns before and after temporary trade barrier (TTB) investigations initiated by the United States between 1993 and 2015. We identify distinct import quantity and price patterns for targeted goods compared to non-targeted goods from the same country and industry. Over the six years preceding TTB investigations, imports from subject countries rise by 50 percent, while prices decline by 16 percent. These pre-trends are characterized by gradual growth, rather than sudden surges. Thus, their counterfactual continuation is crucial for assessing the policy's trade effects. Under the parallel pre-trends assumption, we find large, immediate, and persistent trade destruction. However, assuming the pre-trends had continued in the absence of the policy, the trade destruction effects are significantly larger. This difference is critical for the net trade effect, as trade destruction outweighs trade diversion only when pre-trends are extrapolated. Additionally, we provide new insights into the de facto application of TTBs, such as the lack of discrimination against China and a flattening trend growth leading to TTB investigations over time.

Keywords: Temporary trade barriers, antidumping and countervailing duties, trade protectionism, dynamic treatment effects, pre-trends

JEL codes: F13; F14

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1. Introduction

The use of temporary trade barriers (TTBs) has increased widely across countries and industries during the last three decades. Although the World Trade Organization (WTO) requires each member to apply tariffs equally to all member states, it allows exceptional discriminatory trade remedies that protect specific domestic industries from foreign competition. The most frequently applied TTBs are antidumping (AD) and countervailing duties (CVD). For instance, between 1993 and 2015, the United States imposed TTBs on more than 10 percent of all goods imported.[1] While there is a consensus that TTBs deter trade, their unique institutional features complicate the identification of causal trade effects.[2] In particular, the implementation of TTBs requires evidence of material injury to the domestic industry, making it more likely for investigations to be initiated against import-surging varieties. Uncertainty regarding how trade would have evolved in the absence of a TTB complicates the identification of its trade effects, as its true impact is likely to depend on the strength and shape of pre-trends.

This paper studies trends before TTB investigations and their interactions with the estimated trade effects. To do so, we borrow insights from the recent difference-in-difference (DiD) and event study literature. The event study design is ideally suited for this purpose because it captures the treatment differences before and after initiating an investigation.[3] Thus, it allows us to answer two essential questions. First, are there, and what is the shape of the trade patterns before TTB investigations? Second, how do the potential pre-trends interact with the estimated trade effects of TTBs? While an extensive line of research has studied the quantitative effects of TTBs (Prusa, 1997, 2001; Bown and Crowley, 2007) and another one has documented that the imposition of TTBs is preceded by significant import surges (Bown and Crowley, 2013; McCalman and Hillberry, 2016),

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[1] TTBs are also an essential feature of the recent rise in protectionism. For example, 302 AD and CVD investigations were initiated under the Trump administration, almost three times as many as under the Obama administration.

[2] Most studies concerned with the trade effects of AD and CVD investigations focus on U.S. investigations before 2000. Blonigen and Prusa (2016) provides a comprehensive summary of the estimates.

[3] Other recent papers that use event studies to estimate the dynamic response to trade shocks are Fajgelbaum et al. (2020); Flaaen et al. (2020), and Carter and Steinbach (2020). Fajgelbaum et al. (2020) and Carter and Steinbach (2020) estimate the response to the recent U.S.-China trade war, while Flaaen et al. (2020) studies price responses to the recent wave of AD duties the U.S. imposed on washing machines. This paper is the first to apply the event study design to all U.S. TTB investigations over a long period.

this paper provides new insights into both and demonstrates that the former cannot be identified without assuming how the latter would have evolved in the absence of the TTB. To make our case, we estimate the trade response to the initiation of TTB investigations under three alternative DiD specifications: a static specification that estimates average effects after the investigation, a standard event study specification that assumes parallel pre-trends, and an event study specification that estimates a linear pre-trend and extrapolates it into the post-initiation period.

Our analysis generates three main findings. First, we find sizeable pre-trends in the form of rising volumes and declining prices of imports subject to an investigation. Six years before the TTB investigation, import values (unit values) are significantly below (above) their level at the time of the investigation. Imports rise almost linearly until around two years before the investigation when they experience a rather rapid surge that dissipates in the year before the investigation. Second, the trade effects are very different under the three specifications. Due to the pre-trends, the static estimates are severely downward biased in volumes and muted in the case of prices. The comparison of the two dynamic specifications underscores the importance of the counterfactual continuation of the pre-trends: When estimated as the deviation from the extrapolated linear pre-trends, they are significantly larger than under the parallel trends assumption. This difference is critical for the net per-good effect of TTBs as the trade destruction outweighs trade diversion only under the former. Third, by examining pre-trend heterogeneity, we provide new insights into the de facto application of TTBs. For instance, no pre-trend differences relative to other targeted countries suggest that Chinese exports were not discriminated against, and smaller pre-trends in the second half of our sample periods suggest that the standards that lead to TTBs may have been relaxed over time.

We focus on AD and CVD investigations initiated by the U.S. between 1993 and 2015.[4] In the U.S., decisions about implementing TTBs are administered jointly by the U.S. Department of Commerce (DOC) and the U.S. International Trade Commission (ITC). When a petition is filed – typically by a domestic competitor – the DOC determines whether the subject good is sold at "less than fair value" (AD) or subsidized (CVD). The ITC then decides whether the corresponding domestic

[4] We exclude a third type, safeguards, from the analysis because they are generally non-discriminatory. They are also the least common TTB. Bown and Crowley (2016) report that AD investigations covered 10.3 percent, CVD 5.1 percent, and safeguards 2.8 percent of HS6 goods at some point between 1995 and 2013.

industry has been materially injured or is threatened with material injury by the investigated countries' imports.[5] For a petition to succeed, the two agencies must agree on an affirmative ruling. These institutional features imply that TTB petitions are more likely to succeed if they are preceded by declining prices and rising imports.

Our identification of the trade patterns surrounding TTB investigations follows a common approach in the literature: the differences between targeted and non-targeted goods from the same country-sector-time and differences over time. The second difference distinguishes the static approach from the event study design. While the static method averages trade flows over both the pre- and post-investigation period, the event study fixes the reference period as the quarter before the investigation. The difference between our second and third specifications lies in the assumption regarding the counterfactual continuation of pre-trends into the treatment period. While the standard event study approach yields non-parametric estimates of the trade pattern before the investigation, our third approach estimates a parametric (linear) pre-trend. Thus, it allows one to extrapolate the estimated pre-trend into the treatment period and estimate the trade effects as the deviation between the accumulated trend growth and the estimated per-period effect relative to the quarter before the investigation.[6]

Our findings reveal distinct trade patterns before initiating TTB investigations, both in the volumes and prices of imports from countries named by an investigation. Six years before the investigation, import values (unit values) of targeted varieties were around 50 percent below (16 percent above) their levels in the quarter before the initiation. They then rise (drop) almost linearly at an average of 2.2 (0.5) percent per quarter. This pre-trend is fairly stable until six quarters before the investigation, when import values surge rather rapidly and, within two quarters, grow from -16 percent below their level in the quarter before the investigation to -1 percent, where they remain until the investigation is initiated. We also find significantly larger linear pre-trends in cases where investigations were ultimately affirmed ex-post, compared to those that were rejected or settled. Import

[5] Blonigen (2006) provides an excellent discussion on the precise definition of these requirements.

[6] We borrow this approach from recent advancements in the health economics literature, in which pre-trends are pervasive. In particular, our approach follows Dobkin et al. (2018), who assess the effects of hospitalization on inpatient healthcare spending and personal income.

values in the former category exhibited an average growth of 2.9 percent per quarter leading up to the investigation, whereas the latter averaged of 1.2 percent growth per quarter. An implication of this finding is that the strength of pre-trends not only predicts the initiation of a TTB investigation, but also their outcome. As expected we find no systematic pre-trends in the imports of the same goods by non-targeted countries.[7]

The size of these pre-trends leads to very different estimates of the trade effects of TTB investigations under the three specifications we consider. On the one hand, the static approach produces severe downward biases. In the case of import volumes, it estimates an average trade destruction effect of -14 percent, three times smaller than the average first-year effect under the standard event study approach. In the case of unit values, given that the pre- and post-initiation trade patterns mirror each other, the effects are entirely muted. On the other hand, the difference between the trade effects under two dynamic specifications grows over time as the extrapolated trend growth accumulates under the third approach. Under the parallel pre-trends assumption, the standard event study specification yields a persistent decline in import values of around -42 to -46 percent throughout the six years after the investigation. However, with extrapolated pre-trends, the average trade destruction from TTB investigation is a striking 65 percent five years after the investigation was initiated (versus 43 percent). Given their larger pre-trends, these differences are even more pronounced when comparing the trade effects of investigations that were ex-post ruled affirmatively under the two specifications. The specification choice becomes especially relevant when considering the overall trade effects of TTBs. In contrast with the commonly held view, the per-good destruction of imports from named countries significantly outweighs the trade diversion towards non-named countries when the pre-trends are extrapolated. Thus, under this approach, the intended goal of TTBs to protect domestic industry might be achieved after all.

Finally, we examine the heterogeneity in pre-trends. First, we study differences concerning the most targeted country, China, and the rest. We find no evidence of smaller pre-trends in investigations targeting Chinese imports, suggesting no discrimination in the standards used to implement TTBs.

[7] These results are robust to a wide range of considerations, such as a tighter group of control goods (fixed effects), more aggregated product classifications, the inclusion of zero trade flows or alternative sample designs.

Next, we consider any differences between the most frequent users, the base metals industry, and the rest. In this case, the pre-trends in the base metals industry explain most of the rapid surge in year two before the investigation. At the same time, non-metals display an almost linear growth throughout the entire pre-investigation period. One interpretation of this finding is that its more volatile trade patterns justify why base metals are the most frequent TTB users. At last, we examine differences in pre-trends of investigations initiated before and after 2003. We find that in the second half of our sample periods, the trends leading to the initiation of an investigation have become smaller, consistent with the idea that the requirements to raise TTBs have been relaxed since the turn of the century (Bown and Crowley, 2016).

The gradual nature of the pre-trends we document stands in contrast with common explanations of the drivers of TTBs, such as asymmetric business cycles fluctuations and currency movements, which imply more short-lived and sudden surges (Knetter and Prusa, 2003; Crowley, 2011). To investigate the origins of the pre-trends leading to TTB investigations, we use more aggregated bilateral World trade data that allows us to control for common exporter-supply shocks across destination countries. Perhaps surprisingly, we find that the economic drivers behind these trends are mostly be explained by source-destination specific shocks. Precisely, the linear pre-trend drops only by one-third when controlling for exporter supply shocks. This findings implies more nuanced interpretations of the drivers of TTBs, such as a relatively high demand elasticity of U.S. demand for certain goods compared to the rest of the world. Nonetheless, these results align with the WTO's stipulation that TTBs should be implemented only when distinct cross-destination trade patterns are evident.

The paper contributes to two strands of the literature. First, it challenges earlier estimates of the trade effects of TTBs. While early work employed dynamic panel methods and only considered targeted goods to estimate the trade effects of TTBs (Lasagni, 2000; Prusa, 2001; Konings et al., 2002; Blonigen and Haynes, 2002; Blonigen and Park, 2004; Durling and Prusa, 2006), recent work has relied on difference-in-difference methods to compare targeted and non-targeted goods, as well as time-fixed effects to control for pre-trends (Lu et al., 2013; Nita and Zanardi, 2013; Felbermayr and Sandkamp, 2020). Our paper shows that the most recent methods fail to overcome the parallel pre-trend assumption. The trade effects are considerably larger when the estimated linear pre-trends are extrapolated into the post-initiation period. It also shows that the specification choice is critical to assess the net per-good trade effects of TTBs.

Second, the paper contributes to the literature on the endogeneity of TTBs. For instance, Knetter and Prusa (2003) and Crowley (2011) show that the likelihood of TTB petitions is positively correlated with a weak foreign currency, demand, and employment, while Blonigen and Bown (2003) and Furceri et al. (2021) investigate the role of foreign retaliation. Our results are closest to Bown and Crowley (2013), who show that the likelihood of AD initiations increases with previous import surges and the terms-of-trade motive (Broda et al., 2008; Bagwell and Staiger, 2011). We also find similar results to McCalman and Hillberry (2016), who attribute these surges to exporter supply shock but argue that it is ultimately a negative demand shock immediately before the petition, which determines its filing. In contrast to McCalman and Hillberry (2016), our paper examines imports of targeted goods for a longer time frame before the TTB investigation. It establishes that the estimated almost linear growth trend is critical to determining the trade effects of the policy. Moreover, we show that the heterogeneity in the pre-trends provides novel insights into the de facto application of the policy.

The rest of the paper is organized as follows. Section 2 describes the data and some stylized facts regarding the investigation and implementation of TTBs. Section 3 lays out three alternative empirical specifications to estimate the trade effects of TTBs. Section 4 presents and discusses the main results. Section 5 provides some robustness results and examines heterogeneity over time and across countries and sectors. Section 6 concludes.

2. Data and stylized facts

We work with quarterly U.S. import data between 1990 and 2018 from the U.S. Census Bureau (2021) at the Harmonized Tariff Schedule (HS) 10-digit level. We refer to this level of aggregation as a *good* and denote it by g. We apply the concordance methodology of Pierce and Schott (2012) to create synthetic product lines, including all HS product code revisions applied throughout the sample period. We obtain AD and CVD investigations initiated in the U.S. between 1993 and 2015 from the World Bank's Global Temporary Trade Barriers Database (Bown, 2022). We focus on this sample period of TTBs to allow a minimum of three years to assess the pre- and post-initiation trade effects. Note that each investigation refers to a single country but may include several goods. Besides an investigation identifier, named country, and targeted goods, the database includes the date the investigation was initiated, final decision and revocation dates, and the duty levied (at the TTB investigation level).

Before merging the trade data with the TTB database, we first collapse the original TTB data to the unit of observation of the triplet defined by a targeted good, named country, and year of initiation.[8] We then merge the TTB data with the trade data at the good, source country, and year level. While 95 percent of the targeted goods are defined at the 10-digit tariff-line level, some investigations target goods at a higher aggregation level, e.g., 8-digit goods. In such cases, we merge the TTB data with all 10-digit goods included by the more aggregate good. This leaves us with 715 investigations, 932 distinct targeted goods, and 4,611 distinct targeted country-good pairs, which we refer to as a *variety*.

Our empirical strategy compares trade flows before and after a TTB investigation has been initiated. While some varieties are targeted more than once throughout our sample period, our baseline sample focuses on the sub-sample of targeted varieties affected by at most one TTB investigation. For several reasons, including varieties targeted by two or more investigations in different years is problematic. First, given our interest in the long-run dynamics before and after an investigation, it is challenging to separately identify the trends before a second investigation, given the effects of the first investigation. Second, even if the outcome of the first investigation is negative and no TTB is raised, firms' strategic behavior might be altered for a long period, as the likelihood of another TTB petition in the future increases with the number of previous filings (Blonigen, 2006). Our baseline sample of TTBs is characterized by 566 investigations, 916 targeted goods, and 3,460 targeted varieties.

Table 1 reports the investigation and variety counts at the targeted country level. The odd columns refer to the full sample of TTB investigations, and the even columns refer to our baseline sample. The emerging cross-country facts are broadly consistent with the findings in Bown (2011). For

[8] Online Appendix A describes our procedure to aggregate the TTB database at this level.

one, the primary target of U.S. TTBs is China, which was targeted by more than three times as many investigations as the second-place country, South Korea. This difference is slightly smaller in terms of targeted varieties, but China's share is still around twice as large as South Korea's. Other countries frequently targeted by U.S. TTBs are Japan, India, Taiwan, Mexico, and Canada. Note also that more than half of all investigations were ruled affirmatively. Table 2 reports the same moments at the HS section level. Again, our sample reproduces the well-known fact that base metals and metal goods (steel and aluminum) have been the most active sectors seeking protection through TTBs. In effect, more than half of all investigations involved base metals. Not surprisingly, these metals also drive the difference between the full sample and our baseline sample since it is in this sector that varieties are targeted more than once throughout the sample period.

The baseline sample also reproduces well-known facts about the duration and size of duties in TTBs ruled affirmatively. Figure 1 plots the distribution over the investigation and implementation duration of TTBs. The investigation duration is calculated as the final decision date minus the initiation date, and the implementation duration is calculated as the revocation date minus the finalization date of the investigation. If no revocation date is reported, the end of the sample period is used as the finalization date. Note that the implementation duration refers to investigations ruled affirmatively. Panel (a) shows that more than 95 percent of all investigations are decided within the first six quarters after their initiation. Panel (b) shows that the median duration of TTBs is 20 quarters or five years, coinciding with the 5-year sunset review mandated by the WTO. However, it is noticeable that many TTBs appear to stay in place for much longer.

Figure 2 plots the distribution of ad valorem duties specified by the TTB ruling.[9] The level of duties imposed by TTBs is large and strongly right-skewed, with a median of around 25 percent and an average of about 60 percent. The high average TTB duty is largely driven by investigations targeting China, for which the median is a striking 165 percent. In comparison, the overall median is 24 percent (see the last two columns of Table 1). As discussed in Felbermayr and Sandkamp

[9] Since the 1990s, almost all U.S. investigations have imposed ad valorem duties. We omit specific duties and quotas in any analysis that considers the size of the TTB. One concern with using these duties is that they may vary across exporting firms and over time due to revisions required by those firms. Because we do not observe either of those two variables, we abstract from these variations and focus on the duties reported in Bown (2022).

(2020), China's non-market economy status under U.S. trade laws implies dumping and injury margin calculation methodologies that result in higher levied duties.

3. Empirical strategy

We estimate the trade effects of TTB investigations on named or investigated countries and countries that were not named (Prusa, 1997, 2001). The former response is expected to be negative and is referred to as the *trade destruction* effect of the TTB. In contrast, the latter is expected to be positive and is referred to as the *trade diversion* effect (Bown and Crowley, 2007). Our identification of the trade effects follows a standard difference-in-difference (DiD) approach. The first difference considers trade flows before and after the investigation, and the second compares targeted and non-targeted varieties from the same sector.[10] A critical aspect of this approach is that treated and untreated units have common or parallel trends before the treatment (Angrist and Pischke, 2009). However, given that TTBs are initiated and imposed precisely against varieties surging in volumes and declining prices, this assumption is likely to be compromised. In what follows, we describe three alternative specifications of the DiD approach that differ in their treatment of potential pre-trends.

3.1 Static specification

First, we estimate the static trade effects of TTB investigations using the following two-way fixed effects DiD model:

$$
\begin{aligned}
y_{igt} = {} & \beta_0 \mathbb{1}\{Non - Named_{ig}\}\mathbb{1}\{t > Event_{gt}\} \\
& + \beta_1 \mathbb{1}\{Named_{ig}\}\mathbb{1}\{t > Event_{gt}\} + \alpha_{ig} + \alpha_{st} + \alpha_{is't} + \varepsilon_{igt}, \qquad (1)
\end{aligned}
$$

where the dependent variable, y_{igt}, is the log import value (valued at FOB) (v_{igt}), the log quantity (q_{igt}), or the log unit value (p_{igt}) of exported good g from country i in period t. On the right-hand side, $\mathbb{1}\{Non - Named_{ig}\}$ is one if, at any time in our sample period, good g was targeted by a TTB, but the country i was not named, $\mathbb{1}\{t > Event_{gt}\}$ is one in all periods after the investigation

[10] Similar DiD approaches used to study the effects of TTBs are also found in Lu et al. (2013); McCalman and Hillberry (2016); de Souza and Li (2020), and Felbermayr and Sandkamp (2020).

was initiated,[11] and $\mathbb{1}\{Named_{ig}\}$ is one if at any time in our sample period the variety ig was targeted by a TTB investigation. Note that the event dummies vary at the gt level because investigations are initiated at different periods throughout the sample. Hence, the first term on the right (β_0) estimates the average trade diversion effect, and the second term (β_1) estimates the trade destruction effect. In the baseline we include variety α_{ig}, sector-time α_{st}, and country-sector-time $\alpha_{is't}$ fixed effects. Standard errors are clustered at the ig level where the treatment varies (Abadie et al., 2017).[12]

The specification of the fixed effects controls for various demand, supply, and bilateral factors that are typical in a gravity equation of trade. Variety fixed effects capture good-specific, non-time-varying bilateral trade costs such as distance or common language, along with a non-time varying comparative advantage or level of demand. Sector-time fixed effects account for U.S. demand shocks, multilateral resistance terms, and political economy motives to initiate TTBs (industry concentration, the employment share of the marginal electorate, etc.). The s sector is defined at the HS4 level in the baseline. We use the HS4 sector level to include the variation from investigations ruled at the HS6 level and above. In that sense, given the lack of cross-country variation in the two treatments, using more disaggregated α_{st} fixed effects such as HS10-time fixed effects impedes the identification of separate trade destruction and trade diversion effects and restricts the identification of the net effects of named vs. non-named countries.[13] In addition, the country-sector-time fixed effects account for country-specific supply shocks (e.g., productivity or exchange rate movements) and demand shocks (e.g., preference shocks). In the baseline, we define the s' sector by the 21 HS Sections.[14]

Given these fixed effects, the trade effects of TTB investigations are identified from the trade pattern

[11] Our baseline sample includes investigations that target the same good in different years. Hence, the initiation date of the same good may differ across varieties. We set the initiation date of non-named varieties as the earliest date of all targeted varieties of the same good. All results are robust to restricting the sample to those goods that were targeted by TTB investigations in at most one year.

[12] We also considered estimating the confidence interval as the uniform sup-t band proposed by Montiel Olea and Plagborg-Møller (2019). The baseline results under this approach are reported in Figure B.3.

[13] Out of the 916 HS10 goods affected by TTB investigations in our baseline sample, 94 have a unique HS4 code, and 328 have a unique HS6 code. With $HS4$ fixed effects, for example, the trade effects for named and non-named countries cannot be identified separately for those 94 goods.

[14] Section 5.2 shows that all results are robust to alternative specifications of the fixed effects.

of pre- and post-TTB investigations of targeted and non-targeted varieties within the same sector. While pre-trends that are common within $is't$ and st are controlled by the fixed effects, under the static specification in (1), any systematic growth differences between targeted and non-targeted varieties before the investigations compromise the identification of their trade effects. Moreover, this approach mutes potentially dynamic treatment effects and instead considers the average effect over the specified treatment period.

3.2 Standard event study specification

Second, we estimate the trade effects of TTB investigations under an event study framework. The event study design is ideally suited to capture pre-trends and dynamic effects (MacKinlay, 1997; Freyaldenhoven et al., 2021). In the baseline, we estimate the following specification:

$$\begin{aligned} y_{igt} = & \sum_{n=-24}^{24} \beta_{0,n} \mathbb{1}\{Non-Named_{ig}\} \mathbb{1}\{Event_{gt} = n\} \\ & + \sum_{n=-24}^{24} \beta_{1,n} \mathbb{1}\{Named_{ig}\} \mathbb{1}\{Event_{gt} = n\} + \alpha_{ig} + \alpha_{st} + \alpha_{is't} + \varepsilon_{igt}. \end{aligned} \tag{2}$$

This estimation equation is almost identical to (1). The only difference is that instead of using a single indicator variable for the post-initiation period, an indicator variable for each quarter n before and after the investigation, $\mathbb{1}\{Event_{gt} = n\}$, is used to estimate time-varying trade destruction ($\beta_{1,n}$) and trade diversion ($\beta_{0,n}$) effects. We set the quarter of investigation ($n = 0$) to be the quarter closest to the day the investigation was initiated and estimate the response for periods $n = [-24, 24]$.[15] We bin the beginning and end periods, i.e. $n = -24 \ \forall \ n \leq -24$ and $n = 24 \ \forall \ n \geq 24$. Hence, the end (beginning) coefficients capture the average effect extending six years before (after) the investigation was initiated. We refer to this as the pre- and post-long-run effects, respectively. Finally, we follow the standard convention and set $n = -1$ as the reference period.

Three aspects of this approach are critical. First, an event study allows us to assess the validity of the common or parallel trends assumption. In particular, significant pre-initiation coefficients

[15] The six-year time horizon captures the average year of investigation and five years until the first sunset revision.

$(n < -1)$ reflect distinct trade patterns of the two groups of goods. Second, by fixing the reference period, the estimated trade effects are not subject to biases from averaging the differences between targeted and non-targeted varieties over all pre-initiation periods. Third, assessing per-quarter trade effects, the event study accommodates the potential dynamic effects of TTBs.

3.3 Event study specification with extrapolated pre-trends

Identifying the trade effects of TTB investigations under the standard event study design hinges on the common or parallel trends assumption. As discussed above, this assumption is likely to fail in the context of TTB investigations, given their reactive nature: domestic competitors request the implementation of TTBs against varieties biting into the domestic industry's market share or threatening to do so. Hence, varieties targeted by TTBs will likely be preceded by differential growth patterns. To accommodate for potential pre-trends, our third approach to estimating the trade effects of TTB investigations borrows insights on the treatment of pre-trends from the health economics literature, in which identification challenges from pre-trends are pervasive. In particular, we follow Dobkin et al. (2018) and estimate a linear pre-trend while considering the trade effect relative to the extrapolated (counterfactual) pre-trend.[16] Precisely, we estimate the following equation:

$$\begin{aligned}
y_{igt} =& \sum_{n=0}^{24} \beta_{0,n} \mathbb{1}\{Non-Named_{ig}\}\mathbb{1}\{Event_{gt}=n\} + \sum_{n=0}^{24} \beta_{1,n} \mathbb{1}\{Named_{ig}\}\mathbb{1}\{Event_{gt}=n\} \\
&+ \delta_0 \mathbb{1}\{Non-Named_{ig}\}N_{gt} + \beta_{0,-24}\mathbb{1}\{Non-Named_{ig}\}\mathbb{1}\{Event_{gt}=-24\} \\
&+ \delta_1 \mathbb{1}\{Named_{ig}\}N_{gt} + \beta_{1,-24}\mathbb{1}\{Named_{ig}\}\mathbb{1}\{Event_{gt}=-24\} \\
&+ \alpha_{ig} + \alpha_{st} + \alpha_{is't} + \varepsilon_{igt}. \qquad (3)
\end{aligned}$$

Three differences exist to the standard event study specification in (2). First, only the effects for the post-initiation periods, $n = [0, 24]$, are estimated. Second, a linear trend N_{gt} is included that takes the value of the quarterly difference relative to the quarter in which the investigation was initiated and is set to zero for $n \geq 0$ and $N_{gt} = -24 \; \forall n < -24$. Third, the trade response for

[16] This approach is also discussed in Freyaldenhoven et al. (2021).

$n = -24$ is included to avoid the trend being affected by the fact that periods $n < -24$ are binned into $n = -24$.[17] The trade effect of the investigation is then estimated as the deviation between the estimated per-period effect ($\beta_{i,n}$ for $i = 0, 1$) and the accumulated growth from the extrapolated pre-trend in each period ($\delta_i N$).

Under this approach, the identification of trade effects depends on the assumption that the estimated linear pre-trend would have continued without the TTB investigation. In contrast, under the standard event study specification in (2), the identification requires pre-trends to subside once the investigation is initiated. Hence, the two specifications rely on the two extreme assumptions on the unobserved counterfactual growth of targeted relative to non-targeted varieties without the TTB investigation. Naturally, the difference in their respective estimated trade effects will depend on the strength of the estimated pre-trends.

4. Main results

4.1 Trends prior to TTB investigations

The critical difference in the identified trade effects between the three specifications lies in their assumptions about the counterfactual continuation of pre-trends. Hence, before presenting the estimated trade effects of these three specifications, we first focus on the trade patterns before investigations. We document that targeted varieties experience a distinct growth pattern relative to non-targeted varieties from the same country and sector and to the same good imported from non-named countries. While it is known that TTBs are preceded by import surges Bown and Crowley (2013); McCalman and Hillberry (2016), our results show that the trade patterns that lead to TTB investigations are characterized primarily by long-lived and gradual rises rather than only short-lived surges immediately before the investigation.

Figure 3 plots the results of estimating (2) – the standard event study specification. Focusing on the trade pattern before the investigation (period 0 in the graph), Panel (a) shows that named varieties experience a steady import value growth throughout the considered time. On average,

[17] We also estimate the linear pre-trend considering fewer pre-investigation periods. In these cases, we include the event dummies for the periods excluded from the pre-trend term N_{gt}.

the pre-initiation long-run (binned period 24) level of import values for named varieties is -0.7 log points or $100 \times (e^{-0.70} - 1) = 50$ percent below their level in the quarter before the investigation. In the next four years, imports grow relatively stable until seven quarters before the investigation, when within two quarters, they rise from -16 to -1 percent relative to the quarter before the initiation.[18] Panel (b) shows that this trade pattern is the same but slightly larger in magnitude when considering the quantity of imports. The flip side of the larger magnitude is that unit values, shown in Panel (b), experience a steady decline throughout the period before the investigation. The pre-initiation long-run level of unit values is around 16 percent higher than in the quarter before the investigation; again, the decline is rather gradual. While these effects are highly significant, there are no significant pre-trends in the imports of the same goods from non-named countries, as illustrated by the blue lines.

Figure 4 plots the results of the trade effects when estimating a linear pre-trend as specified in (3) and extrapolating it into the post-TTB initiation period. Panels (a), (c), and (d) overlay the estimated and extrapolated linear pre-trend on the estimates of Figure 3. The estimated linear pre-trend lies within the confidence interval of the non-parametric trade pattern before the investigation in all periods, except after $n = -6$, when imports surge significantly away from the trend. The bottom of panels (b), (d), and (e) report the estimated linear pre-trend. Import values of named varieties grow an average of 2.2 percent per quarter, quantities grow at 2.7 percent per quarter, and unit values decline by 0.5 percent per quarter. Even for the case of unit values, for which the non-parametric pre-initiation pattern was more imprecisely estimated, the estimates of pre-trends are highly significant and economically sizeable.

An important consideration is whether these pre-trends can be eliminated by defining a better group of control goods. Our baseline specification of the fixed effects establishes the trade effects of targeted varieties relative to those from the same country-sector-time. Table 3 reports the linear pre-trends estimated by (3) under alternative fixed effects. Column 1 reports the results from the baseline specification. Column 2 relaxes the fixed effects by eliminating the HS4-time fixed

[18] This surge in the two years before the investigation is similar to the findings in Bown and Crowley (2013) and McCalman and Hillberry (2016).

effect, while column 3 implements a tighter definition of the country-sector-time fixed effects by aggregating sector s at the HS4 level. The overall pattern is unchanged in both cases, although the linear pre-trend of named countries drops slightly. Even under the tightest specification of fixed effects in column 3, the pre-trend is 1.7 percent per quarter for import values, 2.3 percent for quantities, and -0.5 percent for unit values.[19] This indicates that using more similar varieties as the control group does not overcome the persistent difference between targeted and non-targeted varieties. Finally, column 4 shows that the net effect of the difference between named and non-named varieties – obtained by specifying the $\alpha_{s't}$ at the HS10 level – is, if anything, larger than the baseline.

The documented trade patterns before TTB investigations challenge identifying their trade effects and underscore the policy endogeneity. Not only do imports of named varieties surge rapidly in the two years before the investigation, but they are also on a distinct long-run growth path. Their growth trend differs from other varieties of the same country-industry and the same variety of non-targeted countries. These findings presage very different trade effects of TTB investigations under the three specifications.

4.2 The trade effects of TTB investigations

We find sizeable, immediate, and persistent trade destruction effects for varieties targeted by TTB investigation upon their initiation. Nevertheless, quantitatively, the results differ largely under the three specifications described in Section 3. Table 4 summarizes the results. Panel (a) presents the results for the log import values. First, we focus on the trade destruction effects on named countries. Under the static specification, the trade destruction of TTB investigations is around -14 percent (column 1). This treatment effect is much smaller than the effect at any horizon under the two dynamic specifications. For example, the 1-year effect under the standard event study approach is three times larger, at -44 percent. The difference is entirely due to the bias from the pre-trends: the static approach averages over the pre- and post-initiation periods, and a glimpse at Figure 3 reveals that this difference is much smaller than the difference between any post-initiation

[19] Our baseline sample uses only U.S. imports. In subSection 4.4, we extend the analysis to bilateral trade to address whether the documented pre-trends are supplier- or supplier-destination-specific.

period and the quarter before the initiation.

The trade effects estimated by the standard event study design are significantly smaller than those estimated when extrapolating the estimated linear pre-trend. Columns 2 to 4 report the 1-, 3- and 5-year trade effects under (2), while columns 5 to 7 report these effects under (3). In both cases, the impact of the TTB initiation is immediate, as imports drop dramatically and their prior trend growth is interrupted. This rapid decline is completed in the third quarter after the investigation was initiated, coinciding with the average completion of the investigation. Afterward, the trade effects under the standard event study persist at around -42 and -45 percent throughout the entire post-initiation period. In contrast, the effects with extrapolated linear pre-trends increase over time as the extrapolated growth accumulates. While the difference between the two estimates is around 8 percentage points after one year (close to 4×2.2 percent growth per quarter), after five years, the extrapolated trade destruction (65 percent) is 50 percent larger than under the standard approach (43 percent). This difference is statistically significant and economically sizeable. Multiple explanations exist for why one would expect named countries to raise their prices following a TTB investigation. One straightforward explanation is that it allows foreign firms to request a downward revision of the levied duty and thereby collect the higher revenues themselves instead of the U.S. government (Prusa, 2001; Blonigen and Haynes, 2002).[20] The results here indicate that using a dynamic specification is critical to identifying the price effects of TTBs.

The import value effects of TTB initiations are driven by both large quantity and unit value effects. The results for the log of import quantities are reported in Panels (b) of Table 4 and Figure 3. The trade destruction effects are qualitatively the same but quantitatively larger. For example, after five years, the trade destruction measured in import quantities is -52 percent under the standard event study design and -75 percent under the one that extrapolates the linear pre-trend. Under the static approach (column 1), again, large pre-trends lead to sizeable downward biases. The specification choice is especially critical when considering the effects on unit values. Under the static approach, there are no significant price effects for named countries, as can be seen in column 1 of Panel (c) in

[20] Other explanations include the possibility that the outcome of a TTB is a negotiated price undertaking (Blonigen and Prusa, 2016; Bown and Crowley, 2016) and dynamic pricing decisions (Blonigen and Park, 2004) and strategic interactions between domestic and foreign competitors (Prusa, 1992; Blonigen and Prusa, 2003).

Table 4. Panel (e) of Figure 3 explains why the price effects are muted under the static approach: the pre-trend and the dynamic response to the investigations are almost mirrored, thus canceling each other out when averaging over the pre- and post-initiation periods. However, the standard event study design reveals significant price effects of the TTB investigations. One year after the investigation was initiated, prices are 9 percent higher than in the quarter before the investigation, while after five years, the difference has increased to 15 percent. Given the downward trend of prices before the investigation, the effects are larger when extrapolating the linear pre-trend. In this case, prices are 13 percent higher after one year than in the quarter prior to the investigation and 28 percent higher after five years.

The large differences in trade destruction effects under the two dynamic specifications raise the key question: What are the causal effects of TTB investigations? The answer lies in how credible the assumption regarding the counterfactual continuation of the pre-trends is. On the one hand, the standard event study assumes that the counterfactual growth of targeted varieties is the same as non-targeted varieties from the same country-sector pairs after the investigation. At the same time, this is clearly violated in the first five of the six years before the TTB investigation. In the year before, the differences are not significant, as estimates of $[\beta_{1,-5}, \beta_{1,-2}]$ are close to zero. In contrast, the approach that estimates and extrapolates the linear pre-trends assumes that these growth differences would have continued (indefinitely) in the absence of the policy. While this assumption is perhaps more sensitive, it also has drawbacks. Most importantly, the size of the linear pre-trend depends on the pre-investigation period length.

To illustrate these points, we estimate modified versions of (3) in which we estimate a linear pre-trend including periods -4, -6, and -12 to the period before the investigation, instead of all periods as in (3). Panel (a) of Table 5 reports the linear pre-trends. Column 1 indicates that the linear pre-trend that considers only the 4 quarters before the investigation is statistically insignificant and negative. However, including quarters -5 and -6 reverses this result, as the trend term becomes positive and significant, indicating the strength of the import surge typically occurring in the second year before the investigation. In column 3, we consider estimates with a pre-trend for the three years before the investigation. In this case, the trend growth is even larger than in the baseline (column 4). Naturally, these differences result from different trade effects when the pre-trends are extrapolated. We report the 5-year trade effects in Panel (b). Under the definition of N_{gt} in column 2, the trade destruction 5 years after the investigation is 59 percent, while in columns 3 and 4, it is 71 and 65, respectively. These results illustrate that neither of the two identifying assumptions are fully credible, and, therefore, we prefer to view the results of the two dynamic specifications as providing the lower and upper bounds of the causal trade effects of TTB initiations. In Table 6 we summarize the trade effects of TTB initiation for import values, quantities, and unit values for named and non-named countries.

Finally, we find relatively smaller trade diversion effects for the same goods targeted by TTB investigations but imported from non-named counties. Again, the effects vary under the three specifications, albeit to a lesser extent. In terms of import values, the static approach yields larger trade diversion effects than the two dynamic specifications, with an average increase of around 9 percent. Under the standard event study specification, Panel (a) of Figure 3 shows that throughout the post-initiation period, the import value of non-named countries increases by an average and long-run value of 4 percent.[21] This effect peaks ten quarters after initiation at around 8 percent. Given the relatively small pre-trend of non-named countries, when the linear pre-trend is extrapolated into the post-initiation period, the evidence of trade diversion is only found in terms of quantities and only in the first few years after the investigation. We find that the three specifications yield a significant drop of around -3 percent in terms of prices. However, this price drop is temporary, subsiding three years after the investigation.

4.3 Pre-trends and trade effects by investigation outcome

In this subsection, we study the pre-trends and trade effects of TTB investigations, distinguishing investigations that were ex-post ruled affirmatively and those that were either settled, withdrawn, or rejected.[22] To do so, we interact the coefficients of interest in (2) and (3) with indicator variables for whether the TTB investigation was ruled affirmatively and, hence, a TTB was implemented. We find that pre-trends are good predictors of the outcome of the ruling, as affirmatively ruled investi-

[21] The average trade effect is calculated as the average over $\{\hat{\beta}_n\}_{n=1}^{24}$ (de Chaisemartin and D'Haultfoeuille, 2021).

[22] Table 1 shows that affirmatively ruled investigations represent slightly more than half of all the investigations and targeted varieties.

gations display more significant growth before the investigation. Affirmatively ruled investigations lead to large and persistent trade destruction that becomes more sizeable when the pre-trends are extrapolated into the implementation period. While we find evidence of trade destruction effects even for non-affirmative investigations, again, the existence of considerable pre-trends leads to quantitatively different trade effects under the two dynamic specifications.

Figure 5 illustrates three main differences in the pre-investigation trade patterns of affirmatively and non-affirmatively ruled investigation. First, the pre-long-run level of import values of affirmatively ruled investigations is significantly lower, at around 60 percent below the level of the quarter before the investigation, than that of non-affirmatively ruled investigations (30 percent). Second, their growth rate is higher, with an estimated linear pre-trend of 2.9 percent per quarter versus 1.2 percent per quarter in the case of non-affirmatively ruled investigations. Third, non-affirmatively ruled investigations do not display a significant surge in the two years immediately before the investigation. Hence, a low initial level, steeper long-run growth, and a relatively large surge in the immediacy of an investigation initiation appear to predict the outcome of an investigation. In contrast, Figure 6 shows no differences in the pre-trends of unit values, as both affirmatively and non-affirmatively ruled investigations experience a gradual decline in prices of around 0.5 percent per quarter.

Table 7 reports the 1-, 3-, and 5-year trade destruction effects under the two specifications for affirmatively and non-affirmatively ruled investigations. Regarding import values (Panel (a)) of affirmatively ruled investigations, the 5-year trade effect is around -60 percent under the standard event study specification and -80 percent when the linear pre-trend is extrapolated. The specification choice is also critical when considering the effects of non-affirmatively ruled investigations. While the trade effects under the standard approach dissipate after they peak in the first year after the investigation was initiated, trade continues to decline over time when the pre-trend is extrapolated. For several reasons, even ex-post non-affirmatively ruled investigations may result in trade destruction. For example, import declines during the investigation period have been attributed to temporary duties and uncertainty regarding the outcome (Prusa, 1992; Staiger and Wolak, 1994), while the trade destruction after non-affirmative rulings has been attributed to voluntary export restraints (VERs) or price undertaking agreements outside of the courts. Panel (c) reports the results for unit values. Both affirmatively and non-affirmatively ruled investigations experience a gradual rise in unit values. While this increase is larger for affirmative investigations, the difference is statistically insignificant.

There are no significant differences in the trade effects of affirmatively and non-affirmatively ruled investigations for non-named countries.[23] Interestingly, import values of non-named countries in affirmatively ruled investigations also display significant growth before the investigation, although this growth is considerably slower at 0.4 percent per quarter. Again, the pre-trends explain why the trade diversion effects under the static specification are overestimated and much smaller than under the standard event study approach.

4.4 Supplier or supplier-destination pre-trends?

Our baseline results above consider U.S. imports only. The main advantage of this approach is that it allows us to use the exact product lines that TTBs targeted over a long period. However, it prevents the introduction of variety-time fixed effects, raising whether the observed pre-trends are supplier- or supplier-destination-specific trade patterns, an essential question from a policy perspective, given that TTB rulings require evidence of the differential behavior of exporters across destination markets.

We extend the analysis to the bilateral trade data to investigate this question. We obtain the data from the Global Trade Atlas (IHS Markit, 2022) and use a balanced sample of 56 countries between 1999 and 2018.[24] Using bilateral trade data restricts the level of aggregation of a good to the HS6 level, as more disaggregated levels are not harmonized across countries. Moreover, for computational purposes, we aggregate the data at the annual level.[25] To address whether the observed trends prior to the imposition of TTBs are due to the supplier or supplier-destination

[23] For non-named countries, we consider the investigation outcome to be affirmative if a TTB was raised against at least one of the investigated countries.

[24] This includes all 22 countries listed in Table 1, except Vietnam and Ukraine. Table B.4 lists the 56 countries.

[25] Using quarterly data increases the sample size by a factor of three. One drawback of using annual data is that the event indicator in the year of the initiation ($n = 0$) is rather imprecise. This pattern may explain why the effects under this dataset are slightly delayed relative to the baseline.

shocks, we estimate the following equation:

$$y_{ijgt} = \sum_{n=-6}^{6} \beta_{1,n} \mathbb{1}\{Named_{ijg}\} \mathbb{1}\{Event_{jgt} = n\} + \alpha_{ijg} + \alpha_{jgt} + \alpha_{igt} + \alpha_{ijt} + \varepsilon_{ijgt}, \tag{4}$$

where j denotes the destination country. Note that because we include source-good-time and destination-good-time fixed effects and use U.S. TTB investigations only, we effectively estimate the net trade effect of the trade destruction and trade diversion caused by TTBs. To disentangle the effect of controlling for exports of targeted varieties to destinations other than the U.S., we compare the estimates of (4) with and without α_{igt}. Figure 7 plots the results. The dashed red line presents the results of (4); the solid blue line presents the results when α_{igt} is excluded from (4). Panel (a) plots the results for import value. Note that the results of "without *igt*" are analogous to the results using U.S. imports and HS10 time fixed.[26] While the responses after the initiations are nearly identical, the pre-trend is slightly steeper "without *igt*", suggesting that part of the pre-trends are due to supply shocks common across destinations. However, the difference is not statistically significant. This pattern is also captured by the estimate of the linear pre-trend when applying the analogous approach of (3). The linear pre-trend "with *igt*' is around three quarters (7.5 percent per year) of the one "without *igt*" (9.7 percent per year). The importance of supplier shocks driving the pre-trends relative to supplier-destination shock is similar when considering quantities and unit values, as reported in panels (b) and (c).

These findings suggest that the economic drivers behind the pre-trends cannot only be explained by common supplier growth shocks, such as productivity increases or relative wage declines. Instead, the pre-trends are mostly specific to the bilateral relationship. Their economic driving forces are thus more subtle and imply a relatively higher U.S. elasticity of the targeted varieties vis-à-vis the elasticity of the rest of the world. Nevertheless, in terms of policy, these findings are consistent with the WTO's requirements to raise TTBs. As intended by the policy, the U.S. is applying TTBs against varieties that experience differential import surges compared to other destination markets.

[26] The results using U.S. imports and HS10 time fixed are reported in columns 3 and 6 of panel (a) in Table B.2. The results are similar to those in this subsection, indicating that neither aggregation at an annual frequency nor product aggregation plays a critical role.

4.5 Good-level trade effects

Our baseline estimates of the trade destruction and trade diversion effects of TTB investigations refer to per-variety averages. Thus, we cannot infer the net effect of TTB investigations on the imports of a certain. To make progress on this matter, we (1) estimate the response of trade flows of named and non-named countries aggregated at the good level, and (2) consider their respective import shares. Panels (a), (c), and (e) of Figure 8 plot the results of using the sample that aggregates trade flows at the good level and for named and non-named countries and the standard event study specification; while Panels (b), (d), and (f) plot the results when the linear pre-trend is extrapolated.[27] Overall, the estimated trade patterns are very similar to our baseline results: The trade destruction effects are slightly smaller than the baseline, and the trade diversion effects are slightly larger. Under the standard event study specification, the average trade destruction throughout the six years after the investigation was initiated is 35 percent, while the trade diversion is 13 percent. At the same time, non-named countries make up for around two-thirds of the import share of the targeted good in the year before the TTB investigation. Therefore, a simple back-of-the-envelope calculation yields per-good trade destruction of 12 percent and a trade diversion effect of 9 percent.[28] This results in a relatively small net per targeted good trade destruction of 3 percent, as indicated in column 3 of Panel (a) of Table 8.

However, the net effects differ when the estimated linear pre-trends are extrapolated. Panel (b) of Table 8 reports the equivalent overall effect under the specification that extrapolated the linear pre-trend. Because of the large pre-trends of named countries vis-à-vis non-named countries, the trade destruction effects considerably outweigh the trade diversion effects. Considering the average over the six years after the investigation was initiated and applying the same back-of-the-envelope calculation as above, the overall effect yields a per good trade destruction of 10 percent instead of 3 percent. This significant difference is present at all time horizons, as indicated by the different

[27] For this purpose, we modify (2) and (3) to include $\mathbb{1}\{Named_{ig}\}\alpha_g$ and $\mathbb{1}\{Named_{ig}\}\alpha_{s't}$ instead of α_{ig} and $\alpha_{is't}$ fixed effects, respectively. We also restrict the sample only to include goods that were targeted in at most one year of our sample period, the sample called "Restricted" in 5.2. The reason is that aggregating over the same varieties targeted at different periods yields biased time fixed effects that determine the control group.

[28] Figure B.2 plots the distribution of the import share of named countries in the year before the investigation. The distribution is right-skewed, with a median of 29 percent and an average of 36 percent.

columns of Table 8.

4.6 Discussion

We now discuss how our results relate to previous work on the trade patterns before and the trade effects of TTB investigations and their importance for welfare analysis. First, to the best of our knowledge, our paper is the first to establish that the trade effects of TTBs strongly depend on the treatment of pre-trends. Critical to this finding is that the pre-trends correspond to gradual, long-run growth differences between targeted and non-targeted varieties and named and non-named countries. Earlier studies on the endogeneity between TTBs and trade have focused on the year or two years before the TTB investigation, thus abstracting from the long-run dynamics and referring to the trade shocks leading to TTBs as sudden import surges (Bown and Crowley, 2007; McCalman and Hillberry, 2016). Our paper reveals that while these surges may exist, they are preceded by sizeable long-run trend differentials that challenge the identification of the policy's trade effects.

The treatment of the counterfactual continuation of these pre-trends has important policy implications. Firstly, under the standard event study specification, the trade effects are quite similar to the literature: According to the literature review of Blonigen and Prusa (2016) the trade destruction effects of TTBs in terms of import values range between 25 to 40 percent under different methods and samples.[29] In contrast, when extrapolating the estimated linear pre-trend we find trade destruction effects of 64 percent after 5 years or 68 percent in the long run, a sizeable difference relative to previous estimates. Secondly, the specification choice also has important implications for the net per-good trade effects: Under the extrapolated approach, the consensus that import increases from non-targeted countries neutralize trade destruction of TTBs no longer holds, and the intended policy goal to protect the domestic industry is achieved after all.

Our dynamic estimates of the trade effects of TTBs provide another essential and new insight: Despite their intended temporary nature, their trade destruction effects are strikingly persistent. While earlier work on the trade effects of TTBs has considered their effects after up to three years, we find that trade remains persistently below its level prior to the investigation even after more

[29] See for example Staiger and Wolak (1994); Prusa (2001); Konings et al. (2002); Carter and Gunning-Trant (2010).

than six years; in fact, when extrapolating the pre-trends the trade destruction grows significantly over time.[30] These long-lasting effects of TTBs call for further investigation.[31] Our findings also elucidate why previous evidence on the price effects of TTBs has been rather ambiguous. The comparison between the effects under static specification with the two dynamic specifications reconciles the positive, and sometimes large, price effects using dynamic panel methods (Prusa, 2001; Blonigen and Haynes, 2002; Blonigen and Park, 2004) and the lack of evidence in support of price effects obtained under static specifications (Lu et al., 2013; Nita and Zanardi, 2013; Felbermayr and Sandkamp, 2020).

Finally, the dynamic patterns we find have important implications for quantitative work analyzing the welfare effects of TTBs. Naturally, matching the lower or upper bound of our estimated range of the trade effects would have quantitatively very different implications in any quantitative trade model. But more importantly, our findings suggest that the shock process that leads to a TTB cannot be modeled as transitory but instead requires a trend component, such as a differential productivity growth term, to capture the gradual pre-investigation growth documented for targeted varieties. The neglect of this aspect suggests that early work on the welfare losses from TTBs are largely underestimated (Gallaway et al., 1999). In this paper, we refrain from any welfare calculation because we lack an off-the-shelf dynamic trade model capable of capturing the rich dynamics before and after the TTB investigations.

5. Heterogeneity and robustness

5.1 Heterogeneity

Differences before and after 2003 — Our baseline results correspond to a weighted average of the trade effects of all TTB investigations between 1993 and 2015. An interesting question is whether

[30] We have also considered whether the post-long-run effects are different for TTBs that were later revoked; however, we did not find any significant differences.

[31] Recently, Cox (2021) attributes the persistent effect of the 2002 U.S. temporary steel tariffs to the disruption of costly buyer-seller relationships. Another possible explanation is that despite their removal, the introduction of a TTB measure leads to sustained long-run uncertainty that dampens trade even when the actual barriers are removed (Alessandria et al., 2021).

the trade patterns surrounding the initiation of TTBs have changed over time.[32] For example, Bown (2011) documents that the use of TTBs in the United States and the world has risen over the last two decades, that China and developing countries more generally, rather than other developed economies, have become more frequent targeted countries, and that the level of duties imposed by TTBs has increased. To investigate whether there are any changes in the trade patterns surrounding TTB investigations over our sample period, we distinguish the effects of TTB investigations initiated before and after 2003, the middle of our sample period. Figure 9 reports the result for import values. While the two trade patterns after the investigation are almost indistinguishable, there are some subtle differences in the trade patterns before the investigations. First, the pre-trend growth of varieties targeted by investigations before 2003 is larger, at 2.5 percent per quarter, than after 2003, at 1.9 percent. Second, the pre-trend of investigations initiated after 2003 appears more linear than those initiated before 2003. In effect, the surge between quarters 7 and 5 before the investigation found in the baseline is entirely due to investigations initiated before 2003. These findings suggest that there may have been a relaxation in the requirements to implement TTBs.

Discrimination against Chinese imports — Another interesting question is whether cross-country pre-trends differences exist. In particular, as documented above, although China only joined the WTO in 2001, it is the most frequently targeted country in our sample by a large difference. However, Figure 10 shows that the trade pattern of targeted varieties from China does not look too different from that of other countries. While the pre-trend is slightly larger for China, the difference is statistically insignificant. This pattern suggests that in our sample period, there is no evidence of discrimination in the criteria applied for petitioning and implementing TTBs against China. The larger trade effects experienced by targeted varieties from China may be due to the sizeable differential in the duties imposed on China (See Table 1).

Singularity of base metals — Finally, we examine whether there are any significant differences between the base metal sector, the most frequent user of TTBs in the U.S., and the rest. Figure 11 plots the results when distinguishing the base metals sector from other sectors. Qualitatively, the

[32] In the language of the theoretical DiD literature, the policy is staggered, and treatment effects may differ over time (Goodman-Bacon, 2021; Athey and Imbens, 2022).

trade pattern before TTB investigations of the base metals varieties is quite similar to that of other varieties. However, the linear pre-trend is significantly steeper in the case of base metals at 2.6 percent growth per quarter, compared to 1.5 percent in other sectors. Moreover, the import surge in the two years before the investigation appears to be driven mainly by the base metals sector. In terms of trade effects, the trade destruction is considerably larger in the base metals sector. This pattern can only partially be explained by the slightly larger fraction of affirmatively ruled investigations (64 percent of base metals investigations versus 53 percent of others) since affirmatively ruled base metal investigations generally receive lower duties. Interestingly, no significant trade diversion effects exist in the base metals sector, indicating that non-base metal sectors drive the average trade diversion effects.

5.2 Robustness

Fixed effects — The baseline control group measuring the trade effects for varieties from non-named and named countries are varieties from the same HS section. Table 3 establishes that the linear pre-trends that drive the difference between the three estimation methods of the trade effects are virtually unchanged when we consider alternative fixed effects specifications. Hence, it is no surprise that the differences in the trade effects under the three specifications are also present when we consider these alternative fixed effects. In particular, Table B.1 reports the average effect under the static specification, and Tables B.2 and B.3 report the 1- and 5-year trade effects under (2) and (3) when we relax and tighten the sector-country-time fixed effects, and when we consider the net effects of the trade destruction and trade diversion effects.[33] Indeed, the trade effects are very similar under the four fixed effects specifications. Regarding import values, removing the HS4-time fixed effects leaves the trade effects virtually unchanged. Tightening the country-sector-time fixed effects to the HS4 level reduces the trade effects slightly; however, they are statistically still indistinguishable from the baseline. And despite the slightly smaller linear pre-trend, the 5-year effect is still almost an order of magnitude larger under the extrapolated pre-trend approach than under the standard event study approach. Again, the net effects, if anything, tend to be more sizeable than the baseline trade effects, as they capture both the trade destruction and diversion

[33] The figures with the full dynamic response are available upon request from the authors.

effects. The direction of change due to the alternative fixed effects is very similar when considering quantities and unit values.

Sample choice — Our baseline sample eliminates varieties targeted by a TTB investigation more than once throughout our sample period. This choice allows for a clean interpretation of the pre- and post-treatment periods. Nevertheless, the results do not hinge on this sample design. Under two alternative samples, we estimate (2) and (3). First, we consider a more restrictive sample in which we exclude all goods targeted by TTBs initiated in different years. This selection allows for a better definition of the event periods of non-named countries, as in our baseline, these are defined by the period in which the first variety of a good was targeted. This restriction also circumvents the fact that filing a TTB petition may act as a trade-dampening effect for varieties of the same good that haven't been targeted, given the increased likelihood of a future filing once one country has been targeted (Blonigen, 2006). The restricted sample includes 343 investigations and 1,205 targeted varieties. Second, we expand the sample to include all varieties targeted by TTBs, even if they were affected by multiple investigations against the same countries in different years. In this case, we set the initiation date of named countries to correspond to the first affirmatively ruled investigation, if there was one.[34] The initiation for the non-named countries is set to be the earliest date a variety has been targeted. The results for import values are reported in Figure B.4. Panels (a) and (b) plot the results under the restricted sample. Qualitatively, the results are very similar to the baseline, but quantitatively, both the pre-trends and the trade effects are slightly smaller. This pattern is perhaps not surprising since the baseline sample includes goods targeted more frequently by TTBs and for which the endogeneity between pre-trends and TTB investigations is presumably stronger. Nevertheless, comparisons between panels (a) and (b) continue to illustrate that (2) and (3) yield dramatically different trade effects. Panels (c) and (d) indicate that extending the sample to all targeted varieties leaves the baseline results virtually unchanged.

Zero trade flows — A possible concern of using quarterly and disaggregated product data is the pervasive existence of zero trade flows and the resulting missing observations of the log-linear

[34] We also considered the case when the initiation date is the first investigation.

estimation approach (Silva and Tenreyro, 2006).[35] To show that our results are robust to this potential concern, we aggregate trade flows to the named and non-named country levels. This aggregation reduces the issue of missing trade flows substantially. While in our baseline sample, including zero trade flows increases the sample size by more than 200 percent, in the aggregated sample, it increases it by less than 5 percent. The result of estimating (2) with the aggregated sample is shown in Figure 8. While in subSection 4.5 we discuss the quantitative differences, here we highlight that the main differences between applying (2) and (3) are the same as in the baseline. In effect, the linear pre-trend in the aggregated sample is only slightly smaller than in the baseline. This pattern suggests that zero trade flows do not play a critical role in the main findings of the paper. We corroborate this by applying the inverse hyperbolic sine (IHS) transformation to the import value. The IHS transformation allows for zero trade flows by considering $\tilde{x} = x/100,000$ and then taking $\log(\tilde{x} + (\tilde{x}^2 + 1)^{0.5})$ (Carroll and Krane, 2003; Ravallion, 2017; Boehm et al., 2020). Figure B.5 shows that the trade patterns using the aggregated sample excluding zeros in panel (a) and the rectangularized sample including zeros in panel (b) are almost identical.

Other trade costs — In our baseline specification of the trade effects of TTB initiations, we omit other trade costs, such as applied tariffs and shipping charges. As a robustness check, we extend (2) and (3) to include the applied ad valorem tariffs – the log of one plus the duty reported in the census trade data over the FOB value – and the shipping charges – the log of one plus the CIF charges over the FOB value. The results are the same as in the baseline and are reported in Figure B.6.

6. Conclusions

This paper studies the trade patterns before and after the initiation of TTB investigations. Focusing on AD and CVD investigations conducted by the U.S. between 1993 and 2015, we uncover several new facts that precede the filings of TTB petitions. First, imports of targeted varieties

[35] While in general, zero trade flows are related to product seasonality and lumpiness in a firm's ordering behavior (Khan and Khederlarian, 2021), in the case of TTBs, zero trade flows might arise precisely because of the policy. In effect, Besedeš and Prusa (2017) estimate that AD increases the likelihood of an exit by more than 50 percent. Nevertheless, the trade effects under the aggregate of named countries are slightly smaller, indicating that this potential downward bias is, if anything, mostly relevant for less important trading partners.

are characterized by surging volumes and falling prices relative to imports from the same country and sector and the same good from non-subject countries. These pre-trends are a long time in the making and are well proxied by linear growth trends. Second, these trends are larger for investigations that are ex-post ruled affirmatively, as expected by the required proof of material injury to the domestic industry. Third, these trends can only partially be explained by common exporter supply shocks across destinations but are rather specific to exports to the U.S. While this is again consistent with the requirements to raise TTBs stipulated by the WTO, it nevertheless implies that more subtle economic forces drive the trends, such as U.S.-specific market trends. Finally, we provide some suggestive evidence that the requirements to raise TTBs may have been relaxed over the last half of our sample period and that China was not treated differently regarding observed import surges leading to the filings.

The strength and shape of the documented pre-trends challenges identifying the trade effects of TTBs. First, they imply that static frameworks are inadequate due to the downward biases from averaging over the pre- and post-initiation periods. Second, and most importantly, they underscore the importance of the assumption about their counterfactual continuation into the post-initiation periods. The trade effects estimated under a standard event study approach that assumes common or parallel pre-trends are mostly consistent with previous work, with imports dropping by 62 percent in investigations ruled affirmatively and 13 percent when ruled non-affirmatively (Prusa, 1997, 2001; Carter and Gunning-Trant, 2010). However, if instead the estimated linear pre-trend is extrapolated into the post-initiation period, the trade destruction effects are -80 and -33 percent, respectively. This difference also matters for the overall evaluation of the policy: only under the latter approach does the trade destruction considerably outweigh the trade diversion effects, thus challenging the common view that TTBs tend to merely induce substitution across trading partners (Blonigen and Prusa, 2016).

In this paper, we provide estimates of the trade effects under two extreme assumptions: either pre-trends are completely interrupted or continue at the same (linear) pace throughout. However, we do not attempt to distinguish their relative merits and shortcomings here. While assuming their complete interruption is dubious, assuming they continue linearly throughout is similarly problematic. On the one hand, the rather rapid surge in the second year before the filing is steeper than the overall linear pre-trend. On the other hand, a concave functional form of the trend may fit the non-parametric trade pattern just as well. Future research using methods such as those in Abadie et al. (2010); Arkhangelsky et al. (2021) may be able to provide more credible post-initiation counterfactual trade patterns by estimating control groups that fit the pre-trends of targeted varieties (Khederlarian and Steinbach, 2022).

Finally, we believe that the results of this paper are essential to consider when applying general equilibrium models to assess the welfare losses of TTBs (Gallaway et al., 1999). Given the strong pre-trends documented here, static frameworks will likely underestimate the welfare losses. This bias arises because varieties targeted by TTBs undergo rapid import growth and price declines before the investigation. In that sense, modeling the investigated variety as experiencing productivity growth, as in Ruhl (2014), provides a promising path to capture the effects documented here and, thus, a realistic assessment of the true welfare implications of TTBs.

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Tables and Figures

Table 1: TTBs by Investigated Country

	Cases (Count)				Variety (Count)				Duties	
	All		Affirmative		All		Affirmative			
	Full	Baseline	Full	Baseline	Full	Baseline	Full	Baseline	Median	Std. Dev.
China	153	116	109	90	899	449	527	300	165	97
South Korea	51	42	27	25	410	234	246	144	13	10
Japan	45	35	26	19	434	224	263	131	38	22
India	38	34	20	19	316	143	189	90	19	28
Taiwan	33	25	22	17	311	194	183	153	10	11
Mexico	30	27	16	15	158	147	108	100	24	12
Canada	24	20	8	7	94	72	34	26	19	5
Brazil	23	18	14	13	280	90	159	61	27	37
Germany	20	13	8	7	175	111	61	52	26	26
Thailand	20	16	6	6	181	77	37	27	4	7
Indonesia	20	19	11	11	165	120	93	54	33	26
South Africa	19	15	6	6	281	119	54	41	38	37
Russia	17	12	7	5	222	63	67	12	79	79
Turkey	17	11	9	8	238	77	100	59	6	14
Italy	14	12	11	10	130	112	116	101	13	14
France	13	12	5	5	165	147	49	40	11	4
Vietnam	13	10	9	9	98	56	59	53	111	104
Venezuela	12	6	1	0	136	32	2	0		
Malaysia	11	9	5	4	38	34	19	17	3	20
Ukraine	10	10	8	8	108	54	86	51	90	24
Spain	10	6	4	4	115	55	7	7	25	8
Others	113	96	48	44	1278	850	519	387	54	21
Total	706	564	380	332	6232	3460	2978	1906	46	26

Note. Data is from Bown (2022). The construction of both samples is described in Section 2 and Online Appendix A.

Table 2: TTBs by Sector

		Cases (Count)				Variety (Count)					
		All		Affirmative		All		Affirmative		Duties	
	HS Section	Full	Baseline	Full	Baseline	Full	Baseline	Full	Baseline	Median	Std. Dev.
15	Base Metals	398	305	223	195	5118	2552	2454	1481	24	76
6	Chemical Products	98	85	49	42	230	203	93	79	39	84
7	Plastic and Rubber Products	54	44	30	22	156	144	41	31	77	78
16	Machinery and Electrical Equipments	47	38	28	23	202	153	88	66	38	83
4	Prepared Foodstuff	35	23	16	15	66	41	28	25	25	82
1	Animal Products	26	14	5	4	48	26	8	7	64	86
10	Paper Products	22	20	18	18	165	125	149	115	113	94
20	Miscellaneous Manufactures	22	18	10	9	34	24	21	15	121	93
2	Vegetable Products	18	17	7	6	30	28	11	9	11	179
11	Textile Products	14	14	9	9	88	88	50	50	67	117
9	Wood Products	13	8	5	4	23	15	15	11	19	12
17	Vehicles	9	7	2	1	44	33	5	2	384	0
5	Mineral Products	8	8	1	1	12	12	1	1	215	0
13	Stone, ceramic and glass products	5	5	4	4	14	14	12	12	161	87
18	Optimal, photographic, medical, etc.	2	2	2	2	2	2	2	2	61	32
	Total	771	608	409	355	6232	3460	2978	1906	95	73

Note. Data is from Bown (2022). The construction of the full sample is described in Section 2 and Online Appendix A.

Table 3: Linear Pre-trends - Alternative Fixed Effects

	(1)	(2)	(3)	(4)
Panel (a): Linear Pre-Trend, Import Value				
Named	0.022***	0.018***	0.017***	0.022***
	(0.002)	(0.002)	(0.003)	(0.002)
Non-Named	0.002***	0.01	0.002*	
	(0.001)	(0.01)	(0.001)	
Observations	14,447,282	14,448,905	12,917,156	14,333,830
Adjusted R-Squared	0.730	0.722	0.743	0.744
Panel (b): Linear Pre-Trend, Quantity				
Named	0.027***	0.026***	0.023***	0.028***
	(0.003)	(0.002)	(0.004)	(0.003)
Non-Named	0.002	0.001	0.000	
	(0.001)	(0.001)	(0.001)	
Observations	12,464,085	12,465,829	11,037,877	12,351,695
Adjusted R-Squared	0.797	0.791	0.809	0.804
Panel (c): Linear Pre-Trend, Unit Value				
Named	-0.005***	-0.008***	-0.005***	-0.005***
	(0.001)	(0.001)	(0.002)	(0.001)
Non-Named	0.001	-0.000	0.001	
	(0.001)	(0.001)	(0.001)	
Observations	12,464,085	12,465,829	11,037,877	12,351,695
Adjusted R-Squared	0.850	0.848	0.861	0.852
ig FE	✓	✓	✓	✓
ist FE	HS Section	HS Section	HS-4	HS Section
$s't$ FE	HS-4			HS-10

Note. Estimates in all panels report the estimated linear pre-trend from (2), using different fixed effects as specified at the bottom of the table and the dependent variable specified by the panel. For presentation purposes, we omit all other parameter estimates. Standard errors in parentheses are clustered at the ig level, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: Trade Effects - Baseline

	Static	Event Study			Extrapolated Pre-Trend		
	Avg (1)	1-Year (2)	3-Years (3)	5-Years (4)	1-Year (5)	3-Years (6)	5-Years (7)
Panel (a): Import Value							
Named	-0.149***	-0.585***	-0.558***	-0.562***	-0.723***	-0.867***	-1.044***
	(0.037)	(0.045)	(0.049)	(0.054)	(0.047)	(0.061)	(0.077)
Non-Named	0.087***	0.047***	0.050**	0.015	0.008	00.002	-0.045
	(0.014)	(0.018)	(0.020)	(0.022)	(0.018)	(0.024)	(0.030)
Observations	14,447,282		14,447,282			14,447,282	
Adjusted R-Squared	0.730		0.730			0.730	
Panel (b): Quantity							
Named	-0.166***	-0.710***	-0.653***	-0.726***	-0.950***	-1.112***	-1.401***
	(0.046)	(0.056)	(0.061)	(0.068)	(0.065)	(0.082)	(0.102)
Non-Named	0.128***	0.083***	0.092***	0.022	0.056*	0.052	-0.030
	(0.020)	(0.029)	(0.031)	(0.034)	(0.030)	(0.040)	(0.050)
Observations	12,464,085		12,464,085			12,464,085	
Adjusted R-Squared	0.797		0.797			0.797	
Panel (c): Unit Value							
Named	0.009	0.088***	0.057**	0.137***	0.125***	0.132***	0.251***
	(0.017)	(0.026)	(0.027)	(0.030)	(0.029)	(0.035)	(0.042)
Non-Named	-0.033***	-0.031*	-0.025	-0.002	-0.037**	-0.037	-0.019
	(0.010)	(0.019)	(0.020)	(0.021)	(0.018)	(0.024)	(0.029)
Observations	12,464,085		12,464,085			12,464,085	
Adjusted R-Squared	0.850		0.850			0.850	

Note. Estimates in column 1 correspond to the results of (1), columns 2-4 to (2), and columns 5-7 to (3). All regressions include α_{ig}, and α_{st} $\alpha_{is't}$ fixed effects and all dependent variables are in logs. Columns labeled as "1-Year," "3-Years," and "5-Years" correspond to the trade effects in the 4^{th}, 12^{th}, and 20^{th} quarter after the TTB investigation was initiated, respectively. In columns 5-7 they are calculated as $\widehat{\beta}_{i,t} - \widehat{\delta}_i t$, for $t = 4, 12, 20$. Standard errors in parentheses are clustered at the ig level, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: Results with Varying Pre-Trend Length

Quarters included in N_{gt}	-4 to -1	-6 to -1	-12 to -1	-24+ to -1
Panel (a): Linear Pre-Trend				
Named	-0.014	0.014**	0.030***	0.022***
	(0.011)	(0.007)	(0.003)	(0.002)
Non-Named	-0.012 **	-0.002	0.001	0.002**
	(0.006)	(0.003)	(0.002)	(0.001)
Panel (b): 5-Year Extrapolated Trade Effect				
Named	-0.278	-0.898***	-1.251***	-1.047***
	(0.258)	(0.165)	(0.101)	(0.077)
Non-Named	0.264***	0.051	-0.018	-0.051
	(0.128)	(0.080)	(0.044)	(0.031)
Observations	14,447,282			
Adjusted R-Squared	0.730			

Note. Estimates in columns 1-3 correspond to the results of modifying (3) by defining the pre-trend term N_{gt} to include quarters -4 to -1, -6 to -1, and -12 to -1, respectively. In addition, the event dummies are included from -24+ to -5, -24+ to -7, and -24+ to -13. Column 4 plots the results of (3). Panel (a) reports the estimates of the coefficient on the linear pre-trend term N_{gt} and panel (b) the 5-year extrapolated trade effects, i.e. $\widehat{\beta}_{i,20} - \widehat{\delta}_i 20$. Standard errors in parentheses are clustered at the ig level, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 6: Summary of Trade Effects

	Named		Non-Named	
	Event (1)	Extrapolated (2)	Event (3)	Extrapolated (4)
Panel (a): Import Value				
Avg 1^{st} Year	-26%	-32%	3%	0%
Avg 1^{st} 3 Years	-38%	-48%	5%	2%
Avg 1^{st} 5 Years	-40%	-54%	4%	0%
Long Run	-43%	-68%	5%	-2%
Panel (b): Quantity				
Avg 1^{st} Year	-31%	-37%	5%	3%
Avg 1^{st} 3 Years	-43%	-54%	9%	6%
Avg 1^{st} 5 Years	-46%	-60%	8%	5%
Long Run	-50%	-75%	10%	4%
Panel (b): Unit Values				
Avg 1^{st} Year	4%	5%	-2%	-2%
Avg 1^{st} 3 Years	7%	10%	-3%	-3%
Avg 1^{st} 5 Years	8%	13%	-3%	-3%
Long Run	12%	27%	-3%	-5%

Note. Estimates in columns 1 and 3 correspond to the results of (2) and estimates in columns 2 and 4 to (3). The trade effects in the first three rows in each panel are calculated as the average over coefficients $[\widehat{\beta}_{i,n}]_{n=0}^{N}$ for $i = 0, 1$ and $N = 3, 11, 19$, transformed into percentage effects. The long-run effect corresponds to the transformed coefficient of $\widehat{\beta}_{i,24+}$ for $i = 0, 1$.

Table 7: Trade Effects - Affirmative versus Non-Affirmative

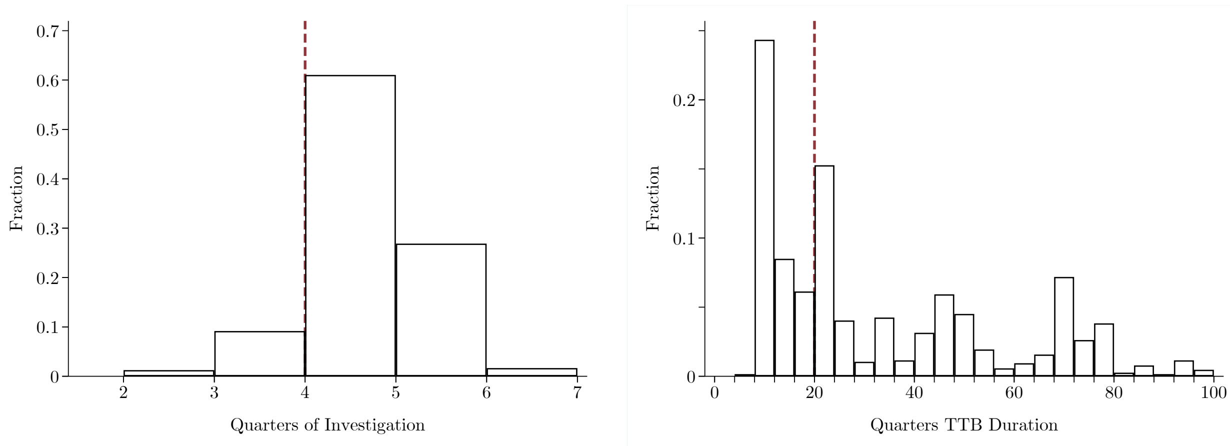
	Static	Event Study			Extrapolated Pre-Trend		
	Avg	1-Year	3-Years	5-Years	1-Year	3-Years	5-Years
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel (a): Import Value							
Affirmative, Named	-0.411***	-0.902***	-0.945***	-0.982***	-1.076***	-1.349***	-1.616***
	(0.052)	(0.064)	(0.071)	(0.081)	(0.068)	(0.088)	(0.112)
Non-Affirmative, Named	0.043	-0.226***	-0.116*	-0.136*	-0.316***	-0.301***	-0.418***
	(0.051)	(0.062)	(0.064)	(0.071)	(0.063)	(0.078)	(0.102)
Affirmative, Non-Named	0.127***	0.054**	0.069**	0.020	0.011	0.010	-0.094**
	(0.018)	(0.025)	(0.027)	(0.029)	(0.024)	(0.032)	(0.041)
Non-Affirmative, Non-Named	0.049**	0.029	0.041	0.048	0.007	0.024	0.037
	(0.020)	(0.026)	(0.030)	(0.031)	(0.026)	(0.036)	(0.046)
Observations	4,761,576		4,761,576			4,761,576	
Adjusted R-Squared	0.723		0.723			0.723	
Panel (b): Quantity							
Affirmative, Named	-0.459***	-1.063***	-1.041***	-1.223***	-1.246***	-1.490***	-1.940***
	(0.063)	(0.077)	(0.085)	(0.100)	(0.083)	(0.107)	(0.135)
Non-Affirmative, Named	0.077	-0.299***	-0.200**	-0.211**	-0.407***	-0.446***	-0.598***
	(0.064)	(0.083)	(0.084)	(0.093)	(0.084)	(0.104)	(0.133)
Affirmative, Non-Named	0.167***	0.089**	0.117***	0.006	0.058*	0.064	-0.073
	(0.025)	(0.037)	(0.040)	(0.043)	(0.036)	(0.047)	(0.059)
Non-Affirmative, Non-Named	0.081**	0.073	0.087*	0.102*	0.080*	0.120**	0.158**
	(0.032)	(0.050)	(0.053)	(0.055)	(0.047)	(0.062)	(0.078)
Observations	4,104,086		4,104,086			4,104,086	
Adjusted R-Squared	0.768		0.769			0.769	
Panel (c): Unit Value							
Affirmative, Named	0.026	0.132***	0.073*	0.210***	0.151***	0.130***	0.306***
	(0.024)	(0.034)	(0.038)	(0.043)	(0.036)	(0.045)	(0.054)
Non-Affirmative, Named	-0.015	0.055	0.058	0.067	0.070*	0.111**	0.160***
	(0.023)	(0.041)	(0.040)	(0.044)	(0.040)	(0.048)	(0.060)
Affirmative, Non-Named	-0.031***	-0.033	-0.037	0.015	-0.044**	-0.058**	-0.016
	(0.012)	(0.023)	(0.024)	(0.026)	(0.021)	(0.027)	(0.034)
Non-Affirmative, Non-Named	-0.029*	-0.013	-0.018	-0.034	-0.030	-0.044	-0.067
	(0.016)	(0.033)	(0.034)	(0.036)	(0.030)	(0.039)	(0.048)
Observations	4,104,086		4,104,086			4,104,086	
Adjusted R-Squared	0.809		0.809			0.809	

Note. Estimates in column 1 correspond to the results of (1), columns 2-4 to (2), and columns 5-7 to (3). All regressions include α_{ig}, and α_{st} $\alpha_{is't}$ fixed effects, and all dependent variables are in logs. Columns labeled as "1-Year," "3-Years," and "5-Years" correspond to the trade effects in the 4^{th}, 12^{th}, and 20^{th} quarter after the TTB investigation was initiated, respectively. Standard errors in parentheses are clustered at the ig level, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 8: Good-Level Trade Effects

	1-Year	5-Year	Average	Long-Run
Panel (a): Event Study				
Trade Destruction	-33%	-38%	-35%	-52%
Trade Diversion	19%	10%	13%	7%
Weighted Sum	2%	-6%	-3%	-13%
Panel (b): Extrapolated Pre-Trend				
Trade Destruction	-41%	-60%	-51%	-71%
Trade Diversion	15%	9%	11%	8%
Weighted Sum	-3%	-14%	-10%	-18%

Note. The column labeled "Average" refers to the average of the event coefficients $\{\hat{\beta}_n\}_{n=1}^{24}$. The rows labeled "Weighted Sum" sum the previous two rows weighted by their import share: one-third for named countries (trade destruction) and two-thirds for non-named countries (trade diversion). See Figure B.2 for the distribution of the import shares of named countries for all targeted varieties.



(a) Investigation Periods **(b) Implementation Periods**

Figure 1: TTB Duration

Note. Panel (a) plots the distribution over the duration of the investigation period. It is calculated as the time difference between the final decision date and the investigation initiation date. For illustration purposes, investigations that took longer than seven quarters are truncated at seven. Panel (b) plots the distribution over the number of quarters TTBs are implemented, calculated as the revocation date minus the finalization of investigation date. If no revocation date is reported, the end of the sample period (2018Q4) is used. The dashed red lines are the median values. All the data are from Bown (2022).

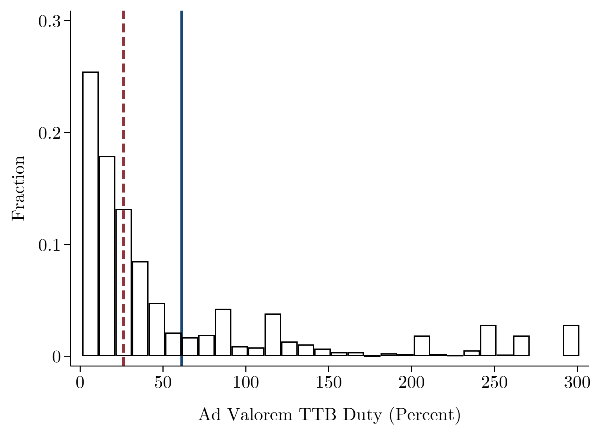
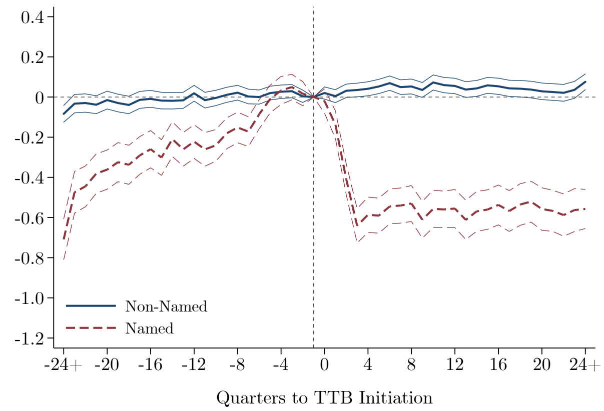


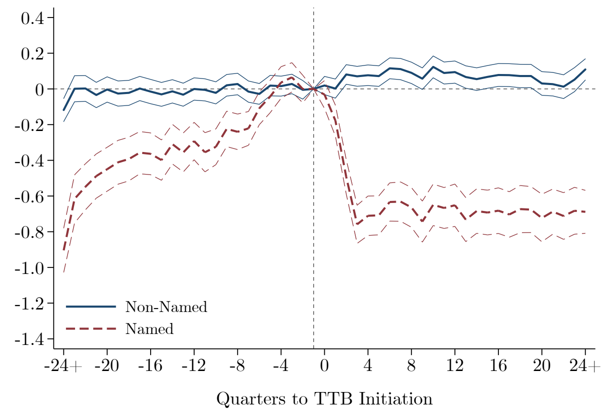
Figure 2: TTB Duties

Note. This figure plots the distribution over the ad valorem duties levied by the TTB investigation. For visualization purposes, duties above 300 percent are binned at 300. The dashed red line is the median value and the solid blue line is the mean. All the data are from Bown (2022).



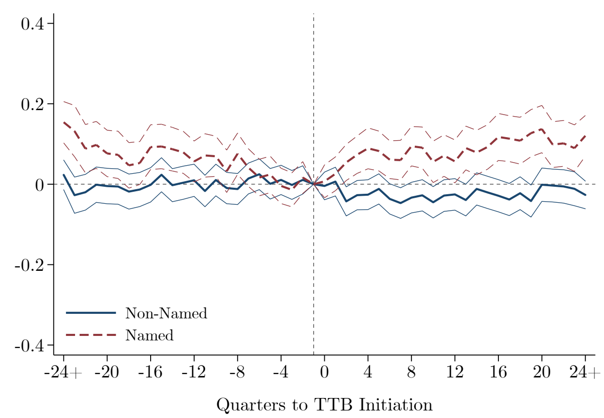
Adj. R-Squared: 0.730 -- Obs.: 14,447,282

(a) Values



Adj. R-Squared: 0.797 -- Obs.: 12,464,085

(b) Quantities

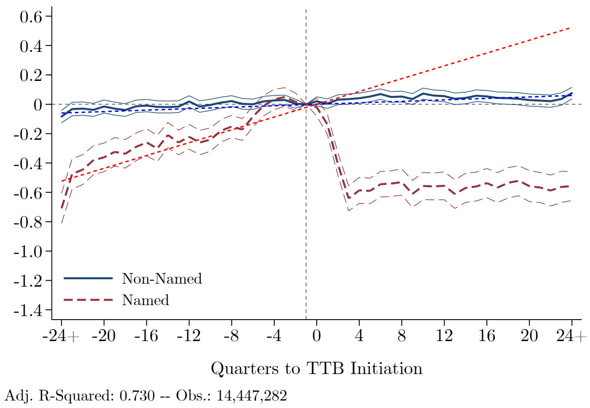


Adj. R-Squared: 0.850 -- Obs.: 12,464,085

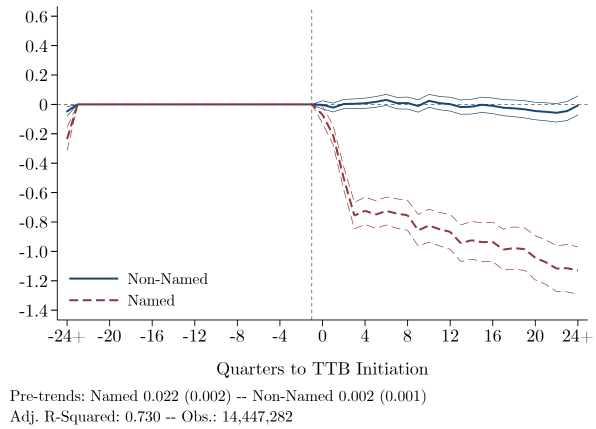
(c) Unit Values

Figure 3: Trade Effects under Event Study

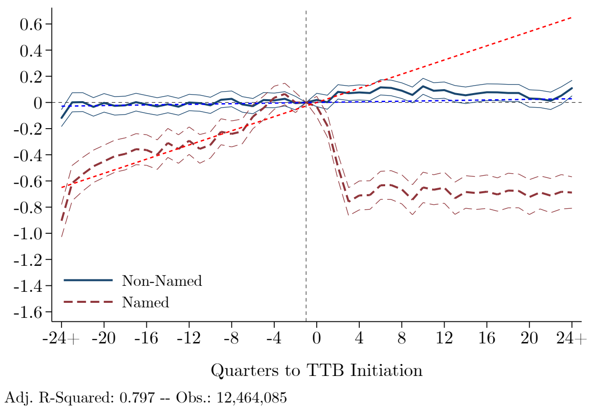
Note. This figure plots the event coefficients estimated by (2) using the log import value as the dependent variable in (a), the log quantity of imports in (b), and the log unit value in (c). Quarters $n < -24$ ($n > 24$) are binned to -24 (24) and $n = -1$ is the reference period. The standard errors that construct the 95 percent confidence intervals are clustered at the ig level.



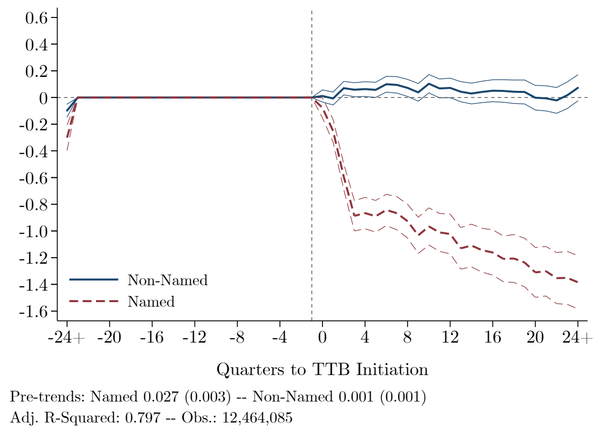
(a) Import Values, Overlaid



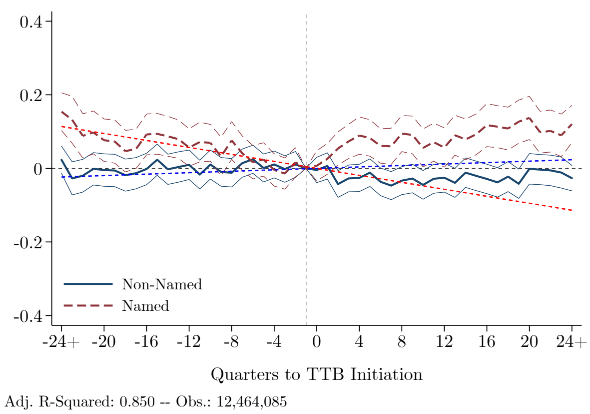
(b) Import Values, Extrapolated



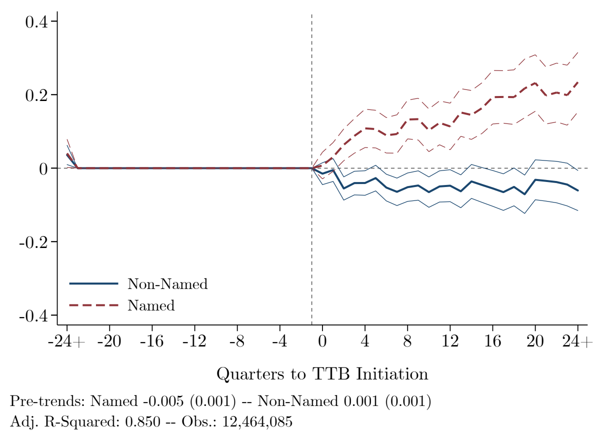
(c) Quantities, Overlaid



(d) Quantities, Extrapolated



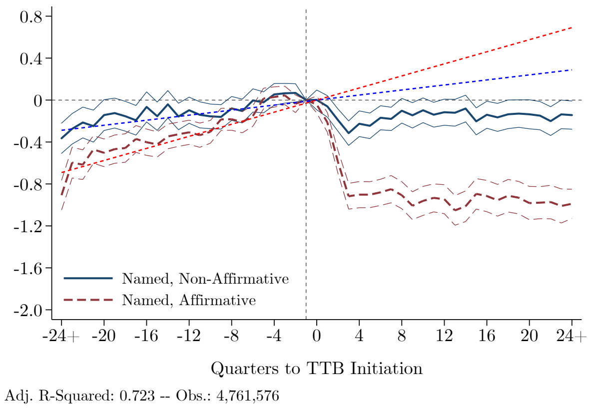
(e) Unit Values, Overlaid



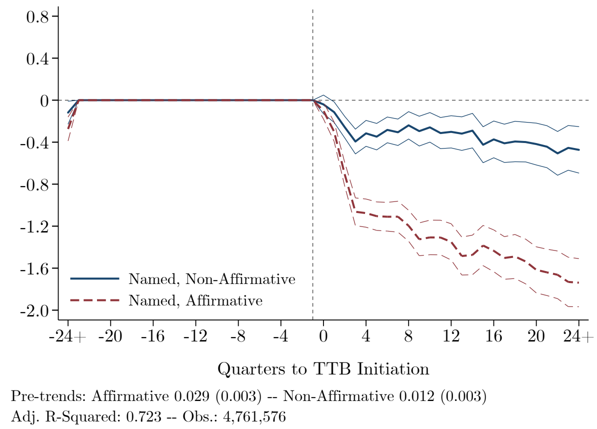
(f) Unit Values, Extrapolated

Figure 4: Trade Effects with Extrapolated Linear Pre-Trends

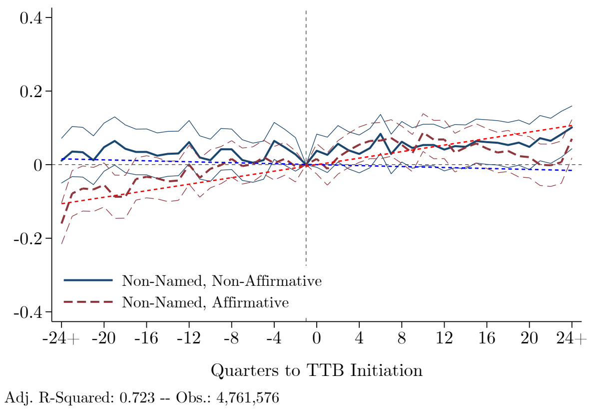
Note. Panels (a), (c), and (d) overlay the linear pre-trends estimated in (3) on the trade effects estimated under the standard event study specification in (2), which were also reported in Figure 3. Panels (b), (c), and (d) plot the trade effects under the event study with extrapolated linear pre-trends in (3), that is $\hat{\beta}_{i,n} - \hat{\delta}_i N$ $i = \{0, 1\}$. The standard errors that construct the 95 percent confidence intervals are clustered at the ig level.



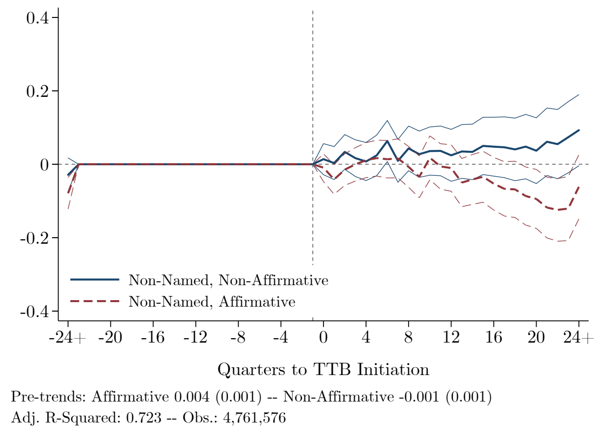
(a) Named, Event Study



(b) Named, Extrapolated



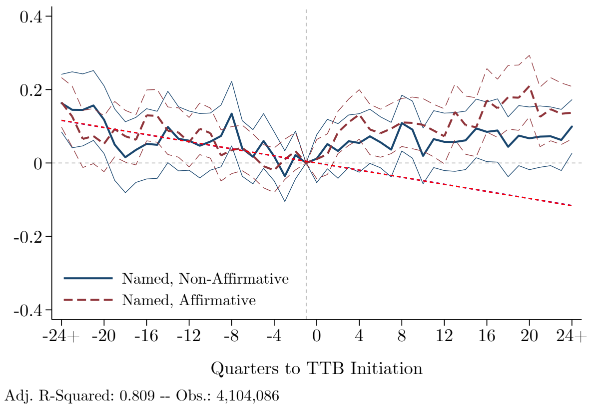
(c) Non-Named, Event Study



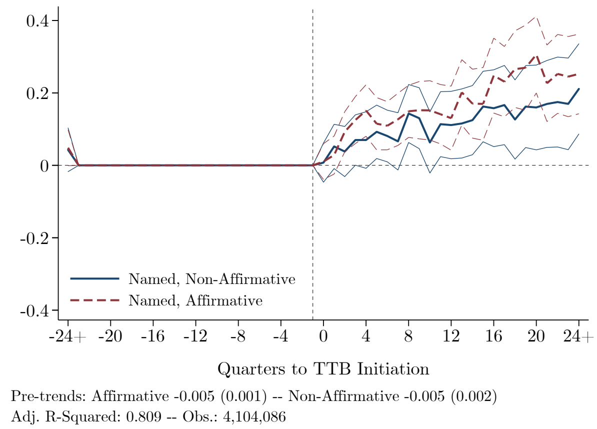
(d) Non-Named, Extrapolated

Figure 5: Import Value Effects by Investigation Outcome

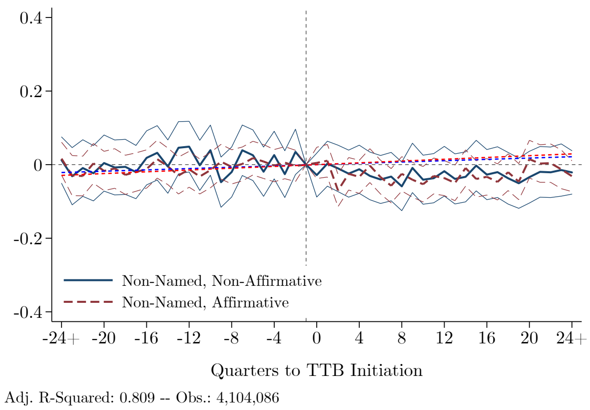
Note. All estimates are obtained by interacting the variables of the coefficients of interest in (2) and (3) with an indicator variable that equals one if the investigation affecting the targeted variety was ruled affirmatively. Panels (a) and (b) plot the results for named countries and panels (c) and (d) do so for non-named countries. The results of panels (a) and (c) are obtained by (2), while the results of panels (b) and (d) correspond to (2) in (3). The linear pre-trend obtained from (3) is overlaid and extrapolated in panels (a) and (c) (dotted lines). The standard errors that construct the 95 percent confidence intervals are clustered at the ig level.



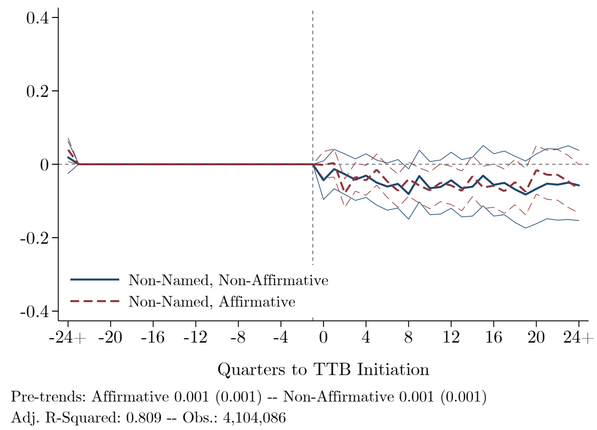
(a) Named, Event Study



(b) Named, Extrapolated



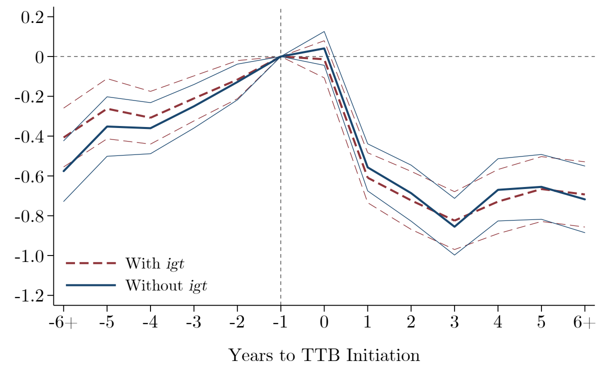
(c) Non-Named, Event Study



(d) Non-Named, Extrapolated

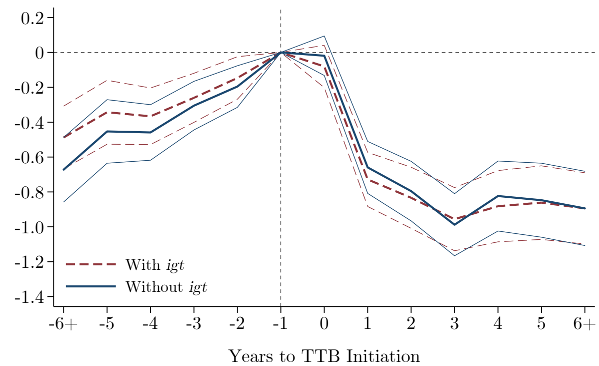
Figure 6: Unit Value Effects by Investigation Outcome

Note. All estimates are obtained by interacting the variables of the coefficients of interest in (2) and (3) with an indicator variable that equals one if the investigation affecting the targeted variety was ruled affirmatively. Panels (a) and (b) plot the results for named countries and panels (c) and (d) do so for non-named countries. The results of panels (a) and (c) are obtained by (2), while the results of panels (b) and (d) correspond to (2) in (3). The linear pre-trend obtained from (3) is overlaid and extrapolated in panels (a) and (c) (dotted lines). The standard errors that construct the 95 percent confidence intervals are clustered at the ig level.



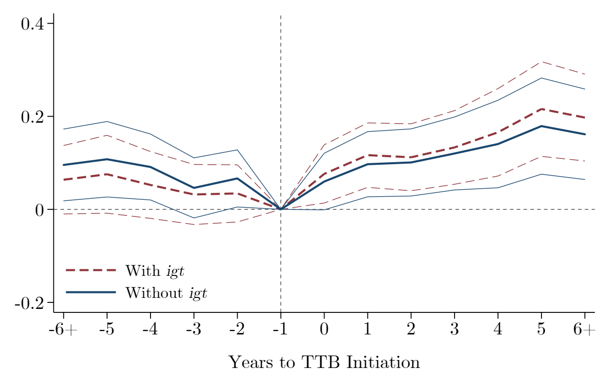
Pre-Trends With *igt*: 0.075 (0.019) -- Without *igt*: 0.097 (0.018)
With *igt*: Adj. R-Squared: 0.786 -- Obs.: 14,687,257
Without *igt*: Adj. R-Squared: 0.776 -- Obs.: 14,714,762

(a) Values



Pre-Trends With *igt*: 0.094 (0.022) -- Without *igt*: 0.121 (0.022)
With *igt*: Adj. R-Squared: 0.866 -- Obs.: 13,423,809
Without *igt*: Adj. R-Squared: 0.860 -- Obs.: 13,454,476

(b) Quantities

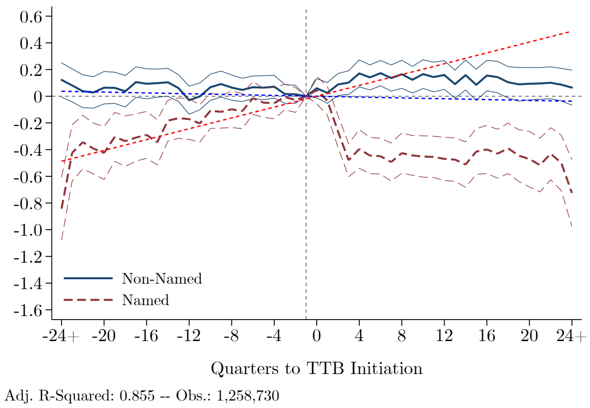


Pre-Trends With *igt*: -0.017 (0.010) -- Without *igt*: -0.024 (0.009)
With *igt*: Adj. R-Squared: 0.925 -- Obs.: 13,421,527
Without *igt*: Adj. R-Squared: 0.923 -- Obs.: 13,452,196

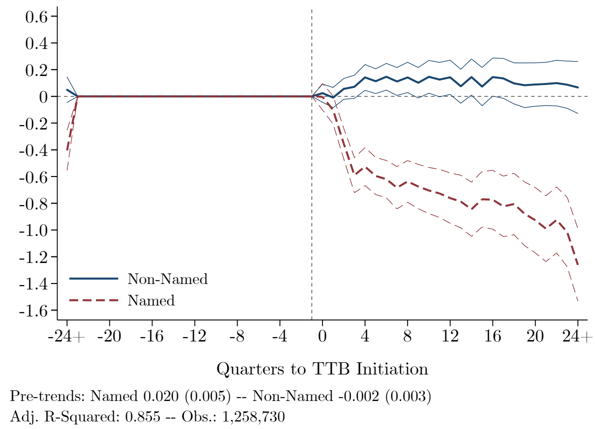
(c) Unit Values

Figure 7: Supplier vs. Supplier-Destination Pre-Trends

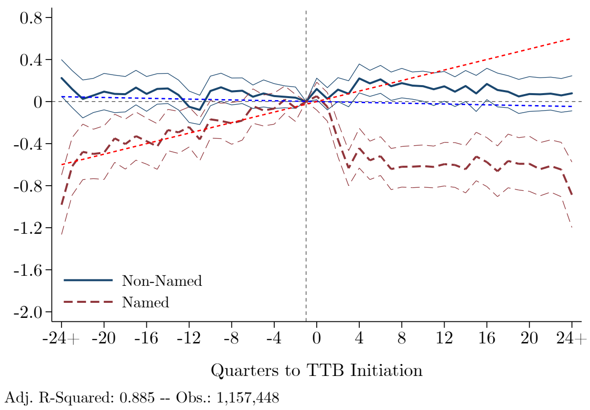
Note. The dashed red line plots the event coefficients estimated by (4). The solid blue line plots the event coefficients when the α_{igt} fixed effect is excluded from (4). Quarters $n < -6$ ($n > 6$) are binned to -6 (6) and $n = -1$ is the reference period. The linear pre-trend reported below the figures is estimated under the analogous approach of (3). The standard errors that construct the 95 percent confidence intervals are clustered at the ijg level.



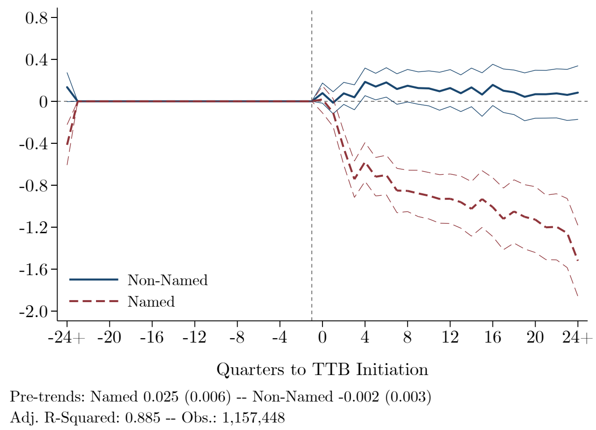
(a) Import Values, Event Study



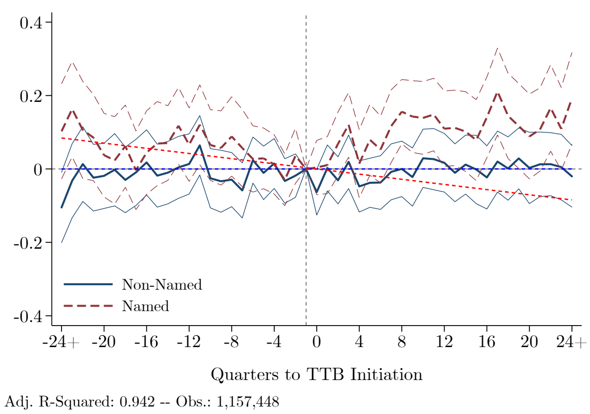
(b) Import Values, Extrapolated



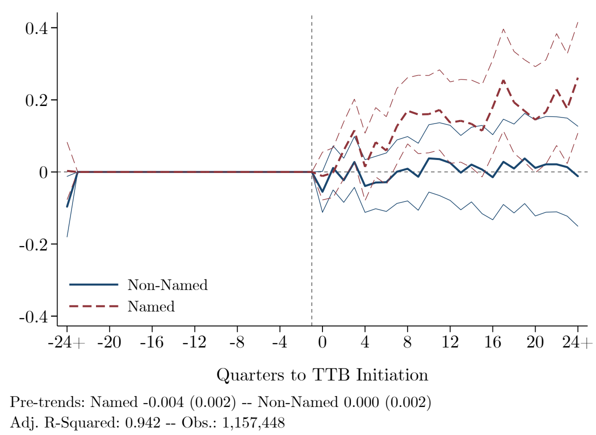
(c) Quantities, Event Study



(d) Quantities, Extrapolated



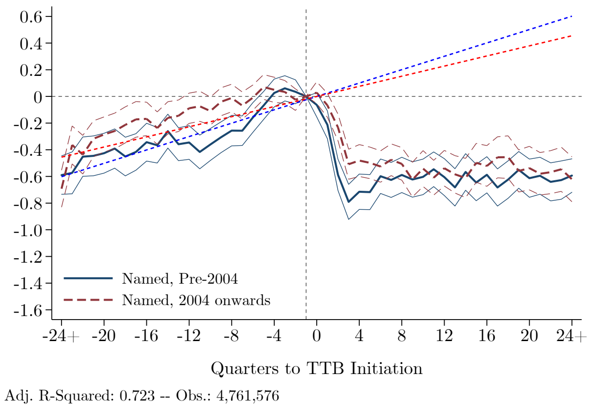
(e) Unit Values, Event Study



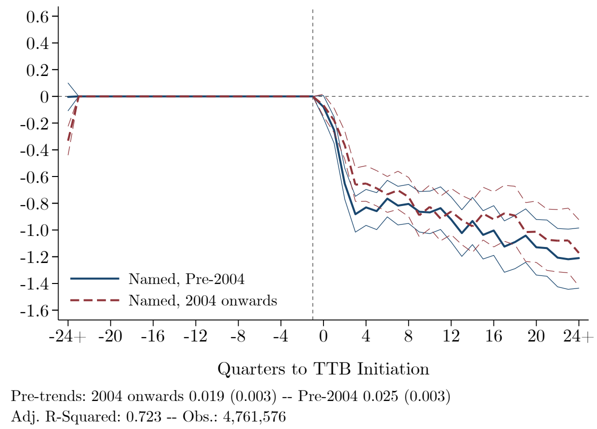
(f) Unit Values, Extrapolated

Figure 8: Good Level Trade Effects

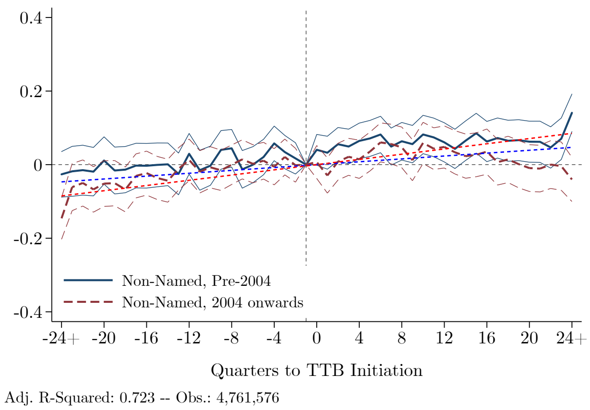
Note. This figure plots the trade effects under specifications (2) and (3) for the sample that aggregates named and non-named countries. Note that instead of ig and ist fixed effects, $\mathbb{1}\{Named\}g$ and $\mathbb{1}\{Named\}st$ fixed effects are used. Panels (a), (c), and (e) correspond to results from (2). Panels (b), (d), and (f) correspond to (3). The linear pre-trend obtained from (3) is overlaid and extrapolated in panels (a), (c), and (e) (dotted lines). The standard errors that construct the 95 percent confidence intervals are clustered at the g level.



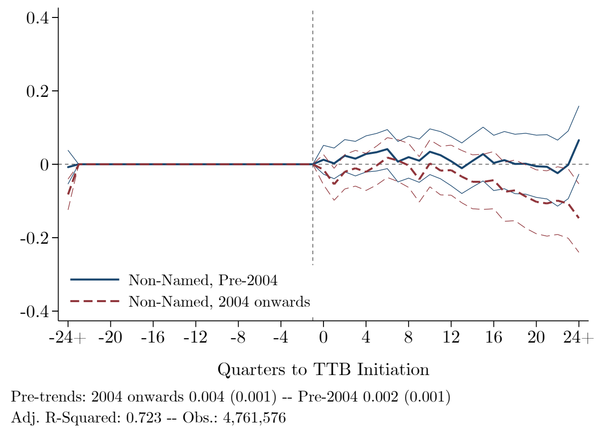
(a) Named, Event Study



(b) Named, Extrapolated



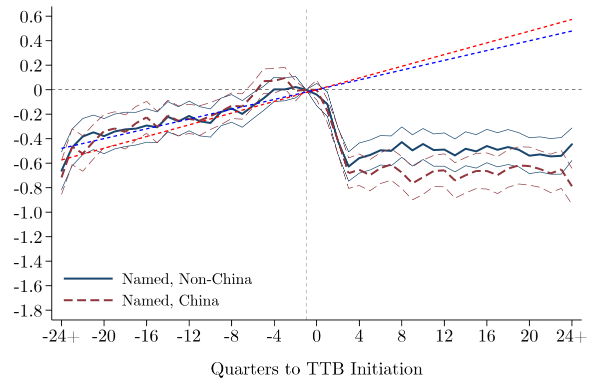
(c) Non-Named, Event Study



(d) Non-Named, Extrapolated

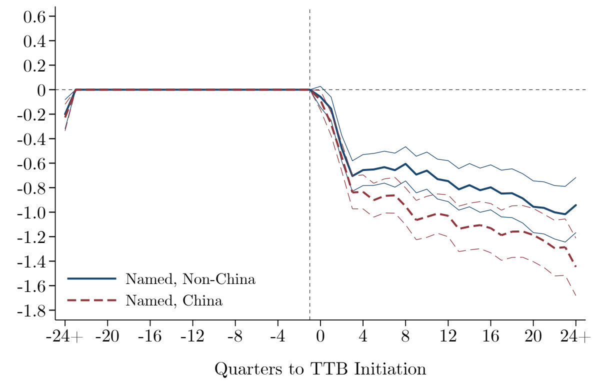
Figure 9: Differences Investigations Pre- & Post 2003

Note. All estimates are obtained by interacting the variables of the coefficients of interest in (2) and (3) with an indicator variable that equals one if the investigation affecting the targeted variety was initiated after 2003. Panels (a) and (b) plot the results for named countries and panels (c) and (d) do so for non-named countries. The results of panels (a) and (c) are obtained by (2), while the results of panels (b) and (d) correspond to (2) in (3). The linear pre-trend obtained from (3) is overlaid and extrapolated in panels (a) and (c) (dotted lines). The standard errors that construct the 95 percent confidence intervals are clustered at the ig level.



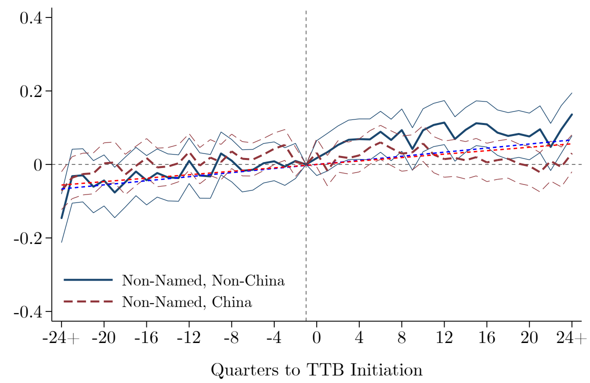
Adj. R-Squared: 0.723 -- Obs.: 4,761,576

(a) Named, Event Study



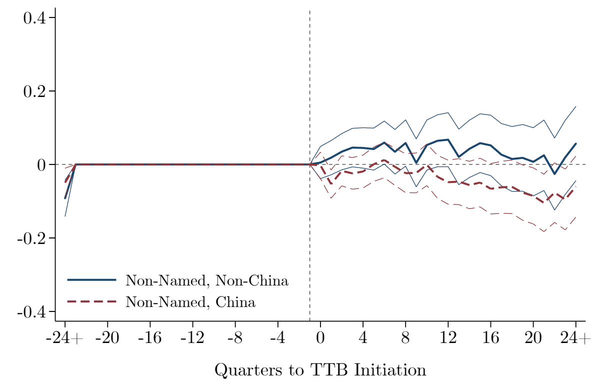
Pre-trends: China 0.024 (0.003) -- Non-China 0.020 (0.003)
Adj. R-Squared: 0.723 -- Obs.: 4,761,576

(b) Named, Extrapolated



Adj. R-Squared: 0.723 -- Obs.: 4,761,576

(c) Non-Named, Event Study

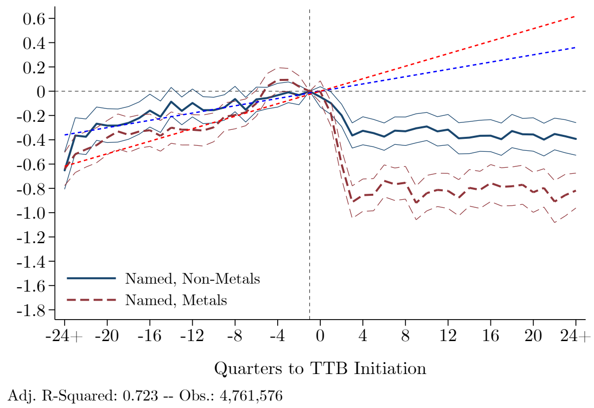


Pre-trends: China 0.002 (0.001) -- Non-China 0.003 (0.001)
Adj. R-Squared: 0.723 -- Obs.: 4,761,576

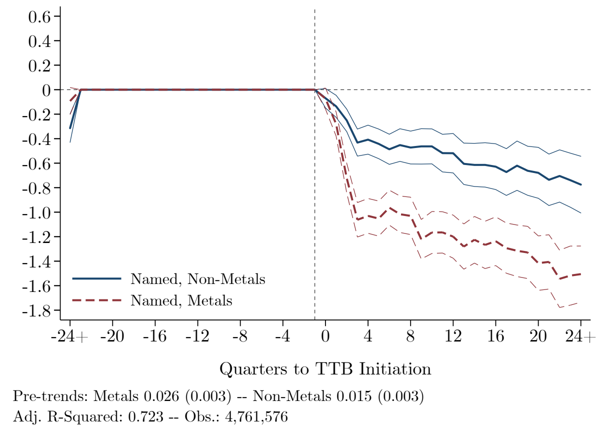
(d) Non-Named, Extrapolated

Figure 10: Differences: China vs. Other Targeted Countries

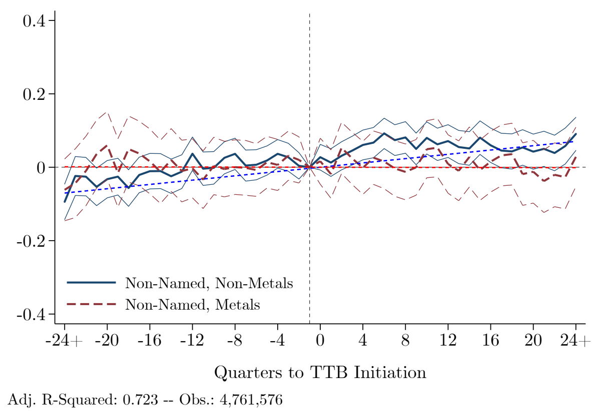
Note. All estimates are obtained by interacting the variables of the coefficients of interest in (2) and (3) with an indicator variable that equals one if the named country is China. Panels (a) and (b) plot the results for named countries and panels (c) and (d) do so for non-named countries. The results of panels (a) and (c) are obtained by (2), while the results of panels (b) and (d) correspond to (2) in (3). The linear pre-trend obtained from (3) is overlaid and extrapolated in panels (a) and (c) (dotted lines). The standard errors that construct the 95 percent confidence intervals are clustered at the ig level.



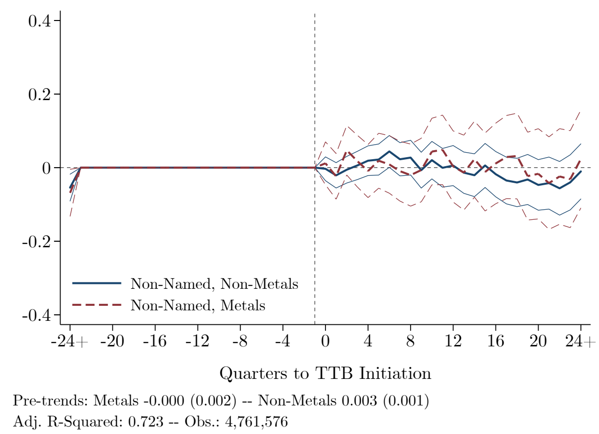
(a) Named, Event Study



(b) Named, Extrapolated



(c) Non-Named, Event Study



(d) Non-Named, Extrapolated

Figure 11: Differences: base metals vs. Other Sectors

Note. All estimates are obtained by interacting the variables of the coefficients of interest in (2) and (3) with an indicator variable that equals one if the targeted variety belongs to the base metal sector (HS section 15). Panels (a) and (b) plot the results for named countries and panels (c) and (d) do so for non-named countries. The results of panels (a) and (c) are obtained by (2), while the results of panels (b) and (d) correspond to (2) in (3). The linear pre-trend obtained from (3) is overlaid and extrapolated in panels (a) and (c) (dotted lines). The standard errors that construct the 95 percent confidence intervals are clustered at the ig level.

Appendix (For Online Publication)

A. TTB Dataset Construction

We use five steps to aggregate the World Bank's Global Temporary Trade Barriers Database (Bown, 2022) at the good, named country, and year of initiation level. The original database of TTBs antidumping and countervailing duties cases initiated by the U.S. between 1993 and 2015 contains 9,864 good-country-year triplets and 875 cases, where the case identifier is set labelled "CASE_ID" in the original database. We then proceed as follows. First, we eliminate duplicates at the good-(named)country-year of initiation-type of TTB (AD or CVD)-TTB ruling-TTB duty level. For example, the U.S. AD cases with identifiers 810 (Live Cattle) and 813 (Live Cattle) against Mexico targeted the same good and were both ruled negatively, but were initiated on separate dates – October 8, 1998, and November 19, 1998. This step reduces the number of cases to 868. Second, among duplicates at the good-(named)country-year of initiation-type of TTB-TTB ruling level, we eliminate the one with the lower ad-valorem duty. For example, the U.S. antidumping case 791 (Stainless Steel Plate in Coils) and 801 (Stainless Steel Sheet and Strip) initiated against South Korea targeted the same goods but imposed a TTB duty of 16.26 and 12.12 percent, respectively. Our procedure assigns the higher duty to the triplet. Third, among duplicates at the good-(named)country-year of initiation-type of TTB level, we eliminate the rejected ones and keep the affirmative ones. For example, the U.S. antidumping case 818 (Cut-To-Length Carbon Steel Plate) and 832 (Cold-Rolled Carbon Steel Products) initiated against Indonesia targeted the same goods in 1998 but the first was ruled affirmatively, while the second was not. Our procedure assigns assigns an affirmative ruling to the triplet. This step reduces the number of cases to 867. Fourth, we merge AD and CVD investigations that target the same variety and are initiated in the same year. For example, the same varieties were targeted in the 1998 AD case 791 and CVD case 447 against South Korea. Our merging procedure assigns the sum of the two duties (if both affirmative) and the earliest initiation date. This step reduces the number of cases to 727. Finally, we apply the product concordance of Pierce and Schott (2012) to the original HS 10-digit product codes and collapse the triplet good-(named)country-year of initiation at the new product code level. This final step leaves us with 712 cases and 6,015 varieties.[36]

[36] The variety count here refers to the level of aggregation of the TTB case, which can be at the 10-, 8-, 6-, and 4-digit level, while in column 5 of Table 1 it is at the 10-digit level. Note also that because in some cases countries are targeted even though there is no trade in those goods, when we merge the TTB database with the trade data the number of cases drops to 706 (column Table 1).

B. Appendix Tables and Figures

Table B.1: Static Trade Effects - Alternative Fixed Effects

	(1)	(2)	(3)
Panel (a): Import Value			
Named	-0.207***	-0.172***	-0.225***
	(0.036)	(0.047)	(0.036)
Non-Named	0.111***	0.069***	
	(0.012)	(0.015)	
Observations	14,448,905	12,917,156	14,333,830
Adjusted R-Squared	0.722	0.743	0.744
Panel (b): Quantity			
Named	-0.161***	-0.174***	-0.269***
	(0.044)	(0.059)	(0.045)
Non-Named	0.204***	0.113***	
	(0.017)	(0.022)	
Observations	12,465,829	11,037,877	12,351,695
Adjusted R-Squared	0.791	0.809	0.804
Panel (c): Unit Value			
Named	-0.045***	-0.001	0.026
	(0.016)	(0.023)	(0.017)
Non-Named	-0.066***	-0.030***	
	(0.008)	(0.011)	
Observations	12,465,829	11,037,877	12,351,695
Adjusted R-Squared	0.848	0.861	0.852
ig FE	✓	✓	✓
ist FE	HS Section	HS-4	HS Section
$s't$ FE			HS-10

Note. All estimates correspond to the static estimation equation specified in (1). Standard errors in parentheses are clustered at the ig level, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B.2: **Event Study Trade Effects - Alternative Fixed Effects**

	1-Year			5-Years		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel (a): Import Value						
Named	-0.542***	-0.495***	-0.636***	-0.522***	-0.468***	-0.600***
	(0.043)	(0.057)	(0.047)	(0.051)	(0.068)	(0.054)
Non-Named	0.057***	0.053***		0.051***	0.008	
	(0.015)	(0.019)		(0.018)	(0.024)	
Observations	14,448,905	12,917,156	14,333,830	14,448,905	12,917,156	14,333,830
Adjusted R-Squared	0.722	0.743	0.744	0.722	0.743	0.744
Panel (b): Quantity						
Named	-0.716***	-0.632***	-0.754***	-0.655***	-0.584***	-0.764***
	(0.051)	(0.074)	(0.059)	(0.063)	(0.087)	(0.071)
Non-Named	0.085***	0.103***		0.097***	0.023	
	(0.024)	(0.032)		(0.028)	(0.037)	
Observations	12,465,829	11,037,877	12,464,085	12,465,829	11,037,877	12,464,085
Adjusted R-Squared	0.791	0.809	0.797	0.791	0.809	0.797
Panel (c): Unit Value						
Named	0.088***	0.098***	0.077***	0.123***	0.075*	0.131***
	(0.023)	(0.035)	(0.028)	(0.027)	(0.040)	(0.034)
Non-Named	-0.025*	-0.039*		-0.024	-0.011	
	(0.015)	(0.021)		(0.017)	(0.023)	
Observations	12,465,829	11,037,877	12,351,695	12,465,829	11,037,877	12,351,695
Adjusted R-Squared	0.848	0.861	0.852	0.848	0.861	0.852
ig FE	✓	✓	✓	✓	✓	✓
ist FE	HS Section	HS-4	HS Section	HS Section	HS-4	HS Section
$s't$ FE			HS-10			HS-10

Note. All estimates correspond to the standard event study estimation equation specified in (2). All dependent variables are in logs. Columns labeled as "1-Year" and "5-Years" correspond to the trade effects in the 4^{th} and 20^{th} quarter after the TTB investigation was initiated, respectively. Standard errors in parentheses are clustered at the ig level, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B.3: Trade Effects with Extrapolated Pre-Trend - Alternative Fixed Effects

	1-Year			5-Years		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel (a): Import Value						
Named	-0.682***	-0.661***	-0.761***	-0.945***	-0.906***	-1.077***
	(0.045)	(0.059)	(0.049)	(0.073)	(0.096)	(0.079)
Non-Named	0.027*	0.021		0.009	-0.028	
	(0.015)	(0.019)		(0.026)	(0.033)	
Observations	14,448,905	12,917,156	14,333,830	14,448,905	12,917,156	14,333,830
Adjusted R-Squared	0.722	0.743	0.744	0.722	0.743	0.744
Panel (b): Quantity						
Named	-0.832***	-0.838***	-0.913***	-1.240***	-1.155***	-1.365***
	(0.056)	(0.077)	(0.063)	(0.090)	(0.122)	(0.100)
Non-Named	0.054**	0.094***		0.045	0.031	
	(0.023)	(0.031)		(0.038)	(0.051)	
Observations	12,465,829	11,037,877	12,351,695	12,465,829	11,037,877	12,351,695
Adjusted R-Squared	0.791	0.809	0.804	0.791	0.809	0.804
Panel (c): Unit Value						
Named	0.118***	0.140***	0.110***	0.274***	0.203***	0.245***
	(0.025)	(0.044)	(0.029)	(0.036)	(0.054)	(0.043)
Non-Named	-0.024*	-0.050***		-0.027	-0.046	
	(0.014)	(0.019)		(0.022)	(0.031)	
Observations	12,465,829	11037877	12,351,695	12,465,829		12,351,695
Adjusted R-Squared	0.848	0.861	0.852	0.861	0.848	
0.852						
ig FE	✓	✓	✓	✓	✓	✓
ist FE	HS Section	HS-4	HS Section	HS Section	HS-4	HS Section
s't FE			HS-10			HS-10

Note. All estimates correspond to the estimation equation with extrapolated linear pre-trends specified in (3). All dependent variables are in logs. Columns labeled as "1-Year" and "5-Years" correspond to the trade effects in the 4$^{\text{th}}$ and 20$^{\text{th}}$ quarter after the TTB investigation was initiated, respectively. Standard errors in parentheses are clustered at the *ig* level, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B.4: Countries with Bilateral Trade Data

Argentina	Australia	Austria	Belgium
Brazil	Bulgaria	Canada	Chile
China	Colombia	Cyprus	Czech Republic
Denmark	Estonia	Finland	France
Germany	Greece	Hong Kong	Hungary
Iceland	India	Indonesia	Ireland
Italy	Japan	Latvia	Lithuania
Luxembourg	Malaysia	Malta	Mexico
Netherlands	New Zealand	Norway	Peru
Philippines	Poland	Portugal	Romania
Russia	Singapore	Slovakia	Slovenia
South Africa	South Korea	Spain	Sri Lanka
Sweden	Switzerland	Taiwan	Thailand
Turkey	United Kingdom	United States	Venezuela

Note. These are the 56 countries for which bilateral trade data between 1999 and 2018 is consistently available from the Global Trade Atlas IHS Markit (2022).

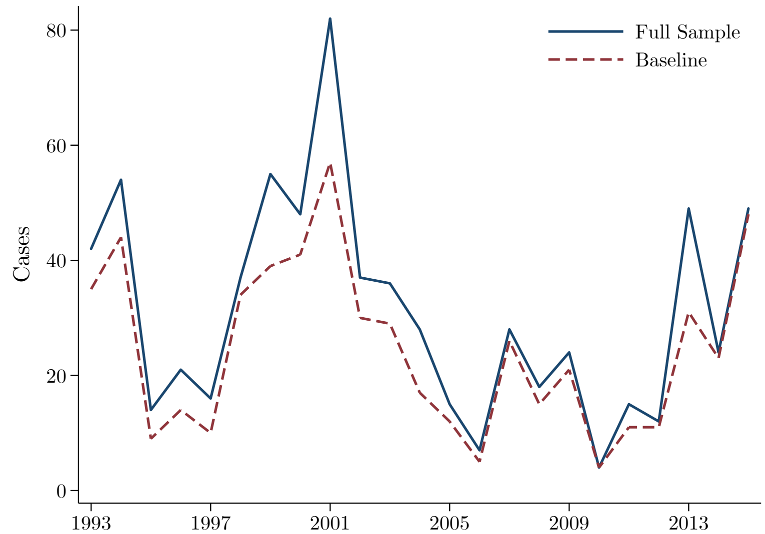


Figure B.1: TTB Investigations over Sample Period

Note. Data is from Bown (2022). Construction of the full and baseline samples is described in Section 2. The baseline excludes varieties that were targeted in more than one year of our sample period.

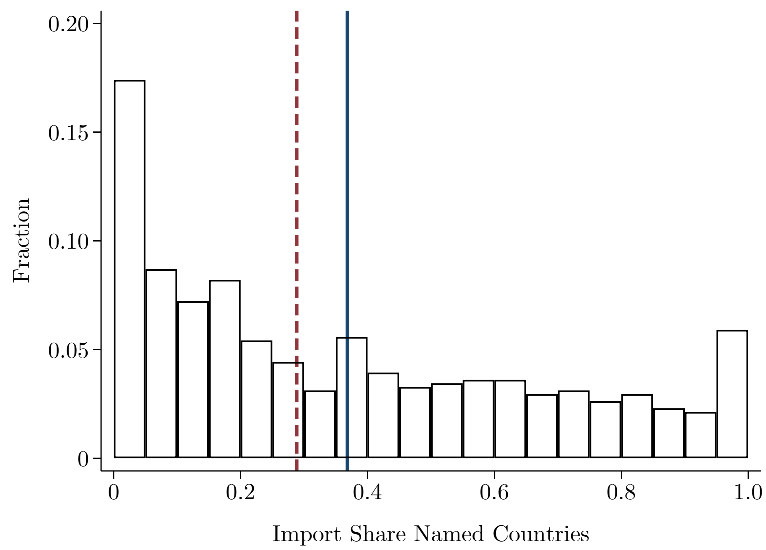
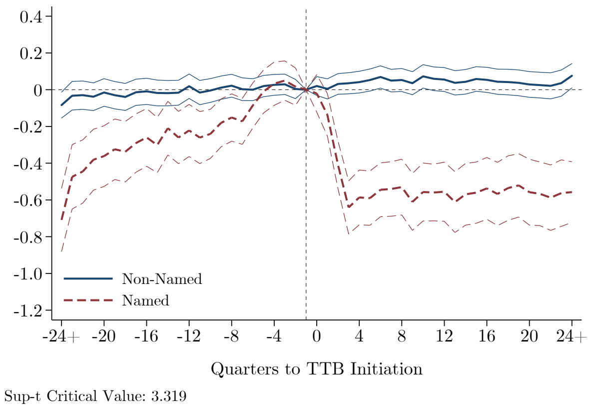
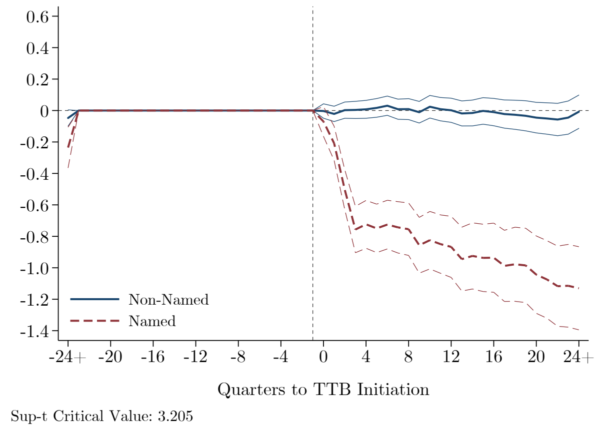


Figure B.2: Import Share of Named Countries

Note. Import share is calculated as the share of imports of targeted goods by named countries in the year before the investigation. The solid blue line is the average and the dashed red line is the median.



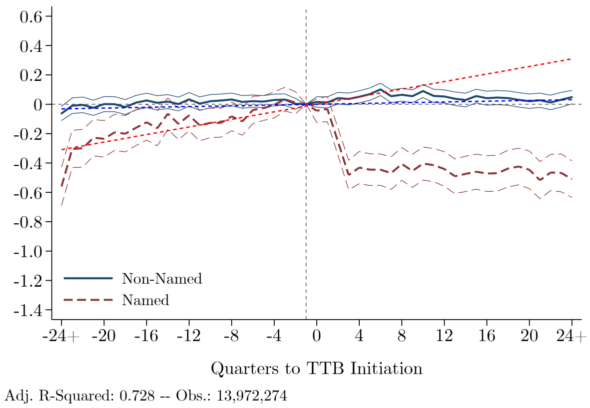
(a) Event Study



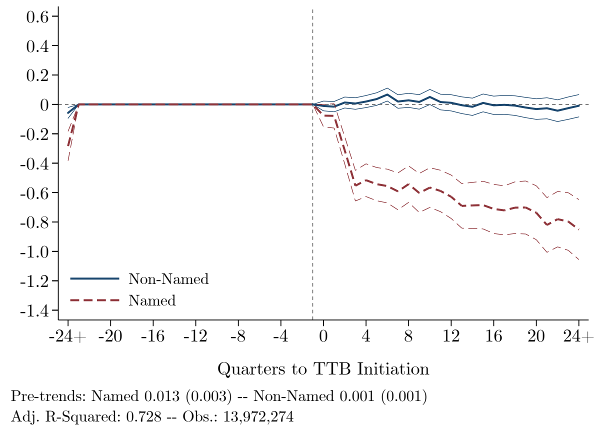
(b) Extrapolated Pre-Trend

Figure B.3: Trade Effects with sup-t Confidence Interval

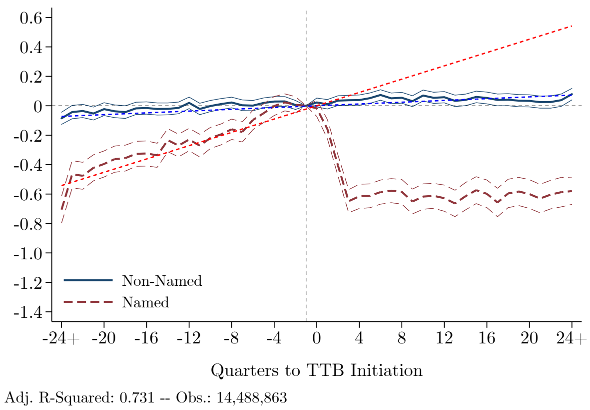
Note. Point estimates and standard errors are the same as in Panels (a) and (b) of Figure 4, and as estimated by (2) and (3), respectively. However, the confidence interval is estimated as the uniform sup-t band (Montiel Olea and Plagborg-Møller, 2019). The estimated critical value is reported at the bottom of each figure.



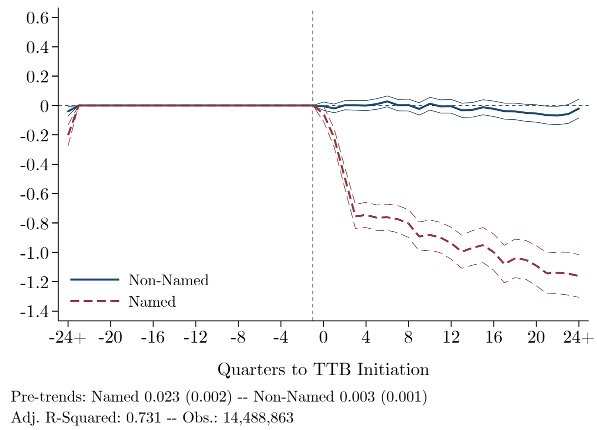
(a) Restricted Sample, Event Study



(b) Restricted Sample, Extrapolated



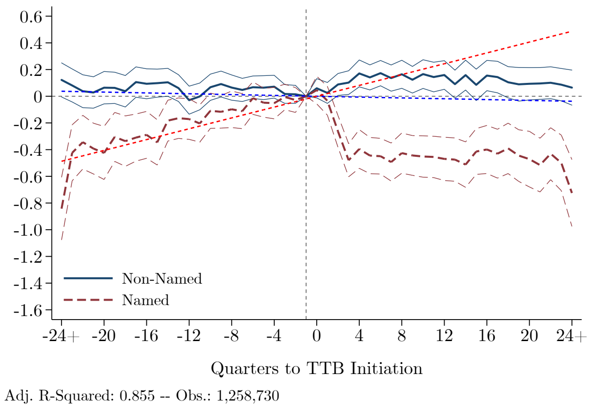
(c) Full Sample, Event Study



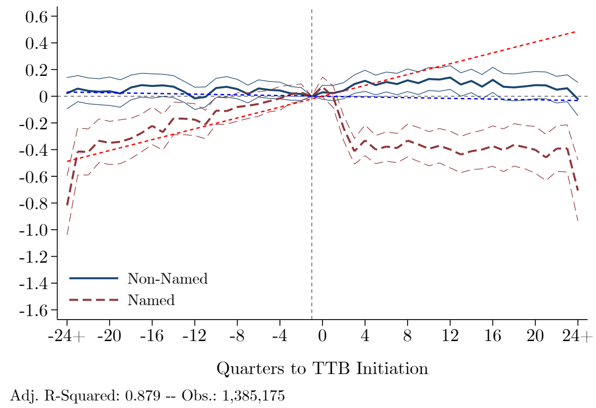
(d) Full Sample, Extrapolated

Figure B.4: Robustness to Alternative Samples

Note. Panels (a) and (b) use the sample that restricts the baseline sample to goods that are affected by investigations targeting different countries in the same year. Panels (c) and (d) use the full sample with all varieties affected by TTBs, including varieties affected by more than one investigation in different years. The results of panels (a) and (c) are obtained by (2), while the results of panels (b) and (d) correspond to (2) in (3). The linear pre-trend obtained from (3) is overlaid and extrapolated in panels (a) and (c) (dotted lines). The standard errors that construct the 95 percent confidence intervals are clustered at the *ig* level.



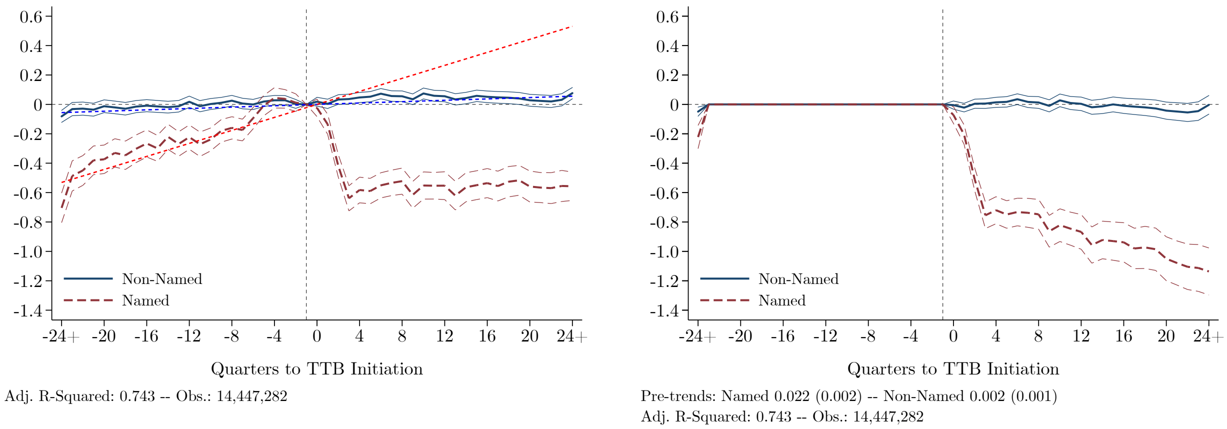
(a) Excl. Zeros



(b) Incl. Zeros

Figure B.5: IHS Transformation of the Import Values

Note. This figure plots the event coefficients estimated by (2), using the inverse hyperbolic sine transformation of the import value as the dependent variable. The sample is aggregated at the good level, and named and non-named country level, as in Figure 8. Panel (a) uses this sample and excludes zeros, while panel (b) rectangularizes this dataset and includes zeros. Quarters $n < -24$ ($n > 24$) are binned to -24 (24) and $n = -1$ is the reference period. The standard errors that construct the 95 percent confidence intervals are clustered at the g level.



(a) Event Study **(b) Extrapolated**

Figure B.6: Including Tariffs and Shipping Costs

Note. Panel (a) plots the event coefficients estimated by (2) extended to include the log of tariffs and shipping costs. Panel (b) plots the analogous modification of (3). Quarters $n < -24$ ($n > 24$) are binned to -24 (24) and $n = -1$ is the reference period. The standard errors that construct the 95 percent confidence intervals are clustered at the g level.