The Hazardous Effects of Antidumping*

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Abstract

We investigate the extent to which antidumping actions eliminate trade altogether. Using quarterly 10-digit HS-level export data for products involved in U.S. antidumping cases we find that antidumping actions increase the hazard rate by more than fifty percent. We find strong evidence of investigation effects with the impact during the initiation and preliminary duty phases considerably larger than once final duties are imposed. There are also important differences with respect to the size of duties with cases with large duties experiencing very large investigation effects. We show the AD-affected countries are less likely to return to the market even after the AD order is removed.

Keywords: antidumping, duration, hazard
JEL Codes: F13, F14

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1 Introduction

For almost four decades antidumping (AD) has been the most important form of discretionary protection,[1] outpacing all other forms of administered protection *combined* (Bhagwati, 1989; Zanardi, 2006; Bown, 2011). AD duties have been shown to significantly reduce exports from named countries, 50–60% on average (Prusa, 2001). This paper examines the impact of AD on the ability of a named supplying country to maintain any market presence. We find that AD investigations often drive export suppliers entirely out of the market. Using U.S. AD case information along with highly disaggregated product-level quarterly export data we estimate the hazard of exports to the U.S. ceasing and find that AD increases the likelihood of exit by more than fifty percent. Considering that over the past two decades more than one-quarter of AD duties have exceeded 100% *ad valorem* it may not be entirely surprising that many AD affected countries are unable to continue to export to the United States.[2] Yet, this aspect of AD protection has been heretofore overlooked. To put it in different words, much of the literature examining the effects of antidumping is concerned with the effects on the intensive margin. The effects on the extensive margin have been largely ignored. We seek to rectify this omission.

A related question is when an AD action affects trade. Is it when the case is initiated? Is it when the preliminary duty is levied? Or, is it when the final duty is levied? We find the most significant effects occur early in the investigation. Interestingly, the investigation effects are larger than those when the final AD duty is levied, implying that by the time the final duty is levied most of the effect on the extensive margin has already happened. Exporters often cease *serving* the market *during* the investigation. We also find important differences with respect to the size of duties. Cases with higher duties are much more likely to result in exit in the preliminary

[1] Antidumping has been estimated to be the most costly form of protection for both the U.S. and EU (Gallaway, Blonigen, and Flynn, 1999; Messerlin, 2001).

[2] Blonigen (2006) analyzes why AD duties are so large.

phase, but there is little additional effect on the likelihood of exit in the final phase. By contrast, cases with lower duties experience a smaller but more persistent effect on the likelihood of exit. The fact that AD activity drives trade to zero is not innocuous and without consequence. We show that countries who were affected by AD are less likely to return to the U.S. market, not only while the AD duty is in place, but even after it was removed. The results imply that AD has a long-run deterrence effect on the behavior of affected suppliers.

We conduct numerous robustness checks. We show that gaps between active spells of trade, left censoring, and observations with many missing explanatory values do not affect our results qualitatively. We also show that the periodic product code changes do not affect our results. We investigate how the effect of AD action changes depending on when during an active spell it is enacted, finding that AD activity has a larger relative effect the older the spell is at the point of filing of an AD petition. Finally, we also show that duration of trade itself is not related to the likelihood of an antidumping petition being filed, thus reducing endogeneity concerns.

2 Related Literature

There is a substantial literature on the intensive margin effects of AD protection. However, no previous study has analyzed AD's impact on the extensive margin. Prusa (2001), Bown and Crowley (2007), and Carter and Gunning-Trant (2010) use annual line-item trade data and estimate a 50–60% reduction in named imports due to U.S. AD duties. Konings, Vandenbussche, and Springael (2001) and Ganguli (2008) find quantitatively similar effects on EU and India imports, respectively.

Several other studies have examined AD's impact on trade during the investigation. Staiger and Wolak (1994) use annual 4-digit industry-level trade data for U.S. AD cases from the early 1980s and conclude that about half of the trade volume effect of an AD order occurs during the period of investigation. This is an important finding as it means that even if named countries are ultimately exonerated the domestic industry gains substantial protection during the 12-month investigation. However, their use of annual industry-level data significantly complicates, and to some extent weakens, their analysis. To begin with, industry level data are far too aggregated for studying AD protection which is levied at the tariff line level. Each industry is comprised of hundreds (or even thousands) of tariff-line codes, most of which are not protected.[3]

While Staiger and Wolak consider investigation effects, they have to make a series of assumptions to estimate the within-year trade effects. Preliminary duties are almost never in effect for a calendar year but rather are in effect for just two or sometimes three quarters. This complicates the derivation of within-year effects using annual data. For each tariff line in each four digit industry Staiger and Wolak count the number of days in each year that each investigation effect is present and then normalize by the total number of days. This index is then used to estimate the various investigation effects. If the trade effects vary across tariff lines and/or the number the number of tariff lines vary by industry (i.e., the number of tariff lines is not related the amount of trade) the index will not accurately capture the actual tariff line trade effects.[4]

Krupp and Pollard (1996) and Baylis and Perloff (2010) also examine investigation effects and avoid some of these complications by using monthly data. Both studies document substantial trade effects during the period of investigation. Lu, Tao, and Zhang (2013) use monthly data on Chinese exports between 2000 and 2006 to examine

[3]In addition to aggregation issues, the results in Prusa (2001) and Bown and Crowley (2007) indicate that the industry metric is a very noisy measure of actual product-level protection due to trade diversion.

[4]For example, suppose an industry has two tariff codes, one with a case (TS1) and one without a case (TS2). Suppose for TS1 the preliminary duty were imposed on August 1. In this case Staiger and Wolak assume TS1 trade was affected during the last five months of the year (152 days); TS2 would have zero days. Therefore, the index for that industry would be $(152 + 0)/(2 * 365) = 0.21$. If the industry had a third (fourth) tariff line the index would fall to 0.14 (0.10).

how Chinese exporters react to U.S. AD cases. They find large distributional effects, with less productive firms exiting the U.S. market, while the surviving firms export more than they did before the order. As we will show, our results are stronger in the sense that we find strong evidence of a complete cessation of exports from countries in products affected by an AD order, irrespective of characteristics of exporting firms.[5]

This paper differs from prior studies in two key dimensions. First, we use quarterly line-item product level data.[6] This allows us to precisely measure the timing of trade effects without the extraneous noise that characterizes monthly data. In addition, the use of quarterly data allows us to avoid some of the partial year bias present in annual data as discussed by Bernard et al. (2013).[7] Second, we are the first to consider the effect of AD actions on duration of trade, a natural way to examine exit caused by an event occurring while a spell of trade is active. The advantage of using a duration model over a simple exit model is that a simple exit model does not take into account that the likelihood of exit is a function of duration. Besedeš and Prusa (2006a) offered the first analysis of duration of trade, showing that the duration of U.S. imports is very short with a median of four years in length. A large number of papers have confirmed that short duration is a common characteristic of international trade at both the product level[8] and the firm level.[9] More important for our purposes is the consistent and robust finding of the duration literature that the likelihood of trade ceasing is a decreasing function of duration with longer lasting relationships less likely to cease.

[5]We did examine whether China's exports of products affected by AD orders behaved differently than those from other countries. We found no significant differences. These results are available on request.

[6]Hillberry and McCalman (2011) use the same data we use to investigate trade patterns prior to an AD investigation.

[7]The bias discussed in Bernard et al. (2013) is likely small in duration studies because the key piece of information on a trade flow in duration studies is its sheer existence, rather than its value.

[8]See Besedeš and Prusa (2006b, 2011), Besedeš (2008, 2011, 2013), Nitsch (2009), Jaud, Kukenova, and Strieborny (2009), and Carrère and Strauss-Khan (2012).

[9]See Görg, Kneller, and Muraközy (2012) and Cadot et al. (2011).

3 A Primer on U.S. AD Procedures

A short discussion of the United States' AD procedures will lay the groundwork for our empirical strategy.[10] In the U.S. an AD investigation begins when the domestic industry simultaneously files a petition with the U.S. Department of Commerce (USDOC) and the U.S. International Trade Commission (USITC). The petition includes two important pieces of information for our study. First, the petition specifies the exact product that is alleged to have been dumped. The product is identified by one (or more) 8- or 10-digit tariff line (HS) codes. If the investigation involved ball bearings, for example, the HS code will identify product size (e.g., 4mm versus 10mm ball bearings), material (metal versus plastic), and chemistry (carbon or alloy steel). Second, the petition indicates which country(-ies) is(are) allegedly dumping. Only countries named in the petition are subject to the investigation and, if they are ultimately levied, to AD duties. We note that for a given named country the AD duty does not vary by HS code, but the duty can vary by named country within a single AD case.[11]

Once the petition is filed the investigation proceeds on a precisely specified statutory timeline (USITC, 2008) which is reflected in our analysis.[12] The investigation continues on a dual track, with the USDOC determining whether the product in question was sold at less than fair value and the USITC determining whether domestic firms suffered a material injury. The first major decision is the USITC's preliminary determination which decides whether the domestic industry is suffering (or is threatened by) material injury. This decision is made within a quarter of the petition

[10]While the WTO Antidumping Agreement provides broad guidelines, the AD process varies substantially from country to country (Blonigen and Prusa, 2003).

[11]Different AD duties can be levied on specific firms from the same source country; however, our trade data does not identify individual exporting companies so we use the weighted average AD margin for the supplying country ("all others" rate).

[12]Since our trade data is quarterly we discuss the timeline in terms of quarters. For example, the USDOC must normally make its preliminary duty determination no later than 160 days (two quarters) from the filing date, or 210 days (three quarters) if the case is deemed "complicated."

initiation; a negative decision ends the case.

The next major decision occurs about one quarter later when the USDOC makes its preliminary duty determination. The USDOC must determine whether the foreign supplier(s) named in the petition sold their product at less than fair value. If the preliminary duty is more than *de minimis*, the AD duty goes into effect at that time.[13] About 95% of the USDOC's preliminary duties exceed the *de minimis* margin. If the preliminary duty is *de minimis* the investigation continues but the preliminary duty is not imposed.

The USDOC's and USITC's final determinations both typically occur two quarters after the preliminary duty determination.[14] If both final determinations are affirmative, the final AD duty is imposed.[15] The preliminary and final duties are very similar with a correlation exceeding 0.9,[16] which means there is little uncertainty about the size of the final duty once the preliminary duty is announced. Therefore, for all intents and purposes once the preliminary AD duty has been determined the only remaining uncertainty involves the USITC's final injury determination.[17] Once imposed, the AD duty can be in place for an indefinite period of time.[18] In our data the average length of an AD order is 36 quarters.

The statutory framework leads us to divide a case into three stages: the initiation phase, the preliminary duty phase, and the final duty phase. The *initiation* phase is the period of time during the investigation before any additional duty is levied. It typically encompasses the time from when the case is initiated until either the

[13]A dumping margin less than two percent is *de minimis*.

[14]If the case is deemed complicated or if there are court challenges, the final determination can be delayed for a quarter or two (or in a few rare cases, even longer).

[15]Unlike some other forms of administrative protection and unlike AD policy in other countries, under U.S. law the AD duty is imposed without any input or approval from the President.

[16]Since 1990 the average (median) preliminary AD duty is 49.16% (30.94%) and the average (median) final AD duty is 53.2% (36.41%).

[17]Since 1990 about 75% of the USITC final injury determinations have been affirmative.

[18]Even though the Uruguay Round of the WTO included a mandatory sunset review, this provision has not resulted in shorter duration of AD duties (Prusa, 2011). The sunset provision simply requires every AD order be reviewed after it has been in place for five years (Moore, 2006).

USITC's negative preliminary determination or the date when the USDOC makes its preliminary duty determination. If the USDOC's preliminary duty is *de minimis* the initiation phase lasts until the final determination is made. Because no additional duties are levied during this phase it is often presumed that AD has no impact. However, this may not be the case. Trade could be affected if the specter of the investigation intimidates U.S. buyers from purchasing from named suppliers, the so-called *in terrorem* effect.

The *preliminary duty* phase encompasses the period when U.S. buyers pay the preliminary AD duty.[19] Once the USDOC and USITC final determinations are made the case is either terminated or a final AD duty is imposed. The *final duty* phase begins the date the final AD duty is imposed and continues until the date the AD order is revoked.

Finally, we note that some AD cases are "settled." Settlements are usually the result of an agreement between the named suppliers and the USDOC and establish maximum export volume and/or minimum sales prices. While many U.S. AD cases were settled in the 1980s (Prusa, 1992) relatively few cases have been settled in the past 20 years.[20] We examine the impact of settled cases on the hazard rate by incorporating information on the duration of settlement agreements. However, we are unaware of any settlement agreement that mandated the cessation of exports.

4 Data

4.1 HS Trade Data

Quarterly trade data are from the U.S. Census' *U.S. Imports of Merchandize Trade* starting with Q2–1990 through Q4–2006. U.S. imports are recorded at the 10-digit

[19]The duty is held in an account and is returned to the buyer if the case is ultimately rejected.
[20]Settlements continue to be quite common in other countries, most notably in the EU.

HS level. The data include information on trade value, quantity shipped, duties collected, and import charges (insurance and freight).

In order to perform duration analysis we translate quarterly trade data into trade relationships and spells of service. A trade relationship is defined as a HS-country pair, while a spell of service consists of consecutive quarters during which a trade relationship is active. There are 10,423,157 quarterly HS-country observations which map into 2,660,147 spells of service reflecting 748,430 trade relationships. The vast majority of observed spells of service are essentially one-off events, with over 58% observed for just one quarter as seen in Table 1. The average observed spell length is 3.91 quarters. Roughly 60% of relationships have multiple spells of service accounting for 89% of all spells. The predominance of multiple-spell relationships is consistent with patterns documented in previous studies of trade duration. They are somewhat more prevalent in this study due to our use of quarterly data.

Two data issues, missing values and left-censoring, cause almost half the quarterly trade observations to be dropped in our estimation. With respect to the former, Census trade data have a large number of missing values. For instance, approximately 17% of observations are dropped due to missing quantity information. We note that because spells are created using the value of trade when other data are missing no artificial exit is created. For example, suppose that in a seven-quarter long spell there are missing observations on quantity in quarters three and five. The missing observation does not preclude us from observing the full seven-quarter long spell. The missing data only preclude us from calculating the unit values in the quarter three and five. In our estimation, the two quarters with missing data are not used but the remaining information is used and exit is correctly identified to have occurred after seven quarters.

With respect to the latter issue, left-censoring is an issue for all studies of trade duration and affects about 30% of all observations. Left-censored spells are all spells

Spells across relationships			Observed Spell Length		
Total # of spells in a relationship	Number of relationships	Frequency	Spell Length	Number of spells	Frequency
1	299,770	40.05%	1	1,563,218	58.76%
2	114,466	15.29%	2	418,337	15.73%
3	74,127	9.90%	3	193,004	7.26%
4	54,965	7.34%	4	95,058	3.57%
5	42,696	5.70%	5	59,857	2.25%
6	33,853	4.52%	6	43,532	1.64%
7	27,147	3.63%	7	34,464	1.30%
8	22,498	3.01%	8	25,423	0.96%
9	18,816	2.51%	9	18,972	0.71%
10	15,628	2.09%	10	16,081	0.60%
11	12,542	1.68%	11	16,916	0.64%
12	9,870	1.32%	12	11,529	0.43%
13	7,521	1.00%	13	8,641	0.32%
14	5,553	0.74%	14	9,622	0.36%
15	3,747	0.50%	15	10,405	0.39%
16	2,392	0.32%	16	6,407	0.24%
17	1,487	0.20%	17	5,448	0.20%
18	766	0.10%	18	5,616	0.21%
19	346	0.05%	19	7,750	0.29%
20	147	0.02%	20	8,688	0.33%
21	65	0.01%	21–26	21,590	0.81%
22	21	<0.01%	27–36	21,047	0.79%
23	4	<0.01%	37–46	16,722	0.63%
24	1	<0.01%	47–56	13,788	0.52%
25	1	<0.01%	57–66	7,348	0.28%
26	1	<0.01%	67	20,684	0.78%
Total	748,430		Total	2,660,147	

Table 1: **Distribution of Active Spells across Relationships**

active in the first observed quarter. By the term "first observed quarter" we mean either the very first quarter of our sample (Q2–1990) or the first quarter of a newly introduced product code.[21] Right censoring is also present (spells observed in the last observed quarter). However, unlike left-censoring, hazard estimation techniques can account for right censoring. After taking into account these two data issues we have 5,417,711 quarterly HS-country trade observations. In section 6.2 we examine both of these issues and show that our results are qualitatively unaffected.

Another data issue may complicate the analysis. As Pierce and Schott (2012) note, on a semi-annual basis the U.S. International Trade Commission revises U.S.

[21] Pierce and Schott (2012) discuss the tendency for HS codes to be created.

import HS codes. These revisions may create artificial exit if the use of a code with positive trade is discontinued. In all such instances, the spells which are ended by code changes are classified as right censored. Right censored spells are spells whose end is unobserved. To ensure this approach does not result in biased estimates, we use the Pierce and Schott (2012) algorithm to concord all HS codes to identify those that are never changed for administrative reasons. We then re-estimate our main specification using codes that were never changed. Our results are quantitatively unchanged.

4.2 AD Case Data

The *Global Antidumping Database* (Bown, 2012) contains information on the precise timing of each case, the HS product codes, the named (also known as "subject") countries, and the size of the preliminary and final duties. Restricting our sample to AD cases during the period covered by our trade data yields 833 AD cases involving 2,179 distinct HS codes and 8,127 HS-country observations.[22] We will say an AD case is "active" if it is in the midst of one of the three phases (initiation, preliminary duty, final duty). Bown's database allows us to create a precise quarterly history for every HS-country pair involved in a case. This history yields 181,804 quarterly observations for active AD cases at the HS-country-product level.[23]

4.3 Other Data

We follow the trade duration literature and include additional control variables. Initial size is controlled by the (log of) the value of trade when a spell starts. We use GDP measured in constant dollars to capture country effects.[24] Other controls include

[22]About half the HS codes are involved in multiple cases, cases where either multiple codes are listed in the petition and/or the petition names multiple named countries.

[23]Additional discussion of the data merging are contained in appendix A.

[24]Obtained from the World Bank's *World Development Indicators*.

standard gravity variables such as weighted distance, common language, and contiguity.[25] Finally, Census trade data allow us to calculate the paid tariff, transportation costs, and unit values. We do not use unit values themselves, but the coefficient of variation of unit values within an HS code in a given quarter, following Besedeš and Prusa (2006b).[26] The variation in unit values captures the extent to which the product is differentiated, with more differentiated products exhibiting a larger variation in unit values.[27]

5 Results

5.1 Methodology

The object of our interest is the hazard of exports to the U.S. of a particular product from a particular country ceasing. The hazard is a conditional probability of exports of product i from country j ceasing at time $t+k$ conditional on it having survived until age t, $P(T_{ij} \leq t + k | T_{ij} \geq t)$, where T_{ij} is a random variable measuring the survived duration of spell ij. Much of the early literature on duration of trade used the Cox semiparametric proportional hazards model as the preferred way to estimate the hazard of trade ceasing (see Besedeš and Prusa 2006b, Besedeš 2008, and Nitsch 2009). As Hess and Persson (2011, 2012) have argued the use of the Cox semiparametric model or fully parametric versions of proportional hazard models potentially leads to biased estimates. Among the issues are that the Cox estimator is ill-suited to discrete time duration analysis as it was developed with continuous time data in mind.

[25] Obtained from CEPII's gravity dataset at `http://www.cepii.fr/CEPII/en/bdd_modele/presentation.asp?id=8`.

[26] This allows us to use spells such as the one mentioned above where quantity information might be missing for one year of the spell. Provided there is sufficient number of other supplying countries, we can still calculate the coefficient of variation of unit values for that particular year of the spell and use it in estimation.

[27] Schott (2004) was one of the first to point out the extent of variation of unit values in U.S. product level trade data and the extent to which it reflected within-product specialization and vertical product differentiation.

Even the discrete-time equivalent of the proportional hazard model is potentially problematic due to the proportional hazard assumption which imposes the constraint that the hazard of two subjects be proportional to each other. This assumption is usually violated in duration of trade studies (see Hess and Persson 2012). Finally, in large data sets the Cox model is impractical if one wants to control for unobserved heterogeneity.

Following Hess and Persson (2012), we estimate the hazard of exports ceasing at time k by estimating a discrete hazard using random effects probit (see Besedeš 2011, 2013) using the following specification

$$\begin{aligned} h_{ijt} &= P\left(T_{ij} \leq t + k | T_{ij} \geq t\right) \\ &= \Phi\left(\mathbf{X}_{ijt}\beta + \gamma_{ij} + \nu_{ij}\right) \end{aligned}$$

where Φ is the standard normal cumulative distribution, $\mathbf{X}_{ijt}$ is the set of explanatory variables, β is the vector of coefficients to be estimated, γ_{ij} are the spell number fixed effects, and ν_{ij} are country-product random effects. This approach does not impose the constraint that the hazard of two subjects be proportional to each other as is the case with proportional hazard estimators and allows for a more computationally feasible accounting of unobserved heterogeneity. We use the log of the number of quarters a spell has been active to specify how the hazard depends on time/duration.

Countries which export products included in AD investigations are organized into three groups: "named," "non-named," and "named, case dropped." An example will clarify our groupings. Suppose there are five countries that export the product named in an AD petition (countries i, j, k, m, and n). Suppose further that three countries (i, j, and k) are named in the petition but at some point country k is dropped. Countries i and j are classified as "named" countries and m and n are classified as "non-named." As long as the case is active against country k it is considered "named."

However, as soon as the case against k is dropped, k is classified as "named, case dropped."

To investigate whether antidumping petitions involve products for which the hazard of exports ceasing is fundamentally different we will control for products that were a part of an antidumping case at some point during the sample period and those that never were. We coded a "product with a case" dummy which identifies all products that were named in a petition at some point during the observed period. To be more precise, for products involved in an AD case, this variable equals 1 irrespective of whether a given exporter of that product was part of a case in that quarter.[28]

We estimate four specifications. In our basic specification we investigate whether a case has an effect on the hazard of exports ceasing (regardless of the case's outcome). To get a better sense as to whether each particular phase of a case has an effect, in the second specification we break out the named country dummy into the three phase dummies, "initiation," "preliminary," and "final." During the initiation phase the specter of the case hangs over the traders but no duty is in place. For this reason Staiger and Wolak (1994) argue that there is no trade effect during the initiation phase. Following Staiger and Wolak's logic, we expect no significant effect during the initiation phase. Staiger and Wolak also find that the trade effect during the preliminary phase is about half the size of the final effect. Our prior, therefore, is that we should find a larger effect on the hazard during the final phase as compared to the preliminary phase.

In our third specification we are interested in whether the size of the duty has a particularly large effect. That is, we expect relationships are significantly more likely to end with large tariffs. To that end, we use two new dummies for each phase. These identify whether either duty (preliminary and final) is below or above the median duty, which is 30.95% for preliminary duties and 36.41% for final duties. We

[28]Our results are virtually identical if we restrict our sample to only those products with a filing at some point. Results available on request.

refer to these three specifications as the basic specification, the phases specification, and the non-linear duties specification.

In the fourth specification we replace the four duty dummies with four variables reflecting the size of the duty in each phase. To maintain the similarity across our specifications, we replace the dummy variable with the log of the size of the duty, below and above the median level, with separate preliminary and final phase effects estimated.

5.2 Control Variables

Before turning our attention to case relevant variables, we summarize the effect of other control variables. Given that the estimates are virtually identical across all specifications and all effects are consistent with results in the literature we expedite our discussion by only presenting them once (Table 2).[29] Time/duration is estimated to have a large negative coefficient, indicating that the hazard decreases with duration. The larger the initial value exported, the lower the hazard, indicating that initial size matters. Distance is estimated to have a statistically significant but small negative effect on hazard — the larger the distance, the lower the hazard. Larger exporter's GDP reduces hazard, while Canada and Mexico, the two countries sharing a border with the U.S. have a significantly lower hazard.

Common language increases the hazard somewhat, while tariffs and transportation costs weakly reduce the hazard. Products with a higher variation in unit values across supplying countries have a lower hazard, reflecting that differentiated products face a lower hazard. The estimated parameter ρ captures the extent of data variation attributable to unobserved heterogeneity. It is estimated to be around 0.2 in most of our specifications, which is in line with the value obtained by Besedeš (2013) using annual 10-digit product level U.S. data.

[29] Full results are available upon request.

Time (ln)	-0.421^{***}
	(0.001)
Initial value (ln)	-0.121^{***}
	(0.000)
Weighted distance (ln)	-0.007^{**}
	(0.003)
COV unit values (ln)	-0.023^{***}
	(0.001)
GDP (ln)	-0.051^{***}
	(0.001)
Contiguity	-0.257^{***}
	(0.007)
Common Language	0.036^{***}
	(0.003)
Tariff (ln)	-0.008^{***}
	(0.000)
Transportation costs (ln)	-0.013^{***}
	(0.000)
Product with a case	-0.061^{***}
	(0.005)
Non-named country	0.059^{***}
	(0.007)
Named, case dropped	-0.085^{***}
	(0.022)
Named country	0.175^{***}
	(0.015)
Constant	3.169^{***}
	(0.033)
Observations	5,417,711
Log-Likelihood	−2,641,464
ρ	0.208^{***}

Standard errors in parentheses with *, **, *** denoting significance at 10%, 5%, and 1%.

Source: Authors' calculations

Table 2: **Basic Specification**

5.3 Basic Specification

We now turn to the estimated AD effects in our basic specification. We begin by noting that products that were a part of an AD petition have a lower hazard (−0.061) than those which did not have a petition filed. Interestingly, the parameter estimate for the "non-named country" (0.059) essentially offsets the product effect which means exports from these countries face the same hazard as products which never were a part of a petition. We find that named countries face a higher hazard (0.175). By contrast, the hazard falls when a named country is dropped from the investigation.

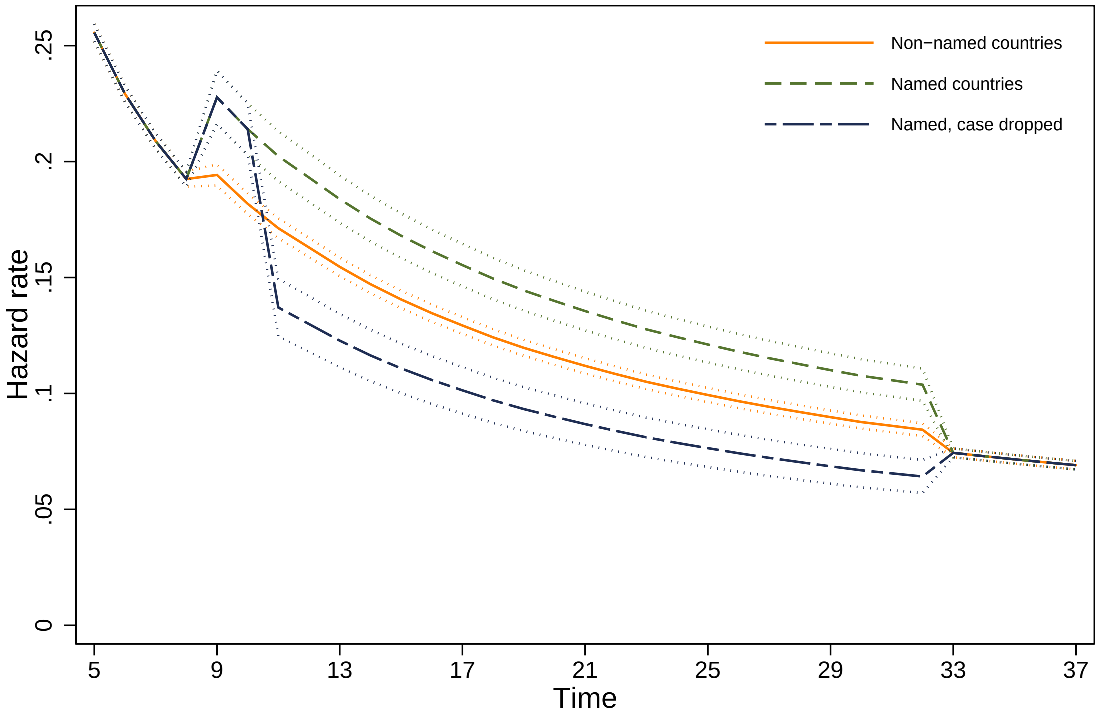
While the use of probit has its advantages, its disadvantage is that the interpretation of coefficients is not straightforward. The effect of every covariate depends on its own estimated coefficient as well as that of time. Since the hazard of two subjects is no longer proportional, the effect of every covariate is not independent of duration. In addition, even if a coefficient is estimated to be statistically significant, its effect on hazard may not be significant as the significance depends not only on its own standard error, but also on that of every other variable.

As is commonly done with this estimation approach we determine whether the effect of a covariate is significant by plotting two estimated hazard functions along with corresponding confidence intervals. All variables are evaluated at the sample mean, except for the covariate of interest which we will evaluate at different values. We then compare the 99^{th} percentile confidence intervals of the two hazard functions. If they do not overlap, the effect of the variable of interest is considered statistically significant.[30]

We show these estimated effects in Figure 1. When simulating the fitted hazard for our plots we assume that an AD case has the following time profile: the petition is initiated in the ninth quarter of an active spell, the initiation phase lasts two quarters, the preliminary dumping duty phase lasts two quarters, and the final duty phase lasts 20 quarters.[31] All but the petition initiation and the length of the final duty phase are determined by the statute. With respect to the initiation timing, we opt for the ninth quarter because that is the observed median duration of a spell of trade at the point of filing of an AD petition. With respect to the duration of the final duty phase our assumption reflects the statute's sunset provision, although in practice final

[30]Sueyoshi (1995) provides a detailed explanation, while Hess and Persson (2011) and Besedeš (2013) use this technique in the context of duration of trade.

[31]The timing assumptions only affect the graphical plots of the estimated parameters. The estimation uses the actual timing for each case.



Simulated effects of AD assuming the following case trajectory: filing occurs in quarter 9, initiation lasts 2 quarters, preliminary duty in place for 2 quarters final duty in place for 20 quarters
Source: Authors' calculations

Figure 1: **Effects of an AD Case (basic specification)**

duties are often imposed for a longer period. Finally, for those countries initially a part of a petition but dropped at some point, we assume they were dropped during the preliminary phase.[32]

As seen, the hazard for a named country is markedly higher during a typical case (24 quarters) by 3.6 percentage points. Relative to the hazard if there was no case our estimate implies an on average 32.7% increase in a named country's hazard (see Table 4).

5.4 Phases Specification

The basic specification constrains the effect of the AD case to be the same for each phase of the case. We now introduce separate dummies for each phase and find that

[32]Countries are most frequently dropped early in the investigation.

the effect varies substantially by phase. The results in Table 3 indicate that the likelihood that exports will cease is higher in the initiation and preliminary phases than in the final phase.[33]

Product with a case	-0.061^{***}
	(0.005)
Non-named country	0.059^{***}
	(0.007)
Named, case dropped	-0.081^{***}
	(0.022)
Initiation phase	0.361^{***}
	(0.031)
Preliminary duty phase	0.448^{***}
	(0.041)
Final duty phase	0.111^{***}
	(0.017)
Observations	5,417,711
Log-Likelihood	−2,641,405
ρ	0.208^{***}

Standard errors in parentheses with *, **, *** denoting significance at 10%, 5%, and 1%.

Source: Authors' calculations

Table 3: **Phases Specification**

The variation by phase is clearly seen in Figure 2 where we plot the hazard profile of a typical case. For comparison purposes, we also include the hazard profile with all phase dummies set to zero which allows us to isolate the pure effect of a case. As seen, the hazard increases when the case is initiated and increases further when preliminary duties are levied. Interestingly, while the hazard is higher during the final phase compared to what it would be in the absence of a case, the effect is considerably smaller than during either of the two early phases.

Table 4 provides perspective on the increase in the hazard during the various phases. The increase in the hazard in the first two phases is remarkable. The hazard during the two-quarter initiation phase increases by 10.7 percentage points, which is a 62.2% increase relative to the "no case" benchmark. The impact during the two-

[33]In Table 3 we only report estimates for the phases and the product and country dummies. Complete results are available on request.

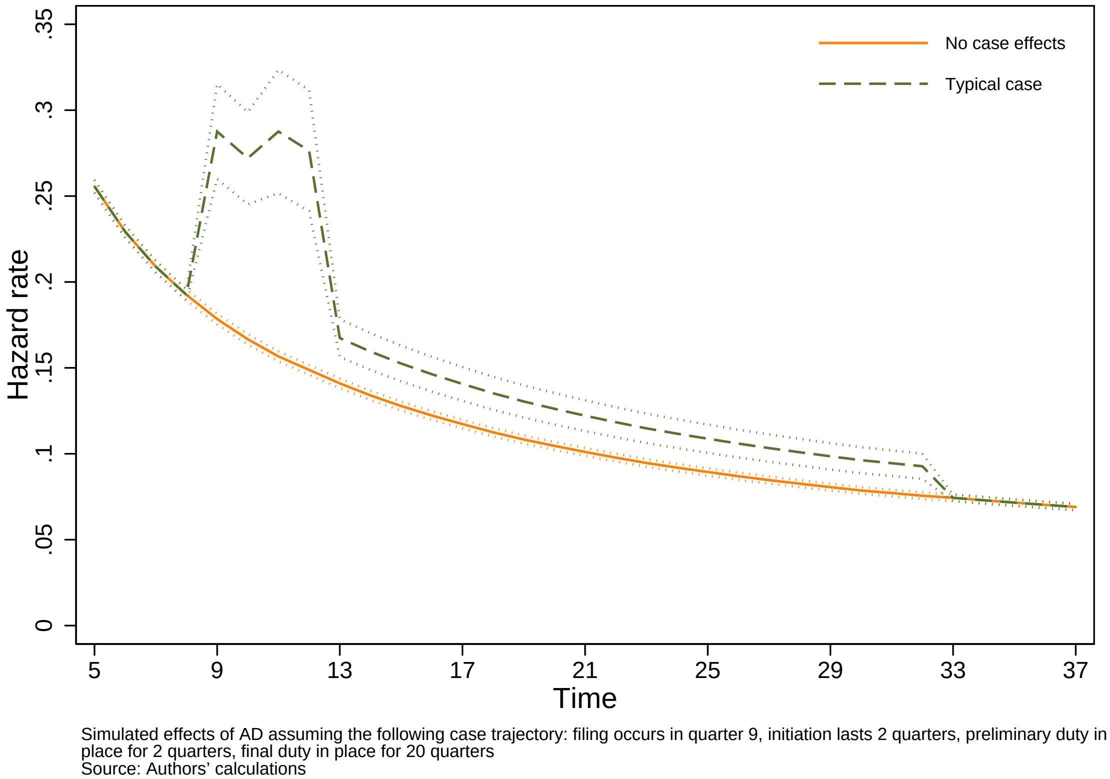


Figure 2: **Phase-Specific Effects**

quarter preliminary duty phase is even larger — an 84.6% relative increase in the hazard. The hazard during the final phase is 20.8% higher than it would be if no case had been filed. Over the entire duration of the case, we find a case increases the hazard by an average of 3.7 percentage points – which compared to its average value (11%) during the duration of a case implies a 33.5% increase in the hazard.

	Average increase in hazard – Phase Specific						Entire Duration of Case	
	Initiation		Preliminary		Final			
Specification	Nominal	Relative	Nominal	Relative	Nominal	Relative	Nominal	Relative
Basic	—	—	—	—	—	—	3.6%	32.7%
Phases	10.7%	62.2%	12.9%	84.6%	2.1%	20.8%	3.7%	33.5%
AD duties								
$\leq$ median	11.3%	59.8%	6.2%	36.9%	4.3%	38.9%	5.1%	41.4%
$>$ median	11.3%	59.8%	24.5%	145.7%	-0.1%	-0.6%	2.9%	23.9%

Source: Authors' calculations

Table 4: **Impact of AD – Average increase in hazard**

Our results differ from those found in Staiger and Wolak (1994). Given that we are estimating hazard effects some differences are to be expected, but what it striking is the relative magnitude across phases. While Staiger and Wolak find no effect during the initiation phase we find a very large effect, almost as large as the preliminary phase effect. Staiger and Wolak find the impact during the preliminary phase is about half the size as that after the final duty is imposed. We, however, find that the hazard effect during the preliminary phase is much larger than in the final phase. We believe there are several explanations for the differences. First, Staiger and Wolak's sample (manufacturing cases filed in 1980–85) is much smaller than ours (all industries, 1990–2006). In addition, steel cases dominated the U.S. caseload during the 1980–85 period, accounting for approximately 60% of all manufacturing cases as compared to about 30% in our sample. Second, Staiger and Wolak use 4-digit industry data in their study; we use line item trade data. Given that AD protection is applied at the tariff line, aggregated data miss important effects at the narrow product level. Third, the use of annual data in the Staiger and Wolak study likely has a greater effect on their estimated investigation effects (which by statute last just one or two quarters) than on the estimated final duty (which can remain in effect for many years). As mentioned earlier, their use of annual data required them to create measures to proxy the quarterly effect. Given that final duties are in effect for many *years* their adjustment method primarily affected the investigation effects which could explain the differences in the relative magnitudes across phases (as compared to our estimates).

We offer an additional approach to quantifying the effect of an AD case: relative survival experience. Consider two identical products — one with and one without an AD case. We then consider how many spells would survive with and without a case during the 24 quarters of a typical case. The results in Table 5 demonstrate the excessive failure due to the AD action. Our basic specification (Table 2) implies only about one-third as many spells will survive the entirety of the AD case as would if there were no case. In the phases specification, we find about 25% more spells fail during the initiation phase and about 46% more spells fail through the end of the preliminary duty phase. Similar to the prediction from our basic specification, under the phases specification only about one-third as many spells will survive the entire AD case as would if there were no case (i.e., about 66% more spells fail).

	Ratio: Spells Surviving AD Case to Spells Surviving with No Case		
Specification	Initiation Phase	Preliminary Phase	Entire Duration
Basic	—	—	36.6%
Phases	75.8%	54.4%	33.9%
AD duties			
$\leq$ median	74.0%	63.4%	23.3%
$>$ median	74.0%	36.8%	37.4%

Source: Authors' calculations

Table 5: **Impact of AD – Spells surviving through end of each phase**

5.5 The Size of AD Duties

We now investigate how the size of preliminary and final AD duties affects the hazard. Rather than using a single variable to identify the duty effect for each phase, we initially use two dummies per phase with the results presented in the first column of Table 6. The dummies correspond to whether the duty is below or above the median.[34] In the second column of Table 6 rather than using dummies identifying duties below and above the median level in the two phases, we use the log level of the duties, but preserving the use of two variable per each phase, one if the duty is below the median and the other if the duty is above the median. There are interesting differences in the effect of the level of duties.

The effect of preliminary duties is consistent with our prior expectations: duties above the median have a greater effect on the hazard than duties below the median.

[34] Alternatively, we divided AD duties into quartiles finding qualitatively similar results. Duties in the bottom two quartiles cause a persistently higher hazard for the entire case, while duties in the upper two quartiles sharply increase the hazard during the preliminary phase only, while having little effect on the hazard during the final phase.

	Dummies specification	Level of duties specification
Product with a case	−0.060***	−0.060***
	(0.000)	(0.000)
Non-named country	0.058***	0.058***
	(0.000)	(0.000)
Named, case dropped	−0.082***	−0.085***
	(0.000)	(0.000)
Initiation phase	0.363***	0.361***
	(0.000)	(0.000)
Preliminary duty ≤ median	0.223***	0.095***
	(0.000)	(0.000)
Preliminary duty > median	0.742***	0.169***
	(0.000)	(0.000)
Final duty ≤ median	0.204***	0.074***
	(0.000)	(0.000)
Final duty > median	−0.004	−0.003
	(0.878)	(0.533)
Observations	5,417,711	5,417,711
Log-Likelihood	−2,641,367	−2,641,364
ρ	0.208***	0.208***

Standard errors in parentheses with *, **, *** denoting significance at 10%, 5%, and 1%.

Source: Authors' calculations

Table 6: **High and Low Duty Specification**

This is clearly seen in Figure 3. A preliminary duty below the median increases the hazard by an average of 6.2 percentage points, an increase of 37% relative to the hazard in the absence of a case (Table 4). When the preliminary duty is above the median, the hazard increases by 24.5 percentage points, which corresponds to a remarkable 146% increase in the hazard relative to the absence of a case.

By contrast, the results involving the impact of final AD duties are surprising. The key insight is that the impact of the final duty depends on what happened *during* the course of the investigation. When a high duty is levied in the preliminary stage, there is very little additional impact when the final duty is levied.[35] By the time the final duty is levied, most of the spells that will fail have already done so. There is very little additional attrition due to the final duty. On the other hand, when a low duty

[35]Cases with preliminary duties above (below) the median almost always also have final duties above (below) the median. In our data, over 90% of the cases where the duties are classified as low–low or high–high.

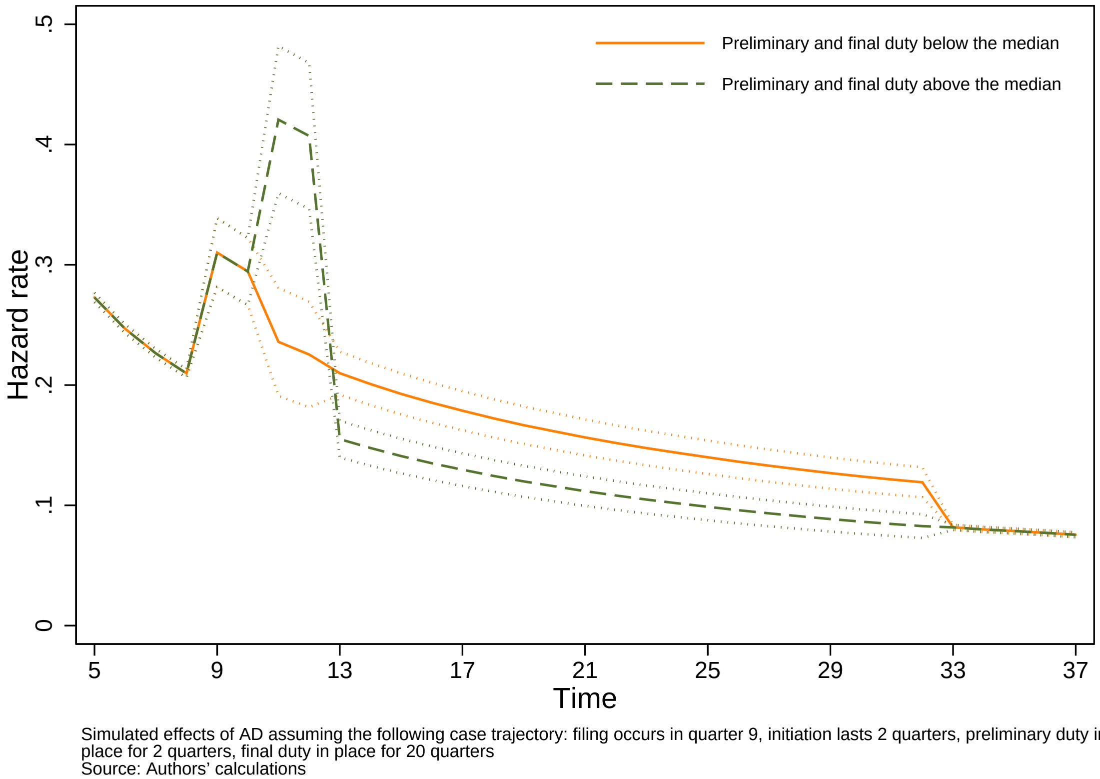


Figure 3: **High and Low Duties**

is levied in the preliminary stage, the final duty has a significant effect on the hazard. A final duty below the median increases the hazard by an average of 4.3 percentage points, an increase of almost 39% relative to the hazard in the absence of a case.

Our findings seemingly indicate lower final duties have a stronger effect than higher duties. This, however, is a bit misleading. Lower duties increase the hazard whenever they are applied, both during the preliminary and final phases. On the other hand, higher duties, have an extraordinarily large impact during the preliminary phase, but have little additional effect during the final stage. For all intents and purposes the entire hazard effect of a high duty case occurs during the preliminary phase.

The simulation results presented in Table 5 further clarify the differential long-run effect of low and high duties. As seen, lower duties have a smaller, but more persistent impact, while higher duties have a large impact for a brief period of time. At the end of the preliminary phase, cases with high duties experience far more failure than those with low duties. However, by the end of the long-run (24 quarters) final duty phase cases with low duties have greater failure.

Estimates shown in the second column of Table 6 results in qualitatively similar results to those from the first column where we use dummies to identify how high AD duties are. In Table 7 we examine how the size of the effect changes with the level of AD duty. With a 10% AD duty, the hazard increases by 37.5% in the preliminary phase and 33.5% in the final phase. Increasing the duty by 20 percentage points increases the hazard by about 20 percentage points in both phases as well. An additional increase of 20 percentage points in the duty, from 30% to 50%, generates different results. The hazard in the preliminary phase is now 134.7% higher than if there were no duties imposed, while in the final phase the hazard decreases slightly, by -2.4%, though the effect is not statistically significant. This large difference occurs because AD duties of 50% are above the median generating different effects. Another increase of 20 percentage points in the duty increases the preliminary phase hazard by 14 percentage points to 148.7%, while the hazard in the final phase barely changes. A further increase to 90% increases the preliminary phase hazard by some 11 percentage points to 159.5%. This indicates that the hazard increases at a decreasing rate as AD duties increase.

	Increase in Hazard Rate	
Size of AD Duty	Preliminary Phase	Final Phase
10%	37.5%	33.5%
30%	57.9%	51.9%
50%	134.7%	-2.4%
70%	148.7%	-2.6%
90%	159.4%	-2.8%

Table 7: **The Size of the AD Duty and the Hazard Effect**

6 Robustness

We now examine the robustness of our results. Due to space considerations we only report results for the phases specification.[36] We first discuss the potential endogeneity of the filing of an AD petition and duration of trade spells. We then perform two additional types of robustness exercises. We begin by re-estimating the model after addressing possible measurement issues in our trade data. We then compute alternative simulation exercises (similar to those in Tables 4 and 5) where we make different assumptions about the timing of the case filing. We also simulate when we evaluate the exogenous variables at values other than their means.

6.1 Concerns with Endogeneity of Filing

In our last robustness check we examine the issue of endogeneity between the duration of a trade spell and the filing of an AD petition. The chief concern is that the filing of an AD petition may itself be a function of the duration of a spell. In order to examine the relationship between the duration of a spell of trade and the filing of an AD case we estimate a different hazard model. We now make the failing condition the *filing* of an AD petition, rather than the cessation of trade. In this alternative specification there are two types of spells. One type involves spells of trade that are never subjected to a filing — these spells are identical to those used in the estimation of the hazard of trade ceasing, except that the failing condition is never observed. The second type involves spells with a filing. Because we are now interested in determinants of filing, not overall duration of trade spells, we only use these spells up to the point of the filing of a case.

We use the same set of variables as in our other specifications with two exceptions. First, since AD investigations usually focus on the named countries' market share, we

[36] We also estimate our other specifications with these adjustments. Results for other specifications are consistent with those discussed in the text and are available on request.

Time (ln)	0.012
	(0.012)
Initial market share (ln)	0.062***
	(0.006)
Change in market share	-0.001
	(0.007)
Weighted distance (ln)	0.401***
	(0.051)
COV unit values (ln)	-0.057***
	(0.006)
GDP (ln)	0.077***
	(0.010)
Contiguity	0.696***
	(0.086)
Common language	-0.203***
	(0.033)
Tariff (ln)	0.023***
	(0.003)
Transport (ln)	0.085***
	(0.011)
Constant	-9.403***
	(0.720)
Observations	3,470,472
Log-Likelihood	-7,520
ρ	0.216**

Standard errors in parentheses with *, **, *** denoting significance at 10%, 5%, and 1%.
Source: Authors' calculations

Table 8: **Probability of Filing an Antidumping Petition**

use the initial market share rather than the initial value of imports. Second, we add the change in the market share from the previous quarter as growth is often a factor in AD investigations. Our findings are reasonable: countries with higher initial market share, further away from the U.S., from larger economies, and a different language are more likely to be subjected to an AD filing. Filings are more likely in products where there is a smaller variation in unit values, and which face higher tariffs and higher transportation costs. The change in the market share has no effect on the likelihood of a filing. Most importantly for our purposes, the length of a spell of imports does not affect the likelihood of a filing. Thus, we conclude that we likely do not have an endogeneity problem in our hazard estimates.

6.2 Alternative Estimation Specifications

Adjusting for Gaps between Spells

We explore whether eliminating short gaps between spells affects our results. The significance of short gaps has been discussed in a number of papers in the duration of trade literature (Besedeš and Prusa, 2006a; Hess and Persson, 2011; Görg, Kneller, and Muraközy, 2012). Our concern is that spells separated by a short period of inactivity (no trade) might be more appropriately treated as one longer continuous spell. For example, suppose a trade relationship has two active spells, five and seven quarters in length, separated by one quarter with no observed trade. Should that one quarter of inactivity be interpreted as a failure? Or might it be more sensible to presume the short gap is not economically meaningful and therefore treat the relationship as having one 13-quarter long spell? If the latter, are our results changed?

We adjust the data to treat short gaps as benign, where we define "short" as a gap of 1-, 3-, or 6-quarters. For short gaps we act as if there were positive trade during the gap which leads us to merge two (or more) separate spells into one longer spell. In the 3-quarter gap adjustment, for instance, four consecutive quarters of no trade would be interpreted as a true break in service while a gap of three or fewer quarters will be viewed benignly and the spells on either side of the gap will be merged.[37]

The gap adjusted results are given in columns (2)–(4) of Table 9. For convenience we also present the relevant coefficients from our phases specification in column (1). If the high frequency of data is driving failures, rather than spells actually failing, we would expect that the gap adjusted data would reflect either smaller hazard effects or a complete lack thereof. Our results indicate the opposite. All coefficients of interest remain significant and most increase in magnitude as the length of the gap adjustment increases.

[37]For example, a relationship with two spells, say five quarters and seven quarters, separated by a three quarter gap will be treated as a single 15 quarter spell.

	Benchmark (1)	Gap Adjustment: One-quarter (2)	Gap Adjustment: Three-quarter (3)	Gap Adjustment: Six-quarter (4)	Unchanged HS Codes Only (5)
Product with a case	−0.061***	−0.059***	−0.047***	−0.055***	−0.073***
	(0.005)	(0.005)	(0.005)	(0.006)	(0.007)
Non-named country	0.059***	0.068***	0.089***	0.119***	0.036***
	(0.007)	(0.007)	(0.008)	(0.009)	(0.012)
Named, case dropped	−0.081***	−0.082***	−0.141***	−0.109***	−0.103***
	(0.022)	(0.024)	(0.029)	(0.035)	(0.039)
Initiation phase	0.361***	0.468***	0.526***	0.451***	0.232***
	(0.031)	(0.034)	(0.037)	(0.041)	(0.069)
Preliminary duty phase	0.448***	0.519***	0.495***	0.293***	0.551***
	(0.041)	(0.046)	(0.051)	(0.056)	(0.081)
Final duty phase	0.111***	0.176***	0.238***	0.288***	0.115***
	(0.017)	(0.019)	(0.022)	(0.025)	(0.033)
Constant	3.168***	2.868***	2.658***	2.811***	3.191***
	(0.033)	(0.032)	(0.034)	(0.038)	(0.045)
Observations	5,417,711	5,260,098	5,515,611	6,023,571	3,115,512
Log-Likelihood	−2,641,405	−2,213,098	−1,787,405	−1,493,467	−1,469,299
ρ	0.208***	0.197***	0.215***	0.278***	0.208***

Standard errors in parentheses with *, **, *** denoting significance at 10%, 5%, and 1%.
Source: Authors' calculations

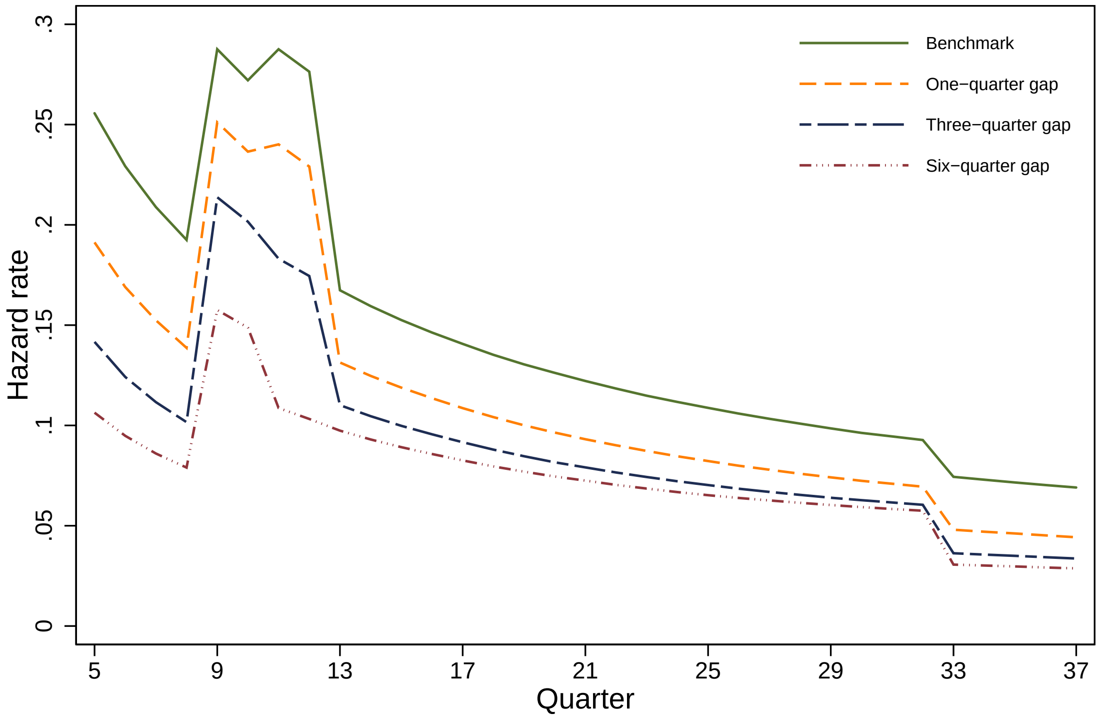
Table 9: **Alternative Specifications**

The hazard profiles for each of the gap adjusted data as well as the benchmark data are plotted in Figure 4. Merging spells separated by gaps reduces the hazard, as one should expect since it creates longer spells. More relevant for our research question is the finding of continued large hazard effects resulting from an AD case under every gap adjustment. The *relative* effects of an AD case with gap-adjusted data is remarkably consistent. With our benchmark data (i.e., no adjustments for gaps) we noted there would be 66% fewer active spells at the end of a typical case than if there was no case. With one-quarter gaps eliminated there would be 69% fewer active spells, while with 3- and 6-quarter gaps eliminated there would be 68% and 62% fewer active spells.[38]

Product Code Changes

We were concerned by the potential role played by product code changes. Codes are often changed for a variety of reasons, with new codes introduced and old codes discontinued. These redefinitions are potentially problematic as they could possibly

[38]Full results reported in Appendix B.



Simulated effects of AD assuming the following case trajectory: filing occurs in quarter 9, initiation lasts 2 quarters, preliminary duty in place for 2 quarters final duty in place for 20 quarters
Source: Authors' calculations

Figure 4: **Typical Case in Gap Adjusted Data**

introduce artificial failure, where a spell ends not because the actual flow of trade ceased, but because the code is no longer used. In our benchmark analysis we code such spells as being either left-censored and right-censored. We now examine whether this benchmark approach affects our results. We do so by limiting our sample to only those codes which were not changed between 1990 and 2007 using the Pierce and Schott (2012) concordance of U.S. 10-digit HS codes.[39] As seen in column (5) of Table 9, our results are qualitatively unchanged as compared to the benchmark results.[40]

[39]We chose to investigate the constant set of codes, rather than reclassifying codes using the Pierce and Schott (2012) algorithm. Their approach involves synthetic HS codes which unify all codes that belong to the same code family. This approach is a problem for our study. In our application a single synthetic code contains a mix of HS codes, some that were included in a case and others that were not. This makes it difficult to characterize whether the synthetic code was included in an investigation.

[40]The HS codes that change in our sample account for 55% of all observed U.S. imports over this time period. These figures are comparable to those reported by Pierce and Schott (2012). They

Left-censoring

Left-censored spells present a challenge in estimation. Since the exact starting date of such spells is not observed, it is difficult to combine them with fully observed spells. We now explore one way of getting a handle on how much our benchmark estimates are affected by dropping the left-censored spells. Our approach will involve using pre-1993 data as initial condition. We begin by creating a new dataset where we presume the first quarter we observe data is Q1–1993. In this new restricted sample any spell observed active in Q1–1993 is considered left-censored. Unlike in our original dataset, however, we can use pre-1993 trade information as a control for left-censoring. In our estimates we use two pieces of information for every relationship: (i) the number of quarters during which a relationship was active between Q2–1990 and Q4–1992 and (ii) the average quarterly value of trade during this period.[41]

In Table 10 we estimate our phases specification on three samples: the original benchmark sample, the restricted sample which starts in 1993 without left-censored spells, and the restricted sample with left-censored spells. The benchmark results are included in the table for ease of comparison.

If we exclude left-censoring spells in our restricted sample (i.e., starting Q1-1993) we have 4,564,033 observations. The lost spells (relative to our benchmark) are spells that started between Q2-1990 and Q4-1992. If we include the left-censored spells our sample size increases to 6,993,583 observations.

As can be seen from Table 10 our attempt to account for left-censored spells yields qualitatively similar results for most variables. In terms of variables relevant for AD cases, there are several interesting findings. First, in our restricted sample with left-censored spells countries supplying an AD-affected product which themselves are not part of the petition face a higher hazard than countries which supply unaffected prod-

report that between 1989 and 2004 43% of all products accounting for 59% of all U.S. imports are in HS codes that changed.

[41] We thank Andrew Bernard for suggesting this approach.

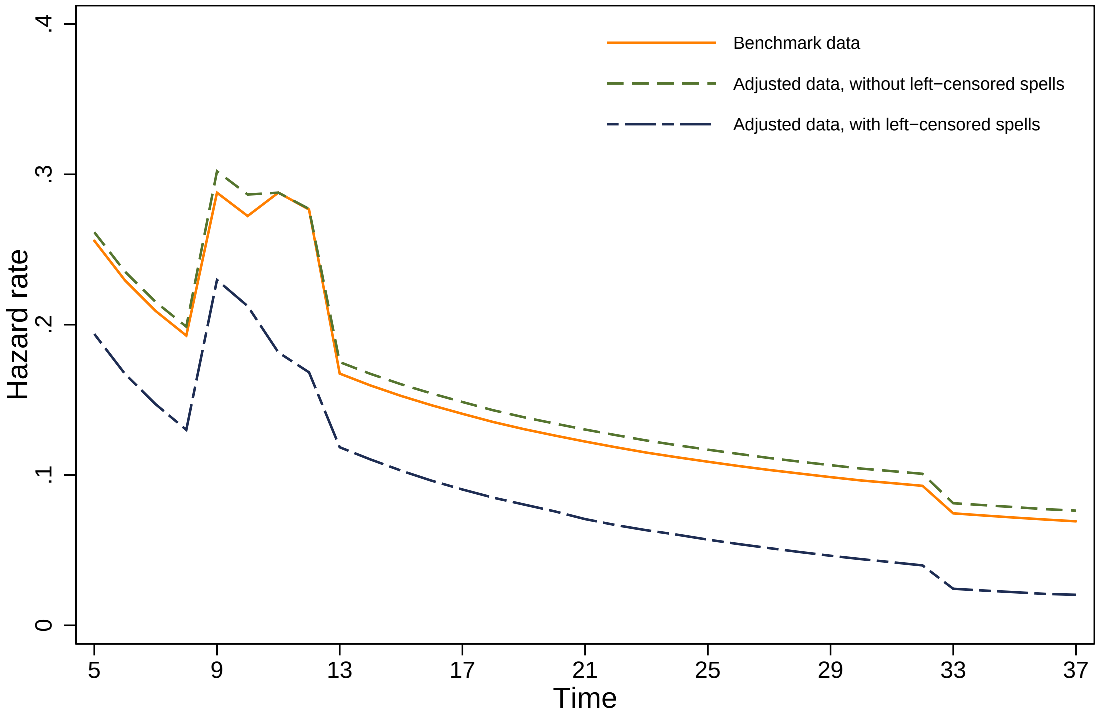
	Benchmark (drop left censored spells)	Restricted sample: Without left censored spells	Restricted sample: With left censored spells	Restricted sample and drop covariates with missing values: Without left censored spells	Restricted sample and drop covariates with missing values: With left censored spells
Product with a case	-0.061***	-0.056***	-0.061***	-0.042***	-0.042***
	(0.005)	(0.005)	(0.004)	(0.004)	(0.004)
Non-named country	0.059***	0.055***	0.108***	0.055***	0.108***
	(0.007)	(0.007)	(0.007)	(0.007)	(0.006)
Named, case dropped	-0.081***	-0.094***	0.031	-0.161***	-0.027
	(0.022)	(0.027)	(0.024)	(0.026)	(0.024)
Initiation phase	0.361***	0.379***	0.445***	0.318***	0.377***
	(0.031)	(0.037)	(0.030)	(0.035)	(0.028)
Preliminary duty phase	0.448***	0.422***	0.386***	0.359***	0.301***
	(0.041)	(0.047)	(0.033)	(0.045)	(0.032)
Final duty phase	0.111***	0.114***	0.201***	0.060***	0.135***
	(0.017)	(0.019)	(0.016)	(0.018)	(0.015)
Constant	3.168***	3.223***	3.110***	1.793***	1.187***
	(0.033)	(0.035)	(0.035)	(0.005)	(0.011)
Observations	5,417,711	4,564,033	6,993,583	5,793,746	9,119,621
Log-Likelihood	-2,641,405	-2,279,043	-2,676,840	-2,889,434	-3,433,640
ρ	0.208***	0.213***	0.230***	0.222***	0.243***

Standard errors in parentheses with *, **, *** denoting significance at 10%, 5%, and 1%.
Source: Authors' calculations

Table 10: **Robustness – Left-censoring and missing values**

ucts. In both our benchmark and our restricted sample excluding left-censored spells the "product with a case" and "non-named country" effects offset each other. Second, in our restricted sample with left-censored spells countries that were named but were dropped from the case experience a small increase in hazard. Third, and most importantly, in terms of the impact of AD, including left-censored spells *increases* the effect of the initiation and final phases, but somewhat reduces the effect of the preliminary duty phase. Nevertheless, our main takeaway from this exercise is that our main finding are robust — AD actions increase the hazard with the effect strongest during the initiation phase, followed closely by the preliminary duties phase, with the final duties phase having roughly half of the effect of the preliminary duties phase.

In Figure 5 we plot the predicted hazard corresponding to estimates in Table 10 using our benchmark simulation assumptions about the evolution of an AD case. As seen, there is very little difference between the estimated hazard using our full sample and the sample that starts in 1993 (excluding left-censored spells). Including left-censored spells reduces the overall hazard profile. This is not surprising given that



Simulated effects of AD assuming the following case trajectory: filing occurs in quarter 9, initiation lasts 2 quarters, preliminary duty in place for 2 quarters final duty in place for 20 quarters
Source: Authors' calculations

Figure 5: **The Impact of Including Left-censored Spells**

a number of left-censored spells have longer duration and some even span the entire sample period. Both considerations serve to reduce the predicted hazard. Nevertheless, the effects of an AD case in the restricted sample (with or without left-censored spells) are qualitatively consistent with our benchmark results.

Covariates with many missing values

As discussed in section 4 several of our control variables have many missing values. There is some concern that our parameter estimates are affected by dropping so many spells due to missing values. In the last two columns of Table 10 we examine the consequence of excluding the handful of covariates that account for nearly all the missing values — GDP, distance, common language, contiguity, and the coefficient of variation of unit values. As seen, when we both exclude these covariates and also account for left-censoring our sample includes almost 90% of the 10,423,157 quarterly HS-country observations. The remaining observations are used to compute the two variables reflecting left censored spells. As seen in Table reftab::leftcensoring the parameter estimates are qualitatively similar to those in our benchmark sample.

6.3 Alternative Starting Point of a Case

In all our graphical depictions of the effect of an AD action we have assumed a specific timing for each event. Most of the timing assumptions were based on statutory provisions. However, we had to pick a starting date for the filing. In our benchmark simulations we have assumed the investigation started in quarter nine of a spell which is the median starting point for an active spell in our sample. One can certainly consider alternative simulations where the investigation starts in a different quarter. To see the impact, we compare the simulated effects of a case from our benchmark plots (quarter 9) with those where the simulations assume different starting times for the investigation: in quarter 5, quarter 13, etc.

In Figure 6 we plot the percentage change in the hazard associated with each phase relative to the baseline hazard when there is no case. For example, look first at the hazard effect of the preliminary phase. In our benchmark simulation (quarter nine starting point) the hazard increases by 84.6% relative to what it would have been had there been no case. By contrast, if the case were initiated in the fifth quarter we find the hazard increases by almost 73%. If the case were initiated in quarter 13, the hazard increases by almost 92% relative to what it would have been if there were no case. In other words, the relative impact of AD during the preliminary phase is larger the longer is the duration of relationship when the filing is initiated.

A similar pattern is seen for the AD's effect during the initiation phase. Namely, the impact of AD increases in the duration of the affected spell. On the other hand AD's effect during the final phase is fairly constant as starting time varies. Thus, our

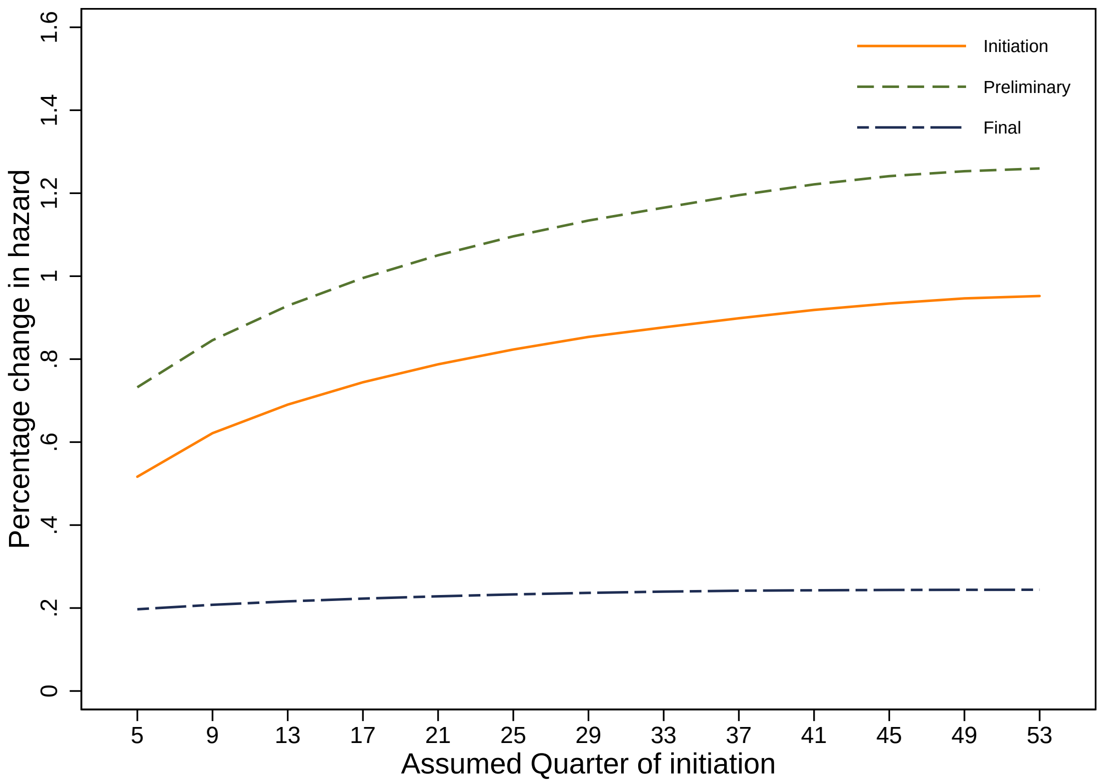


Figure 6: **Hazard Effects Relative to "No case" by Investigation Phase and Quarter of Filing**

analysis shows that the relative effect actually increases the later the case is initiated.

7 Discussion and Implications

We have shown that AD petitions and investigations have a harmful effect on the ability of affected trade spells to survive, drastically increasing the hazard of trade ceasing and driving many affected countries to abandon the U.S. market. We now offer another perspective on whether these effects are meaningful. We examine this issue by creating a new dataset where we estimate the hazard of market absence ceasing (i.e., the supplying country returning to the U.S. market). This will allow us to comment on whether AD causes named countries to be less likely to return to the U.S. market. For this exercise spells are defined by absences from the U.S.

market after the first and any subsequent failure. In this new dataset the spells of absence "fail" when the relationship becomes active again and the supplying country returns to the U.S. market. Our dataset is limited to only those spells where we have observed imports of that product from that country.[42] Thus, we estimate the likelihood of re-entering the market conditional on the length of absence from the market.

Table 11 presents results from estimating the equivalent of our phases specification. We use the same explanatory variables as in our previous regressions, except for those variables which are defined by active spells (e.g., the initial value, tariffs, and transportation costs). In lieu of these control variable we instead use average tariffs and transportation costs associated with active spells from other countries present in the market. The coefficient of variation of unit values is also based on the observed unit values of other countries' participation in any given quarter. Finally, to get a full picture of the effect of AD on market return we add a dummy for countries subject to an AD order indicating the period after the order was revoked. This variable identifies whether AD activity has an effect on named countries even after the AD order is removed, potentially indicating a long run effect of AD which persists even after the original order is revoked.

We begin with some general comments. The results indicate that the longer a supplying country is absent from the market, the less likely it is to return to it. Distance has no effect on the likelihood of re-entry, while contiguity and common language both make a return more likely. Larger countries are more likely to return to the market. Returns are more likely for products where there has been a case. Countries that were not named in a petition are less likely to return to the market. Similar to our results on the effect of AD on market exit, countries not named in a petition do not look any different from suppliers of products which never were affected

[42]In other words, we do not include instances when a country was never observed to have exported a product to the U.S. during this time period.

Time (ln)	-0.251***
	(0.001)
Weighted distance (ln)	-0.004
	(0.003)
GDP (ln)	0.145***
	(0.001)
Contiguity	0.142***
	(0.006)
Common language	0.032***
	(0.003)
COV unit values (ln)	0.030***
	(0.000)
Average tariff (ln)	0.005***
	(0.000)
Average transport cost (ln)	0.000
	(0.001)
Product with a case	0.069***
	(0.004)
Non-named country	-0.069***
	(0.006)
Named, case dropped	0.092***
	(0.028)
Initiation phase	-0.095***
	(0.035)
Preliminary duty phase	-0.307***
	(0.033)
Final duty phase	-0.188***
	(0.016)
After AD order removal	-0.111***
	(0.025)
Constant	-4.609***
	(0.029)
Observations	12,271,942
Log-Likelihood	-3,779,030
ρ	0.242***

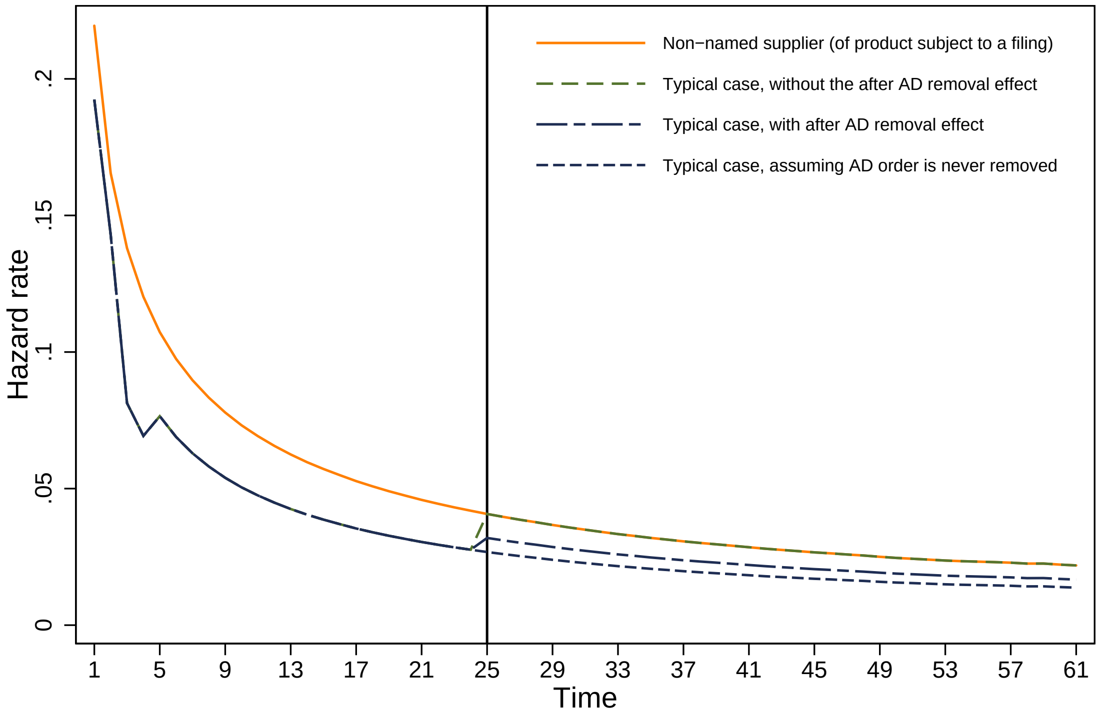
Standard errors in parentheses with *, **, *** denoting significance at 10%, 5%, and 1%.
Source: Authors' calculations

Table 11: **Hazard of Re-entry**

by an AD filing. Countries which were named in a petition, but were dropped from the case are more likely to return to the market.

In Figure 7 we illustrate the hazard of market absence ceasing assuming that a named supplying country exits the U.S. market as soon as the AD petition is filed. This is denoted as quarter 1 in the figure. Following the statutory provisions, we assume the initiation and preliminary duty phases last two quarters each; we also assume the final duty phase lasts 20 quarters. The key event, therefore, happens at the end of quarter 24 — when the AD duty is revoked. Therefore, quarter 25 is the first time when the supplying country can service the market without the order.[43]

[43] We note again that the specific timing is chosen for illustrative purposes. The effects we discuss are present irrespective of the actual length of each phase.



Simulated effects of AD assuming the following case trajectory: initiation lasts 2 quarters, preliminary duty in place for 2 quarters, final duty in place for 20 or all remaining quarters
Source: Authors' calculations

Figure 7: **Effects of an AD Case Market Re-entry**

Four hazards are presented. First, in order to provide some context for the case effects, we plot the profile of a supplying country of the same product who is not named in the petition. The other profiles are for three types of named countries. For one type we assume there are no effects on "re-entry" after the AD order is removed. For the second type of named country we simulate the chance of "re-entry" allowing for the post-revocation effect to be present. For the third type of named country we assume the AD order is not revoked.

As seen, countries named in an AD petition are less likely to return to the market throughout the duration of an AD case. During the initiation phase they are 13% less likely to return than a supplying country who was not named in the petition. The effect is even larger during the preliminary duty phase, where we find a named supplying country is 42% less likely to return than a non-named supplying country.

During the final duty phase a named supplying country is 32% less likely to return to the market. That countries named in AD petitions are less likely to return while the case is ongoing is not surprising.

However, it is surprising that the effects of an AD case lingers even after the order is removed. As our estimates clearly indicate, countries named in an AD petition are less likely to return *after* the order is removed: 22% less likely to return than a non-named countries. Note that the likelihood of the return after the AD order is removed does increase relative to the likelihood of the return during the final duty phase, by about 17%. This increase erases about one-third of the difference in the likelihood of return between non-named countries and named countries during the final duty phase. In other words, the removal of the AD order leaves in place approximately two-thirds of the effect of a final duty order. This is a striking indication of the long run effect of an AD order. In the case of AD, long run is longer than the duration of the actual order. This is evidence that having been subjected to an AD order makes named countries more reluctant to return to the market both during and after the case. This reluctance may stem from either a fear of future AD action, in essence a long-term *in terrorem* effect

8 Conclusion

In this paper we set out to examine whether antidumping petitions and duties have an effect on the extensive margin of U.S. imports. Using quarterly trade flow data in combination with detailed case-specific data we find that AD actions have a large effect, causing named countries to completely abandon the U.S. market. We find not only that is the extensive margin effect is large, but also that there are large differences across different stages of a case. An AD action increases the hazard in every stage, but interestingly we find that the smallest effect is during the final AD duty phase and larger effects during the initiation and preliminary phases.

We also find that the effect varies by the size of the AD duty. Cases with lower duties have a more persistent effect throughout the duration of the case, while cases with large duties have a much more dramatic effect during the initiation and preliminary phase but little effect in the final phase. Our results are robust to a number of concerns, including gaps between spells, changes in HS codes, left-censoring, omission of observations due to missing covariates, and different assumptions about the timing of a case's initiation. We also show that the likelihood of filing an antidumping petition against a particular country does not depend on the length of that country's exports to the U.S.

While we do not directly test for sunk costs, our results can be interpreted in light of the literature on heterogeneous firms and sunk costs of trade (Melitz, 2003). From the sunk costs perspective, the costs on exporters from AD are larger than those captured by standard price and volume effects. We have shown AD causes trade relationships to fail and we have also found a lower likelihood of a return. Both findings suggest AD may have long lasting welfare effects for both domestic consumers (who may have to pay higher prices due to a lack of competition) and also foreign suppliers (who may not recover the sunk costs required to service the destination market). Consequently, AD may have far greater welfare effects than generally recognized.[44] From an importing country point of view, if AD eliminates efficient foreign suppliers, less efficient domestic firms will continue to service the market which imposes additional costs on the economy (Pierce, 2011). Using a calibrated model of trade with heterogeneous firms and monopolistic competition Ruhl (2014) also argues antidumping has larger welfare effects than generally recognized.

[44]Vandenbussche and Zanardi's (2010) results are consistent with this concern. They focus on trade flows (i.e., the intensive margin) and find AD reduces trade in products where no duty is imposed but which are similar to those where duties are imposed. We note that Egger and Nelson (2011) also study AD spillover effects and come to the opposite conclusion – namely, that there are no spillover effects from AD duties

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A Adjustments to AD Case Data

A few adjustments to the AD case data were required, generally involving cases that were settled or were filed multiple times. AD duty orders are eventually revoked when it is determined that dumping or injury is no longer present. For some cases, the revocation date is missing; this is often true for settled cases. If no revocation information is available, we assume cases were revoked five years after the last decision date recorded in the *Global Antidumping Database* (Bown, 2012). A five year duration was chosen as that is the length of time of the sunset provision.

A separate issue occurs when the same HS-country pair is named in multiple cases. This most typically occurs when the initial petition was rejected very quickly (e.g., in the first quarter). In such cases the domestic industry generally corrects the deficiencies in its petition and re-files at some later date. In such circumstances it seems sensible to focus on the trade impact of the re-filed case. When this occurred we focused on the case which resulted in the imposition of a final duty (i.e., we dropped the quickly rejected petition).

A related issue occurs when the same HS-country pair is named in cases involving different products. For example, a HS code might be included in a case involving "steel plate" and also a case involving "steel sheet." As disaggregated as the HS classification is, it is well documented that the HS system nonetheless has codes that are broader than desired. In the handful of cases where the same HS-country pair was named in multiple cases and the cases involved different product names, we assigned the HS code to the case involving a larger value of trade.

B Extended Summary Tables of Results

	Average increase in hazard – Phase Specific						Entire Duration of Case	
	Initiation		Preliminary		Final			
Specification	Nominal	Relative	Nominal	Relative	Nominal	Relative	Nominal	Relative
Basic	—	—	—	—	—	—	3.6%	32.7%
Phases	10.7%	62.2%	12.9%	84.6%	2.1%	20.8%	3.7%	33.5%
AD duties								
≤ median	11.3%	59.8%	6.2%	36.9%	4.3%	38.9%	5.1%	41.4%
> median	11.3%	59.8%	24.5%	145.7%	-0.1%	-0.6%	2.9%	23.9%
Gap adjustment								
1 qtr	12.1%	98.8%	12.8%	119.2%	2.6%	38.5%	4.2%	56.4%
3 qtr	11.8%	130.7%	10.0%	127.8%	2.9%	59.2%	4.3%	77.0%
6 qtr	8.3%	118.2%	4.4%	71.9%	3.2%	78.9%	3.7%	83.2%
Unchanged codes	6.3%	39.2%	15.9%	112.4%	2.0%	22.1%	3.5%	34.9%
Fitted at								
25^{th} pctile	14.3%	33.9%	17.7%	45.6%	3.9%	13.6%	5.9%	19.3%
median	13.5%	44.9%	16.5%	60.7%	3.1%	16.7%	5.1%	25.1%
75^{th} pctile	12.2%	53.8%	14.7%	73.0%	1.9%	20.6%	3.8%	34.1%
Initiation qtr								
5	12.5%	51.7%	14.7%	73.2%	2.3%	19.7%	4.2%	31.1%
9	10.7%	62.2%	12.9%	84.6%	2.1%	20.8%	3.7%	33.5%
13	9.5%	69.0%	11.6%	92.9%	1.9%	21.6%	3.4%	35.0%
17	8.6%	74.4%	10.6%	99.6%	1.8%	22.3%	3.1%	36.2%
21	7.8%	78.8%	9.8%	105.0%	1.7%	22.8%	2.9%	37.1%
25	7.3%	82.3%	9.2%	109.6%	1.6%	23.3%	2.7%	37.9%
29	6.8%	85.4%	8.7%	113.4%	1.5%	23.7%	2.6%	38.4%
33	6.5%	87.7%	8.3%	116.5%	1.5%	24.0%	2.4%	38.9%
37	6.1%	89.9%	7.9%	119.5%	1.4%	24.2%	2.4%	39.1%
41	5.9%	91.9%	7.6%	122.1%	1.4%	24.2%	2.3%	38.9%

Source: Authors' calculations

Table 12: **Impact of AD – Average increase in hazard (Complete Results)**

	Ratio: Spells Surviving AD Case to Spells Surviving with No Case		
Specification	Initiation Phase	Preliminary Phase	Final Phase*
Basic	—	—	36.6%
Phases	75.8%	54.4%	33.9%
AD duties			
≤ median	74.0%	63.4%	23.3%
> median	74.0%	36.8%	37.4%
Gap adjustment			
1 qtr	74.3%	54.6%	31.1%
3 qtr	75.8%	60.2%	32.1%
6 qtr	82.9%	75.3%	38.3%
Unchanged codes	85.5%	56.8%	36.1%
Fitted at			
25^{th} pctile	56.6%	28.6%	6.7%
median	65.1%	39.0%	17.8%
75^{th} pctile	70.9%	47.2%	30.6%
Initiation qtr			
5	69.6%	46.4%	27.2%
9	75.8%	54.4%	34.0%
13	79.2%	75.2%	65.3%
17	81.6%	63.4%	42.9%
21	83.4%	66.3%	46.1%
25	84.7%	68.6%	48.7%
29	85.8%	70.4%	50.8%
33	86.5%	71.8%	52.5%
37	87.2%	73.1%	53.9%
41	87.8%	74.2%	54.8%

* Entire duration of case

Source: Authors' calculations

Table 13: **Impact of AD – Spells surviving through end of each phase (Complete Results)**