Antidumping and the Death of Trade

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Abstract

We investigate the extent to which antidumping actions eliminate trade altogether. Using U.S. antidumping case data along with product–level data on exports to the U.S. at the quarterly level we investigate the effect antidumping petitions and cases have on the hazard of exports ceasing. We find that antidumping cases have a large effect, increasing the hazard by more than fifty percent. We identify important differences in the effect of each specific phase of a case, with the initiation and the preliminary duty phases having a larger effect than the final phase, when the final antidumping duty is levied. There are also important differences with respect to the size of duties. Cases with higher duties face a much higher hazard in the preliminary phase, but display almost no effect on hazard in the final phase. In other words, the effect of large duties on the hazard rate occurs before the final duty is invoked. By contrast, cases with lower duties display a lower, but more persistent effect on the hazard, which proves to be more detrimental in the long run as more spells are eliminated during the duration of the case.

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1 Introduction

For more than two decades antidumping (AD) has been the “go to” policy for trade protection. While other trade policies sometimes garner the headlines, AD is easily the most frequently initiated form of administrative protection (Bhagwati, 1989; Zanardi, 2006). Since the mid-1970s, AD has consistently outpaced all other forms of administrative protection combined (Prusa, 2001; Bown, 2011a). One reason AD is so popular is that it delivers substantial protection with exports from subject countries falling (on average) by 50–60% (Prusa, 2001). Moreover, in an important finding Staiger and Wolak (1994) find that the trade is significantly reduced during the course of the 12-month investigation so even if the dumping claim is ultimately rejected, the domestic industry benefits due to the “investigation” effect.

In this paper we examine a different, and heretofore overlooked aspect of AD protection: namely the impact of AD on the ability of a subject supplier to maintain any market presence. Specifically, we examine whether AD protection drives export suppliers entirely out of the market. The earlier empirical literature on the trade effects of AD has focused on the impact of AD on the intensive margin of trade (Hansen and Prusa, 1993; Lichtenberg and Tan, 1994; Staiger and Wolak, 1994; Prusa, 1997, 2001). In this paper, by contrast, we focus on the extensive margin. With AD duties often exceeding 65%, and in many cases exceeding 100%, one would expect the firms located in the country subject to such high duties to be unable to compete and have their export trade volume (to the AD using country) fall to zero. If this is indeed the case, the literature on heterogenous firms and sunk costs of trade imply the costs associated with AD are larger than those captured by the standard (price and volume) trade effects. From the exporter point of view, the firm may not be able to recover the sunk costs required to service the destination market.

1In our sample of cases, the average duty, either preliminary or final, is over 53%, while some 15% of cases have either duty in excess of 100%.
If exporting firms could anticipate this happening (i.e., because certain destination markets or industries were known to be aggressive users of AD or because of the firms’ prior experience with AD in other destination markets) then they would be unwilling to commit the sunk costs in the first place. Consequently, AD might have far greater effects than the prior literature suggests.\(^2\) From an importing country point of view, if AD eliminates efficient foreign suppliers then less efficient domestic firms will continue to service the market (Melitz, 2003) thereby imposing additional costs on the economy (Pierce, 2011).

Using U.S. antidumping case data along with highly disaggregated product–level quarterly export data to the U.S. we investigate the effect antidumping petitions and cases have on the hazard of exports ceasing. We estimate duration models of export trade (Besedeš and Prusa, 2006b; Besedeš, 2008, 2011) augmented with information on AD cases. We find that antidumping cases have a large effect, increasing the hazard by more than fifty percent. We then allow for differential effects by stage of the investigation. This generates important and insightful differences in our understanding of the impact of AD. Our estimates imply the initiation and the preliminary duty phases have a larger effect than when the final antidumping duty is levied. By the time the final duty is levied most of the effect on the extensive margin has already happened – exporters have ceased serving the market. There are also important differences with respect to the size of duties. Cases with higher duties face a much higher hazard in the preliminary phase, but display almost no effect on hazard in the final phase. In other words, the harmful effect of large duties on the hazard rate occurs before the final duty is invoked. By contrast, cases with lower duties have a lower, but more persistent effect on the hazard rate. In the long run, this persistent effect proves to be more detrimental as more spells are eliminated during the duration of

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\(^2\)Zanardi and Vandenburgu (2010) results are consistent with this concern. While they focus on trade flows (i.e., the intensive margin) they nonetheless find AD reduces trade in products where no duty is imposed but which are similar to those where duties are imposed.
2 Background and Related Literature

AD is easily the most popular form of administered protection, accounting for more trade actions than all other forms of discretionary protection combined. Gallaway, Blonigen, and Flynn (1999) estimate that AD imposes more costs on the U.S. economy than any other trade policy. Messerlin (1985, 1989) argues the costs of the EU’s use of AD is equivalent to the costs of the agricultural protection.

Beyond its obvious public relations attractiveness (what politician could be in favor of unfair trade?), AD has a number of characteristics that prove advantageous to domestic industries. First, an AD petition is quite likely to deliver protection. In the United States over the last decade about 70% of AD petitions have resulted in final duties. In other countries, the “success” rate of AD can exceed 90%. Second, AD duties are quite large. Blonigen (2006) documents that the average AD duty in the U.S. now exceeds 65%. About one-fifth of U.S. AD duties exceed 100%. Third, AD protection is quite timely. From the date the petition to the date of the final order takes about 12 months (Staiger and Wolak, 1994). Fourth, “temporary” or “preliminary” duties are imposed quite early in the investigation. In the U.S. three out of four petitions result in preliminary duties and these duties are generally imposed about six months after the petition filing. Because the methods for calculating preliminary and final AD duties are similar, preliminary AD duties are usually similar to final AD duties, which is to say they are also very high. As a result, petitioning industries can get sizeable tariffs imposed on their foreign competitors even if the case is ultimately rejected. Fifth, AD duties can be imposed for an indefinite period of time. In the Uruguay Round revisions to the antidumping code included a mandatory sunset review. Yet, this review has not resulted in shorter

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3 In other countries, the “success” rate of AD can exceed 90%.
4 Double check
5 The correlation between the two in our data is 0.92.
duration of AD duties (Prusa, 2011). It is rare for AD duties to be removed sooner than five years after the imposition of final duties. More typically, AD duties in the U.S. are imposed for more than 10 years.

There are also several characteristics that reduce the attractiveness of AD. To begin with, the higher duties are only imposed on the specific countries named in the petition and found to have caused material injury. Said differently, AD protection is country-specific. Over time, U.S. industries have learned to compensate for this characteristics and now often file against multiple supplying countries in a single petition (Prusa, 2011). In some cases, U.S. industries file a sequence of petitions to expand the scope of the protection (Durling and Prusa, 2006). Second, AD protection is product-specific where the products are generally identified at the 8- or 10-digit tariff line code. In some cases a dispute will involve a single code and in others a product (e.g., ball-bearings) might involve a dozen or more codes. As a result, two very similar products – 4mm steel ball-bearings and 4mm aluminum ball-bearing – might have very different tariff levels depending if one has an AD duty and the other does not.

Given the frequency of use and the sizeable duties associated with AD protection, a substantial literature has emerged studying the trade effects of AD protection. Most of the literature uses the tariff product codes to analyze trade effects although several significant early papers based their analysis on industry trade data (Hansen and Prusa, 1993; Lichtenberg and Tan, 1994; Staiger and Wolak, 1994). With one exception (discussed below) all the literature has used annual trade to examine trade effects. In this study, on the other hand, we use quarterly export data which allow us to more precisely measure when AD duties were imposed.

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6. The tariff codes often distinguish by product size (4mm versus 10mm), material (metal versus plastic), and chemistry (high carbon or high alloy steel).

7. We have found that our duration results are qualitatively unaffected by the aggregation issue. The more highly aggregated data give more precise results and more accurately capture the actual trade affected by the duties.
Regardless of the differences in data used, researchers have consistently found significant trade effects. Using annual line-item tariff data from the 1980s Prusa (2001) found that AD duties reduce the volume of trade by 50–60%. Prusa (1997) documented that much of lost trade was diverted to other suppliers. Bown and Crowley (2007) found similar effects and, in addition, found that subject suppliers redirect trade to other destination markets. Konings, Vandenbussche, and Springael (2001) use annual line-item tariff codes to study the effects of European AD on import diversion from subject importers to non subject countries. In contrast to the U.S. they find trade diversion in the EU caused by AD actions to be rather limited.

Staiger and Wolak (1994) is the only paper that focuses on the impact of preliminary duties. Using annual industry level trade data they find about half of the trade volume effect of the AD order occurs during the period of investigation. Thus, even if subject imports are found not to have caused material injury, the domestic industry will have gained substantial protection for a substantial period of time. In fact, the benefit to the domestic industry during the course of the investigation Staiger and Wolak dub some industries “process filers” as they “gain” even if the dumping allegation is ultimately rejected.

We use quarterly trade data at the line-item product level. Not only does this allow us to more precisely measure the timing of importing survival, it also allows us to identify the timing of the impact of AD. When do buyers stop purchasing from subject countries? Is it when the case is initiated? Is it when the preliminary duty

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8Prusa (1997, 2001) used product definitions based on the TSUSA schedule which was the official U.S. coding scheme until the late 1980s. Since the late 1980s all WTO members have used the “harmonized system” (HS) to classify trade.

9Bown and Crowley use the term “trade depression” when referring to the decrease in trade from subject suppliers, “trade diversion” when referring to the increase in trade from non subject suppliers, and “trade deflection” when referring to the redirection of trade from subject suppliers to other destination markets.

10Preliminary duties are almost always in effect for less than a year. This complicated their attempt to derive within–year effects using annual data. They adjusted the annual data for the number of months within a calendar year with no protection, with preliminary protection, and with final protection.
is levied? Or, is it when the final duty is levied? Given that nearly all the preceding literature uses annual trade data, these questions have not been addressed. Staiger and Wolak (1994) is the exception. While they use annual data, they assume there is no trade effect until the preliminary duty is levied and they then interpolate trade effects based on the timing of the preliminary and final duties.\footnote{If the preliminary duty were imposed on August 1st, then Staiger and Wolak assume trade was unaffected for seven months (and therefore occurred as it did prior to the investigation) and was unimpeded for the last five months of the year. Trade flow following the preliminary duty is derived from this identifying restriction.}

Our approach merges the antidumping literature with the duration of trade literature. Besedeš and Prusa (2006a) offered the first analysis of duration of trade, showing that the duration of U.S. imports was very short with a median of four years in length. Short duration has been shown to be a universal characteristic of international trade in both product–level investigations\footnote{see Nitsch (2009) for imports of Germany, Jaud, Kukenova, and Strieborny (2009) for exports of a large set of countries, Besedeš and Prusa (2011) and Carrère and Strauss-Khan (2012) for developing countries’ exports.} or firm–level investigations\footnote{Görg, Kneller, and Muraközy (2012) for Hungarian firm–level export data or Cadot et al. (2011) for exporters from four African nations.}.\footnote{Görg, Kneller, and Muraközy (2012) for Hungarian firm–level export data or Cadot et al. (2011) for exporters from four African nations.}

Besedeš and Prusa (2006b) and Nitsch (2009) showed that the hazard of exports of differentiated goods to the U.S. ceasing is lower than that for homogeneous goods, while Besedeš (forthcoming) showed that the signing of NAFTA had a differential effect on the hazard of exports ceasing of the three member countries. To our knowledge, this is the first attempt to investigate the effect of a temporary trade policy on the duration of trade.

\section{Data}

\subsection{Data Sources}

We use data from two sources. Trade flow data come from the U.S. Census \textit{U.S. Imports of Merchandize Trade}. We use data from April 1990 through December 2006.
U.S. imports are recorded at the 10-digit HS level from virtually every trading partner. While trade flow data are available on a monthly basis, we aggregated the data to the quarterly level with the aim of reducing the noise that is present in monthly data. After aggregating to the quarterly level we have 10,423,157 quarterly product–supplier observations. Along with trade volume information, data also include information on the quantity shipped,\textsuperscript{14} duties collected, as well as the cost, insurance, and freight value of imports. These additional data allow us to construct information on unit values, (collected) tariff rates, and transportation costs.

The second source of data is the \textit{Global Antidumping Database} constructed by Chad Bown (2010). Bown’s database contains information on all U.S. antidumping cases from January 1979 through April 2010. Over the whole period a total of 1,229 antidumping cases were initiated in the U.S. However, given the availability of quarterly import data our main analysis only looks at cases initiated between April 1990 until December 2006: a total of 833 cases involving 2,179 distinct HS codes. Some HS codes are involved in multiple cases.\textsuperscript{15} Once we link each case with every HS product involved we have 8,127 observations on a country–case–product triplet. We use two pieces of information on every antidumping petition: the precise timing of the case as well as the size of any duty order while the case is active. Prior to explaining the arduous process of linking the two data files, we provide a brief overview of how an antidumping investigation in the U.S. is carried out and describe possible outcomes.

\textsuperscript{14}While value is always coded, it is not uncommon for quantity information to be missing. Approximately 17\% of the observations have missing quantity data.

\textsuperscript{15}While 947 codes are involved in exactly one case, most are involved in multiple cases. For example, the domestic industry might file a ball-bearing case against Germany and Italy. The HS codes in that investigation would be involved in two cases. A single antidumping investigation typically involves multiple countries (which in our terminology means multiple cases) and hence most HS codes are involved in multiple cases.
3.2 U.S. Antidumping Procedures

Once the domestic industry (or a subset of firms in an industry simultaneously) files an antidumping petition with the U.S. Department of Commerce (USDOC) and U.S. International Trade Commission (USITC), the case proceeds on a precisely specified statutory defined timeline. The USDOC determines whether the foreign firm(s) named in the petition sold their product(s) at less than fair value, i.e., “dumped”. The USITC determines whether the less than fair prices charged by foreign firm(s) resulted in material injury to the petitioning industry. Over the course of about a year each of these agencies makes a series of decisions that determine the size of the dumping duty and whether the duty will be applied. For purposes of our empirical analysis we divide a case into three stages: the initiation phase, the preliminary duty phase, and the final duty phase.

The initiation phase encompasses the time from when the case is initiated until the date the USDOC makes its preliminary duty determination. The initiation stage lasts on average two quarters. During the initiation phase no additional duty is in place so trade may not be affected. However, it is conceivable that the specter of the investigation will either intimidate U.S. buyers from purchasing from the subject firm(s)/country or spur an increase in purchases in order to buy before the duty is imposed.

The preliminary duty phase encompasses the time from the point of the USDOC’s preliminary duty determination to the date of the final dumping determination. If the dumping determination at the end of the initiation phase is affirmative, as it is in the vast majority of cases, a preliminary antidumping duty is levied. From that point

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16There are two exceptions to this. All cases reported in the Global Antidumping Database which were rejected in the initiation stage were rejected after one quarter due to adverse information being revealed or a negative preliminary USITC determination. In some instances, there is no affirmative decision on dumping (i.e., preliminary estimate of zero percent dumping), yet the USITC makes an affirmative decision with respect to injury. In this situation the case proceeds but no preliminary duty is levied. In such cases the initiation phase lasts on average about 4 quarters.
on U.S. buyers will pay an antidumping duty in addition to any other tariffs. This means duties are in place while the investigation is taking place and before any final judgement is made. As Staiger and Wolak (1994) document, the preliminary duty has a significant impact on trade flows. In the vast majority of cases the USITC’s final injury determination is the critical factor as to whether final duties are or are not imposed.

The final dumping and injury determinations are both usually made about four quarters after the initiation date.\(^{17}\) Consequently, the average length of the preliminary duty phase is generally about two quarters. As with its preliminary dumping determination, the USDOC’s final dumping determination is almost always affirmative. In most cases the preliminary and final duty margins are comparable, with a correlation over 0.9 in our sample.

Once the USDOC and USITC final determinations are made the case is either terminated (if either determination is negative) or duties are imposed (if both determinations are affirmative). Unlike some other forms of administrative protection and unlike policy in other countries, under U.S. law the final duty is imposed without any need for input or approval from the President. Once the final duty is imposed (which happens in about two-thirds of U.S. antidumping cases) the case enters what we refer to as the final duty phase, or simply the final phase.

The length of the final phase depends on how long the duty is in place. Historically an antidumping duty was revoked after the foreign firm demonstrated that it was no longer dumping. In some cases this meant the duty was revoked after just a few years but in other cases the duties were in place for multiple decades. The Uruguay Round of the WTO included a sunset provision which required every antidumping order be reviewed after it had been in place for five years.\(^{18}\) Mandatory sunset review went

\(^{17}\)Due to special circumstances such as an exceedingly complicated case or court challenges, the final determination can be delayed for a quarter or two.

\(^{18}\)As Moore (2006) documents, mandatory sunset review is far from equivalent to mandatory termination.
into effect in January 1995. In our data, the average length of an antidumping order is 36 quarters or nine years. In our analysis the final phase is in effect from the date of the final determination to when the order was revoked.\footnote{Prusa (2011) discusses the effect of mandatory sunset on the duration of U.S. antidumping orders.}

Overall, U.S. antidumping cases can be classified in one of six ways based on outcomes:

1. the petition may be immediately rejected during the initiation phase, most often due to the case being rejected at the preliminary injury determination;
2. final duties imposed after having had preliminary duties in place;
3. final duties imposed without having preliminary duties in place;
4. case ultimately rejected after having had preliminary duties in place;
5. case ultimately rejected without having had preliminary duties in place; and

Table 1 contains information on the average length of each phase of a case, depending on its ultimate outcome. It will be useful for our purposes to define a “typical” case. We will assume that a typical case consists of a 2-quarter initiation phase, a 2-quarter preliminary duty phase, and a 20-quarter final duty phase. The typical case begins in quarter nine of an active spell of trade, the median point of filing a petition in our data.

### 3.3 Linking the Data

Bown’s *Global Antidumping Database* includes information on the 10-digit HS code of products involved in each antidumping case over the time period we investigate. We
are therefore able to link the two datasets and match trade flow data with antidumping case data. Bown’s database provides precise dates on every facet of virtually all AD cases. We use these dates to define the three phases a case may go through (initiation, preliminary, and final). Rather than having a single observation on each case, we create quarterly observations while the case is ongoing. For example a case which goes through the initiation phase for two quarters and is rejected at the injury determination after two quarters of the preliminary phase will consist of four observations, one for each quarter while the petition was investigated. A case which is not rejected but results in an antidumping duty order, which is in place for five years, will consist of 24 observations, two each for the initiation and preliminary phase, and twenty for the final antidumping duty phase. The quarterly case data consist of five key variables: the subject country, the product it is accused of dumping, and three dummies identifying which quarters belong to which phase of the case. Such transformation of antidumping case data results in our sample growing from 8,127 singular observations on country–case–product triplets to 196,865 quarterly observations on all country–case–product triplets between 1989 and 2007. When making this transformation we ignored whether there was positive trade in a given product and quarter from a particular country. Any zero trade quarter will be eliminated when antidumping case data are merged with trade flow data.

For certain types of cases we needed to make a few adjustments to the data, pri-

\[\begin{array}{|c|c|c|}
\hline
\text{Case Rejected at Prelim Phase} & \text{Initiation phase} & \text{Preliminary duty phase} \\
\hline
\text{Case Rejected at Final Phase, No Prelim Duties} & 3.6 & \text{Antidumping duty phase} \\
\hline
\text{Case Rejected at Final Phase, Prelim Duties} & 2.2 & 1.7 \\
\hline
\text{Duty Imposed, No Prelim Duties} & 4.3 & 36.3 \\
\hline
\text{Duty Imposed, Prelim Duties} & 2.2 & 1.9 \\
\hline
\text{Settled/Withdrawn} & 1.8 & 2.2 \\
\hline
\end{array}\]

Table 1: Average Length of Every Stage by Case Outcome

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marily those that were settled and those that were filed multiple times. Antidumping
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, which is most
often true for settled or withdrawn cases, which may never have had an antidumping
duty order in place. We arbitrarily decided to code such cases as revoked five years
after the last date recorded in the data. Five years were chosen as that is the length
of time of the sunset provision, while being fully cognizant that the sunset provision
was not in place in the earlier part of our data. Imposing a cap on the length of a
(settled) case eliminates the possibility of overlapping cases. We chose this approach
as we are interested in whether any aspect of an antidumping case hastens the demise
of a trade relationship. In the rare instance of the same country–product pair be-
ing named in multiple cases we only kept data on the first case which resulted in at
least the imposition of a final duty. In most instances of such multiple cases the first
petition was rejected in the first quarter, and was refiled at some later point.\textsuperscript{21}

When merging antidumping case data with trade flow data we successfully match
54,736 case–relevant observations or some 28\% from a total of 196,865 dumping ob-
servations. Of the 54,736 matched observations, 6,875 belong to cases for which every
quarterly observation is matched to an active trade flow observation, or put differently
to uninterrupted spells of exporting to the U.S. In other words, some 87\% of case–
relevant observations belong to instances where a country subject to an investigation
ceases to export the product in question while the investigation or case is still active.
Such a large number of failed spell of exporting to the U.S. suggests that antidumping
investigations do cause exports to cease. Among the 142,489 observations that are
not matched to any trade flow while the case is ongoing, 55,375 (39\%) observations
belong to trade flows which are not observed while the case is ongoing, with 36,472
belonging to country–product pairs which are never observed in trade flow data. Put

\textsuperscript{21} In such circumstances it seems sensible to focus on the trade impact of the refiled case.
differently, 66% of unmatched observations are associated with products listed in a petition that a country never actually exported to the U.S. The remaining 87,114 of unmatched case–relevant observations belong to country–product pairs which do not have active exports while the case is ongoing, but are observed to have exported to the U.S. at some other point, with 26,036 observe before the case was filed.

### 3.4 Descriptive Statistics

With the antidumping case information matched to quarterly trade data, in order to be able to estimate the hazard of exports to the U.S. ceasing, we converted quarterly data to active spells of service for every trade relationship. A trade relationship is defined as a country–product pair, while a spell of service consists of consecutive quarters during which the particular trade relationship is active. There are a total of 2,660,147 spells of service reflecting 748,430 trade relationships. There are 448,660 trade relationships with multiple spells of service, roughly 60% of all relationships. Multiple spells account for 2,360,037 spells or 89% of all spells. Such dominance of multiple–spell relationships owes in part to the quarterly nature of our data and in part to the fact that many trade relationships are not active on a consistent basis. In terms of the distribution of the number of spells per trade relationship (see Table 2, there are 299,770 one–spell relationships and 114,466 two–spell relationships, which jointly account for some 55% of all trade relationships. The largest observed number of active spells in a relationship is 26, the longest of which is three quarters long (on three separate occasions).\(^{22}\)

The 54,376 observations on antidumping cases are contained in 10,033 spells reflecting 4,724 trade relationships, of which 2,615 are characterized by multiple spells. While our interest is in examining the hazard of exports to the U.S. ceasing and the effect of antidumping activities on that hazard, the nature or our data compels us to

\(^{22}\text{This particular relationship involves French exports of Boys’ suit–type jackets and blazers made from material other than cotton, corduroy, or blue denim (HS code 6203322050).}\)
Table 2: Distribution of Active Spells across Relationships

<table>
<thead>
<tr>
<th>Spells across relationships</th>
<th>Observed duration</th>
</tr>
</thead>
<tbody>
<tr>
<td>Total number of spells in a relationship</td>
<td>Number of relationships</td>
</tr>
<tr>
<td>1</td>
<td>299,770</td>
</tr>
<tr>
<td>2</td>
<td>114,466</td>
</tr>
<tr>
<td>3</td>
<td>74,127</td>
</tr>
<tr>
<td>4</td>
<td>54,965</td>
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<tr>
<td>5</td>
<td>42,696</td>
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<tr>
<td>6</td>
<td>33,853</td>
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<tr>
<td>7</td>
<td>27,147</td>
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<td>8</td>
<td>22,498</td>
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<td>9</td>
<td>18,816</td>
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<td>10</td>
<td>15,628</td>
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<td>12,542</td>
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<td>7,521</td>
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<td>3,747</td>
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<tr>
<td>25</td>
<td>1</td>
</tr>
<tr>
<td>26</td>
<td>1</td>
</tr>
<tr>
<td>Total</td>
<td>748,430</td>
</tr>
</tbody>
</table>

The table above shows the distribution of active spells across relationships. Spells across relationships are observed for varying durations, with 58.76% of spells lasting only one quarter. The average observed duration is 3.91 quarters. The maximum observed length of a spell is 67 quarters, with 0.78% of the spells lasting this long. This suggests that most spells are short-lived, with the majority lasting just one quarter.
data reveal that trade is even shorter than one observes using annual data, the standard time metric in the duration of trade literature. It may be puzzling that such a large number of spells last less than the starting point of a typical case. However, the distribution of spell lengths prior to a filing is different.\textsuperscript{23} The average duration prior to a filing is 12.09 quarters, while the median is 9 quarters. For comparison purposes, to reach 92\% of spells in this distribution one needs to look at all spells which at most 32 quarter prior to a filing.

\section*{3.5 Other Data}

We utilize some additional data in the following section where we estimate the hazard of exports ceasing. As has become standard in analysis of trade duration, we use the volume of trade with which a spell starts measured on a logarithmic scale. We use GDP measured in constant dollars obtained from the World Bank’s \textit{World Development Indicators}. Gravity variables, weighted distance, common language, and contiguity come from CEPII’s gravity variables dataset. Finally, we use the additional information available in the Census trade data 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 the approach of Besede\v{s} and Prusa (2006b). The variation in unit values speaks to the extent to which the product is differentiated, with more differentiated products exhibiting a larger variation in unit values.

\textsuperscript{23}Not that this is not the distribution of overall spell lengths, until a spell fails. Rather, it simply taking account of how long spells have been active for at the point of filing of an antidumping case.
4 Results

4.1 Methodology

We estimate the hazard of exports to the U.S. ceasing by using random effects probit. It does not impose the constraint that the hazard of two subjects be proportional to each other, as is the case with proportional hazard estimators, the semiparametric Cox for continuous data or the cloglog specification for discrete data. Unlike the Cox estimator, unobserved heterogeneity is more easily accounted for with random effects probit. In order to use probit we need to specify how time/duration enters the regression. Not including duration explicitly as an explanatory variable would imply assuming that the hazard does not depend on duration, which has been shown not to be true for duration of trade. We chose to use the natural logarithm of time, where time is measured in quarters.

While the use of probit has the aforementioned advantages, it comes with a complication that the interpretation of estimated 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 it depends not only on its own standard error, but also on that of the time/duration variable, as well as every other variable used in the regression. The appropriate way to ascertain the significance of the effect of a covariate is to plot the estimated hazard evaluated at some specific values (usually means of every variable, or a specific profile for a subject of interest) as well as the estimated hazard with all variables held at the same values except the variable of interest. Along with these two fitted hazards, one needs to plot the corresponding confidence intervals. If the two confidence intervals do not overlap, the effect of the variable of interest is significant. Sueyoshi (1995) provides a detailed
explanation, while Hess and Person (2011) and Besedeš (forthcoming) use it in the context of duration of trade. Since the primary tool of the effect of a variable is to plot the fitted hazards, our main focus in presenting our results will be to describe the fitted hazards, though we also present estimated coefficients for every specification we use.

In our estimation, in addition to the data and variables outlined in the previous section, we differentiate between two types of products and three types of countries. There are two types of products in our analysis: products which at some point were a part of an antidumping case and those which never were. To investigate whether antidumping petitions involve products for which the hazard of exports ceasing is fundamentally different at any point in time we coded a ‘product with a case’ dummy. This variable identifies all products which at some point were listed in a petition. Our results are virtually identical if we only use information on all products with a filing at some point.\textsuperscript{24}

We can differentiate between three types of countries, based on their role in the petition. A petition specifically names firms and countries in which those firms are based. These are the subject or named countries. In our simplest specification, one dummy variable (named country) is used to identify countries named in the petition. This dummy is equal to one as long as the case is ongoing, irrespective of the exact phase it is in. Thus, the subject country dummy could be called an active case dummy. In our preferred specification we break up the named country dummy into three mutually exclusive dummies identifying each of the three possible phases of a case (initiation, preliminary, and final). A second country type is a country which was initially named in the petition, but had the case against it dropped or settled at some point. We refer to these countries as 'named, case dropped’ and identify them with a dummy variable. In a given case a country which had the case against

\textsuperscript{24}Available on request.
it dropped, while the case continued against other countries will have two dummy variables equal to one. As long as the case is active against this particular country, only the named country dummy will equal one. As soon as the case against it is dropped, but it continues against some other country named in the original petition, the named country dummy equals zero, but the 'named, case dropped' dummy equals one. The last country type are those countries that were not named in the original petition but had positive exports to the U.S. If the case against all countries is dropped then all case-relevant dummies are equal to zero.

A note on the observations we can use in estimation. From the 10,423,157 quarterly observations in our data, we use 5,417,711 in estimation, or 52%. This reduction occurs for two reasons: left censoring and missing variables. Left censored spells are all spells active in the first observed quarter. They are left censored since their exact start is unobserved by the econometrician. The first observed quarter comes in two flavors. One is the very first quarter we observe, the second quarter of 1990, while the other is the first quarter of a newly introduced product code. There is a similar issue for all spells observed in the very last quarter in the data, the right censored spells. Unlike right censoring, left censoring cannot be dealt with easily in estimation forcing us to drop all such spells. This causes us to lose 3,525,356 observations. The remaining 1,480,090 quarterly observations that we do not use are lost due to missing explanatory variables unrelated to case information. Our results are not significantly different if we do not use all the other variables, either qualitatively or quantitatively.

We estimate three different specifications. In the basic specification we investigate whether the sheer existence of a case has an effect on the hazard of exports ceasing. Thus, in terms of antidumping case information, we use the filed product dummy and the three country type dummies, of which one, subject country, identifies the

\footnote{As documented by Pierce and Schott (forthcoming) a not uncommon occurrence.}

\footnote{Available on request.}
effect of a case/petition. 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 subject country dummy into the three phase dummies, initiation, preliminary, and final. Since in the second specification we use only dummies to pick out the effect of a phase, in our third specification we investigate whether the size of both the preliminary and final duties have a differential effect. To that end, instead of using dummies for the preliminary and final phases, we use two new dummies for each phase. These identify whether either duty is below or above the median duty, which is 30.95% for preliminary duties and 36.41% for final duties. We are interested in whether the size of the duty has a nonlinear effect. We refer to these three specifications as the simple specification, the phases specification, and the non–linear duties specification. We discuss each in turn.

4.2 Simple Specification

Before turning our attention to case relevant variables, we summarize the effect of all other variables. These are virtually identical across all specifications, so we describe them here only to save space. Time/duration is estimated to have a large negative coefficient, indicating that the hazard of exports ceasing decreases with time, a standard result in the duration of trade literature. The larger the initial volume 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 GDP reduces hazard, while Canada and Mexico, the two countries sharing a border with the U.S. have a significantly lower hazard. Countries sharing a common language with the U.S. have a somewhat larger hazard, while tariffs and transportation costs weakly reduce the hazard. Finally, products with a higher variation in unit values across suppliers have a lower hazard, which is expected as long as such variation speaks to the extent of product differentiation
across suppliers. All of these effects are in line with the duration of trade literature. The estimated parameter $\rho$ captures the extent of data variation attributable to unobserved heterogeneity. It is estimate to be around 0.2 in most of our specifications, which is in line with the value obtained by Besedes (2012b) using annual 10-digit level U.S. data.

<table>
<thead>
<tr>
<th>Parameter</th>
<th>Coefficient</th>
<th>Standard Error</th>
</tr>
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<tbody>
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<td>(0.001)</td>
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<tr>
<td>Initial volume (ln)</td>
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</tr>
<tr>
<td>Weighted distance (ln)</td>
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<td>(0.003)</td>
</tr>
<tr>
<td>COV unit values (ln)</td>
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<td>(0.001)</td>
</tr>
<tr>
<td>GDP (ln)</td>
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<td>(0.001)</td>
</tr>
<tr>
<td>Contiguity</td>
<td>-0.257***</td>
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</tr>
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<td>Common Language</td>
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<td>(0.003)</td>
</tr>
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<td>Tariff (ln)</td>
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<td>(0.000)</td>
</tr>
<tr>
<td>Transportation costs (ln)</td>
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<td>(0.000)</td>
</tr>
<tr>
<td>Product with a case</td>
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<td>(0.005)</td>
</tr>
<tr>
<td>Non–named country</td>
<td>0.059***</td>
<td>(0.007)</td>
</tr>
<tr>
<td>Named, case dropped</td>
<td>-0.085***</td>
<td>(0.022)</td>
</tr>
<tr>
<td>Named country/case dummy</td>
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<td>(0.015)</td>
</tr>
<tr>
<td>Constant</td>
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<td>(0.033)</td>
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<table>
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<th>Summary Statistics</th>
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</tr>
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<tbody>
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<td>Observations</td>
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<td></td>
</tr>
<tr>
<td>Log-Likelihood</td>
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<td></td>
</tr>
<tr>
<td>Number of id</td>
<td>518,717</td>
<td></td>
</tr>
<tr>
<td>$\rho$</td>
<td>0.208***</td>
<td></td>
</tr>
</tbody>
</table>

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

Table 3: Simple Specification

As results in Table 3 indicate, products which were a part of an antidumping
petition at some point do have a different hazard than those which did not have a petition filed. The hazard of exports to the U.S. ceasing associated with such products is lower. As described above, whether the difference is statistically significant depends on whether the fitted hazard of the two types of products lie outside each other’s confidence intervals. To that end in Figure 1 we plot the fitted hazard for both product types with all variables held at their overall sample mean and all case relevant dummies set to zero. In this and every subsequent figure the confidence interval is plotted with dotted lines around the fitted hazard. While the difference between the two fitted hazards is statistically significant, as there is no overlap between confidence intervals, the difference is not economically meaningful. It averages 1.1 percentage points across all 61 quarters plotted and ranges from a high of 2.4 percentage points in the first quarter to the low of 0.007 in quarter 57.  

Case relevant dummies indicate that countries not named in a petition face a higher hazard. Exports from these countries face the same hazard as exports of products which never were a part of a petition. The two coefficients are statistically not different from each other and the plots of fitted hazards for products not involved in a petition and non–named countries exporting products listed in a petition are virtually indistinguishable. Countries named in a petition, subject countries, face a larger hazard, while countries which were part of a petition but were eventually dropped from a continuing case face a lower hazard. Figure 2 indicates that the hazard for the three different roles a country may find itself in during an active antidumping case are significantly different from each other. The difference between a named and non–named country averages 2.5 percentage points higher hazard for subject countries during the course of a typical case (24 quarters), and varies between a high of 3.4 percentage points in the first quarter of a case and a low of 2 percentage

\[27\] In this instance as well as any other such comparison of the effect of a variable, the difference between the two hazards identifying the effect of a variable decreases with duration.  

\[28\] Available of request.
points in the last quarter of an active case.

When plotting the fitted hazard we make a set of specific assumptions about the timing of a case. We assume that a case starts in the ninth quarter of an active spell. Nine quarters is the median point in duration of a spell when a petition is filed. We then assume the case proceeds along the lines of what we termed a typical case above: initiation lasts two quarters, the preliminary dumping duty phase lasts two quarters, and the final duty phase lasts 20 quarters, for a total of 24 quarters during which a case is active. We are clearly assuming here that all cases go through all phases, which is true of the majority of U.S. antidumping cases. As a result of this assumption, the hazard for the three roles a country may play in a case is identical at any point in a spell while the case is not active (either before quarter 9

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29 We obtain this figure by examining duration spells until a petition is filed.
30 Space constraints preclude us from examining case–outcome specific differences.
Filing occurs in quarter 9, initiation lasts 2 quarters, preliminary duty in place for 2 quarters final duty in place for 20 quarters
or after quarter 32). We maintain this timing structure for all subsequent plots and investigate in the robustness section whether the size of the effect of a case depends on when the petition is filed. In the case of countries initially a part of a petition and dropped at some point, we assume they were dropped from the case during the preliminary phase, though it varies from case to case.

### 4.3 Phases Specification

The simple specification does not allow for the possibility that different phases of a case have a different effect on the hazard of exports ceasing. In the phases specification we introduce separate dummies for each phase, initiation, preliminary, and final. Summing the three dummies would recreate the named country dummy from our simple specification. All other variables are as before. We remind the reader that rather than using the actual duties levied in the preliminary and final phases, we use a dummy to identify cases which progressed to those phases. As we will show later, the size of a duty does have a differential effect on the hazard. However, if one were to plot a fitted hazard from our phases specification with dummies and a specification with actual duties and fitted them at their means, the two fitted hazards would be rather similar.\(^{31}\) Since the only change we are making in this specification is to split the subject dummy into three new dummies, in Table 4 we only report the case relevant dummies, along with the product and country role dummies.\(^{32}\)

Coefficients for dummies identifying products with a case and non-named countries are unchanged, while that for named, case dropped countries slightly decreases in absolute value. The phase specific dummies indicate that the effect of an antidumping case on the hazard of exports ceasing depends on the phase of the case. The

\(^{31}\)Available on request.

\(^{32}\)The only other variable whose coefficient changes is time/duration which is not unexpected since the three new dummies may result in a different behavior of the hazard. However, even that coefficient changes only in the third decimal spot by one unit. Complete results are available on request.
## Table 4: Phases Specification

<table>
<thead>
<tr>
<th>Phase</th>
<th>Coefficient</th>
<th>Standard Error</th>
</tr>
</thead>
<tbody>
<tr>
<td>Product with a case</td>
<td>-0.061***</td>
<td>(0.005)</td>
</tr>
<tr>
<td>Non–named country</td>
<td>0.059***</td>
<td>(0.007)</td>
</tr>
<tr>
<td>Named, case dropped</td>
<td>-0.081***</td>
<td>(0.022)</td>
</tr>
<tr>
<td>Initiation phase</td>
<td>0.361***</td>
<td>(0.031)</td>
</tr>
<tr>
<td>Preliminary duty phase</td>
<td>0.448***</td>
<td>(0.041)</td>
</tr>
<tr>
<td>Final duty phase</td>
<td>0.111***</td>
<td>(0.017)</td>
</tr>
</tbody>
</table>

| Observations   | 5417,711   |
| Number of id   | 518,717    |
| Log-Likelihood | -2,641,405 |
| \( \rho \)     | 0.208***   |

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

Hazard is higher throughout an ongoing case. However, the likelihood that exports will cease is higher in the initiation and preliminary phases than it is in the final phase. It is maximized in the preliminary phase when preliminary duties are levied. Phase specific effects are clearly seen in Figure 3 where we plot the hazard profile of a typical case as defined above, against the hazard profile with all phase dummies set to zero, thus isolating the pure effect of a case. The hazard increases immediately in the initiation phase, and even more so in the preliminary phase. The first four quarters pack a mighty punch in terms of the increase in the hazard. By the time the final phase starts, the increase in the hazard decreases below either of the two early phases, but still remains above what the hazard would be in the absence of a case.

The increase in the hazard during an ongoing case is statistically significant. It is also economically significant. Relatively speaking, the increase in the hazard in the first two phases is tremendous. The hazard in quarter nine in the absence of a case is 17.9%. The filing of an antidumping petition increases the hazard to 28.8%. While an increase of almost 11 percentage points may appear nominally small, it represents
Filing occurs in quarter 9, initiation lasts 2 quarters, preliminary duty in place for 2 quarters, final duty in place for 20 quarters

Figure 3: **Phase-Specific Effects**
a sudden 61% jump in the likelihood that a spell will cease. At the start of the preliminary phase the jump is even larger relative to the absence of a case at 83.5% increase in the hazard. The hazard increases in the final phase, but at a relatively modest 19% at the start of the phase. The quarterly average nominal increase in the hazard due to a case is 3.7 percentage points, or almost a third of the hazard (29.7%). The initiation phase increases it by an average of 10.7 percentage points or 62.2%, the preliminary phase increases it by an average of 12.9 percentage points or 84.6%, and the final phase increases it by an average of 2.1 percentage points or 21%.

To put things in a different perspective, we offer two calculations. We first ask whether we can identify an equivalent increase in the hazard that we can account for with changes in another variable. We then examine how many additional spells disappear due to a case. For the first exercise we look to three variables commonly used in the duration of trade literature and intuitively easy to apply. We ask whether common border, GDP, and the initial volume of exports can give us the same increase in the hazard as an antidumping case, and if the answer is yes, then how large should those differences be. For common border this is a matter of obtaining the fitted hazards for the two values it can take, while for GDP and the initial volume of exports require us to experiment with different values. We collect our results in the three panels of Figure 4.

Common border is estimated to reduce the hazard. To see whether it has a similar effect to an antidumping case, we plot the fitted hazard for a typical case for a country sharing the border with the U.S. We then take away the existence of a case, but introduce a lack of a common border for the same duration (of a typical case). Unlike the case, the lack of a common border generates a consistent effect over time. On average the lack of a common border generates a higher hazard, but in the two most critical phases (from the perspective of the effect on the hazard), initiation and preliminary, the lack of a common border only generates about a half as much of an
Filing occurs in quarter 9, initiation lasts 2 quarters, preliminary duty in place for 2 quarters, final duty in place for 20 quarters

Figure 4: Equivalent Hazard Effects

28
increase in the hazard.

For changes in the GDP and the initial volume of exports we experimented with different values before we arrived to a set which we present. The increase in the hazard due to a change in the GDP which comes close to matching the effect of the initiation and preliminary phases is a reduction in GDP by three orders of magnitude. This would imply that exports subject to an antidumping investigation have the same hazard profile as a German exporter suddenly relocated and forced to operate as an exporter from Benin, Armenia, or Barbados, or and exporter from Sweden now having the same experience as an exporter from the Comoros or Guinea–Bissau. In terms of the initial volume of exports, if it is reduced by one order of magnitude, it will generate a lower increase in the hazard in the first two phases, but a larger one in the last phase.

The other approach to demonstrating the size of the effect of a case is to conduct the following thought experiment. Start with 10,000 active spells in quarter 9, the quarter when the petition is filed. Then apply the hazard rate with and without a case and compare the number of remaining cases at the end of each phase, assuming all 10,000 spells are part of the investigation, a clearly unrealistic assumption. Entering the preliminary phase, there would be 1,660 spells fewer when an investigation is conducted (5,186 vs 6,846), or 24% less than in the absence of an investigation. Entering the final phase there would be 46% fewer active spells (2,674 vs 4,914). By the time a typical case has run its course and an antidumping duty order is revoked, there would be 66% fewer active spells than if the case never took place (220 vs 638). An antidumping case decimates active trade spells, creating a kind of killing field, and causes exporters to stop exporting.
4.4 Nonlinear Duty Effects

In our last specification we investigate whether the effect of antidumping duties, both preliminary and final, depends on their size. In other words, we investigate whether there are any potential nonlinearities in their effect. Rather than having a single dummy identifying the preliminary duty phase we use two new dummies identifying whether the preliminary duty is below or above the median, which is 30.94%. We similarly define two new dummies identifying whether the final antidumping duty is below or above the median, which is 36.41%. As results in Table 5 indicate there is an interesting dichotomy in the effect of the level of duties, indicating significant nonlinearities. The effect of preliminary duties is arguably expected, with duties above the median increasing the hazard more than duties below the median, by a factor of almost three. This is clearly seen in Figure 5. The hazard during the preliminary duty phase when the duty is below the median is actually lower than during the initiation phase, while in cases where it is above the median, the hazard is much higher than during the initiation phase. While the preliminary duty below the median increases the hazard by an average of 6.2 percentage points, the average increase when it is above the median is four times as large at 24.6 percentage points.

The dichotomy occurs in the effect of the final antidumping duties. The hazard in the final duty stage is always lower than it is during both the initiation and preliminary duty phases. When the final duty is below the median the hazard is on average 4.3 percentage points higher than it would be in the absence of a case. However, when the final duty is above the median, it has no meaningful effect on the hazard. Thus, lower final duties have a stronger effect than do the higher ones. Antidumping duties have two varied effects on the hazard. Lower duties increase the hazard whenever they are applied, with the effect higher for preliminary duties. Higher duties, those above the median, have a tremendous impact during the preliminary phase, as if the entire might of the effect of a case is loaded into the preliminary phase. But they
have no effect during the final stage.

<table>
<thead>
<tr>
<th></th>
<th>Coefficient</th>
<th>Std. Error</th>
</tr>
</thead>
<tbody>
<tr>
<td>Product with a case</td>
<td>-0.060***</td>
<td>(0.004)</td>
</tr>
<tr>
<td>Non-named country</td>
<td>0.058***</td>
<td>(0.007)</td>
</tr>
<tr>
<td>Co-named country</td>
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<td>(0.022)</td>
</tr>
<tr>
<td>Initiation phase</td>
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<td>(0.031)</td>
</tr>
<tr>
<td>Preliminary duty below the median</td>
<td>0.223***</td>
<td>(0.057)</td>
</tr>
<tr>
<td>Preliminary duty above the median</td>
<td>0.742***</td>
<td>(0.061)</td>
</tr>
<tr>
<td>Final duty below the median</td>
<td>0.204***</td>
<td>(0.024)</td>
</tr>
<tr>
<td>Final duty above the median</td>
<td>-0.004</td>
<td>(0.025)</td>
</tr>
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</table>

<p>| | | |</p>
<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
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<tbody>
<tr>
<td>Observations</td>
<td>5,417,711</td>
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<tr>
<td>Number of id</td>
<td>518,717</td>
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<tr>
<td>Log-Likelihood</td>
<td>-2,641,361</td>
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</tr>
<tr>
<td>ρ</td>
<td>0.208***</td>
<td></td>
</tr>
</tbody>
</table>

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

Table 5: The Nonlinear Effects of Antidumping Duties

This dichotomy leaves us with an unresolved question. Which antidumping duty profile is worse and in what sense? The lower duties have a smaller, but persistent impact, while higher duties have a large impact for a brief period of time. We can again perform the same thought experiment and ask how many of the initial (entering quarter nine) 10,000 spells survive through each phase of the case under different antidumping duties. Since the initiation phase is identical under both duty scenarios, the initiation phase eliminates some 52% of spells under both scenarios (4,867 spells survive it). There is a large difference between the two scenarios in the preliminary duty phase, as one would expect. While 2,881 spell survive the preliminary phase under duties below the median, only 1,672 spells do so under duties above the median, almost 42% less. As the case progresses, the higher hazard associated with lower duties causes the number of surviving spells to drop faster, and starting with the
Filing occurs in quarter 9, initiation lasts 2 quarters, preliminary duty in place for 2 quarters, final duty in place for 20 quarters

Figure 5: Nonlinearity of Antidumping Duties’ Effect
beginning of the third year of final duties (tenth quarter to be precise) there are fewer surviving spells under duties below the median than under duties above the median. Duties above the median result in 159 spells surviving the entire length of the case, while duties below the median result in only 99 spells surviving the entire case, some 38% less. Thus, over the length of the case, the persistently higher hazard resulting from duties below the median causes fewer spells to survive than under duties above the median. Such high duties only cause a large reduction during the preliminary phase.

The difference between the two duty scenarios is illustrated in Figure 6 where we plot the number of spells surviving each quarter of a case under the two duty scenarios as a percentage of spells surviving the same quarter in the absence of a case. Thus, the value of 0.86 for both trajectories in quarter 9 implies that the number of active spells after the first quarter of the initiation phase amounts to 86% of spells that would be active in the absence of a case. In other words, the initiation phase eliminates some 14% of spells. Duties below the median have a persistent (almost linear) effect on the relative survival of spells, ever decreasing the number of surviving spell relative to what happens in the absence of a case. While duties above the median cause a steep decline in the number of surviving spells during the preliminary phase and have no effect during the final phase. In the latter phase, in any quarter the number of spells still active amounts to 37% of spells that would be active if there was no case. This number does no change since final duties above the median do not have a distinguishable effect on the hazard.

Of course this need no imply that lower duties are less damaging from an economic welfare point of view. It is possible that the slower rate of decline in the number of active spells under lower duties allows for the beneficial effects (such as lower prices, greater variety) of trade flows to accrue, whereas with high duties the large reduction in spells during the preliminary phase immediately eliminates some of those gains.
We also divided our data in quartiles of the antidumping duties 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 increase the hazard during the preliminary phase only, while having no effect on the hazard during the final phase. These differences are statistically significant whenever two non-consecutive quartiles are compared.

5 Robustness

5.1 Adjusting for Gaps between Spells

We examine the robustness of our results across several different dimensions. We report robustness results only for the phases specification.\textsuperscript{33} We begin by examining

\textsuperscript{33}Robustness results for other specifications do not affect our conclusions qualitatively and are available on request.
whether eliminating short gaps between spells of the same trading relationship affects our results. It has become standard in the duration of trade literature to examine whether short gaps between spells are responsible for high failure rates. Suppose that a trade relationship has two active spells, one of 5 and one of 7 quarters in length, separated by one quarter of no observed activity. It is possible that the one quarter of inactivity is simply a recording mistake and should be more properly treated as a quarter with active service, resulting in one 13-quarter long spell. With quarterly data an argument could also be made that one quarter of inactivity should not be interpreted as a failure of a relationship as not enough time has passed to allow for complete depreciation of all relationship specific information which allows a firm to easily export or renew its exports. We examine three approaches to adjusting gaps between active spells. We, in turn, eliminate all 1-, 3-, and 6-quarter gaps between spells. We do so by treating every 1-, 3-, or 6-quarter gap as having had positive trade service, thus merging together the previously separate spells on either end of the gap. Limiting the adjustment to just single-quarter gaps may be insufficient given the relative shortness of one quarter. It strikes us that examining gaps of more than 6 quarters in length is inappropriate as even a 6-quarter gap might be too long of a period to be entirely attributable to recording errors.

We collect our results in Table 6 where we also present the relevant coefficients from our unadjusted (benchmark) phases specification. 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. Among the coefficients of interest, controlling for the various case-related effects, not a single one looses significance and most increase in magnitude as the length of the adjusted gap increases. The hazard profiles for each of the gap adjusted data as well as the benchmark data are plotted in the upper left panel of Figure 7. Merging together spells separated by gaps reduces the hazard,
as one should expect since it creates longer spells, but more importantly for our immediate purposes, the higher hazard resulting from an antidumping case is quite visible under every adjustment.

A quick side note on the number of observations in the gap–adjusted data is in order. One would expect the number of observations used in estimation to increase when gap–adjusting the data. A new quarter of positive trade service is being added to the data set after all. However, the number of observations decreases from the benchmark to one–quarter gap–adjusted data, and then increases progressively with the length of the gap. The 150,000 observation decrease is due to some one–quarter gaps merging a left–censored spell (the earlier spell) with another spell. While the latter was used in the benchmark estimation, the former was not as all left–censored spells are excluded. The new merged spell is in its entirety a left–censored spell and is excluded from estimation, thus resulting in a decreased number of observations used. We performed our analysis on a constant sample of spells which appear throughout the gap–adjusted data yielding identical qualitative results.34

5.2 Product Code Changes

Our second robustness check has to do with ensuring that failures are not driven by product code changes. The U.S. Census Bureau administers the 10–digit HS codes used to record U.S. trade data. The Census changes import codes on at least an annual basis for a variety of reasons, with new codes being introduced and old codes being discontinued. For any longitudinal study such redefinitions are potentially problematic as they may introduce an artificial failure, where the spell ends not because the actual flow of trade ceased, but because the code is no longer used. Our analysis so far implicitly controlled for certain aspects of code changes. Any code which is discontinued would result in spells active in the last quarter of a code’s

34Available on request.
<table>
<thead>
<tr>
<th></th>
<th>Benchmark gap</th>
<th>One-quarter gap</th>
<th>Three-quarter gap</th>
<th>Six-quarter gap</th>
<th>Unchanged gap codes</th>
<th>Without irregular cases to annual data matched</th>
<th>Quarterly Annualized data</th>
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<tr>
<td><strong>Product with a case</strong></td>
<td>-0.061***</td>
<td>-0.059***</td>
<td>-0.047***</td>
<td>-0.055***</td>
<td>-0.073***</td>
<td>-0.061***</td>
<td>-0.043***</td>
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<td>(0.005)</td>
<td>(0.005)</td>
<td>(0.005)</td>
<td>(0.006)</td>
<td>(0.007)</td>
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<td><strong>Non-named country</strong></td>
<td>0.059***</td>
<td>0.068***</td>
<td>0.089***</td>
<td>0.119***</td>
<td>0.036***</td>
<td>0.059***</td>
<td>0.012</td>
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<tr>
<td></td>
<td>(0.007)</td>
<td>(0.007)</td>
<td>(0.008)</td>
<td>(0.009)</td>
<td>(0.012)</td>
<td>(0.007)</td>
<td>(0.008)</td>
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<tr>
<td><strong>Co-named country</strong></td>
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<td>-0.082***</td>
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<td>-0.138***</td>
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<td></td>
<td>(0.022)</td>
<td>(0.024)</td>
<td>(0.029)</td>
<td>(0.035)</td>
<td>(0.039)</td>
<td>(0.022)</td>
<td>(0.028)</td>
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<td><strong>Initiation phase</strong></td>
<td>0.361***</td>
<td>0.468***</td>
<td>0.526***</td>
<td>0.451***</td>
<td>0.232***</td>
<td>0.355***</td>
<td>0.345***</td>
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<td></td>
<td>(0.031)</td>
<td>(0.034)</td>
<td>(0.037)</td>
<td>(0.041)</td>
<td>(0.069)</td>
<td>(0.032)</td>
<td>(0.045)</td>
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<tr>
<td><strong>Preliminary duty phase</strong></td>
<td>0.448***</td>
<td>0.519***</td>
<td>0.495***</td>
<td>0.293***</td>
<td>0.551***</td>
<td>0.431***</td>
<td>0.451***</td>
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<tr>
<td></td>
<td>(0.041)</td>
<td>(0.046)</td>
<td>(0.051)</td>
<td>(0.056)</td>
<td>(0.081)</td>
<td>(0.042)</td>
<td>(0.062)</td>
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<td><strong>Final duty phase</strong></td>
<td>0.111***</td>
<td>0.176***</td>
<td>0.238***</td>
<td>0.288***</td>
<td>0.115***</td>
<td>0.107***</td>
<td>0.092***</td>
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<tr>
<td></td>
<td>(0.017)</td>
<td>(0.019)</td>
<td>(0.022)</td>
<td>(0.025)</td>
<td>(0.033)</td>
<td>(0.017)</td>
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<td><strong>Constant</strong></td>
<td>3.168***</td>
<td>2.868***</td>
<td>2.658***</td>
<td>2.811***</td>
<td>3.191***</td>
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<td>(0.032)</td>
<td>(0.034)</td>
<td>(0.038)</td>
<td>(0.045)</td>
<td>(0.033)</td>
<td>(0.035)</td>
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</table>

<table>
<thead>
<tr>
<th></th>
<th>Observations</th>
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<th>Log-Likelihood</th>
<th>( \rho )</th>
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<td><strong>Product with a case</strong></td>
<td>5,417,711</td>
<td>518,717</td>
<td>-2,641,405</td>
<td>0.208***</td>
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<td>-2,213,098</td>
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<td>5,515,611</td>
<td>478,296</td>
<td>-1,787,405</td>
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<td>6,023,571</td>
<td>461,912</td>
<td>-1,493,467</td>
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<td></td>
<td>3,115,512</td>
<td>243,335</td>
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<td>5,417,495</td>
<td>518,694</td>
<td>-2,641,259</td>
<td>0.208***</td>
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<tr>
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<td>3,266,646</td>
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<td>-1,723,044</td>
<td>0.181***</td>
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<tr>
<td></td>
<td>1,656,008</td>
<td>436,078</td>
<td>-908,111</td>
<td>0.252***</td>
</tr>
</tbody>
</table>

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

Table 6: Robustness Results
Typical Case in Gap Adjusted Data

Different Fitted Hazard Profiles

Typical Case with Quarterly and Annual Data

Figure 7: Robustness Plots
use to be classified as right-censored, which our estimation approach can take into account. Any code which is introduced at any point during our sample is classified as left-censored and is excluded from estimation.

We now limit our sample to only those codes which have not been changed between 1990 and 2007. To that end we use the Pierce and Schott (2012) concordance of U.S. 10-digit HS codes over time. As column 5 of Table 6 indicates, our results are qualitatively unchanged. Restricting the sample to just the unchanged codes reduces it by some 40% in terms of observations and by more than a half in terms of relationships. An alternative approach is to use the Pierce and Schott (2012) concordance to connect all the changing codes. However, this presents a challenge of a different sort. Their concordance works by introducing what they refer to as a synthetic HS code which unifies all codes that can be tracked as belonging to the same code family.\(^{35}\) In our specific application a difficulty arises from the possibility that within the synthetic code created by the concordance some original codes were a part of a case, but others were not. It is not immediately clear to us that in all such instances we should be treating all such codes as part of a case.

### 5.3 On the Definition of a Typical Case

Throughout our analysis we have used what we defined as a typical case, one in which both the initiation and preliminary phases last two quarters each and the final phase lasts twenty quarters. While most cases correspond to such timing structure\(^ {36} \), three cases stand out in the length of the preliminary phase in particular.\(^ {37} \) In two of these cases the preliminary round lasted at least six years, while in the third it lasted more

\(^{35}\) As an example, a code may be split into three new codes. All four codes, the single old one and the three new ones, would belong to the same synthetic code. Four codes could be combined into a single code. All five of these would belong to the same synthetic code.

\(^{36}\) The main exception to this is the length of the final phase, especially prior to the mandatory sunset review was put in place in January 1995 as a result of the Uruguay Round of the WTO. The average length of the final phase in our data is 36 quarters.

\(^{37}\) These are cases denoted USA–AD–673, USA–AD–753, and USA–AD–806.
than two years. In all three there was a suspension agreement which was terminated after several years, with a duty imposed years later. In column 6 of Table 6 we exclude these cases yielding qualitatively identical results. While we do not employ any sort of control for cases in which the final phase lasts for more than five years, we note that in such instances the effect of the final phase shown in all our figures would simply be longer lasting, resulting in more failed spells.

5.4 On the Starting Point of a Case

Our categorization of a typical case deserves closer scrutiny from a different point of view as well. In all our simulations of the effect of an antidumping investigation and case we have assumed that the investigation started in quarter nine, the median starting point for an active spell in our sample. We now investigate to what extent does the average quarterly effect of a case depend on when during an active spell it commences. We simulate what the hazard would have been had the case started in quarter five first, and then examine how it changes as the start of the case is pushed back in 4–quarter increments. In our benchmark approach, with the case commencing in quarter nine, the average hazard is 14.8% for the 24 quarters during which the case develops. When the case starts in quarter five, the average hazard is 17.7%. With the case starting in quarter thirteen, the average hazard decreases to 12.9 percentage points. Broadly speaking, the average hazard of exports to the U.S. ceasing while an antidumping case is active looks very much like the hazard itself: decreasing at a decreasing rate as the starting time is pushed back during the life of a spell.

In the upper panel of Figure 8 we plot the average hazard during each phase of a case as it varies with the quarter of initiation. For each quarter of initiation we plot the average hazard for the duration of the case for the particular phase of the case. The 34.8 percent average hazard for the preliminary phase plotted for quarter 5 is not the hazard due to the preliminary phase in quarter five, but rather the average
Figure 8: Average Hazard Effects by Phase and Quarter of Filing
hazard during the preliminary phase when the case is initiated in quarter five. The average overall effects just described hide a large variation across the three phases of a case. While the average hazard throughout a case starting in quarter five is 17.7%, it is composed of a 36.8% hazard during initiation, 34.8% hazard during the preliminary duty phase, and a 14.1% hazard during the final phase. As the quarter of initiation of a case increases, the average hazard during each phase decreases. While the hazard during the preliminary phase for a case initiated in quarter five is 34.8%, it decreases to 28.2% for a case initiated in quarter nine, then to 24.1% for a case initiated in quarter thirteen, and on until it reaches an average of 11% for a case initiated in quarter 53 of an active spell.

The bottom panel of Figure 8 reveals that while the average hazard of each phase of a case decreases with the quarter of initiation, the relative effect increases with the quarter of initiation. We plot the percentage change in the hazard associated with a phase relative to the hazard when there is no case as the quarter of initiation increases. The preliminary phase increases the hazard of a spell failing by almost 73% if the case is initiated in the fifth quarter. If the case is initiated two years later, in quarter thirteen, the preliminary phase increases the hazard by 92%. A filing taking place one more year later and the preliminary phase doubles the hazard. While the upper panel suggests that longer lived spells enjoy some protection from their length as the average hazard associated with a case decreases the later the case is initiated, the bottom panel shows that the relative effect of a case actually increases the later the case is initiated.

5.5 Fitting the Hazard at Values other than Sample Means

Throughout our analysis we have persistently used the fitted hazard evaluated at the average value of each variable across all observed spells. It is worthwhile to investigate where does the fitted hazard at means lie relative to the hazard fitted at other points
in the distribution of every variable. In the bottom left panel of Figure 7 we plot the hazard fitted at means, medians, the 25\textsuperscript{th}, and the 75\textsuperscript{th} percentiles. The fitted hazard evaluated at means, which as argued produces a quite rapid reduction in the number of surviving spells, is a relatively rare occurrence given our data. It lies below the other three fitted hazard curves, with the hazard evaluated at the 25\textsuperscript{th} percentile displaying the highest hazard, almost everywhere at least twice as large as that evaluated at means. Thus, using the average value of every variable we are quite possibly understating the actual hazard faced by a typical exporter to the U.S. More importantly for our immediate purposes, which values are used to fit the hazard does not affect whether an antidumping case has an effect on the hazard.

5.6 Annualized Data

Our final robustness check examines whether quarterly data simply overstate the extent of the hazard of exports to the U.S. ceasing. The high frequency of data may result in an inadvertent observation of a failure. In other words, we examine an alternative approach to gap–adjusted data by estimating the hazard using annualized data. We first aggregate our quarterly trade flow data to annual observations. We then annualize the antidumping data by simply observing the year in which each phase of a case takes place. Annualizing the case data presents us with a challenge we would have to face if our entire analysis were conducted at the annual frequency which is how to properly code the length of each phase. The initiation and preliminary phases typically last two quarters. But with annualized data the average length of each phase is a year and a half. This is a consequence of many cases filed towards the end of the calendar year, thus resulting in a phase being observed over a two year period. Since our use of annualized data is mainly for illustrative and robustness purposes we chose to largely ignore this issue and simply use the data as they are after our annualization procedure without attempting to correct for this issue.
For the purpose of generating a hazard profile associated with a typical case with annual data, we assume that the filing takes place in year two and that the initiation and the preliminary phase each last a year and that they take place in the same year. We assume that the final phase lasts for five years. As the bottom right panel of Figure 7 indicates this results in a somewhat awkward comparison between the quarterly and annual data as the case goes on for a total of six years with annual data, one more than with quarterly data. As the fitted hazards indicate, quarterly data do not overstate the extent of spell failure since the hazard of exports ceasing with annual data is higher than it is with quarterly data. Using our thought experiment again, starting with 10,000 active spells when the antidumping investigation begins, after the initiation phase there are 21% fewer spells relative to when there is no antidumping case, while the corresponding figure with quarterly data is 24%. After the preliminary phase, there are 27% fewer spells with annual data versus 46% under quarterly data. After the five years worth of final antidumping duties in place, there are 55% fewer spells with annual data as compared with 66% under quarterly data. As the last column of Table 6 indicates there are no qualitative differences in terms of estimated coefficients, except that the final duty phase has a larger effect than the preliminary phase. In the immediately preceding column we use quarterly data which are used to create the annualized data used in estimation. For proper comparison we need to take into account the possibility that a five year long spell in annual data is composed of multiple spells in quarterly data, one of which could be left-censored. This reduces our quarterly sample by some 40% but results in no qualitative differences when compared to our benchmark results.

\[38\] This occurs because the effect of the final phase begins as soon as the spell enters its third year, in year 2.25, but with annual data that is observed only in year 4, since fractions of a year are unobservable. Or to use a different example, while with quarterly data once the case ends at the end of the seventh year the effect of the case disappears within the first quarter of the eight year, with annual data it takes the entire eight year for the effect of the case to disappear.
6 The Specificity of Steel Products

We devote our last set of result to examining whether cases involving steel products exhibit different effects than cases which involve products other than steel. Cases involving steel products constitute the majority of our case observation data, some 60% of the 196,865 case–relevant observations (116,484 to be precise). Rather than estimating a single hazard regression across all product we split our sample in two parts, one containing only steel products (HS codes starting with 72 and 73) and one with all other products. We then replicate our phases and nonlinear duty effects specifications on each sample. To save space we only report the fitted hazards from these four regressions in Figure 9. In every case the fitted hazard is computed using the sample means values for the appropriate subsample (for steel products this entails the average of every variable across all steel product observations only.)

The effect of an antidumping case is quite different depending on which type of a product, or more precisely industry it involves. For non–steel products only the preliminary duty phase results in a higher hazard than the hazard in the absence of a case. During the initiation stage and the final duty stage, the hazard for products involved in a case is lower than what it would be if there were no case. The upper left panel of Figure 9 indicates that while the lower hazard during the initiation phase is not statistically significantly different, that during the preliminary and final phases is. For steel products every phase of a case results in a both statistically and economically significant higher hazard, with a larger increase during the initiation and preliminary phases than during the final duty phase. Thus, the overall results we identified above seem to a large extent to be driven by steel products.

Examining the nonlinear effect of antidumping duties in the lower panels of Figure 9 reveals that there is no differential effect of either preliminary or final duties for non steel products as the confidence intervals for the two fitted hazard, with duties

\[39\text{Complete results are available on request.}\]
Steel vs non–steel products

Filing occurs in quarter 9, initiation lasts 2 quarters, preliminary duty in place for 2 quarters, final duty in place for 20 quarters

Figure 9: Differences Between Steel and Non–Steel Products
below and above the median, overlap. Although the difference in the fitted hazard for the preliminary phase is much larger. This is not entirely the case for steel products, where there is no overlap in the confidence intervals for preliminary duties. Above median preliminary duties increase the hazard relative to the initiation phase, while below median duties reduce it relative to the initiation phase, though still keep it higher than what it would be in the absence of a case. The effect of the final duty appears different, with above median duties resulting in a higher hazard, but the differences are not statistically significant (though they are jointly statistically different from the hazard that would prevail in the absence of a case).

Note that the dichotomy in the effect of antidumping duties is not present for steel products. Above median duties, whether preliminary or final, result in a higher hazard. The dichotomy does exist for non–steel products, which generates the dichotomy we identified across all products. The lower hazard resulting from initiation and final duties for non–steel products is surprising, particularly when one considers that duties levied on non–steel products tend to be higher. The median duty levied on non–steel products is 61.96% while it is only 25.77% for steel products.\textsuperscript{40}

7 Conclusion

Our goal in this paper was to examine whether antidumping petitions and cases have an effect on the extensive margin of U.S. imports, in contrast to most of the antidumping literature which has focused on effects on the intensive margin. Using quarterly trade flow data in combination with detailed case–specific data we have shown that antidumping actions have a large, almost prohibitive effect on the hazard of exports to the U.S. ceasing. Given the detailed nature of our data, we have shown that not only is this effect large, but it also varies with the stage of a case. Across all products, an antidumping case increase the hazard in every stage, but its effect is

\textsuperscript{40}The median is 36.41\% across all products.
minimized in the final antidumping duty phase. It is maximized in the preliminary duty phase, and its effect in the initiation phase, prior to the imposition of any antidumping duty, is larger than during the final duty phase. The large effect an antidumping case has during its initiation and preliminary phases can be matched by GDP of the exporting country being reduced by three orders of magnitude.

The size of the antidumping duty has a nonlinear effect. Cases with below median duties having a more persistent effect through the case, while cases with above median duties have much larger effect during the initiation and preliminary phase, but have no effect in the final phase. In terms of extensive margin effects, duties below the median are more damaging as they result in fewer spells surviving a five year case. Duties below the median result in fewer surviving spells provided the case enters at least the third year of final antidumping duties being in effect. This, of course, need not imply that duties below the median are more damaging from an economic welfare point of view. Our results are robust to a number of concerns, including gaps between spells, changes in HS codes, omission of irregular cases, and annualized data. The relative effect of a case increases as the filing of a petition takes place later in the life of an ongoing spell. Finally, some of our results are due to different effects antidumping cases have on steel and non–steel products.

We devote our final words on providing a broader perspective on our results. Given that the existing literature has focused on evaluating the effects of antidumping actions by focusing on the intensive margin, our discovery of significant effects on the extensive margin raises a question whether current estimates of welfare effects associated with antidumping are underestimated. If antidumping actions, as we have shown, cause exporters to abandon the U.S. market there are long term consequences in the form of persistently lower competition and higher prices which need to be taken into account. In addition, as the heterogeneous firm and sunk costs literature would suggest, if a foreign exporter is forced to abandon the U.S. market due to antidumping,
it may be reluctant to return at a future point. This reluctance would be due to
the uncertainty associated with the ability of the exporter to recover prospective
sunk costs when considering re-entry from the fear to facing another antidumping
claim in the future. It may also be possible that in products or industries where
antidumping activity is particularly high and frequent, such as steel, prospective
exporters never enter the market from fear of facing an antidumping petition and
experiencing significant financial losses. In such instances there may be significant
welfare consequences (for specific markets) from potential, but never realized trade.
References


