How Do Exporters Cope With Violence? Evidence from Political Strikes in Bangladesh*

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Abstract

We examine the impact of political strikes on the export-oriented garments industry in Bangladesh. To do so, we use the universe of political strikes and export transactions in Bangladesh during 2010 to 2013. These strikes are uniquely targeted episodes of violence that allow us to identify its effect on exports through the transport disruption channel alone. Despite the violence involved in these strikes, our results suggest that they do not have a cumulative effect on a firm's decision to export or the value of its exports. To explain this, we develop a model where an exporter trades off the added risk of exporting during a strike with the cost of switching the shipment date on short notice and the reputation penalty from delaying the shipment date. Our model provides the following sharp predictions: small exporters will disproportionately ship during a strike, medium-sized exporters will disproportionately ship the day before, and large exporters will disproportionately ship the day after. Our empirical results support these predictions and confirm that the risk burden of these strikes are disproportionately borne by small exporters.

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1. Introduction

Political violence is an endemic feature in many developing countries. For instance, Strauss and Taylor (2009) find that 58 percent of elections in Sub-Saharan Africa during 1990 to 2007 involved some form of violence. Apart from the human cost, such political violence has a disruptive effect on commercial activity in general and export activity in particular. The latter is especially problematic for developing countries, given the important role that exports potentially play in driving their economic growth (WTO, 2003). While a series of studies have examined the adverse effects of violence on trade (Glick and Taylor, 2010; Ksoll, Machiavello, and Morjaria, 2014; Machiavello and Morjaria, 2015), this literature has two key limitations. First, it has mainly exploited catastrophic episodes of violence that affect trade through many channels. This makes it difficult to precisely identify the mechanisms that are driving these results. Second, this literature has not fully examined the distributional effects of such violence. In particular, the question of whether particular types of exporters bear a disproportionate burden of exporting amidst such violence remains an open one.

In this paper, we address these limitations in the literature by examining the impact of political strikes, known locally as *hartals*, on garments export activity in Bangladesh. As in many other developing countries, democracy in Bangladesh is characterized by a culture of confrontational politics. A particularly egregious example of this is the use of *hartals*, which are designed to disrupt the country's transportation network and are typically used by opposition parties to pressure the government to accept its demands.[1] To examine the impact of *hartals* on export behavior, we use self-collected, daily data on all *hartals* in Bangladesh during our sample period of 2010 to 2013. Further, we also collect data on when a *hartal* was announced, why it was announced, whether it spanned a single day or multiple days, and the number of injuries and deaths during each *hartal*. Such detailed data on political violence in a developing country over an extended period of time are rare. We pair our *hartal* data with the universe of export transactions in Bangladesh during our sample period. These data, which are collected by the National Board of Revenue, allow us to construct an exporter-level, daily panel.

We use these data to examine the impact of a *hartal* on the timing of a firm's decision to export, the value of its shipments, and its decision to use air transport. Our baseline event window

[1] A related form of protest is prevalent in India and Nepal today, where they are referred to as *bandhs*. Further, the disruptive effects of *hartals* share some similarities with general strikes in Bolivia and elsewhere.

begins the day before each *hartal* and ends six days after it. This means that, not only are we able to examine the impact of a *hartal* on the day of a *hartal* itself, we are also able to examine an exporter's adjustment behavior on the days immediately before and after a *hartal*. Thus, a key advantage of our setting is that our high-frequency data allow us to precisely understand the manner in which exporters adjust over a short event window around a *hartal*.[2,3]

A second advantage of our setting is that the targeted nature of *hartals* allows us to cleanly isolate a single channel (transport disruptions) through which such political violence affects exporters. As discussed in greater detail below, the typical study in the literature examines the economic impact of either a war, internal conflict, or intense political violence. The drawback of utilizing such episodes of severe violence is that they can affect economic activity through many channels such as transport disruptions, damage to utility infrastructure, damage to factories, and worker absenteeism. In contrast, the *hartals* that we examine in this paper are targeted towards disrupting transport networks alone. There is no direct damage to infrastructure and factories. Further, the mainly female garments workers in Bangladesh live near their factories. As a result, there is very little worker absenteeism as a result of a *hartal* (Ashraf et al., 2015). Thus, *hartals* provide a uniquely "clean" shock that is free of other confounding factors and allow us to isolate the effect of political violence on exporters through the transport disruption channel alone.

Our baseline results suggest that a *hartal* lowers the probability that a firm in our sample will export on that day by 1.20 percentage points. However, we also find that a *hartal* leads to a 0.70 percentage point increase in the probability that a firm will export on the day before. In fact, over our eight-day event window, we find that there is no cumulative effect of a *hartal* on the probability that an exporter in our sample will make a shipment. Our results suggest that some exporters in our sample reallocate their shipments away from *hartal* days to minimize the risk of its shipment being damaged due to the violence. We also find that there is no cumulative effect of *hartals* over our event

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[2] Our use of a short event window is essential because the median *hartal* in our data was announced with three days' notice. Thus, if exports do engage in any adjustment behavior, it will be during a short period around a *hartal*. Further, the need for a short event window validates our decision to focus on exports rather than production. While high-frequency export data for a large number of firms are now available for several countries (see for example the data used in Eaton, Kortum, and Kramarz, 2011), this is not the case for high-frequency production data. A notable exception to this are the data used by Ashraf, Machiavello, Rabbani, and Woodruff (2015), although their data only cover 33 factories.

[3] The average exporter in our sample makes approximately 93 shipments a year with an average gap between shipments of about 6 days. Thus, they ship at a sufficiently high frequency such that an eight-day event window is long enough for us to observe their adjustment behavior.

window on the value of goods shipped and the probability of using air shipment. These results are robust to including exporter fixed effects, to controlling for daily, international shipping prices, and to the use of alternate controls for seasonality.

Given the violence involved in a typical *hartal*, the small adjustment behavior we observe in our baseline results is a puzzle. To explain this, we develop a model where a representative exporter faces the choice between exporting during a *hartal* and changing the shipment date to the day before or after a *hartal*. In choosing the optimal shipment date, this exporter faces the following trade-off. First, if it chooses to export during a *hartal* on day d it must bear the risk that its shipment will be damaged. In contrast, if it chooses to export the day before a *hartal*, it must pay a shipment cost premium, τ_{d-1}. Lastly, if it chooses to ship on the day after a *hartal*, it must bear both an added shipment cost, $\tau_{d+1} < \tau_{d-1}$, as well as a reputational penalty for a delayed shipment.[4] We assume that the added shipment cost is independent of exporter size and that the reputational penalty is declining in exporter size. This means that larger exporters will enjoy both economies of scale in changing shipment dates and suffer a smaller penalty for any delays.

Our model identifies three different types of adjustment behavior based on exporter size in response to a *hartal*. First, it predicts that sufficiently small exporters that are not large enough to enjoy economies of scale in shipping and/or suffer a high reputational penalty for any delays will choose to ship during a *hartal* despite the added risk of doing so. To the extent that the vast majority of the exporters in our sample have shipment sizes below this cutoff, this result provides an explanation for the limited adjustment behavior we observe in our baseline results.[5] Our model also provides sharp predictions about the heterogeneous adjustment behavior of medium and large exporters respectively. In particular, we show that there exists a range of shipment sizes where exporters are large enough to enjoy economies of scale in arranging shipments on the day before a *hartal* but are not large enough to face a small reputational penalty from shipping the day after a *hartal*. Our model predicts that such medium-sized exporters will disproportionately change their shipment dates to the day before a *hartal*. In contrast, large exporters that enjoy both economies of

[4] The assumption of a cost premium is justified by the fact that, given previously scheduled shipments, there is likely to be limited excess shipping capacity the day before and after a *hartal*. Further, since the median hartal is announced with three days' notice, exporters have very little time to organize an alternate shipment date. Further, our assumption that $\tau_{d+1} < \tau_{d-1}$ is justified by the fact that exporters have two additional days to organize a shipment the day after a *hartal* relative to the day before.

[5] This is an especially plausible explanation for our baseline results given that the median shipment size during our sample period was USD 38,000.

scale in shipping and a low reputational penalty for delays will disproportionately ship the day after a *hartal*.

To test whether these predictions are supported by our data, we first divide our sample into three equal tertiles based on an exporter's average shipment value over the entire sample period. We then define exporters in the first tertile as small, exporters in the second tertile as medium sized, and exporters in the third tertile as large. We find that small exporters do not demonstrate any statistically significant adjustment behavior. In contrast, both medium and large exporters do lower their shipments on the day of a *hartal* and increase them during the remainder of the event window. Importantly, we find that medium-sized exporters transfer 72.70 percent of their reduced export shipments during a *hartal* to the day before while large exporters transfer only 59.10 percent of their reduced shipments. In addition to supporting our model's predictions, these results are important because they highlight the fact that the risk burden of a *hartal* are borne disproportionately by smaller exporters. This distributional effect of political violence has been under-studied in the previous literature.

Next, we exploit the richness of our data to examine the effect of *hartal* characteristics on the adjustment behavior of exporters. We find that the greater the gap between the announcement of a *hartal* and the date of the *hartal* itself, the more likely it is that exporters will change their shipment dates away from a *hartal*. This is consistent with the idea that a greater notice period provides exporters with more time to organize shipments on alternate dates. We also find that *hartals* that span multiple days are much more disruptive than *hartals* that span a single day.

Our paper is related to a growing literature that documents the adverse, microeconomic effects of political violence on firms. A pioneering paper in this literature is Ksoll, Machiavello, and Morjaria (2014), who use daily export data to examine the impact of election-related violence in 2008 on Kenya's floriculture industry. They find that weekly export values in affected regions decreased by 38 percent. They then use self-reported, recall data to show that worker absenteeism, and not transportation problems, was the key mechanism driving their results. As mentioned above, the key distinguishing feature of our paper is that we examine a form of political violence that is targeted towards disrupting transport networks alone. As a result, we are able to cleanly isolate the effect of political violence on exporters that operates through this channel alone.

Our paper is also related to a literature that examines the impact of political violence and instability on other aspects of firm performance. For instance, Shonchoy and Tsubota (2015) use annual, firm-level data to show that *hartals* in Bangladesh lower firm productivity. Machiavello and Morjaria (2015) examine how election-related violence affected the relationship between Kenyan flower exporters and its foreign buyers. Collier and Duponchel (2012) examine how the greater intensity of fighting in Sierra Leone affects firm output. Similarly, Guidolin and La Ferrara (2007) and Abadie and Gardeazabal (2003) examine how the sudden end of civil conflict in Angola and a truce announced in the Basque region of Spain respectively affected the stock-market returns of firms operating in these regions.

Next, our paper is also related to an earlier literature that examines the effect of terrorism and conflict on bilateral trade (Nitsch and Schumacher, 2004; Blomberg and Hess, 2006; Martin, Mayer, and Thoenig, 2008; Glick and Taylor, 2010).[6] It is also related to a literature that documents the negative effect of political instability on growth (Alesina, Özler, Roubini, and Swagel, 1996). Finally, our paper is related to a literature that documents the trade-reducing effects of transportation delays (Djankov, Freund, and Pham, 2010; Hummels and Schaur, 2013) and to a literature that uses natural disasters to identify the causal effect of transport disruptions on trade (Volpe Martincus and Blyde, 2013; Besedes and Murshid, 2015).[7]

We structure the remainder of the paper as follows. In section 2 we provide further background on *hartals* in Bangladesh as well as on its export-oriented garments industry. In section 3 we describe our *hartal* data and discuss the evolving nature of these *hartals* during our sample period. We also discuss our export data in this section. In section 4 we introduce our econometric specification, discuss some econometric issues, and present our baseline results. In section 5 we examine, both theoretically and empirically, the heterogeneous adjustment behavior of exporters in response to a *hartal*. In section 6 we explore the robustness of our baseline results and examine the

[6] The relationship between economic globalization and conflict has also been extensively studied by political scientists. See the papers cited in Barbieri and Reuveny (2005). See also Blattman and Miguel (2010) for a survey of studies that examine the broader economic impact of conflict.
[7] These natural disasters provide exogenous shocks to transport infrastructure and therefore address endogeneity concerns in an innovative manner. However, its use has the drawback that natural disasters are a rare type of transport disruption. As a result, they do not necessarily inform us about how exporters respond to less intense and more common disruptions such as poor weather, inadequate infrastructure etc. In contrast, our results based on low-intensity and targeted disruptions are a better guide to how exporters in developing countries respond to the typical transport disruptions that they face.

effect of *hartals* on the value of exports and an exporter's choice of air shipment. Finally, in section 7 we provide a conclusion.

2. Background

2.1. *Hartals* in Bangladesh

A *hartal* is a political protest that has a long history in both Bangladesh as well as in South Asia. For instance, *hartals* were first used as early as 1919 by Mahatma Gandhi as a voluntary and largely non-violent method of civil disobedience against British colonial rule.[8] In Bangladesh's pre-independence period (1947 to 1971), *hartals* were seen as a legitimate method of protest against misrule by West Pakistan. As a result, *hartals* during this period had relatively greater popular support. Next, in the 1980's, *hartals* were used to protest the authoritarian, military ruler at the time and also enjoyed widespread support. This historical success and popular support lends contemporary *hartals* a degree of legitimacy in the eyes of Bangladeshi political parties (Suykens and Islam, 2013).

While Bangladesh has a tradition of using *hartals* to protest misrule, in recent years its use has become more widespread. This is because, despite being a parliamentary democracy since 1991, Bangladesh's democracy is characterized by a general intolerance for the views of the opposition parties. As a result, institutional mechanisms for addressing the grievances of opposition parties either do not exist or do not work well. In the Bangladeshi context, the main grievance is regarding the fairness of general elections. As in the case in other illiberal democracies, opposition parties in Bangladesh do not trust the incumbent to hold fair elections. As a result, *hartals* are viewed as the only viable way to force the incumbent to either enact electoral reforms or to resign and allow a neutral government to hold fair elections (Sobhan, 2004a).

Despite its past history of popular support, it is the case that *hartals* today are deeply unpopular among ordinary Bangladeshis. A 2013 poll conducted jointly by the Asia Foundation and a local newspaper found that 31 percent of all respondents considered *hartals* and political violence to be the country's leading problem (Daily Star, 2013) So why do political parties use them? There are three main reasons. First, a successful *hartal* sends a signal to the government that the opposition

[8] A related form of protest is prevalent in India and Nepal today, where they are referred to as *bandhs*.

party is sufficiently powerful and organized and poses an electoral threat to the government. It is the typically the case that other non-violent political activities such as processions, meetings, etc. are also scheduled to coincide with a *hartal*. As a result, a *hartal* is seen as a tool with which to regroup opposition political activists and to place pressure on the incumbent government to accept the opposition's demands. Second, Bangladeshi politics is dominated by two main political parties: the *Awami* League and the Bangladesh Nationalist Party. This duopoly engenders a belief that the voter will not punish opposition parties that call *hartals* since their choice is between the opposition and a typically unpopular incumbent (Sobhan, 2004a).[9] Moreover, both political parties have built a sizeable base of loyal supporters. This means that the probability of losing significant political support as a result of staging a violent *hartal* is low. Lastly, given the typical heavy-handed response by police, *hartals* are viewed by opposition parties as an effective method with which to garner greater voter support (Sobhan, 2004b). As described below, a common tactic adopted by opposition activists during a *hartal* is to goad the police into violent confrontations. The resulting response by police, which typically involves the use of excessive force, generates widespread sympathy for injured opposition activists.

So what happens during a *hartal*? As described in greater detail in Ahmed and Mortaza (2005), *hartals* are enforced by activists that include armed mercenaries along with hired protestors. The latter are typically drawn from various urban slums. The main aim of these activists is to restrict vehicular movement in key urban areas. This is done in three ways. First, the armed activists goad the police into violent confrontation. Second, *hartal* activists set off homemade grenades and other improvised explosives at various urban areas (Human Rights Watch, 2014). Finally, a third tactic used by opposition activists is to torch vehicles (private cars, buses, vans etc.) that ignore the *hartal* restrictions and are seen on city streets.[10] These activities typically start the night before the *hartal* itself and its aim is to create a sense of fear among everyday citizens and entrepreneurs and to discourage them from using motor vehicles. Importantly, because *hartals* are most strictly enforced in urban areas away from industrial zones, there is typically no damage to factories or to utility

[9] This is supported by the observation that in all four general elections held in Bangladesh in which both parties participated, the opposition used *hartals* extensively prior to the election and was still voted to office.

[10] It is evident that to successfully stage a *hartal*, where success is measured by the amount of disruption caused, opposition parties need to have the organizational capacity to hire a sufficient number of armed activists and other individuals. This work is typically the responsibility of mid and low-level party operatives. Demonstrating competence in organizing disruptive *hartals* is considered by these party operatives to be highly valuable as it often leads to patronage if the party is voted to government. As a result, *hartals* tend to be very popular among such operatives (Suykens and Islam, 2013).

infrastructure. Thus, as documented in greater detail below, apart from the transport disruption, factories are otherwise unaffected.

2.2. The Ready-Made Garments Industry in Bangladesh

The disruptions caused by a *hartal* are particularly problematic for the export-oriented, ready-made garments industry (RMG or garments from here on) in Bangladesh. This industry has played a vital role in driving the country's recent economic growth. It emerged in the late 1970's through a partnership between a local firm, Desh Ltd., and a South Korean manufacturer, Daewoo Corporation. At the time, the low export of garments from Bangladesh meant that it was not subject to quotas in Western markets. Daewoo's objective was to use Bangladesh as an export platform to circumvent the quotas that applied to its exports from South Korea. According to Quddus and Rashid (2000), as part of this venture, Desh sent 130 of its employees to South Korea to participate in an eight-month training program. The vast majority of these employees then went on to start their own garments factories. From this humble beginning, the garments industry in Bangladesh has grown at a dramatic rate over the last four decades (Heath and Mobarak, 2015) and has emerged today as one of the leading garments exporters in the world. According to McKinsey (2011), Bangladesh's garments industry in 2011 employed around 3.60 million workers, most of whom were women.

During this period in which the garments industry in Bangladesh has expanded, the nature of garments sourcing has changed dramatically. Traditional garments sourcing methods resulted in orders being placed by Western retailers to overseas factories approximately six months before a season in the West (Birtwistle, Siddiqui, and Fiorito, 2013). The size of the orders was forecasted based on sales from previous years. Errors in these forecasts created a mismatch between the demand for an item and its available stock in retail outlets. To lower such inefficiency, an increasing number of Western garments retailers switched to quick-response (QR) methods of supply-chain management starting in the 1990's (Taplin, 2014). QR methods are designed to reduce the gap between when an order is placed to factories and the date at which the customer purchases the item. A lower gap allows retailers to better predict what the trendy items are likely to be in any given season. It also means that once it becomes evident that an item is popular, retailers can quickly order a new batch from its supplier. The use of QR methods meant that the typical order to an overseas supplier changed from having a predictable several-month lead time to a series of small and frequent orders with low lead times that better reflect real-time demand.[11] While QR methods lower costs for retailers and prices for consumers, it places a greater strain on suppliers as they have to be flexible enough to respond to volatile changes in fashion trends. The use of QR methods also place a greater emphasis on timely delivery as any delays may cause popular items to be understocked in retail stores.

2.3. The Disruptive Effect of *Hartals*

An important feature of *hartal*s is that, while they are costly to exporters, these costs are almost only due to transport disruptions. By making motor vehicle movement more risky, *hartal*s lead to higher transport prices to compensate transport companies for the added risk they bear. They also lead to longer transit times as drivers avoid violence-prone areas in cities. Finally, there is also a non-negligible probability of shipment loss in the event that a shipment is damaged or destroyed by political activists.[12]

In contrast, *hartals* do not make it significantly more costly for garments workers to travel to their factory. The mainly female workforce in the garments industry tends to live very close to their place of work. This is supported by the results in Ashraf et al. (2015) who find that *hartals* do not affect worker absenteeism or productivity in garments factories. *Hartals* also do not adversely affect port operations for export shipments. While precise data regarding this are difficult to find, media reports suggest that any adverse effects on port operations are restricted to the import side (Haroon, 2012). On a typical day, an imported container is offloaded from a ship and then placed on a truck for transport to the relevant factory. During a *hartal*, these containers are placed in port storage as trucks are less able to transport them to the factories. In contrast, if a container intended for export is

[11] Lead time is defined in this context as the gap between an order date and the required delivery date.

[12] This discussion assumes that the time to transport goods from the factory to the port is sufficiently long for such transport disruptions to be costly. Unfortunately the export data that we use do not record the location of each exporter's factory. As a result, we cannot use these data to calculate an exporter's transport time or distance from the factory to the port. Instead, to gauge the location of export activity, we use the results from Fernandes (2008). She shows that 67 percent of garments firms in her sample are located in either Dhaka or the Dhaka Export Processing Zone. Next, our export data, which are described in detail below, suggest that 73.68 percent of garment export shipments are made through Chittagong port, which is located in the south east of the country. Thus, the majority of exporters, who are located near Dhaka, ship their goods through Chittagong port. The distance between Dhaka and Chittagong Port is approximately 263 kilometres. On a typical day, it can take a truck up to 24 hours to reach Chittagong Port from a factory in Dhaka (World Bank, 2016). It follows that, for the majority of exporters in our data, we can rule out the possibility that the transport disruption caused by *hartals* represents a trivial additional cost.

already in the port premises on the day of a *hartal*, they are loaded on to ships. The delay in export shipments occur due to the inability of some shipments to reach the port itself during a *hartal*.

3. Data

3.1. *Hartal* Data

To examine the effects of *hartals* on export behavior, we compiled a database of *hartals* during the period 2005 to 2013 using the two most popular Bengali and English language daily newspapers in Bangladesh. These are *The Daily Ittefaq* and *The Daily Star* respectively. We used two research assistants, who independently went through the archives of these newspapers for each day of our sample period to collect information on *hartals*. In order to avoid data collection errors, we then compared the entries of both research assistants and corrected any discrepancies. Apart from collecting the date on which the *hartal* occurred, we also collected the announcement date of the *hartal,* the length of the *hartal*, the political party/parties announcing the *hartal* and the official reason for announcing the *hartal*. Our data yield the following stylized facts about *hartals* in Bangladesh.

Hartals Are Mainly Timed Around Elections

Figure 1 illustrates the annual trend in *hartals* during the period 2005 to 2013. In the first half of this time period (2005 to 2009), there were a total of 53 *hartals* in Bangladesh. The prevalence of *hartals* during this period reached its peak immediately before the general elections that were scheduled for 22nd January, 2007. In the face of increasingly violent unrest, the Bangladeshi military intervened on 11th January, 2007 and installed a military-backed caretaker government. This government remained in power until the general elections held on 29th December, 2008. As Figure 1 illustrates, this period of military-backed rule was free from *hartals*. In the second half of this period (2010 to 2013), there were 99 *hartals* in Bangladesh. As before, the prevalence of *hartals* again increased during the year preceding the general elections that were held on 5th January, 2014.[13]

Hartals Have Become Increasingly Disruptive

Next, as Table 1 demonstrates, not only have *hartals* become more frequent during the second half of this period, they have also become more disruptive. When announcing a *hartal*, a political

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[13] Over the entire 2005 to 2013 period there were approximately 17 *hartals* per year. This is almost the same as the number of public holidays per year (19).

party can stipulate whether the *hartal* is going to be span a single day or whether they will span multiple days. Our data suggest that the percentage of single-day *hartals* decreased significantly during the second half of our sample period. For instance, during the period 2005 to 2009, 72 percent of *hartals* spanned a single day while 14 percent spanned two-days and 14 percent spanned more than two days. In contrast, during the period 2010 to 2013, 60 percent of *hartals* spanned a single day, 21 percent spanned two days, and 19 percent spanned more than two days. Parties that announce a *hartal* can also stipulate the number of hours during which the *hartal* will apply. Our data suggest that the average length of *hartals* increased from 14.60 hours during the first half of the sample period to 16.13 hours during the second half.

Further, the *hartals* in the second half of our sample period were also announced with less notice. For instance, during the period 2005−2009, *hartals* were announced 7.28 days before the *hartal* itself. However, during the period 2010−2013, *hartals* were announced 4.62 days before the *hartal* itself. In fact, the median gap between the announcement date and the *hartal* date was three days during the second half. Lastly, during the first half of our sample period, there were about 0.5 deaths per *hartal* whereas in the second half, there were about two deaths per *hartal*. Thus, along all dimensions reported in Table 1, *hartals* have become more disruptive in Bangladesh in recent years.

Lastly, Table 1 also documents the fact that the disruptive effect of *hartals* varies markedly. In particular, the variance in (a) the length of a *hartal*, (b) the notice provided, and (c) the number of deaths and injuries during a *hartal* are large. To highlight this in another way, note that the length of a *hartal* varies between 2 and 24 hours, the notice provided varies between 0 and 39 days, and the number of deaths and injuries varies between 0 and 36.5 and 0 and 1598 respectively.[14]

3.2. Transaction-Level Export Data

We combine our *hartal* database with transaction-level export data. These administrative data represent the universe of export transactions during our sample period and are collected by the National Board of Revenue (NBR). These data were digitized using the Automated System for Customs Data designed by the United Nations Conference on Trade and Development (UNCTAD). The NBR records the bill of entry details associated with each export shipment. These bills of entry

[14] When the two newspapers we use to construct our *hartal* database provides conflicting estimates of deaths and injuries, we take an average of these two estimates. This is why some *hartals* in our sample have a reported number of deaths/injuries that are in fractions.

provide the date of an export shipment, the exporters' unique identification number, the total value of export, the 8-digit HS code of the product that is exported, the port through which the product is exported, and the destination of the export shipment. As a result, we can use these data to construct a panel of export transactions during the period 2005 to 2013. To construct our working sample, we omit observations that do not include the date of export (0.0005 percent of the raw data). We then aggregate each firm's export by day. In some cases, firms have multiple consignments on the same day, often for the same product.[15] We aggregate these to ensure that there is only one observation per firm per day.

To gauge the reliability of these data, we compare the aggregate exports calculated from our customs data with that reported by the World Bank. This comparison is demonstrated in Table 2. In both columns (1) and (2) we report the total annual exports from Bangladesh for the period 2005 to 2013. In column (1) we use the customs data while in column (2) we use the World Bank data. In column (3) we report the ratio of annual exports from the customs data to the annual exports from the World Bank data. Over the entire sample period, this ratio takes the value of 0.995. Thus, over this entire period, the customs data accurately captures almost all export transactions from Bangladesh. However, if we examine this ratio by year, certain anomalies become evident. In particular, the ratios in 2006 and especially 2007 are outliers. In fact, the customs data suggest that there was a decrease in exports in 2007, which is surprising given the widely reported inexorable rise of exports in Bangladesh during this period. Due to these concerns about data quality, we chose to restrict our working sample to the period 2010 to 2013.[16] Lastly, we also restrict the sample to firms in the ready-made garments industry. During our sample, ready-made garments exports represented 79.40 percent of all Bangladeshi exports and ready-made garments exporters represented 76.31 percent of all Bangladeshi exporters.

[15] We define a shipment as the value of goods that a firm exports on a given day. In contrast, we define a consignment as the value of goods reported in each bill-of-entry. To make this clearer, suppose that an exporter is planning to export 1,000 units of a product on a given day. She decides to transport them to the port in four trucks consisting of 250 units per truck. In our analysis, each truck is considered a consignment while the total quantity exported on that day (1,000) is the shipment.

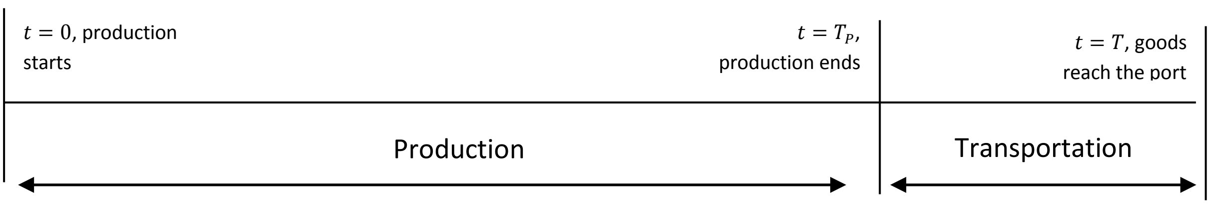
[16] An alternate approach would be to include 2008 and 2009, which consists of 6,380,879 observations (8,777 firms × 727 days). However, as Figure 1 illustrates, there were no *hartals* during these two years. This means that adding these years will increase our sample size by almost 6.38 million observations without adding any additional variation to our *hartal* variables. Due to this, we chose to omit these years from our working sample.

In Table 3 we report some descriptive statistics of the exporters in our working sample. This sample consists of 8,161 garments firms that have exported at least once during our sample period of 2010 to 2013.[17] On average, 624 firms export on any given day. The average exporter in our sample exports 5.41 products per year, where a product is defined at the HS6 level. Such a firm also exports to 5.40 destinations per year and makes 92.75 shipments per year. The average firm in our sample uses air transport for 22 percent of its shipments. Thus, our sample consists of high-frequency, multi-product, and multi-destination exporters.[18]

4. Econometric Strategy and Baseline Results

4.1. The Timing of the Effect of *Hartals* on Exports

Since *hartals* provide a transportation shock to Bangladeshi exporters, we would expect there to be a reduction in export shipments on the day of the *hartal* itself. What is less well understood is the lag structure with which *hartals* will affect the decision to export. To explore this, consider an exporter's operational phase, as illustrated below. This operational phase consists of two segments: (a) a production segment where the goods intended for export are produced and (b) a transportation segment where the goods intended for export are transported to the port. In the diagram below, the period between $t = 0$ and $t = T_P$ represents the production segment while the period between T_P to T represents the transportation segment.



As discussed above, *hartals* are a unique form of political violence because they are targeted towards disrupting transportation and do not cause any direct disruption to production. Thus, if a *hartal* falls on an exporter's production segment, then we should not observe a disruption to its production and therefore should not observe an effect on its probability of exporting or the value of

[17] Thus, for each of these firms we have 1,453 daily observations during the period 2010 to 2013. This excludes seven dates that are missing in our sample. These missing dates typically coincide with *Eid*, which is the main public holiday in Muslim-majority Bangladesh.

[18] The most common destination for these exports is the United States, which accounts for 29.93 percent of all Bangladeshi garments exports. This is followed by Germany, which accounts for 24.96 percent of all exports.

its exports at day T.[19] On the other hand, if a *hartal* falls on an exporter's transportation segment, an exporter's ability to transport its goods to the port in a timely manner will be adversely affected. As a result, the amount it exports on day T will be reduced. It follows that the exports on day T will only be affected by a *hartal* that is scheduled on day T itself or by *hartals* on days within a short window before T. This will motivate the use of a relatively short event window in our econometric specification below.

4.2. Econometric Specification

To capture the impact of *hartals* on exports, we estimate the following specification:

$$\Pr[X_{it} > 0] = \alpha_1 + \sum_{s=-1}^{6} \beta_s H_{t-s} + \theta_d^w + \theta_d^Y + \theta_y + \varepsilon_{it} \tag{1}$$

where X_{it} is the value of total exports for exporter i on day t. Our aim here is to capture whether an exporter responds to a *hartal* by the choosing not to export at all on a *hartal* day. Thus, the regression in (1) estimates the effect of a *hartal* on the extensive margin. To identify the effect on the intensive margin, we also use as a dependent variable a firm's total daily exports in natural logarithm. Lastly, to explore other coping mechanisms, we also replace the dependent variable in (1) with an indicator for whether a firm uses air transport on day t.

When $s = 0$, H_t is an indicator variable for whether there was a *hartal* on that day. For all other values of s, H_{t-s} takes the value of one if there was a *hartal* $t - s$ days ago and there wasn't a hartal on day t. Thus, each coefficient β_s captures the impact of a *hartal* that occurred s days ago on today's exports. The use of lagged *hartal* indicators allows us to capture the extent to which exporter's reallocate their shipment away from *hartal* days and towards days immediately before and after a *hartal*. Thus, if such reallocation were absent we would expect β_s to equal zero for all $s \neq 0$.

Our specification also extensively controls for any seasonal patterns in the data. In particular, we include day-of-week fixed effects (θ_d^w), which will capture any secular variation in exports during the week. We also include day-of-year fixed effects (θ_d^Y) to control for any seasonal factors that might be correlated with exports. For instance, exports for particular products might exhibit strong seasonal patterns during the year (e.g. summer or winter clothing). Thus, by not including day-of-year fixed

[19] If we do observe such long-lagged effects, it must be due to disruptions in access to imported inputs during the production segment.

effects, our regression estimates might be picking up spurious changes in the data. Further, we include year fixed effects (θ_y) to capture macro-level factors that are correlated with *hartals* as well as a firm's export decision. Lastly, ε_{it} is a classical error term.[20]

In section 4.1 above, we discussed the rationale for examining a short event-window around a *hartal*. Nonetheless, our choice of an exact, eight-day event window ($s = -1$ to $s = 6$) in our baseline specification merits further discussion. In choosing our baseline event window length, we faced the following trade-off. On the one hand, we do not want our event window to be too short as this will prevent us from capturing an exporter's full adjustment behavior. For example, a two-day event window won't allow us to capture an exporter's full adjustment behavior if this exporter responds to a *hartal* by shifting its export shipment to three days after the *hartal*. On the other hand, we do not want our event window to be too long as this will introduce other confounding factors. As discussed above, a *hartal* will only affect an exporter's export shipment with a long lag if it disrupts the exporter's access to imported inputs. Given that our aim here is to capture the direct effect of a direct disruption to export shipment alone, a relatively short event window is appropriate.

With this trade-off in mind, we take a data-driven approach to selecting a baseline event window length. In particular, we choose the minimum event window needed for the total effect of a *hartal* on the decision to export of an average firm to approach zero. This approach has the advantage that it allows us to be agnostic about the inherently difficult question of what is the "correct" event window length. Nonetheless, as discussed below, we show that our key results are robust to alternate event window lengths.

Finally, our choice of an eight-day event window requires that firms in our sample export at a relatively high frequency. If the typical shipment gap is greater than eight days then our event window may not be long enough to observe the adjustment behavior of firms. To examine whether this is the case, we plot the distribution of the number of days shipped per year in Figure 2. This figure suggests that the average exporter in our sample exports 92.75 days per year. Further, our data suggests that the average gap between shipment days is 6.29 days. Both of these numbers suggest

[20] Given that *hartals* are exogenous to the exporter, we are not concerned about the correlation between them and unobservable, time-invariant exporter characteristics. As a result, in the interest of parsimony, we do not include exporter fixed effects in our baseline specification. We do however include them as a robustness check. As we discuss below, adding the firm fixed effects does not change the key results of the paper.

that the firms in our working sample export with relatively high frequency and therefore an eight-day event window is long for us to observe the adjustment behavior of firms.

4.3. Econometric Issues

Our identification of (1) relies on two key assumptions. The first is that *hartals* are not announced because of adverse economic shocks. To the extent that these adverse shocks also lower exports, this would result in us picking up a spurious negative correlation between *hartals* and exports. To examine whether this is the case, we explore the reasons for announcing a *hartal.* Recall that when we constructed our *hartal* database we recorded the official reason for announcing each *hartal*. We group these reasons into various categories and illustrate their frequencies in Figure 3. As this figure clearly demonstrates, the main reasons for announcing a *hartal* are political. The most common reason is a demand for election reforms. Since the beginning of electoral democracy in Bangladesh in 1991, elections there have been marred by distrust and violence. As a result, many pre-election *hartals* are motivated by the desire for electoral reforms to minimize any advantage for the incumbent party. Other common reasons for announcing *hartals* are to protest police violence against opposition activists and to protest a recent War Crimes trial. Importantly, of the 99 *hartals* during our sample period, only four were motivated by economic factors. In all four cases, the motivation for announcing the *hartal* was the rising price of essential goods and, therefore, it was not directly related to exports.

While *hartals* may be announced due to political reasons, they may be timed around important economic periods. Thus, the second identifying assumption required for (1) is that the timing of *hartals* is not related to economic conditions. For instance, if particular months of the year represent peak exporting periods, opposition parties may refrain from announcing *hartals* then to minimize any adverse effect on exporters. To examine whether this is the case, we plot the share of exports by month and the share of *hartals* by month in Figure 4. As this figure illustrates, garments exports in Bangladesh are fairly evenly spread out throughout the calendar year. This is mainly a function of the fact that garments are in demand throughout the year. While the exact product to be exported will vary throughout the year (e.g. summer vs. winter clothing), the export of garments overall are unlikely to be specific to any seasons. In contrast, *hartals* are more prevalent at the end of the calendar year. This is because of two main reasons. First, elections in Bangladesh are typically held in January and February. Further, the dryer and cooler weather at the end of the year is more conducive to staging a *hartal*.[21] For these reasons, *hartals* in Bangladesh peak around November and December. Thus, there is no evidence in Figure 4 of *hartals* being timed around peak export periods, mainly because the uniform nature of garments exports throughout the year in Bangladesh means that significant peak periods are non-existent.

4.4. Baseline Results and Event-Window Selection

We begin by estimating equation (1) for various event windows using a linear probability model. Our aim here is to examine how the effect of a *hartal* evolves over various event windows and to pick the shortest event window needed for the cumulative effect of a *hartal* to approach zero. This shortest event window will then serve as our default event window for the rest of our analysis. This data-driven approach to selecting a default event window has the advantage that it does not require us to take a stand on what the default event window should be.

In column (1) of Table 4 we report the contemporaneous effect of a *hartal*. The dependent variable is an indicator that takes the value of one if a firm exports on day t and is zero otherwise. The coefficient of the *hartal* indicator, H_t, is negative and statistically significant. It suggests that a *hartal* reduces the average firm's probability of exporting by 1.30 percentage points.[22] In column (2) we include the first lead of the hartal indicator, H_{t+1}, which takes the value of one if there is *hartal* tomorrow but no *hartal* today. This indicator will capture the extent to which exporters bring forward their export shipment date to lower their exposure to a *hartal*. The results in this column suggest that the contemporaneous effect alone does not capture the true effect of a *hartal* on an average firm's export behavior. In particular, we find here that while there is 1.20 percentage point reduction in the probability of making an export shipment on the day of the *hartal* itself, there is a 0.80 percentage point increase in this probability the day before the *hartal*. That is, the average firm responds to a *hartal* by increasing its shipments the day before the *hartal* to make-up for some of the reduced shipments on the day of the *hartal*.

We can use our estimates in column (2) to introduce our method of calculating the cumulative effect of a *hartal* on export shipments. This approach relies on the following logic. Suppose there is a

[21] Our extensive seasonality controls will capture the secular effect of weather patterns and elections on the timing of *hartals*.

[22] We cluster our standard errors at the day level, which is the level at which our dependent variables of interest are measured. As we show later in the paper, our standard errors remain largely unchanged if we cluster them at the two-way level (firm and day) instead.

hartal on day t. From our estimates in Table 4, we know that this *hartal* will have an effect on the probability of making a shipment on day t. We also know that this *hartal* will affect an exporter's probability of making a shipment on the day before. Further, as we show below, this *hartal* will also affect an exporter's probability of making a shipment on the days immediately after. Thus, the sum of these three effects represents the cumulative effect of a *hartal*. More precisely, the cumulative effect is given by $\sum_{s=-1}^{6} \beta_{t-s}$. This cumulative effect is reported at the bottom of column (2). For the two-day event window examined here, the cumulative effect is a 0.50 percentage point reduction in the probability of making an export shipment.

In columns (3) to (4) of Table 4 we extend our event window sequentially by two days at a time. For instance, in column (3) we include the lagged *hartal* indicators H_{t-1} and H_{t-2} while in column (4) we add the lagged *hartal* indicators H_{t-3} and H_{t-4}. The coefficients on these additional lagged indicators are both relatively small in magnitude and statistically insignificant. In both cases, the cumulative effect is a 0.20 percentage point reduction in the probability of making an export shipment.

Finally, in column (5) we extend our event window to eight days by including the lagged hartal indicators H_{t-5} and H_{t-6}. As above, the coefficients on these lagged indicators are both relatively small in magnitude and statistically insignificant. However, the cumulative effect of a *hartal* during this eight-day event window is a 0.02 percentage point increase in the probability of making an export shipment. Importantly, this result suggests that the cumulative effect of *hartal* approaches zero during the eight-day event window. This adjustment behavior is illustrated more clearly in Figure 5, which plots the cumulative effect over the entire eight-day event window. The takeaway message here is that, over an eight-day event window, a *hartal* does not have a cumulative effect of the probability of making an export shipment. This is because the average exporter in our sample compensates for the reduced probability of making an export shipment on the day of the *hartal* by increasing the probability of making a shipment the before a *hartal* as well as the days after.

To demonstrate that our core result is not sensitive to restricting the event window to eight days, we extend our event window further in column (6). In particular, we add two additional lead indicators, H_{t+2} and H_{t+3}, as well as two additional lagged indicators H_{t-7} and H_{t-8}. As the results demonstrate, the coefficients of these additional *hartal* indicators are both small in magnitude and statistically insignificant.[23] Further, the cumulative effect over this 12-day event window is also statistically insignificant. Thus, extending our default event window in this manner does not change our key takeaway message.

5. Heterogeneous Adjustment Behavior

5.1. Theory

Our results thus far suggest that *hartals* lead to a minor displacement of export shipments. In particular, we find that both the decrease in the probability of making a shipment on the day of the *hartal* itself and the increase in this probability the day before the *hartal* are relatively small. Given the degree of violence and uncertainty involved in a typical *hartal*, the lack of a displacement effect is surprising.[24] In this section, we use a simple model to provide an explanation for why exporters may choose to continue to ship during a *hartal* despite the added risk of doing so. A further advantage of our model is that it will provide sharp predictions about the heterogeneous adjustment behavior of exporters and will highlight the fact that the risk burden of a *hartal* is borne disproportionately by small exporters. Such heterogeneous effects of political violence on firm export activity raises important distributional concerns.

To begin, consider an exporter i that enters into an agreement with a foreign buyer. The agreement stipulates the quantity of a product that the exporter must produce, the price of each unit, and the date d on which the shipment is to be made. The price and quantity of goods to be shipped

[23] Note that the median notice period provided for a *hartal* is three days in our sample period of 2010 to 2013. Our informal discussions with garments exporters reveal that the transportation segment is typically 1 to 2 days and includes transportation related logistic preparations. Therefore, a *hartal* that occurred at day T was typically announced during the final few days of an exporter's production segment. These last few days of the production segment involves sorting and packaging of goods, third party inspection, as well as the buyer's own inspection. Most importantly, the schedule for each production line in a factory gets tighter as its moves closer to its completion date. Therefore, the typical exporter has very little scope to bring forward its shipment date by more than one day.

[24] The lack of a displacement effect could also reflect the fact that *hartals* are not that disruptive to begin with. We can rule this possibility out for two reasons. First, survey data indicate that firms in Bangladesh do consider *hartal*s to be a constraint. For instance, according to the 2013 round of the *Enterprise Surveys* collected by the World Bank, 97.80 percent of Bangladeshi firms in their sample report political instability to be an obstacle. In fact, 69.90 percent report political instability to be either a major or severe obstacle. Second, a survey of chief purchasing officers (CPO) of several European and US apparel retailers conducted by McKinsey suggest that political instability is one of the five main challenges to the growth of garments exports in Bangladesh (McKinsey, 2011). Approximately half of these CPO's report that they will decrease their sourcing from Bangladesh if political instability were to increase, as such instability leads to greater delays.

are determined by the buyer and are typically non-negotiable. As a result, we assume that the exporter takes the total value of the shipment as given. Let this value be V^i.[25] In contrast, we assume that the exporter can deviate from the agreed shipment date depending on whether or not there is a *hartal* on day d.

When deciding whether or not to export during a *hartal*, a risk-neutral exporter must weigh the added costs of doing so. This added cost reflects the probability that *hartal* activists may vandalize transport vehicles or set them on fire. In such an event, an exporter's shipment will be damaged. More precisely, an exporter's expected payoff from transporting goods during a *hartal* is

$$\pi_d^i = \rho(V^i - D^i) + (1-\rho)V^i = (1-\rho)V^i$$

where ρ is the probability that an exporter's shipment will be damaged. Recall from section 3.1 that there is significant variance in the intensity with which a *hartal* is implemented. This means that, while the date of a *hartal* is known to an exporter, the severity of the *hartal* is not. The parameter ρ captures this risk. D^i is the damage caused to an exporter's shipment in the event that it is vandalized. Without loss of generality, we assume that $V^i = D^i$. That is, we assume that the entire shipment is lost when it is vandalized.[26]

Absent any adjustment cost, the added risk of transporting during a *hartal* should lead to a complete displacement of export activity to days immediately adjacent to the *hartal*. The fact that we do not observe this in our data suggests that there are significant costs associated with changing the date of a shipment. We model these adjustment costs associated as follows. First, we assume that exporters can change their shipment date from d to $d-1$, but must incur an additional cost, $\tau_{d-1} < V^i$, of doing so. This cost reflects the added fixed cost of hiring a standard-sized shipping container on day $d-1$ due to the sudden increase in demand. To put it slightly differently, we know that there were shipments already scheduled for day $d-1$ before a *hartal* was announced for day d. This means that there are limited excess shipping containers available to accommodate any displaced shipments that were originally scheduled for day d. As a result, the desire of exporters to transfer their shipments to day $d-1$ will bid up the cost of these containers. τ_{d-1} is designed to capture this

[25] For simplicity, we assume that an exporter's shipment size accurately reflects its overall size. As a result, we use the terms shipment size and exporter size interchangeably.
[26] The central predictions of this model will go through for any $D^i < V^i$, but the assumption that $V^i = D^i$ allows us to economize on the number of parameters in the model.

surcharge. We assume that each exporter in our sample is sufficiently small that they take τ_{d-1} as given. It follows that an exporter's payoff from exporting on date $d-1$ is

$$\pi_{d-1}^{i} = V^{i} - \tau_{d-1}$$

Second, exporters can also change their shipment date from d to $d+1$, but doing so incurs two costs.[27] The first cost is the penalty associated with the shipment delay. Missing the agreed shipment date may lead to an exporter being categorized as unreliable by the buyer and therefore adversely affect the exporter's chances of receiving an order in the future. We assume that this reputation penalty is equal to $e^{-\delta V^{i}}V^{i}$, where $e^{-\delta V^{i}}$ is the probability that an exporter will suffer a reputation penalty and is decreasing in an exporter's size. Further, δ is a positive constant that governs the rate at which the probability of receiving a reputation penalty declines with exporter size. The negative relationship between the probability of receiving a penalty and exporter size captures the fact that larger exporters are more likely to have a longer export history and a longer relationship with their buyers. For such exporters, it is reasonable to assume that one delayed shipment will likely have a very small effect on its reputation for reliability. One may wonder whether it is reasonable to assume a reputation penalty at all in this context, given that any delay will be caused by political violence that is beyond an exporter's control. However, as we discussed in section 2.2, this assumption is consistent with foreign buyers being more inclined to source their garments from other countries if there are repeated delays caused by political violence in Bangladesh (McKinsey, 2011). Thus, the reputation penalty in this case represents the probability that a buyer will switch to another Bangladeshi supplier or a supplier in a different country in response to a delayed shipment.

The second cost of changing the shipment date to $d+1$ is that an exporter must incur an additional transport cost of $\tau_{d+1} < V^{i}$, with $\tau_{d+1} < \tau_{d-1}$. The intuition for the latter restriction is that one would expect the cost surcharge associated with arranging shipment on an alternate date to be

[27] Instead of allowing exporters to choose between three possible shipment dates ($d-1, d$, and $d+1$), we could instead allow for there to be n possible shipment dates. However, such a generalization will not add any empirical insight in our context. Further, given the reputation penalty associated with a delayed shipment, it is unlikely that exporters will change their shipment date to one that is too far removed from the agreed date.

higher when an exporter has less time to make such arrangements.[28] As before, we assume that each exporter takes the value of τ_{d+1} as given. Thus, an exporter's payoff from exporting on date $d+1$ is

$$\pi_{d+1}^{i} = \left(1 - e^{-\delta V^{i}}\right) V^{i} - \tau_{d+1}$$

In Figure 6 below we plot an exporter's payoff as a function of its shipment size (V^i) for each of the three shipments dates $d-1$, d, and $d+1$ respectively. This figure illustrates two important cutoff values of V^i, V_1^* and $V_2^* > V_1^*$.[29] The first of these cutoff values, V_1^*, identifies the shipment size at which an exporter is indifferent between shipping during the *hartal* on day d and shipping on day $d-1$. We derive this by setting $\pi_d^i = \pi_{d-1}^i$, which yields

$$\frac{\tau_{d-1}}{V_1^*} = \rho \tag{2}$$

Thus, only exporters with $V^i < V_1^*$ will choose to export during a *hartal*. This follows from our assumption that the added cost of transporting on date $d-1$, τ_{d-1}, is independent of the shipment size. This means that smaller exporters ($V^i < V_1^*$) do not enjoy the economies of scale in transportation needed to make this change of shipment date attractive. For these exporters, the expected utility from shipping during a *hartal* exceeds the utility from shipping on day $d-1$. This yields the following hypothesis:

Hypothesis 1

Only sufficiently large exporters with shipment values greater than V_1^* will choose not to export during a *hartal*. All other exporters will continue to ship during a *hartal* despite the added risk of doing so.

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[28] The $\tau_{d+1} < \tau_{d-1}$ restriction is necessary to ensure that some export shipments are displaced to the days after a *hartal*. With a positive probability of receiving a penalty for shipping on day $d+1$, $\tau_{d+1} \geq \tau_{d-1}$ would imply that no exporter will change their shipment date to $d+1$.

[29] As we discuss more formally in the appendix, a sufficient condition for this second cutoff, V_2^*, to be greater than V_1^*, is for δ to be of an intermediate value. The intuition for this restriction on δ is as follows. If δ is too high, then the probability of receiving a penalty falls very quickly with exporter size. This means that once an exporter chooses not to ship during a *hartal*, she will always find it advantageous to ship on day $d+1$ since $\tau_{d+1} < \tau_{d-1}$. For such high values of δ, no exporter will ship on day $d-1$. Similarly, if δ is too low, then the probability of receiving a reputation penalty does not fall much with exporter size. This means that once an exporter chooses not to ship during a *hartal*, she will always find it advantageous to ship on day $d-1$. Thus, if δ is too low, then no exporter will ship on day $d+1$. It follows that δ has to take an intermediate value (the range is derived in the appendix) for V_2^* to be greater than V_1^*. When this happens, some exporters will choose to ship on day $d-1$ and some exporters will choose to ship on day $d+1$ when there is a *hartal* on day d.

This hypothesis suggests that if the vast majority of exporters in our sample have shipment values below V_1^*, then we should observe very little displacement of export shipments away from *hartal* days. Next, the second cutoff value of V_2^* identifies the shipment size at which exporters are indifferent between shipping on day $d-1$ and day $d+1$. This cutoff only applies to exporters that are large enough that they choose not to ship during a *hartal*. We derive this cutoff by setting $\pi_{d+1}^i = \pi_{d-1}^i$which yields

$$e^{-\delta V_2^*} = \frac{\tau_{d-1} - \tau_{d+1}}{V_2^*} \tag{3}$$

This cutoff separates medium-sized exporters with $V_1^* < V^i < V_2^*$ who choose to export on day $d-1$, from large exporters with $V^i > V_2^*$ who choose to export on day $d+1$. The intuition for this separation is that the medium-sized exporters are large enough ($V^i > V_1^*$) that they enjoy economies of scale in transportation. This means that they prefer to ship on day $d-1$ rather than during a *hartal*. However, these medium-sized exporters are small enough ($V^i < V_2^*$) that their probability of receiving a reputation penalty is still high. Thus, for these exporters, $e^{-\delta V^i} > (\tau_{d-1} - \tau_{d+1})/V^i$ holds, which means that the probability of receiving a penalty is higher than the cost savings from shipping on day $d+1$. As a result, they find it optimal to ship on day $d-1$.

In contrast, large exporters with $V^i > V_2^*$ are large enough that they enjoy economies of scale in transportation. Thus, these exporters will also not ship their goods during a *hartal*. In addition, due to their size, these exporters have a low probability of receiving a reputation penalty. When combined with the fact that $\tau_{d-1} > \tau_{d+1}$, it follows that these large exporters will always choose to ship on day $d+1$ rather than on day $d-1$. The discussion above results in the following hypothesis:

Hypothesis 2

Compared to large exporters, medium-sized exporters will disproportionately shift their exports to the day before a *hartal*.

The heterogeneous adjustment behavior identified in hypotheses 1 and 2 has two important implications. First, it suggests that accounting for adjustment costs is important for us to understand the true heterogeneous behavior of exporters in the face of transport uncertainty. Second, the heterogeneous adjustment behavior identified above implies that the adverse effects of a *hartal* are not uniformly distributed among Bangladeshi garments exporters. Instead, the risks associated with transporting goods during a *hartal* are borne disproportionately by smaller exporters as these are the exporters that are less likely to change their shipment dates away from the *hartal* day. This is an important result because it highlights a key distributional consequence of such forms of political violence.

5.2. Heterogeneous Adjustment Behavior: Empirics

In Table 5 we examine whether or not our data support the heterogeneous adjustment behavior predicted by our model. To do so, we first categorize exporters as either small, medium, or large. In particular, we classify an exporter as small if its average shipment value over the entire sample period is below the 33rd percentile. We classify an exporter as medium sized if its average shipment value is between the 33rd and 67th percentile and classify an exporter as large if its average shipment value is above the 67th percentile. We then estimate our baseline specification for each of these groups of exporters separately.

We report the results from these regressions in Table 5. In column (1) we restrict the sample to small exporters while in columns (2) and (3) we restrict the sample to medium and large exporters respectively. The results in column (1) suggest that the effect of a *hartal* on the probability of making an export shipment is not statistically significant for small exporters. This is also the case for all lead and lagged effects of a *hartal*. Thus, we observe very little adjustment behavior by small exporters in response to a *hartal*. In contrast, in both columns (2) and (3), we find that the coefficient of H_t is negative and statistically significant. Further, the size of the coefficient of H_t in these three columns suggest that the reduction in the probability of exporting on the day of a *hartal* is lowest for small exporters and highest for large exporters. This suggests that both medium and large exporters are more likely to change their shipment dates away from a *hartal* relative to small exporters. These results are fully consistent with the prediction in hypothesis 1.

Now consider hypothesis 2. This hypothesis states that medium-sized exporters will disproportionately change their shipment dates to the day before a *hartal* when compared to large exporters. To see whether this is the case, note that we can think of the ratio $|\beta_{t+1}/\beta_t|$ as the fraction of the reduced export shipments on the day of a *hartal* that is transferred to the day before the *hartal*. From the coefficients in columns (2) and (3), we can see that this ratio is equal to 0.727 for medium exporters and is equal to 0.591 for large exporters. Thus, medium exporters transfer 72.70 percent of their reduced export shipments during a *hartal* to the day before while large exporters transfer only 59.10 percent of their reduced shipments. This strongly supports the prediction in hypothesis 2.

We next examine the role of other exporter characteristics. To make this more concrete, consider a stylized version of our model where the size of an export shipment is fixed at some level $\tilde{V}$. Suppose that an exporter faces a choice of either exporting during a *hartal* or on a different day. That is, there is no distinction between shipping on day $d-1$ and $d+1$. However, now assume that the added cost of exporting on a non-*hartal* day, τ^i, is exporter specific. In this version of the model, exporters with a sufficiently low τ^i will choose to change their shipment date away from the *hartal* date.

To examine whether such types of heterogeneous adjustment behavior is supported by our data, we proxy heterogeneity in τ^i in two ways. First, we assume that exporters that have a longer exporter history are better able to negotiate a lower shipping cost premium τ^i. This could reflect the fact that such exporters are likely to have a longer working relationship with shipping companies, which they can leverage to negotiate a lower cost premium. To the extent that this is the case, we should observe greater adjustment behavior among more experienced exporters. In our data, we only observe each firm's export history during the period 2005 to 2013 and therefore do not know its full export history. Nonetheless, we can distinguish between exporters that first appear in our sample in the later period (inexperienced) and exporters that appear earlier (experienced).

In columns (1) and (2) of Table 6 we restrict the sample to firms that first exported in or after 2010 (inexperienced) and those that first exported before 2010 (experienced) respectively. We chose 2010 as the cut-off as this ensures that the sample sizes in columns (1) and (2) are relatively similar. The results in column (1) suggest that there is a comparatively small decrease in the probability of making an export shipment during a *hartal* for inexperienced exporters. Thus, these exporters demonstrate very little reallocation of export shipments during the event window. In contrast, in column (2) we observe a much larger reduction in the probability of exporting on the day of a *hartal* among experienced exporters. As before, the cumulative effect over the event window is close to zero and statistically insignificant for both types of exporters.

A second source of heterogeneity in τ^i may be due to the frequency with which exporters make shipments. For instance, all else equal, an exporter that ships more frequently is likely to have a better working relationship with shipping companies, which can allow them to negotiate a lower shipping cost premium. If this is the case then we should observe that exporters that ship frequently are better able to reallocate their export shipments. To examine this, we compare the results for infrequent versus frequent shippers in columns (3) and (4) of Table 6. To categorize firms as either frequent or infrequent shippers, we first calculated the average number of annual shipments made by each exporter during our sample period. We then classified an exporter as an infrequent shipper if its average annual number of export shipments was below the sample median. All other exporters were classified as frequent shippers. The results in column (3) and (4) confirm that frequent shippers are more likely to reduce the probability of exporting on the day of a *hartal* and increase the probability of exporting the day before a *hartal*. As before, the cumulative effect over the event window is close to zero and statistically insignificant for both types of exporters.

5.3. *Hartal* Type

In this section we examine whether the effect of a *hartal* depends on the characteristic of the *hartal* itself. We begin by examining whether the notice provided to exporters affects their adjustment behavior. Recall that, in addition to the date of the *hartal*, we also know when a *hartal* was announced. The difference between these dates represents the time that exporters had to make alternate transport arrangements. It is likely that a *hartal* announced with a longer notice period will give exporters the ability to organize transport on alternate shipment dates in a more cost-effective manner. In turn, this will better allow exporters to reallocate their shipments away from *hartal* days.[30] To explore this, we define a short-notice *hartal* as one that was announced with three of fewer days' notice. Three days is the median gap between when a *hartal* is announced and when it takes place during our sample period of 2010 to 2013. The remaining *hartals* were classified as having a long notice. In column (1) of Table 7 we examine the effect of short-notice *hartals* on an exporter's adjustment behavior. To ensure that our counterfactual is the export shipment probability on a

[30] To see this more precisely, consider a version of our model where the shipping cost premium associated with changing the shipment date depends on the length of notice provided. In particular, let $\Delta > 0$ represent the gap (in days) between a *hartal* date and when it was announced. Thus, the time an exporter has to organize transport on the day before a *hartal* is $\Delta - 1$. To allow the length of notice provided to affect shipment costs, suppose that the shipping cost premium associated with changing the shipment date to the day before a *hartal* is $\tau/(\Delta - 1)$. Thus, a longer notice period (high Δ) lowers this shipping cost premium. In this version of the model, the cutoff V_1^* is derived from the expression: $V_1^* = \tau/\rho(\Delta - 1)$. This means that a longer notice period will lower the cutoff shipment size at which changing the shipment date to $d - 1$ becomes feasible. It follows that, all else equal, a longer notice period will lead to greater export adjustment activity in response to a *hartal*.

seasonally-adjusted non-*hartal* day, we omit from our sample in column (1) days in which there was a long-notice *hartal*.[31]

Next, in column (2) we examine the effect of long-notice *hartals*. As before, we omit short-notice *hartal* days to ensure that our counterfactual is a seasonally-adjusted non-*hartal* day. By comparing the results in columns (1) and (2), we find that the reduction in the probability of making an export shipment on the day of a *hartal* is greater in the case of a long-notice *hartal* when compared to a short-notice *hartal*. This result is consistent with the idea that a longer-notice *hartal*, by providing an exporter with greater time to arrange transportation on alternate dates, will facilitate an exporter's ability to reallocate its shipment dates. In contrast, short-notice *hartals* are less likely to give the average exporter the time needed to make these alternate arrangements. To the extent that this is the case, exporters will have lesser scope to reallocate their shipment dates.

Next, we examine the differential impact of single-day and multiple-day *hartals* in Table 7. We begin in column (3) by examining the effect of single-day *hartals*. We define a single-day *hartal* as one where there was a hartal on day t and there wasn't a *hartal* the day before as well as the day after. We then estimated equation (1) with this definition of a *hartal*. Here, the lagged variable H_{t-s} takes the value of one if there was a single-day *hartal* on day $t-s$ for $s=-1$ to $s=6$ and no *hartal* on day t. For the reason stated above, we omit non-single-day *hartals* from our sample before estimating the regression in column (3). The results suggest that, for the average exporter, the likelihood of making a shipment increases on the day before a *hartal*. However, unlike the earlier results, there is no significant reduction in the probability of making a shipment on the day of the *hartal* itself. Overall, the results here suggest that single-day *hartals* are much less disruptive to exporters relative to the baseline.

In column (4) we consider the effect of *hartals* that spanned between two and four days. To see how these *hartal* indicators are defined, consider a sequence of *hartals* that span four days. Here, H_t takes the value of one on the first day of the *hartal* sequence, H_{t-1} takes the value of one on the

[31] In our baseline estimation in section 4.4, we compared the export shipment probability of firms in our sample on the day of a *hartal* with the shipment probability on a seasonally-adjusted non-*hartal* day. Thus, seasonally-adjusted non-*hartal* days were our counterfactual. In column (1) of Table 7, the unadjusted counterfactual includes all seasonally-adjusted long-notice *hartals*. In other words, without any further restrictions, it includes both non-*hartal* days as well as *hartal* days where a longer notice was provided. As a result, to ensure that our counterfactual is appropriate, it is important to exclude long-notice *hartal* days from our sample in column (1). This adjusted counterfactual now only includes non-*hartal* days, as was the case with the baseline results above.

second day of the *hartal* sequence, H_{t-2} takes the value of one on the third day of the *hartal* sequence, and H_{t-3} takes the value of one on the fourth day of the *hartal* sequence. We define our *hartal* indicators in a similar manner in the case of two and three-day *hartals*.[32] Since the *hartal* period now spans up to four days, we extend our baseline event window by four additional days as well. In particular, we now define our *hartal* indicators, H_{t-s}, for $s = -3$ to $s = 8$. The results suggest that, for the average exporter, the probability of making an export shipment drops significantly on the second day of the *hartal* sequence. While there is a reduction in the probability of making an export shipment on the first, third, and fourth days of the *hartal* sequence, these effects are both relatively small in magnitude and also statistically insignificant. Interestingly, we do not observe any days in our event window with a significant increase in the probability of making a shipment. This suggests that these multiple-day *hartals* are much more disruptive to exporters in the sense that it does not allow them to fully reallocate the reduced shipments as a result of the *hartals*.

Lastly, in column (5) we examine whether *hartals* that occurred during the period that preceded the nationwide elections of 5th January, 2014 had different effects on export behavior. To explore this, we restrict the sample to the period between July and December, 2013. Of the 99 *hartals* that took place between 2010 and 2013, 40 occurred during this six-month period. As a result, the *hartals* during this period were announced with greater frequency and shorter notice to exporters and are likely to be more disruptive. The results in column (5) of Table 7 suggest that exporters exhibited adjustment behavior during this period that was is similar to the baseline. That is, *hartals* lowered the probability of making a shipment on the day of the *hartal* itself, but did not have a statistically significant cumulative effect over the event window. The main difference between these results and the baseline is the larger decrease in the probability of making a shipment during a *hartal* and the fact that shipments were reallocated to the day after the *hartal* rather than the day before.

[32] As Table 1 demonstrates, the majority of *hartals* in our sample period span a single-day and relatively few span two, three, and four days respectively. Thus, if we were to consider two, three, and four-day *hartals* separately we will be left with *hartal* indicators with very little variation. To avoid this problem, we group together two, three, and four-day *hartals*.

6. Further Results

6.1. Robustness Checks

In Table 8, we subject our primary results to a series of robustness checks. Thus far we have assumed that a *hartal* provides a uniform ``treatment" to all exporters in the sample and that an exporter's response to it may be heterogeneous based on their individual characteristics. But it could be the case that an exporter's exposure to a *hartal* is also heterogeneous. For instance, an exporter that is located close to Chittagong port may have a lower exposure to a *hartal* compared to an exporter located in Dhaka. In such cases, it is more useful to think of the exposure to a *hartal* as having both a uniform as well as an exporter-specific component. More precisely, we can rewrite our *hartal* indicator as $H_{it} = H_t + h_i$, where H_t is the component of a *hartal's* treatment that applies uniformly to all exporters while h_i is the exporter-specific component that is heterogeneous across exporters. Substituting this new *hartal* indicator into the baseline econometric specification yields

$$\Pr[X_{it} > 0] = \alpha_2 + \sum_{s=-1}^{6} \beta_s H_{t-s} + \theta_i + \theta_d^w + \theta_d^Y + \theta_y + \vartheta_{it} \tag{6}$$

where θ_i are firm fixed effects and controls for any time-invariant ability that an exporter may have to minimize its exposure to a *hartal*. A second advantage of estimating (6) relative to the baseline is that the inclusion of θ_i allows us to control for any correlation between the timing of *hartals* and unobservable, time-invariant exporter characteristics. The results from estimating equation (6) are reported in column (1) of Table 8.[33] As is evident from the table, the key results of the paper remain highly robust to including firm fixed effects. This suggests that neither *hartal* treatment heterogeneity nor the possible correlation between the timing of *hartals* and unobserved, time-invariant exporter characteristics is a first-order concern in this application.

Next, in column (2) we estimate a version of equation (1) where we cluster the standard errors at both the firm and day level. Recall that our default approach thus far has been to cluster at the day level alone. As the results in this column confirm, clustering at the two-way level does not

[33] Due to the large sample size and the presence of two high-dimensional fixed effects (firm and day-of-year), we estimate (4) using the STATA command `reg2hdfe`. This command implements the procedure described in Guimaraes and Portugal (2010). With large sample sizes, this procedure imposes a much lower computational and memory burden relative to a standard within estimator.

change the key results of the paper. In fact, the two-way clustered standard errors are almost identical to the one-way clustered standard errors up to three decimal places.

In column (3) of Table 8 we account for the fact that a firm's shipment date choice may be driven by short-term fluctuations in shipping prices. To examine whether this affects our primary results, we estimate an augmented version of (1) where we include the natural logarithm of the daily Baltic Dry Index (BDI). This index captures the price of shipping a range of primary commodities by sea using dry bulk carriers. It is constructed by the London Baltic Exchange based on daily information from a panel of shipbrokers. We use this as a proxy for short-term changes in the cost of shipping internationally. As the results in column (3) suggest, adding the BDI to the baseline specification does not significantly alter the key results of the paper. In particular, we still find a decrease in the probability of making a shipment on the day of a *hartal* and an increase on the day before. We also find that the BDI has a negative and statistically significant effect on the probability of exporting. This suggests that a higher BDI (i.e. higher average freight charges) lowers the probability that a firm will export on any given day. Note that the BDI data are not available for all days in our sample. Hence the sample size in column (3) is lower than the baseline.

A limitation of the BDI data is that it only captures freight charges and not necessarily fuel costs. To account for the latter, we add the natural logarithm of the daily crude oil price in addition to the BDI in column (4) of Table 8. The crude oil price data are from the Federal Reserve Bank of St Louis and capture the daily spot price in U.S. dollars of a barrel of crude oil. As before, the daily crude oil price is not available for all days in our sample, which is why the sample size in column (4) is smaller than column (3). Nonetheless, it is the case that adding the daily crude oil price also does not change the key results of the paper.

Lastly, we examine whether our key results are robust to alternate methods of controlling for seasonal trends in our data. Up to this point we have controlled for seasonal trends using a combination of day-of-year fixed effects, day-of-week fixed effects, and year fixed effects. To examine whether our results are robust to alternate methods of controlling for seasonality trends, we replace the day-of-year fixed effects with month fixed effects and an indicator for public holidays in Bangladesh. The results from this regression are reported in column (5) of Table 8. As these estimates clearly demonstrate, our key results go through when we use this alternate method of controlling for seasonality trends.

6.2. The Effect of *Hartals* on Export Value and Mode

Our analysis thus far has focused on the effect of *hartals* on an exporter's choice of shipment date. In other words, we have examined whether a *hartal* affects a firm's export decision at the extensive margin. In Table 9 we examine whether *hartals* affect a firm's export decision at the intensive margin. That is, we now ask whether a *hartal* alters the value of the goods that a firm exports on any given day. In column (1) we restrict the sample to observations with positive exports and then estimate a version of equation (1) with the natural logarithm of a firm's daily exports as the dependent variable. The sign of the coefficient of the *hartal* indicator, H_t, is negative and large in magnitude.

A possible limitation of the results in column (1) is that it does not account for day-to-day changes in the composition of the sample. This means that the negative coefficient of the H_t variable in column (1) could reflect a true intensive margin reduction or it could reflect the fact that larger firms disproportionately lower their probability of exporting on the day of a *hartal*. To account for this, we introduce firm fixed effects in column (2). These fixed effects will control for time-invariant firm characteristics such as size, export history etc. and will allow us to account for day-to-day compositional changes in the sample.

The coefficient of the *hartal* indicator, H_t, in column (2) remains negative, but is now statistically significant. In addition, we observe an increase in the value of exports the day after the *hartal* itself. The magnitude of this adjustment is very similar to the size of the reduction in export value on the day of the *hartal*. Interestingly, in both the OLS regression in column (1) and the fixed-effects regression in column (2), we find that the cumulative effect of a *hartal* on the intensive margin over the eight-day event window is positive and statistically significant. When combined with our finding in Table 5 that the cumulative effect of a *hartal* on the probability of making an export shipment is close to zero, we can conclude that a *hartal* does not reduce the cumulative value of a firm's exports over our event window.

Next, we examine whether a *hartal* affects an exporter's choice of transport mode. That is, we ask whether the exporters in our sample are more likely to use air transport to make up for the disruption caused by a *hartal*. Given that air transport is significantly more expensive (Hummels and Schaur, 2013), to the extent that *hartals* lead to greater use of such transport, it can have an adverse effect on the profit margin of garments exporters. To examine whether this is the case, we first estimate a version of our baseline econometric specification in (1) where the dependent variable is now an indicator that takes the value of one if an exporter using air transport on any given day and is zero otherwise. In column (3) we report the results based on an OLS estimation of this new specification while in column (4) we report the results after including firm fixed effects.

The results in both columns suggest that the coefficient of the *hartal* indicator, H_t, is positive, statistically significant, and large in magnitude. This suggests that the average exporter in the sample does increasingly use air transport on the day of a *hartal*. Interestingly, we find that these exporters compensate for the higher costs associated with this increased use of air transport by lowering the use of air shipment on other days in the event window. In particular, we find that the coefficient for H_{t-5} is negative and statistically significant. Further, we also find that the cumulative effect of a *hartal* on the decision to use air transport over the entire event window is small in magnitude and statistically insignificant. Thus, the results in columns (3) and (4) of Table 9 indicate that while *hartals* do cause exporters to increasingly switch to expensive air transport, these exporters are able to attenuate the effects of this on their profitability by lowering their use of air transport on subsequent shipments.

7. Conclusion

In this paper we examined the impact of political strikes, locally known as *hartals*, on the behavior of garments exporters in Bangladesh. In particular, we examined whether these *hartals* affected the timing of export shipments, the size (in value) of shipments, and the use of air transport. To do so, we used data on all *hartals* during the years 2010 to 2013. In particular, we collect the date of a *hartal*, when it was announced, why it was announced, and whether it spanned a single day or multiple days. We pair these data with the universe of export transactions that occurred during our sample period. These data, which were collected by the National Board of Revenue, allows us to construct a working sample consisting of a daily panel of 8,161 exporters over 1,453 days.

These high-frequency data allowed us to identify whether an exporter adjusted its shipment date, shipment size, and transport mode during a short event window around each *hartal*. To the extent that the adjustment behavior we observe is expensive, our analysis allowed us to identify an additional cost of political violence that has been under-studied in the literature. A second advantage of our setting is that we were able to isolate a single channel through which political violence affects exporters. Unlike other forms of political violence, *hartals* are targeted towards disrupting Bangladesh's transport network. There is no damage to utility infrastructure or factories during a *hartal*. Further, since the mainly female workers in the garments industry live close to their factories, *hartals* do not cause worker absenteeism or other forms of production disruptions (Ashraf et al., 2105). As a result, *hartals* provide an unusually clean shock to Bangladesh's transport network that allowed us to isolate how such violence affects exporters through a single channel.

We found that *hartals* lowered the probability that a firm in our sample will export on that day by 1.20 percentage points. However, we also found that there was no cumulative effect of a *hartal* on the probability of making an export shipment over our eight-day event window. That is, the exporters in our sample were resilient enough to reallocate their shipment dates to ensure that there was no overall reduction in shipments as a result of a *hartal*. While this result held for *hartals* that spanned a single say, we also found that during *hartals* that spanned between two to four days, there was a cumulative reduction in the probability of making an export shipment. Having established that the typical *hartal* does not significantly disrupt export shipments, we then calculated the impact of *hartals* on the cost of transporting goods to the port. Our calculations suggest that this cost increases by 69 percent due to a *hartal*. We then examined whether these effects varied according to exporter characteristics. Here we found that regardless of size, export history, and frequency of shipments, all exporters use the adjustment behavior that we identified in our baseline results. Lastly, we also found that *hartals* due to decrease the value of exports and does not have a cumulative effect on the probability of using air transport, even though it does increase the likelihood of using air transport on the day of the *hartal* itself.

Our results provide important insights on how policy makers in other developing countries can shield their firms from the adverse effects of political violence. Because we are able to isolate the effect of political violence through the transportation channel alone, we can conclude that the transport disruptions caused by such violence do not have a first-order effect on the shipment behavior of exporters. These exporters are sufficiently resilient that they use costly adjustment behavior to minimize the adverse effects of *hartals*. The key feature of *hartals* that allows this to happen is that it does not disrupt an exporter's production and only disrupts its transportation. This suggests that preventing political violence from disrupting production may be an important way in which to attenuate its adverse effects on firms.

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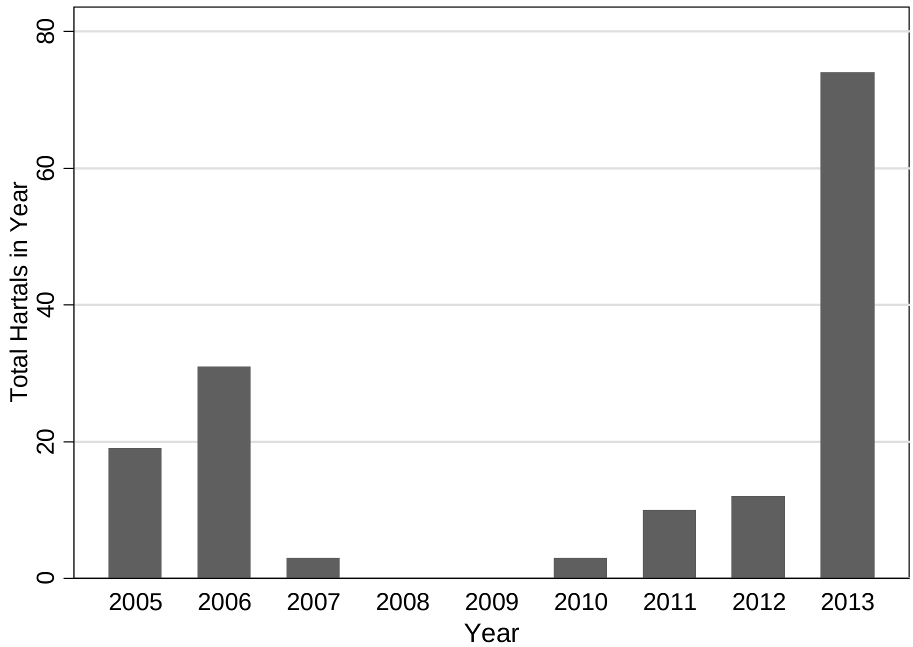

Figure 1: Annual trend in *hartals*.

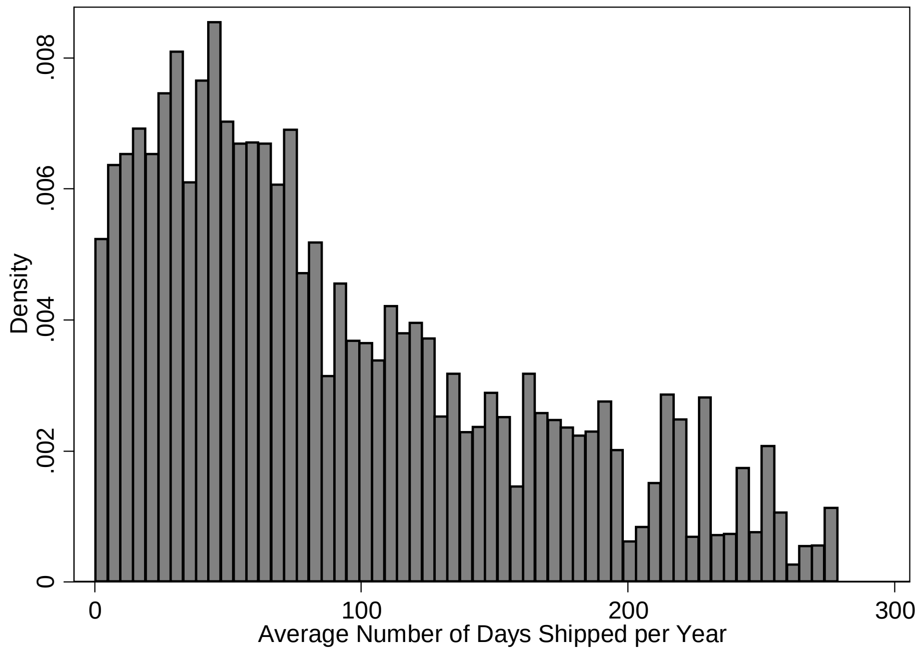

Figure 2: The average number of days shipped per year.

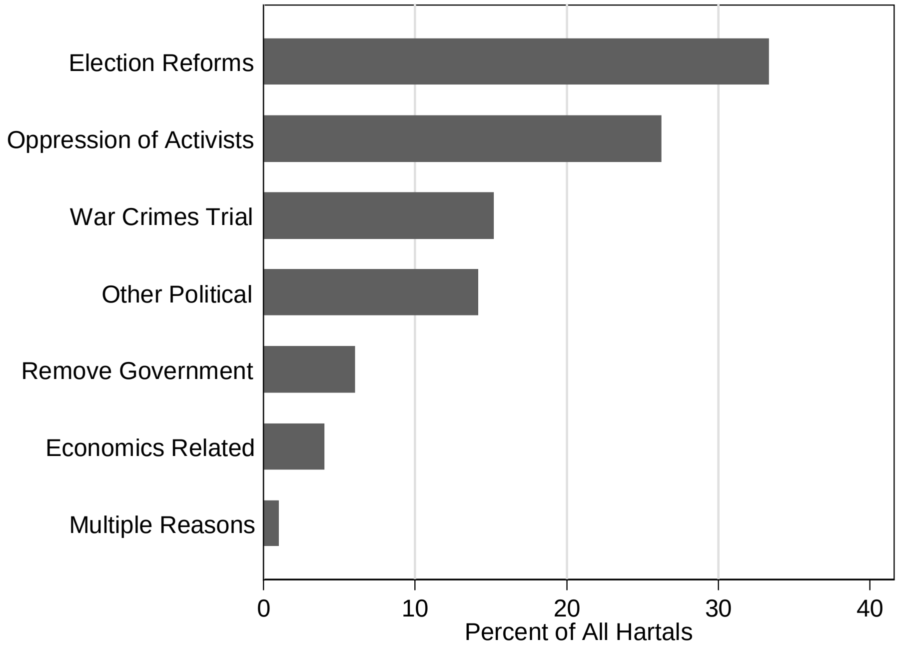

Figure 3: The stated reasons for calling a *hartal* (2010 to 2013).

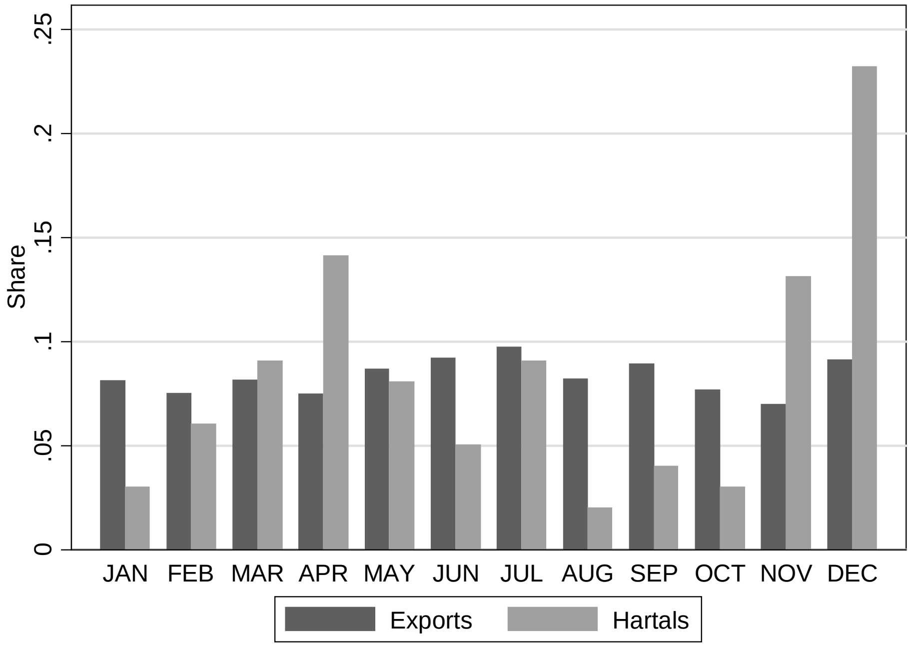

Figure 4: Distribution of *hartals* and daily exports by month. The correlation coefficient between these two variables is 0.05.

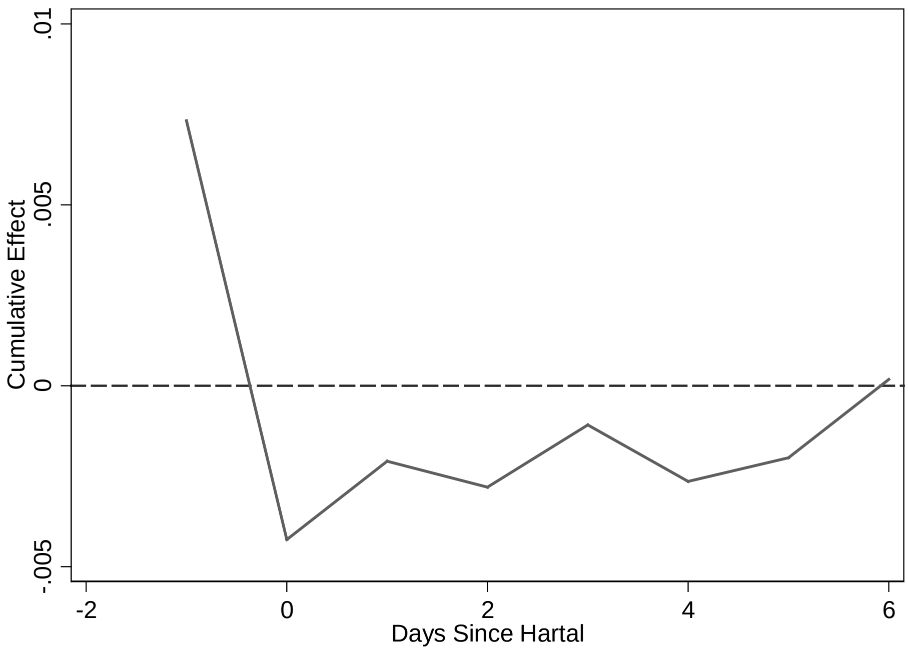

Figure 5: Cumulative effect of a *hartal* on the probability of exporting. A zero on the horizontal axis represents the day of the *hartal*.

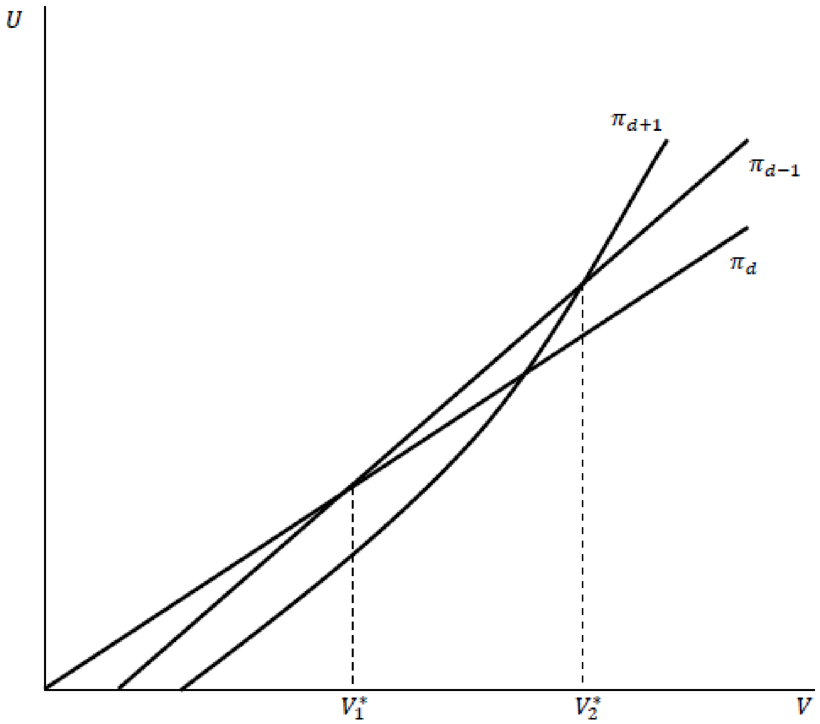

Figure 6: The payoff from exporting on dates $d-1$, d, and $d+1$ respectively.

Table 1: *Hartals* in Bangladesh

	(1)	(2)	(3)
Years Included	2005-2013	2005-2009	2010-2013
Total *Hartals*	152	53	99
Fraction of *Hartals* that spanned:			
Single Day	0.65	0.72	0.60
Two Day	0.18	0.14	0.21
Greater than Two Days	0.17	0.14	0.19
Length of *Hartals* (in hours)	15.60	14.60	16.13
	[6.31]	[6.65]	[6.09]
Notice Provided (in days)	5.55	7.28	4.62
	[7.35]	[8.02]	[6.82]
Number of Deaths	1.49	0.52	2.01
	[3.86]	[1.73]	[4.53]
Number of Injuries	112.68	132.92	101.84
	[174.37]	[232.81]	[133.23]

Notes: the reported numbers are authors' calculations using data collected from two leading Bangladeshi newspapers: *The Daily Star* and the *Ittefaq*. The numbers in brackets are standard deviations.

Table 2: Validation of the Customs Exports Data

	(1)	(2)	(3)
Year	World Bank	Customs	World Bank/ Customs
2005	577,769	571,766	1.011
2006	914,655	792,638	1.154
2007	631,699	860,018	0.735
2008	1,050,898	1,054,508	0.997
2009	1,059,283	1,037,734	1.021
2010	1,340,978	1,327,932	1.010
2011	1,803,050	1,739,932	1.036
2012	2,168,282	1,988,230	1.091
2013	2,212,223	2,327,139	0.951
All Years	11,758,837	11,699,897	0.995

Notes: In columns (1) we report the aggregate annual exports for Bangladesh as reported by the World Bank. These are based on balance of payments calculations. In column (2) we report the aggregate annual export data based on our customs data. The correlation coefficient between the two is 0.98. In column (3) we report the ratio of the World Bank aggregate to the customs aggregate. The monetary values are in millions of Bangladeshi Takas. One US dollar was approximately equivalent to 61.5 Takas in 2005.

Table 3: Descriptive Statistics of Exports Data

	Mean	Median
Total Number of Exporters	8,161	-
Exporters per Day	623.57	624.00
	[148.97]	
Daily Firm Exports	4.55	2.31
	[7.28]	
Number of HS6 Products	5.41	4.00
per Firm per Year	[5.03]	
Number of Destinations	5.40	4.00
per Firm per Year	[6.20]	
Number of Firm Shipment	92.75	73.00
Days per Year	[69.12]	
Fraction of Shipments Made	0.22	-
Using Air Transport	-	

Notes: In column (1) we report the mean of each variable along with its standard deviation in brackets. All monetary values are in millions of constant 2005 Bangladeshi Takas. One US dollar was approximately equivalent to 61.5 Takas in 2005.

Table 4: The Impact of *Hartals* on The Probability of Exporting

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable			Indicator for Exporter			
H_t	-0.013***	-0.012***	-0.012***	-0.012***	-0.012***	-0.012***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
H_{t+1}		0.008***	0.007***	0.008***	0.007***	0.008***
		(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
H_{t-1}			0.002	0.002	0.002	0.002
			(0.002)	(0.002)	(0.002)	(0.002)
H_{t-2}			-0.0003	-0.001	-0.001	-0.001
			(0.002)	(0.003)	(0.002)	(0.002)
H_{t-3}				0.002	0.002	0.002
				(0.002)	(0.002)	(0.002)
H_{t-4}				-0.001	-0.002	-0.001
				(0.002)	(0.002)	(0.002)
H_{t-5}					0.001	0.001
					(0.002)	(0.002)
H_{t-6}					0.002	0.003
					(0.002)	(0.002)
H_{t-7}						-0.001
						(0.002)
H_{t-8}						-0.003
						(0.002)
H_{t-9}						-0.001
						(0.002)
H_{t-10}						0.0001
						(0.002)
Cumulative effect ($\sum H_{t+s}$)	-	-0.005	-0.002	-0.002	0.0002	-0.003
P-value ($H_0: \sum H_{t+s} = 0$)	-	[0.155]	[0.580]	[0.677]	[0.973]	[0.662]
R-squared	0.002	0.002	0.002	0.002	0.002	0.002

Notes: N = 11,857,933. The dependent variable in all columns is an indicator for whether a firm exports on a given day. All regressions include day-of-year fixed effects, day-of-week fixed effects, year fixed effects, and a constant that is not reported. Robust standard errors in parentheses are clustered at the day level. *** p < 0.01, ** p < 0.05, * p < 0.1

Table 5: Heterogeneous Adjustment Behavior

	(1)	(2)	(3)
Dependent Variable	Indicator for Exporter		
Exporter Characteristic	Small	Medium	Large
H_t	-0.002	-0.011***	-0.022***
	(0.001)	(0.002)	(0.005)
H_{t+1}	0.0003	0.008***	-0.013***
	(0.001)	(0.002)	(0.005)
H_{t-1}	0.001	0.004	0.002
	(0.001)	(0.003)	(0.005)
H_{t-2}	-0.001	-0.001	-0.0005
	(0.001)	(0.002)	(0.005)
H_{t-3}	-0.0003	0.001	0.005
	(0.002)	(0.002)	(0.005)
H_{t-4}	0.0002	-0.001	-0.004
	(0.002)	(0.002)	(0.005)
H_{t-5}	-0.0001	-0.0002	0.002
	(0.001)	(0.002)	(0.004)
H_{t-6}	0.0001	0.002	0.004
	(0.001)	(0.002)	(0.004)
Cumulative effect ($\sum H_{t+s}$)	-0.001	0.003	-0.001
P-value (H_0: $\sum H_{t+s} = 0$)	[0.708]	[0.617]	[0.943]
Observations	3,912,929	4,030,622	3,914,382
R-squared	0.0004	0.001	0.005

Notes: small exporters are firms with an average shipment value over our sample period that is below the 33rd percentile. Medium exporters are firms with an average shipment value over the entire sample period that is between the 33rd and 67th percentiles. We classify all other exporters as large. All regressions include day-of-year fixed effects, day-of-week fixed effects, year fixed effects, and a constant that is not reported. Robust standard errors in parentheses are clustered at the day level. *** p<0.01, ** p<0.05, * p<0.1

Table 6: The Role of Other Exporter Characteristics

	(1)	(2)	(3)	(4)
Dependent Variable	Indicator for Exporter			
Exporter Characteristic	First Export In or After 2010	First Export Before 2010	Infrequent Shipper	Frequent Shipper
H_t	-0.006***	-0.018***	0.001	-0.024***
	(0.002)	(0.004)	(0.001)	(0.005)
H_{t+1}	-0.001	0.017***	0.0001	0.015***
	(0.003)	(0.004)	(0.002)	(0.005)
H_{t-1}	0.002	0.003	0.0001	0.004
	(0.004)	(0.005)	(0.002)	(0.005)
H_{t-2}	0.000	-0.002	-0.0001	-0.001
	(0.003)	(0.005)	(0.002)	(0.005)
H_{t-3}	0.002	0.001	0.001	0.003
	(0.003)	(0.005)	(0.002)	(0.005)
H_{t-4}	-0.001	-0.002	0.001	-0.004
	(0.003)	(0.005)	(0.002)	(0.005)
H_{t-5}	0.002	-0.0005	0.001	0.0005
	(0.003)	(0.004)	(0.002)	(0.004)
H_{t-6}	0.005	-0.001	0.002	0.002
	(0.003)	(0.004)	(0.002)	(0.004)
Cumulative effect ($\sum H_{t+s}$)	0.003	-0.003	0.005	-0.005
P-value (H_0: $\sum H_{t+s} = 0$)	[0.759]	[0.802]	[0.201]	[0.636]
Observations	6,359,781	5,498,152	5,973,283	5,884,650
R-squared	0.032	0.007	0.002	0.004

Notes: first export in or before 2010 refers to exporters whose first export shipment was in 2010 or after. The remaining exporters are classified as having first exported before 2010. Infrequent shippers are exporters with average number of annual shipment days over the entire sample period that is at or below the sample median. The remaining exporters are classified as frequent shippers. All regressions include day-of-year fixed effects, day-of-week fixed effects, year fixed effects, and a constant that is not reported. Robust standard errors in parentheses are clustered at the day level. *** p<0.01, ** p<0.05, * p<0.1

Table 7: *Hartals* and Export Shipments by *Hartal* Characteristic

	(1)	(2)	(3)	(4)	(5)
Dependent Variable	Indicator for Exporter				
Type of *Hartal*	Short-Notice	Long-Notice	Single-Day	Two to Four Day	Pre-Election
H_t	-0.009***	-0.018***	0.002	-0.006	-0.021**
	(0.003)	(0.003)	(0.002)	(0.004)	(0.010)
H_{t+1}	0.008***	0.007***	0.009***	0.006	0.005
	(0.003)	(0.003)	(0.002)	(0.005)	(0.014)
H_{t-1}	0.003	0.001	0.001	-0.030***	0.029**
	(0.003)	(0.003)	(0.002)	(0.007)	(0.012)
H_{t-2}	-0.001	-0.0005	-0.000	-0.005	-0.007
	(0.003)	(0.003)	(0.003)	(0.007)	(0.009)
H_{t-3}	-0.0002	0.005	0.001	-0.001	0.003
	(0.003)	(0.003)	(0.003)	(0.005)	(0.009)
H_{t-4}	-0.003	0.0005	-0.002	-0.0002	-0.003
	(0.003)	(0.003)	(0.003)	(0.005)	(0.010)
H_{t-5}	-0.002	0.006	0.001	-0.001	0.016*
	(0.002)	(0.003)	(0.002)	(0.004)	(0.009)
H_{t-6}	0.003	0.002	0.004	-0.0004	-0.002
	(0.002)	(0.003)	(0.002)	(0.004)	(0.008)
H_{t-7}				0.003	
				(0.004)	
H_{t-8}				-0.007	
				(0.005)	
H_{t+3}				-0.004	
				(0.005)	
H_{t+2}				-0.006	
				(0.005)	
Cumulative effect ($\sum H_{t+s}$)	-0.0002	0.003	0.016**	-0.051***	0.020
P-value ($H_0: \sum H_{t+s} = 0$)	[0.982]	[0.652]	[0.037]	[0.002]	[0.513]
Observations	11,572,298	11,335,629	11,311,146	11,205,053	851,605

Notes: short-notice *hartals* are those that were announced with three or fewer days' notice. All remaining *hartals* are classified as long notice. A single-day *hartal* is an episode in which there was a *hartal* on a given day but there wasn't a *hartal* on either the preceding or the next day. Two to four-day *hartals* are episodes in which there was a *hartal* on two to four consecutive days. Pre-election *hartals* are *hartals* that were announced during July to December, 2013. This is the six-month period that preceded the January, 2014 elections. The sample used in column (5) is restricted to this six-month period, which is why the sample size is significantly smaller. The regressions in columns (1) to (4) include day-of-year fixed effects, day-of-week fixed effects, and year fixed effects. The regression in column (5) includes an indicator for public holidays, a week-of-year fixed effects, and day-of-week fixed effects. Robust standard errors in parentheses are clustered at the day level. *** p<0.01, ** p<0.05, * p<0.1

Table 8: Robustness Checks

	(1)	(2)	(3)	(4)	(5)
Dependent Variable	Indicator for Exporter				
Estimation Method	FE	OLS			
H_t	-0.012***	-0.012***	-0.014***	-0.014***	-0.012***
	(0.002)	(0.002)	(0.003)	(0.003)	(0.002)
H_{t+1}	0.007***	0.007***	0.006*	0.006*	0.006***
	(0.002)	(0.002)	(0.003)	(0.003)	(0.002)
H_{t-1}	0.002	0.002	0.003	0.002	0.003
	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)
H_{t-2}	-0.001	-0.001	-0.001	-0.001	0.0001
	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)
H_{t-3}	0.002	0.002	0.002	0.002	0.002
	(0.002)	(0.002)	(0.003)	(0.003)	(0.002)
H_{t-4}	-0.002	-0.002	0.000	0.000	-0.002
	(0.002)	(0.002)	(0.003)	(0.003)	(0.002)
H_{t-5}	0.001	0.001	-0.001	-0.001	0.001
	(0.002)	(0.002)	(0.002)	(0.003)	(0.002)
H_{t-6}	0.002	0.002	0.003	0.003	0.001
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Cumulative effect ($\sum H_{t+s}$)	0.0002	0.0002	-0.003	-0.003	-0.001
P-value (H_0: $\sum H_{t+s} = 0$)	[0.973]	[0.973]	[0.630]	[0.683]	[0.825]
Observations	11,857,933	11,857,933	8,005,941	7,842,721	11,857,933
R-squared	0.203	0.002	0.001	0.001	0.003

Notes: in column (1) we report the estimates from a version of the baseline specification that includes firm fixed effects. In column (2) we report the estimates of the baseline specification where the standard errors have been clustered at both the firm and day level. In column (3) we estimate an augmented specification that controls for the daily Baltic Dry Index while in column (4) we control for both the Baltic Dry Index and the daily global crude oil price. These data are not available for every day in our working sample, which is why the sample sizes in columns (3) and (4) are smaller. In column (5) we estimate a version of the baseline specification where the day-of-year fixed effects have been replaced by month fixed effects and an indicator for a public holiday. All regressions include day-of-week fixed effects, year fixed effects, and a constant that is not reported. Robust standard errors in parentheses in columns (1) and (3) to (5) are clustered at the day level. *** p<0.01, ** p<0.05, * p<0.1

Table 9: The Impact of *Hartals* on Export Value and Export Mode

	(1)	(2)	(3)	(4)
Dependent Variable	Ln(Daily Exports)		Indicator For Air Shipment	
Estimation Method	OLS	Firm FE	OLS	Firm FE
H_t	-0.033	-0.037**	0.031***	0.031***
	(0.020)	(0.017)	(0.007)	(0.006)
H_{t+1}	0.017	0.028	-0.013	-0.004
	(0.024)	(0.022)	(0.010)	(0.007)
H_{t-1}	0.019	0.039*	-0.014*	-0.010
	(0.022)	(0.021)	(0.009)	(0.007)
H_{t-2}	0.020	0.022	0.008	-0.003
	(0.019)	(0.018)	(0.008)	(0.006)
H_{t-3}	0.012	0.006	-0.004	0.004
	(0.020)	(0.018)	(0.008)	(0.005)
H_{t-4}	0.006	-0.003	0.006	0.006
	(0.020)	(0.019)	(0.008)	(0.007)
H_{t-5}	0.048**	0.031*	-0.026***	-0.020***
	(0.020)	(0.019)	(0.009)	(0.007)
H_{t-6}	0.007	0.022	-0.004	0.006
	(0.019)	(0.017)	(0.017)	(0.020)
Cumulative effect ($\sum H_{t+s}$)	0.096*	0.109**	-0.017	0.011
P-value (H_0: $\sum H_{t+s} = 0$)	[0.090]	[0.019]	[0.400]	[0.481]
Observations	826,858	826,858	826,858	826,858
R-squared	0.009	0.248	0.013	0.196

Notes: the dependent variable in columns (1) and (2) are each firm's daily exports in natural logarithm. The sample in these columns is restricted to firm-day pairs with positive exports. The dependent variable in columns (3) and (4) is an indicator that takes the value of one if a firm uses air transport on any given day and is zero otherwise. The sample in these columns is also restricted to firm-day pairs with positive exports. All regressions include day-of-year fixed effects, day-of-week fixed effects, year fixed effects, and a constant that is not reported. Robust standard errors in parentheses are clustered at the day level. *** p<0.01, ** p<0.05, * p<0.1

Appendix

A.1. Sufficient Conditions for $V_2^* > V_1^*$

To ensure that the second cutoff, V_2^*, is greater than the first cutoff, V_1^*, we need the following sufficient conditions to hold. First, it must be the case that π_{d+1}^i is below π_{d-1}^i at $V^i = V_1^*$ and that that the slope of π_{d+1}^i is greater than the slope of π_{d-1}^i. The first of these conditions requires that

$$\left(1 - e^{-\delta V_1^*}\right)V^i - \tau_{d+1} < V^i - \tau_{d-1}$$

or

$$\delta < \frac{1}{V^i}\ln\left[\frac{V^i}{\tau_{d-1} - \tau_{d+1}}\right] \tag{A1}$$

The second sufficient condition requires that

$$1 - e^{-\delta V^i} + \delta V^i e^{-\delta V^i} > 1$$

or

$$\delta > \frac{1}{V^i} \tag{A2}$$

Thus, using (A1) and (A2), we can conclude that a sufficient condition for V_2^* to be greater than V_1^* is that the following condition holds:

$$\frac{1}{V^i} < \delta < \frac{1}{V^i}\ln\left[\frac{V^i}{\tau_{d-1} - \tau_{d+1}}\right]$$