How Does Violence Affect Exporters? Evidence from Political Strikes in Bangladesh*

Reshad N. Ahsan†
University of Melbourne

Kazi Iqbal‡
Bangladesh Institute of Development Studies

November, 2017

Abstract

Using novel, high-frequency data on political strikes, we examine the impact of political violence on the export-oriented garments industry in Bangladesh. Our data cover the universe of political strikes and export transactions in Bangladesh during 2010 to 2013. Using an event-study approach, we find that there is a 16.26 percent reduction in the baseline probability of exporting on the day of a strike. Most of these shipments are reallocated to the day before the strike, while the remaining shipments are delayed by up to six days. We then use country- and product-level data to examine the broader implications of these disruptive effects. We show that countries with greater political violence tend to have a comparative disadvantage in higher-priced garments. This suggests that political violence can prevent a country from moving up the garments value chain.

Keywords: Exports, Political Violence, Garments.
JEL Codes: F14, F50, D74

---

* We thank Emma Aisbett, Arpita Chatterjee, Shahe Emran, Rachel Heath, Raja Junankar, Fahad Khalil, Phil McCalman, Laura Puzzello, and various seminar and conference participants for helpful comments and suggestions. Mahbuba Khatun and Amin Bin Hasib provided excellent research assistance. We gratefully acknowledge funding from the International Growth Centre for this project. The standard disclaimer applies.
† Department of Economics, University of Melbourne 3010, Victoria, Australia; email: rahsan@unimelb.edu.au.
‡ E-17 Agargaon, Sher-e-Bangla Nagar, Dhaka 1217, Bangladesh; email: kiqbal@bids.org.bd.

1. Introduction

Political violence is an endemic feature in many developing countries. According to Strauss and Taylor (2009), 58 percent of elections in Sub-Saharan Africa during 1990 to 2007 involved some form of violence. The human cost of such violence can be catastrophic. For instance, political violence in 2007 and 2008 in Kenya alone killed approximately 1,200 people and displaced a further 500,000 (Ksoll, Machiavello, and Morjaria, 2014). Similarly, in the run-up to the January, 2014 elections in Bangladesh, hundreds of people were killed or injured due to political violence (Human Rights Watch, 2014). In addition to the tragic human toll, such political violence has important economic implications. One such implication is its effect on export activity. This is especially important for developing countries, where exports have played an important role in improving recent economic performance (WTO, 2003).[1] Thus, fully understanding both the short-run and long-run impact of political violence on exports is of first-order importance.

In this paper, we make two contributions to our understanding of the impact of political violence on export activity. First, using novel, high-frequency data, we examine the effects of political violence on garments export activity in Bangladesh. Our data cover the universe of political strikes and export transactions in Bangladesh between 2010 and 2013 and allow us to document the adverse effects of political violence at a highly granular level. We find that such violence significantly disrupts an exporter's ability to ship on the scheduled date. Next, using country- and product-level data, we show that the disruptions caused by such violence leads to a country having a long-run comparative disadvantage in higher-priced garments. This suggests that political violence can prevent countries from moving up the garments value chain.[2]

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 political strikes, which are known locally as *hartals*. These strikes are designed to disrupt the country's transportation network

[1] Further, exports represent a large share of overall economic activity in developing countries. According to the World Bank's *World Development Indicators*, the average ratio of exports of goods and services to GDP during the period 1995 to 2015 was 0.271 for lower-middle-income countries and 0.214 for low-income countries.

[2] As mentioned below, a previous literature has examined the effect of conflict and violence on *how much* countries trade. However, this literature has not examined whether violence affects *which products* countries export.

and are typically used by opposition parties to pressure the government to accept its demands.[3,4] To examine the impact of these strikes on export behavior, we use self-collected, daily data on all political strikes in Bangladesh during our sample period of 2010 to 2013. Our data include 99 such strikes during this period. Thus, a key innovation of our analysis is the use of such high-frequency data on political violence in a developing country. We pair our political strikes data with the universe of export transactions in Bangladesh during the same period. These data, which are collected by the National Board of Revenue, allow us to construct a daily panel of over 5,500 exporters. We use our high-frequency data to examine the impact of a strike 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 begins the day before each strike and ends six days after it. This means that, not only are we able to examine the impact of a strike on the day of a strike itself, we are also able to examine an exporter's adjustment behavior on the days immediately before and after a strike. Thus, our novel, high-frequency data allow us to precisely understand the way in which exporters adjust over a short event window around a strike.[5,6]

A second advantage of our setting is that the targeted nature of political strikes in Bangladesh 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 political strikes that we examine in this paper do not lead to direct damage to infrastructure and factories and creates very little worker absenteeism (Ashraf et al., 2015). Thus, these political strikes provide a uniquely "clean" shock that is free of other confounding factors

---

[3] 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.

[4] Note that these strikes are entirely political in nature and do not involve any labor unrest or work stoppages.

[5] Our use of a short event window is essential because the median political strike 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 strike. Further, the need for a short event window validates our decision to focus on exports rather than production. While high-frequency export data 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.

[6] The average exporter in our sample makes approximately 95 shipments a year. Thus, they ship at a frequency that is high enough for us to observe their adjustment behavior over an eight-day event window.

and allow us to isolate the effect of political violence on exporters through the transport disruption channel alone.

Our baseline results suggest that these political strikes are highly disruptive to garments export activity in Bangladesh. We find that the probability that a firm in our sample will export on the day of a strike is 1.60 percentage points lower than a comparable non-strike day. This represents a 16.26 percent reduction from the baseline probability of exporting. We also find that the majority of these reduced shipments are reallocated to the day before a strike, while the remaining shipments are delayed by up to six days. Due to this reallocation, there is no cumulative reduction in the value of goods exported over our eight-day event window. Nonetheless, given that the median political strike is announced with three days' notice, changing shipment dates with such short notice is highly disruptive.

Having documented the disruptive effects of these strikes, we then examine its broader implications on Bangladesh's garments exports. In particular, we ask whether these political strikes affect the type of products that are exported from Bangladesh. This question is motivated by surveys of purchasing officers representing Western retailers. These purchasing officers cite political violence and instability as a factor that affects where they source their garments from (McKinsey, 2011; McKinsey, 2017). We are particularly interested in whether such violence diminishes Bangladesh's ability to move up the garments value chain. That is, away from the export of low-price, low-quality garments, which has typically been its area of comparative advantage, to high-price, high-quality garments.[7]

To examine whether political violence affects Bangladesh's pattern of comparative advantage, we first use HS6 product-level data to document the evolution of Bangladesh's garments exports. We show that during the period in which political strikes in Bangladesh have become more prevalent and disruptive, Bangladesh's garments exports have become increasingly concentrated in low-priced products. To confirm that this reflects a causal relationship, we use cross-country, HS6 product-level data to examine the extent to which this evolution in Bangladesh's comparative advantage is due to its

[7] To the extent that higher-priced garments are less generic, it will have relatively lower sales volume than a lower-priced, generic garment. In turn, this means that such products will have more variable demand (Abernathy, Dunlop, Hammond, and Weil, 1999). Thus, if political strikes create greater uncertainty about delivery dates, then it will disproportionately affect Bangladesh's exports of higher-priced, variable-demand garments. Evans and Harrigan (2005) show that the demand variability of a garment affects where it is sourced from.

worsening political violence.[8] We find that countries with greater political violence, as measured by the World Bank's *World Development Indicators*, have a comparative disadvantage in higher-priced garments products. This result is robust to allowing a country's comparative advantage in higher-priced garments to be driven by its level of human capital, it's income per capita, it's physical capital stock, its quality of contract enforcement, as well as its level of financial development. Thus, our results confirm that greater political violence can prevent a developing country such as Bangladesh from moving up the garments value chain.

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. Thus, we can cleanly isolate the effect of political violence on exporters that operates through this channel alone and do not have to worry about other channels through which such violence might affect exporters.

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 political strikes in Bangladesh lower firm productivity. Machiavello and Morjaria (2015) examine how election-related violence affects 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

[8] Our econometric approach is based on Campante and Chor (2017), Chor (2010), and Nunn (2007). See Nunn and Trefler (2014) for a comprehensive review of the literature on the effect of institutions on comparative advantage.

Thoenig, 2008; Glick and Taylor, 2010) and to a literature that examines the impact of supply-chain uncertainty on trade (Clark, Kozlova, and Schaur, 2016).[9] 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).

We structure the remainder of the paper as follows. In section 2, we provide further background on political strikes in Bangladesh as well as on its export-oriented garments industry. In section 3, we describe our political strikes data and discuss the evolving nature of these strikes during our sample period. We also discuss our export data in this section. In section 4, we introduce our econometric specification and discuss some econometric issues. In section 5, we describe our baseline results. In section 6, we explore the robustness of our baseline results and examine the effect of *hartals* on the value of exports and an exporter's choice of air shipment. In section 7, we use country and HS6 product-level data to examine whether political violence affects a country's pattern of comparative advantage. Finally, in section 8 we provide a conclusion.

2. Background

2.1. *Hartals* in Bangladesh

Political strikes, or *hartals* from hereon, are a form of 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. 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

[9] 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.

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 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).[10] 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

[10] 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.

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.[11] 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.

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 riskier, *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. Further, there is also a non-negligible probability of shipment loss if a shipment is damaged or destroyed by political activists.[12]

In contrast, *hartals* do not make it significantly costlier for garments workers to travel to their factory. The mainly female workforce in the garments industry tends to live very close to their place

[11] 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).

[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. Thus, 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. 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 most exporters in our data, we can rule out the possibility that the transport disruption caused by *hartals* represents a trivial additional cost.

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 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*.

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 (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 binding 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.[13] 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.

3. Data

3.1. *Hartal* Data

To examine the effects of *hartals* on export behavior, we compiled a database of all nation-wide *hartals* in Bangladesh during the period 2005 to 2013 using two popular Bengali and English language daily newspapers. 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

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

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.[14]

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 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 to 2009, *hartals* were announced 7.28 days before the *hartal* itself. However, during the period 2010 to 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*.[15] Thus, along all dimensions reported in Table 1, *hartals* have become more disruptive in Bangladesh in recent years.

[14] 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).

[15] 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.

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 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. These data allow us to construct a daily, exporter-level panel for the period 2005 to 2013.

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 0.99. 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 uninterrupted 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]

To construct our working sample, we restrict our data to exporters in the ready-made garments industry. During our sample, ready-made garments exports accounted for 79.40 percent of all Bangladeshi exports and 76.31 percent of all Bangladeshi exporters. We also omit observations that do not include the date of export and drop exporters whose average number of shipments per year is less

[16] When we use the entire period (2005 to 2013), we still observe that a reduced probability of making an export shipment during a *hartal*, but the magnitude of the effect is considerably larger than what we report below. Further, with the entire period, we observe a large, negative cumulative over our eight-day event window. Thus, by restricting the sample to the period 2010 to 2013, we are effectively erring on the side of caution by presenting more conservative estimates.

than or equal to the 3rd percentile and exporters whose average value of annual shipments is less than or equal to the 3rd percentile.[17] 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.[18] We aggregate these to ensure that there is only one observation per firm per day.

In Table 3 we report some descriptive statistics of the exporters in our working sample. Our sample consists of 5,551 garments firms that have exported during our sample period of 2010 to 2013.[19] On average, 598 firms export on any given day. The average exporter in our sample exports 5.46 products per year, where a product is defined at the HS6 level. Such a firm also exports to 5.48 destinations per year and makes 94.75 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.[20]

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

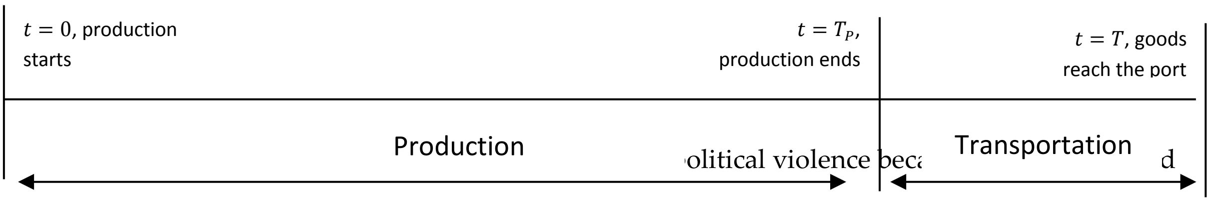
[17] These omissions are motivated by the presence of exporters that send samples to Western buyers to demonstrate the quality of their work. These exporters are not responding to an actual purchase order from a buyer, but are instead trying to establish a reputation for quality in the hopes of obtaining a future purchase order. We chose to omit these exporters as they engage in transactions that are fundamentally different from the remaining exporters in our data. These omitted exporters account for just 2.18 percent of total garments exports in Bangladesh over our entire sample period.

[18] 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.

[19] 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.

[20] 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.

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.



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 its exports at day T.[21] 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. 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 the natural logarithm of a firm's total daily exports. 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.

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

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 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.[22]

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

[22] 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.

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. Fortunately, we find that the average exporter in our sample exports 94.75 days per year. Further, the average gap between shipment days is 5.65 days. Both of these numbers suggest that the firms in our working sample export with relatively high frequency and therefore an eight-day event window is sufficient 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 2. 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, 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 3. 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*.[23] For these reasons, *hartals* in Bangladesh peak around November and December. Thus, there is no evidence in Figure 3 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.

5. Results

5.1. 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 1.80 percentage points.[24] This represents an

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

[24] 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 robust if we cluster them at the two-way level (firm and day) instead.

18.29 percent decline from the baseline probability of exporting.[25] 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 a 1.70 percentage point reduction in the probability of making an export shipment on the day of the *hartal* itself, there is a 1.10 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 *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 0.60 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

[25] The magnitude of this displacement effect is consistent with survey responses by Bangladeshi exporters and Western purchase officers. 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 both relatively small in magnitude and statistically insignificant. In both cases, the cumulative effect is a 0.30 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. Importantly, we observe a 1.60 percentage point reduction in the probability of exporting during a *hartal*, which is a 16.26 percent decline from the baseline probability of exporting. In Figure 4, we illustrate the evolution of the cumulative effect of a *hartal* over our eight-day event window. As is evident from this figure, most of the reduced shipments during a *hartal* are reallocated to the day before, while the remaining shipments are delayed by up to six days.

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.[26] 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.

5.2. Results by *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. To explore this, we

[26] 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.

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 5 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 seasonally-adjusted non-*hartal* day, we omit from our sample in column (1) days in which there was a long-notice *hartal*.[27]

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 5. 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.

[27] In our baseline estimation in section 5.1, 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 5, 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, we 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.

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 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*.[28] 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*.

6. Further Results

6.1. Results by Exporter Size

Our results thus far suggest that *hartals* lead to a significant displacement of export shipments. Exporters in our sample respond to a *hartal* by reducing their shipment probability on the day of a *hartal*. However, we also find that exporters reallocate their shipments in a way such that *hartals* do not have a cumulative effect on the probability of exporting during an eight-day event window. We now explore whether this displacement effect and resulting adjustment behaviour varies by exporter size. To do so, we first calculate each exporter's average daily shipment value over the entire sample period. We then classify an exporter as small if its average daily shipment value over the entire sample period is at or below the sample median. All other exporters are classified as large. We then estimate our baseline specification for each of these groups of exporters separately. We report the results from these

[28] 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*.

regressions in Table 6. In column (1) we restrict the sample to small exporters while in column (2) we restrict the sample to large exporters respectively. The results suggest that both small and large exporters reduce their probability of making an export shipment on the day of a *hartal* and increase it the day before. Thus, both types of exporters engage in qualitatively similar adjustment behavior.

6.2. Robustness Checks

In Table 6, we also 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

$$\Pr[X_{it} > 0] = \alpha_2 + \sum_{s=-1}^{6} \beta_s H_{t-s} + \theta_i + \theta_d^w + \theta_d^Y + \theta_y + \vartheta_{it} \tag{2}$$

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 (2) are reported in column (3) of Table 6.[29] 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.

[29] 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.

Next, in column (4) 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 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.

6.3. 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 7 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 statistically significant.

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 partially account for day-to-day compositional changes in the sample.

The coefficient of the *hartal* indicator, H_t, in column (2) remains negative and 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*, although the former is not precisely estimated. Interestingly, in both the OLS regression in column (1) and the fixed-effects regression in column (2), we find that there is no cumulative reduction in the total value of goods exported during our eight-day 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 7 indicate that while *hartals* do cause exporters to increasingly switch to expensive air transport, these exporters attenuate the effects of this on their profitability by lowering their use of air transport on subsequent shipments.

7. Political Violence and Comparative Advantage

Our results thus far suggest that political violence disrupts a Bangladeshi garments exporter's ability to make a shipment on the targeted day. We now examine the broader implications of these disruptive effects. In particular, we ask whether the disruption and uncertainty caused by political violence affects the type of garments that are sourced from Bangladesh. This question is motivated by survey evidence that suggests that political violence and instability is a factor that determines where Western retailers source their garments from (McKinsey, 2017).[30] To examine whether this is the case, we use trade data at the HS6 digit product level. These data are originally from UN Comtrade and have been collated by CEPII (Gaulier and Zignago, 2010). It includes data from 222 countries over the period 1995 to 2015. We restrict the data to garments products, which yields a dataset of 233 products.[31] With these data in hand, we first document some stylized facts about the evolution of Bangladesh's

[30] Muhammad, D'Souza, and Amponsah (2013) show that EU importers switched some of their flower imports away from Kenya and to other countries in the aftermath of the electoral violence in Kenya in 2007.
[31] Garments products are defined as products with a 2-digit HS code of 61 or 62.

garments exports. Our primary interest is in evaluating the extent to which Bangladesh has managed to move up the garments value chain. That is, away from the export of low-priced/low-quality garments, which has been its traditional area of comparative advantage, to high-priced/high-quality garments.[32]

To document these trends, we first classify garments products into low-price and high-price categories. We do using the CEPII trade data, which provides the quantity of goods exported in tonnes for all products in the data. This allows us to calculate the unit price per kilogram for each HS6 product.[33,34] We then designate a product as low priced if its median unit price is at or below the sample median, while we designate all other products as high priced. To be consistent with our regression analysis below, we use the median price for each product between 1995 and 1999 to create our low price and high price categories.

The evolution of Bangladesh's garments exports is illustrated in Figure 5. This figure show that during the period 1995 to 2015, Bangladesh's garments exports have become increasingly concentrated in low-priced products. In 1995, Bangladesh's exports of low-priced garments represented 2.19 percent of world exports of these products. By 2015, this percentage had increased to 9.86 percent. In contrast, Bangladesh's export share of high-priced garments increased from 1.11 percent to 2.95 percent over the same period. As Figure 5 also demonstrates, Bangladesh's export divergence is much more striking than the divergence for other lower-middle-income countries.[35] Interestingly, the divergence in Bangladesh's exports is especially pronounced from the mid-2000's onwards. Recall from Figure 1 and Table 1 that this is approximately the same period in which *hartals* became increasingly prevalent and disruptive in Bangladesh.

---

[32] To the extent that higher-priced garments are less generic, they will have relatively lower sales volume compared to a low-priced, generic garment. All else equal, the lower sales volume will likely lead to its demand being more volatile (Abernathy et al., 1999). It follows that if political violence creates greater uncertainty in delivery dates, it will have a disproportionate effect on Bangladesh's exports of high-priced, variable demand garments. Evans and Harrigan (2005) show both theoretically and empirically that the demand variability of a garments product affects where it is sourced from.

[33] Unit prices calculated in this manner is commonly used as a measure of quality in the trade literature (e.g. see Schott (2004) and Hallak (2006)). In our regression analysis, we show that our results are robust to using an alternate measure of quality estimated using the method proposed by Khandelwal, Schott, and Wei (2013).

[34] To minimize measurement error, we omit unit prices that are either below the 5th percentile or above the 95th percentile.

[35] We classify countries in to income groups based on the data provided by Beck, Demirgüç-Kunt, and Levine (2000). They classify Bangladesh as a lower-middle-income country, which is why we show the trends for other lower-middle-income countries in Figure 5.

7.1. Econometric Strategy

Of course, we cannot conclude from Figure 5 alone that political violence has affected Bangladesh's pattern of comparative advantage in garments. It could be the case that the trends depicted in this figure are determined by other factors such as human capital, physical capital, financial development etc. Further, Bangladesh's share of world export in any product will also be a function of shocks in other countries. For instance, Bangladesh's rising share of low-quality garments exports could be mainly driven by changes elsewhere, such as rising labor costs in China. Thus, to examine whether political violence has affected Bangladesh's pattern of comparative advantage in garments, we need to account for other sources of comparative advantage and include export data from other countries. To do so, we follow Campante and Chor (2017) and Nunn (2007) to estimate the following specification:[36]

$$\ln(X_{cjr}) = \alpha_2 + \gamma_1 PV_{cr} \times \ln(P_j) + \gamma_2 HC_{cr} \times SI_k + \gamma_3 \ln(K_{cr}) \times KI_k + \theta_c \times \theta_r + \theta_j + \eta_{cjr} \tag{3}$$

where X_{cjr} is country c's exports of HS6 product j in period r.[37] PV_{cr} is an increasing measure of a country's political violence and is based on the World Bank's *World Governance Indicators* (WGI). The WGI data includes a political stability measure, which captures "perceptions of the likelihood of political instability and/or politically-motivated violence, including terrorism." This measure is particularly well suited to our application given that it captures a form of political violence that is closely related to the *hartals* that we have examined earlier in the paper. For ease of interpretation, we multiply the raw data with minus one to ensure that a higher value captures greater political violence.

Next, P_j is the median unit price for each HS6 product. We use the median price to ensure that our quality measure is not sensitive to price outliers. Further, we calculate this median price over the period 1995 to 1999 to ensure that it is as exogenous as possible to changes in political violence and exports during our sample period.[38] The resulting measure, P_j, is a time-invariant, product-specific

---

[36] A static version of this specification is used in Nunn and Trefler (2014), who provide a thorough review of the literature on the institutional determinants of comparative advantage. See also Chor (2010), who derives a theory-consistent version of (3).

[37] While the trade data we use are available annually from 1995 to 2015, the political violence data that we describe above are available for 1996, 1998, 2000, 2002, and then annually from 2003 to 2015. To ensure that the time-periods are consistent and to account for any idiosyncratic time variation in the political violence data, we follow Campante and Chor (2017) and average our data over multi-year periods. These periods are 1995 to 1999, 2000 to 2004, 2005 to 2009, and 2010 to 2015.

[38] Our results are robust to using the median price of each product in 1995.

measure of price. Our key coefficient of interest is γ_1. We expect γ_1 to be negative, which would indicate that greater political violence causes a country to have a comparative disadvantage in high-priced garment products.

In addition to political violence, we control for a country's factor endowments to account for other sources of its comparative advantage. In particular, we include an interaction between a country's level of human capital, HC_{cr}, with the skill intensity of production for each industry (k), SI_k. We also include the interaction between the natural logarithm of a country's physical capital, K_{cr}, and the capital intensity of production for each product, KI_k.[39, 40] Summary statistics for all variables used in these comparative advantage regressions are provided in Table A.1 in the Appendix.

We include in (3) country and period interaction fixed effects to flexibly control for any time-varying, country-specific shocks that might be correlated with both a country's exports and its political violence. For instance, as countries become wealthier we would expect its politics to become more stable and for its patterns of comparative advantage to evolve. Our interaction fixed effects will capture the effect of such changes. Further, an importing firm's decision on where to source their garments from will also depend on an exporter's labor costs as well as transportation costs. Both costs will vary over time and will be captured by our country and period interaction fixed effects. Thus, conditional on including these interaction fixed effects, we expect our measure of political violence to be exogenous to exports. Next, we include product fixed effects to account for time-invariant, product characteristics that may determine where a garment is sourced from. For instance, all else equal, a bulkier garment is more likely to be sourced from nearby countries to minimize transportation costs. By including product fixed effects in (3), we will be capturing the effect of bulk and other time-invariant product characteristics on exports. Lastly, η_{cjr} is a classical error term.

[39] We proxy a country's human capital using the human capital index provided by the *Penn World Tables* while we proxy a country's physical capital using its capital stock at constant 2011 US dollars also provided by the *Penn World Tables*.

[40] We define an industry's skill intensity as the ratio of non-production to all workers while we define its capital intensity as the natural logarithm of capital stock in millions of US dollars divided by the number of employees. These variables are taken from the NBER-CES database. The raw data are the 4-digit 1987 SIC level. We merge these to our trade data using a converter provided by the *World Integrated Trade Solution*.

7.2. Results

We present the results from estimating equation (3) in Table 8. In column (1), the coefficient of the interaction between political violence and product price is negative and statistically significant. This suggests that, all else equal, countries with greater political violence have a comparative disadvantage in higher-priced garments products. To gauge the magnitude of this interaction coefficient, consider the HS6 product with the average price [ln (Price) = 3.80]. Now suppose Bangladesh, which has an average political violence index of 1.30 over the last period in our data (2010 to 2015), were to have the average index value of India of 1.14 over the same period. The coefficient of the interaction term in column (1) suggests that this improvement in political violence would lead to a 10.09 percent increase in Bangladesh's exports of the product with the average price.[41]

Recall that in our baseline specification we've included a product's median price during the initial period of our sample (1995 to 1999). This was done to minimize any simultaneity bias between price and trade value. To address this concern further, we now drop the first period from our sample and re-estimate equation (3). The results from this are reported in column (2) of Table 8. As these results suggest, even after dropping the first period from our sample, our coefficient of interest remains robust with a magnitude that is relatively close to that of column (1).

Thus far we have used unit price as a proxy for product quality. We now examine the robustness of our core result by using an alternate measure of product quality. This measure is adopted from Khandelwal et al. (2013).[42] They show that if one allows a buyer to have a utility function that incorporates product quality and take logs of the resulting product demand function, it is possible to write down an expression that can be used to estimate product quality directly. The regression analogue of this expression is

$$\ln q_{cjr} + \sigma \ln p_{cjs} = \alpha_j + \alpha_c \times \alpha_r + \epsilon_{cjr} \tag{4}$$

where q is the quantity of an HS6 product j that is exported by country c in period r. σ is the elasticity of substitution between varieties. Following Khandelwal et al. (2013), we assume that $\sigma = 4$. p is the unit price of product j exported by country c, α_j are product fixed effects, and $\alpha_c \times \alpha_r$ are country and

[41] This is calculated by multiplying the improvement in political violence (1.14 – 1.30) with the coefficient in column (1) of -0.166 and the ln (Price) of the average product of 3.80.

[42] See also Khandelwal (2010), who offers an alternate approach to infer product quality from the demand side.

period interaction fixed effects. Khandelwal et al. (2013) show that the residual in (4) can be used to directly estimate the quality of a product, $\hat{\lambda}_{cjr}$.[43] The underlying intuition behind this method is that, conditional on its price, the greater the quantity of a product that is exported the greater is its quality. To be consistent with our baseline proxy for quality, we estimate (4) over the period 1995 to 1999. This yields a value of $\hat{\lambda}$ for each country-product-year in our data. We then calculate the median value of $\hat{\lambda}$ for each product over this period. Thus, this measure of quality varies by product alone and not by period or country. We then replace P_j with $\hat{\lambda}_j$ and re-estimate (3). The results from this regression are listed in column (3) of Table 8. As is clear from this column, the interaction between political violence and product quality remains negative and statistically significant.

Up to this point, we have only allowed a country's comparative advantage in higher-priced garments to be a function of its degree of political violence. This is an important restriction to the extent that political violence is correlated with other country characteristics that are the true determinants of a country's comparative advantage. For instance, in our data, a country's degree of political violence is correlated with the overall level of human capital there. Thus, what we've estimated so far could be capturing the fact that countries with greater human capital, and not political violence, specialize in higher-priced garments products. To examine this, we now allow other country characteristics to play a role in our baseline specification (equation (3)). More precisely, in column (1) of Table 9 we augment the baseline specification by adding the interaction between a country's human capital and the unit value of an HS6 product. This interaction term is positive and statistically significant, which suggests that countries with greater human capital do indeed have a comparative advantage in higher-priced garments. Importantly, however, our coefficient of interest remains negative and statistically significant even after this additional interaction term is included.

A country's comparative advantage in higher-priced garments could also be explained by its overall income. Higher-income countries may specialize in higher-priced products due to (a) having local demand that is skewed towards higher-priced products (Linder hypothesis) or (b) being relatively skill abundant. Dingel (2017) shows that the former is as important a determinant of a country's specialization in higher-priced products as the latter. This means that human capital alone will not control for the way in which income affects the patterns of specialization. To account for this, we include in column (2) an interaction between the natural logarithm of a country's real GDP per capita

[43] More precisely, they show that $\hat{\lambda}_{cjr}$ can be written as $\ln \hat{\lambda}_{cjr} = \epsilon_{cjr}/(\sigma - 1)$.

(in constant 2011 US dollars) and the unit price of a product. As the coefficients in this column demonstrate, our primary result is robust to including this additional interaction term.

In column (3) we add the interaction between a country's capital stock (in natural logarithm) and the unit price of a product. The coefficient of this additional interaction term is small and statistically insignificant. Further, the coefficient of interest remains highly robust. Next, in columns (4) and (5) we add the interaction between a country's quality of contract enforcement and the unit price of a product and the interaction between a country's degree of financial development and the unit price respectively.[44,45] In both cases, the additional interaction terms are small and statistically insignificant. Importantly, in both cases, the coefficient of interest remains highly robust. Thus, the results in Table 9 suggest that introducing other commonly used determinants of a country's comparative advantage to our baseline specification does not change our key result.

8. 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 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, allow us to construct a working sample consisting of a daily panel of 5,551 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.

[44] We measure a country's contract enforcement using the Enforcing Contracts variable from the World Bank's *Doing Business* dataset. The precise variable used captures the gap in contract enforcement between each country in the sample and the country with the best contract enforcement (i.e. the frontier). These data are available annually from 2004 onwards, which is why the sample size in column (4) is considerably smaller than the baseline.
[45] We follow Manova (2013) and measure a country's financial development as the ratio of private credit by deposit money banks and other financial institutions to GDP. These data are from Beck et al. (2000).

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 1.60 percentage points. This represented a 16.26 percent reduction from the baseline probability of exporting. Our results suggested that the vast majority of these reduced shipments were reallocated to the day before a *hartal* while the remaining shipments were delayed by up to six days. We also found that there was no cumulative reduction in the value of goods exported because of a *hartal* during our eight-day event window. Overall, these results suggested that while Bangladeshi garments exporters were resilient enough to ensure that there was no overall reduction in shipments as a result of a *hartal*, these episodes of political violence did create significant disruptions for both Bangladeshi garments exporters as well as foreign buyers.

We then examined the broader implications of such violence on Bangladesh's garments exports. In particular, we asked whether these political strikes affect the type of products that are exported from Bangladesh. We were particularly interested in whether such violence diminished Bangladesh's ability to move up the garments value chain. To examine this, we used country- and HS6-product-level data to examine the effect of political violence on a country's comparative advantage in garments. We found that countries with greater political violence, as measured by the World Bank's *World Development Indicators*, had a comparative disadvantage in higher-priced garments products. This result was robust to allowing a country's comparative advantage in higher-priced garments to be driven by its level of human capital, it's income per capita, it's physical capital stock, its quality of contract enforcement, as well as its level of financial development. Thus, our results confirmed that greater political violence can prevent a developing country such as Bangladesh from moving up the garments value chain.

References

Abadie, A., Gardeazabal, J., 2003. "The Economic Costs of Conflict: A Case Study of the Basque Country." *American Economic Review*, 93(1): 113−132.

Abernathy, F.H., Dunlop, J.T., Hammond, J.H., Weil, D., 1999. *A Stitch in Time: Lean Retailing and the Transformation of Manufacturing – Lessons from the Apparel and Textile Industries.* Oxford: Oxford University Press.

Ahmed, I., Mortoza, G., 2005. "The Anatomy of Hartal: How to Stage a Hartal," in *Beyond Hartal: Toward Democratic Dialogue in Bangladesh*. Dhaka: United Nations Development Programme.

Ashraf, A., Machiavello, R., Rabbani, A., Woodruff, C., 2015. "The Effect of Political and Labour Unrest on Productivity: Evidence from Bangladeshi Garments." Mimeograph.

Alesina, A., Özler, S., Roubini, N., Swagel, P., 1996. "Political Instability and Economic Growth." *Journal of Economic Growth*, 1(2): 189−211.

Barbieri, K., Reuveny, R., 2005. "Economic Globalization and Civil War." *The Journal of Politics*, 67(4): 1228−1247.

Beck, T., Demirgüç-Kunt, A., and Levine, R., 2000. "A New Database on Financial Development and Structure." *World Bank Economic Review*, 14(3): 597−605.

Besedes, T., Murshid, A., 2015. "Experimenting with Ash: The Trade Effects of Airspace Closures in the Aftermath of Eyjafjallajkull." Mimeograph.

Birtwistle, G., Siddiqui, N., Fiorito, S., 2013. "Quick Response: Perceptions of UK Fashion Retailers." *International Journal of Retail & Distribution Management*, 31(2): 118−128.

Blattman, C., Miguel, E., 2010. "Civil War." *Journal of Economic Literature*, 48(1): 3−57.

Blomberg, S., Hess, G., 2006. "How Much Does Violence Tax Trade? *The Review of Economics and Statistics*, 88(4): 599−612.

Campante, F.R., Chor, D., 2017. "'Just Do Your Job': Obedience, Routine Tasks, and the Pattern of Specialization." Mimeograph.

Chor, D., 2010. "Unpacking the Sources of Comparative Advantage: A Quantitative Approach." *Journal of International Economics*, 82(2): 152−167.

Clark, D.P., Kozlova, V., Schaur, G., 2016. "Supply Chain Uncertainty in Ocean Transit as a Trade Barrier," Mimeograph.

Collier, P., Duponchel, M., 2012. "The Economic Legacy of Civil War: Firm-Level Evidence from Sierra Leone." *Journal of Conflict Resolution*, 57(1): 65−88.

Daily Star, 2013. “National Public Perception Study: A Special Supplement.” November 2.

Dingel, J., 2017. “The Determinants of Quality Specialization.” *The Review of Economic Studies*, 84(4): 1551−1582.

Djankov, S., Freund, C., Pham, C., 2010. “Trading on Time.” *The Review of Economics and Statistics*, 92(1): 166−173.

Eaton, J., Kortum, S., Kramarz, F., 2011. “An Anatomy of International Trade: Evidence from French Firms.” *Econometrica*, 79(5): 1453−1498.

Evans, C., Harrigan, J., 2005. “Distance, Time, and Specialization: Lean Retailing in General Equilibrium.” *American Economic Review*, 95(1): 292−313.

Fernandes, A., 2008 “Firm-Level Productivity in Bangladesh Manufacturing Industries.” *World Development*, 36(10): 1725−1744.

Gaullier, G., Zignago, S., 2010. “BACI: International Trade Database at the Product Level. The 1994-2007 Version.” *CEPII Working Paper No. 2010-23*.

Glick, R., Taylor, A., 2010. “Collateral Damage: Trade Disruption and the Economic Impact of War.” *The Review of Economics and Statistics*, 92(1): 102−127.

Guidolin, M., La Ferrara, E., 2007. “Diamonds Are Forever, Wars are Not: Is Conflict Bad for Private Firms?” *American Economic Review*, 97(5): 1978−1993.

Guimaraes, P., Portugal, P., 2010. “A Simple Feasible Procedure to Fit Models with High-Dimensional Fixed Effects.” *The Stata Journal*, 10(4): 628−649.

Hallak, J.C., 2006. “Product Quality and the Direction of Trade.” *Journal of International Economics*, 68(1): 238−265.

Haroon, J., 2012. “Hartal Halts 4,000 Containers Daily at Ctg Port.” *The Financial Express*, April 30.

Heath, R., Mobarak, A.M., 2015. “Manufacturing Growth and the Lives of Bangladeshi Women.” *Journal of Development Economics*, 115: 1−15.

Human Rights Watch, 2014. *Democracy in the Crossfire*.

Hummels, D., Schaur, G., 2013. “Time as a Trade Barrier.” *American Economic Review*, 103(7): 2935−2959.

Khandelwal, A.K., 2010. “The Long and Short (of) Quality Ladders.” *The Review of Economic Studies*, 77(4): 1450−1476.

Khandelwal, A.K., Schott, P.K., Wei, S., 2013. “Trade Liberalization and Embedded Institutional Reform: Evidence from Chinese Exporters.” *American Economic Review*, 103(6): 2169−2195.

Ksoll, C., Machiavello, R., Morjaria, A., 2014. "Guns and Roses: Flower Exports and Electoral Violence in Kenya." Mimeograph.

Machiavello, R., Morjaria, A., 2015. "The Value of Relationships: Evidence from a Supply Shock to Kenyan Rose Exports." *American Economic Review*, 105(9): 2911−2945.

Manova, K., 2013. "Credit Constraints, Heterogeneous Firms, and International Trade." *The Review of Economic Studies*, 80(2): 711−744.

Martin, P., Mayer, T., and Thoenig, M., 2008. "Civil Wars and International Trade." *Journal of the European Economic Association*, 6(2−3): 541−550.

McKinsey, 2011. *Bangladesh's Ready-Made Garments Landscape: The Challenge of Growth*.

McKinsey, 2017. *The Apparel Sourcing Caravan's Next Stop: Digitization*.

Muhammad, A., D'Souza, A., Amponsah, W., 2013. "Violence, Instability, and Trade: Evidence from Kenya's Cut Flower Sector." *World Development*, 51(1): 20−31.

Nitsch, V., Schumacher, D., 2004. "Terrorism and International Trade: An Empirical Investigation." *European Journal of Political Economy*, 20(2): 423−433.

Nunn, N., 2007. "Relationship-Specificity, Incomplete Contracts, and the Pattern of Trade." *Quarterly Journal of Economics*, 122(2): 569−600.

Nunn, N., Trefler, D., 2014. "Domestic Institutions as a Source of Comparative Advantage," in the *Handbook of International Economics Volume 4*. Amsterdam-Elsevier.

Quddus, M, Rashid, S., 2000. *Entrepreneurs and Economic Development: The Remarkable Sotry of Garment Exports from Bangladesh*. Dhaka: University Press Limited.

Schott, P.K., 2004. "Across-Product Versus Within-Product Specialization in International Trade." *Quarterly Journal of Economics*, 119(2): 647−678.

Shonchoy, A.S., Tsubota, K., 2015. "Economic Impact of Political Protests (Strikes) on Manufacturing Firms: Evidence from Bangladesh." IDE Discussion Paper No. 523.

Sobhan, R., 2004a. "Structural Dimensions of Malgovernance in Bangladesh." *Economic and Political Weekly*, 39(36): 4101−4108.

Sobhan, Z., 2004b. "The Mathematics of Hartals." *The Daily Star*, March 24.

Strauss, S., Taylor, C., 2009. "Democratization and Electoral Violence in Sub-Saharan Africa." Mimeograph.

Suykens, B., Islam, A., 2013. "*Hartal* as a Complex Political Performance: General Strikes and the Organisation of (Local) Power in Bangladesh." *Contributions to Indian Sociology*, 47(1): 61−83.

Taplin, I., 2014. “Global Commodity Chains and Fast Fashion: How the Apparel Industry Continues to Re-Invent Itself.” *Competition and Change*, 18(3): 246−264.

Volpe Marincus, C., Blyde, J., 2013. “Shaky Roads and Trembling Exports: Assessing the Trade Effects of Domestic Infrastructure Using a Natural Experiment.” *Journal of International Economics*, 90(1): 148−161.

World Bank, 2016. *Doing Business in 2016: Measuring Regulatory Quality and Efficiency*. Washington, D.C.

WTO, 2003. *World Trade Report 2003: Trade and Development*. Geneva: WTO.

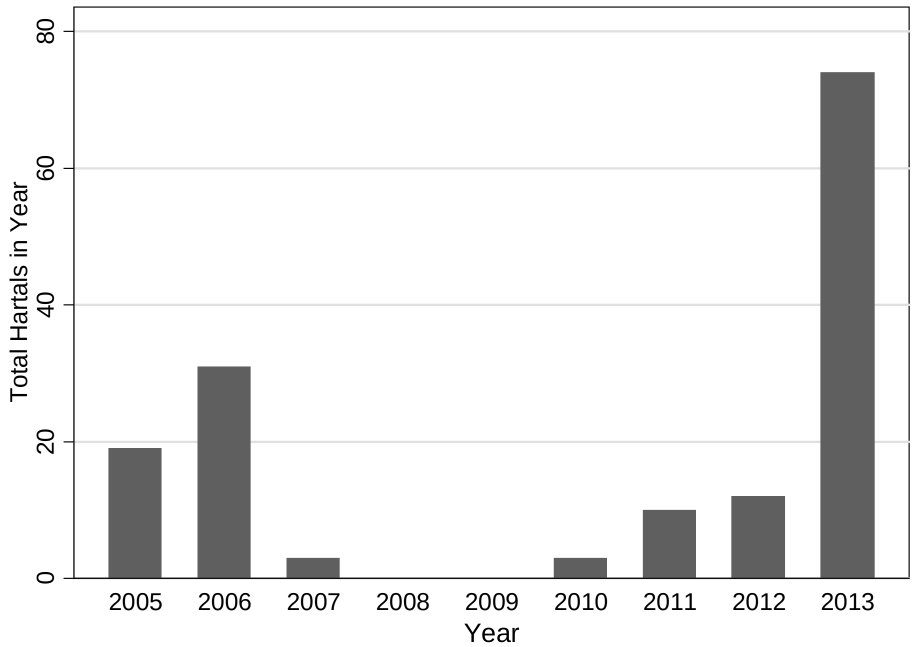


Figure 1: Annual trend in *hartals*.

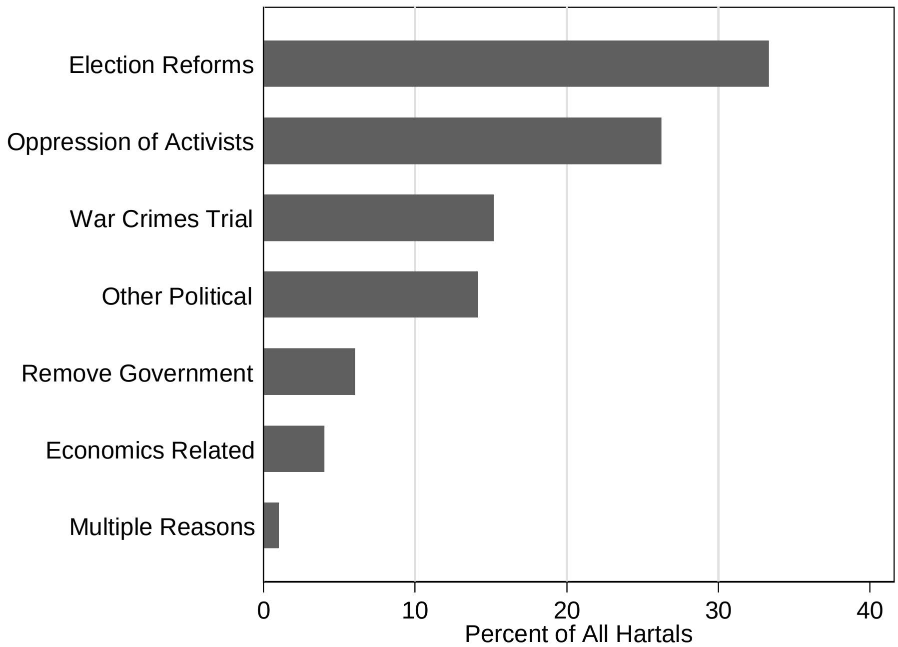


Figure 2: The stated reasons for calling a *hartal* (2010 to 2013).

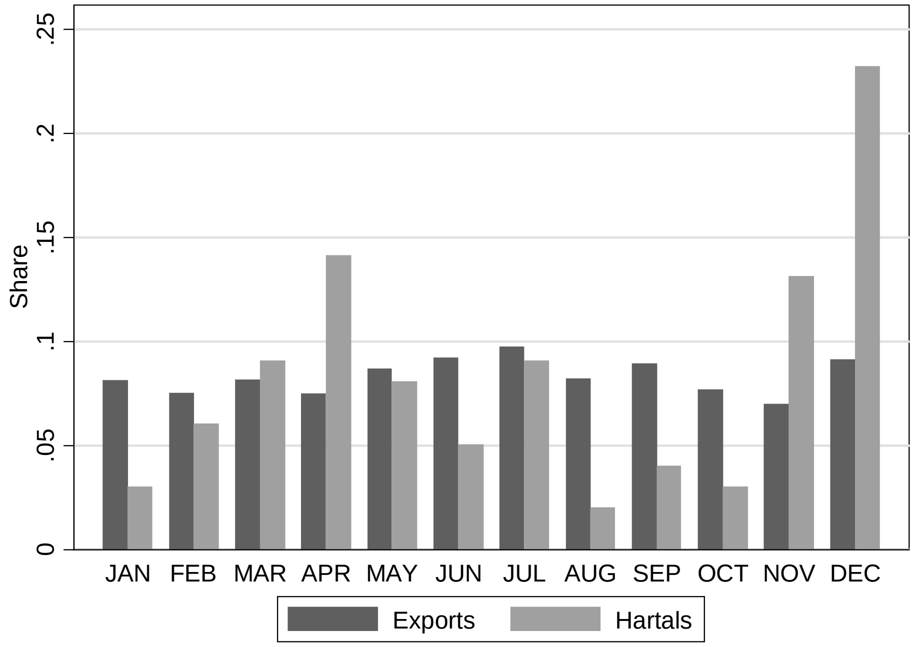


Figure 3: Distribution of *hartals* and daily exports by month. The correlation coefficient between these two variables is 0.05.

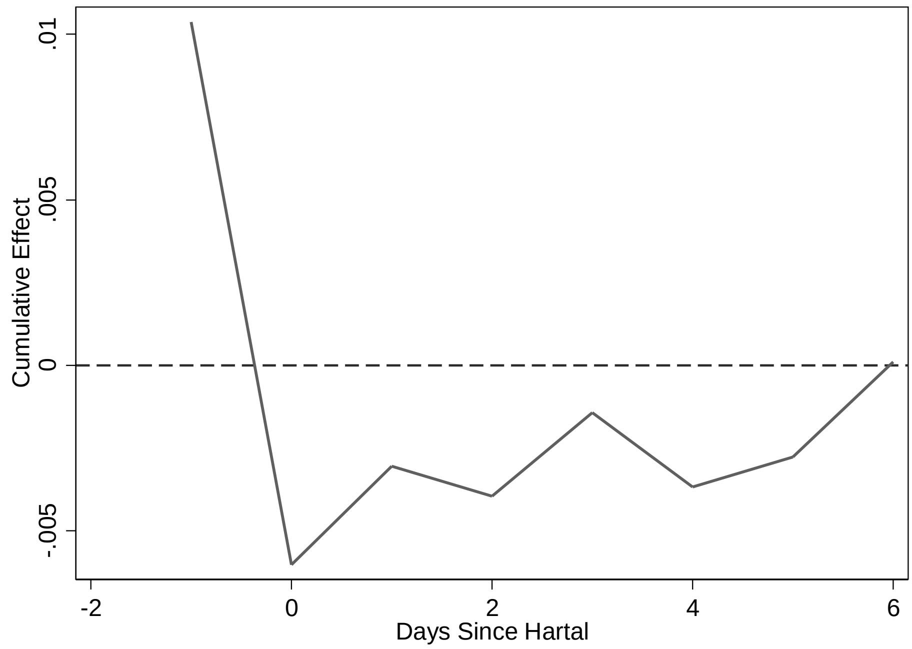


Figure 4: Cumulative effect of a *hartal* on the probability of exporting. A zero on the horizontal axis represents the day of the *hartal*.

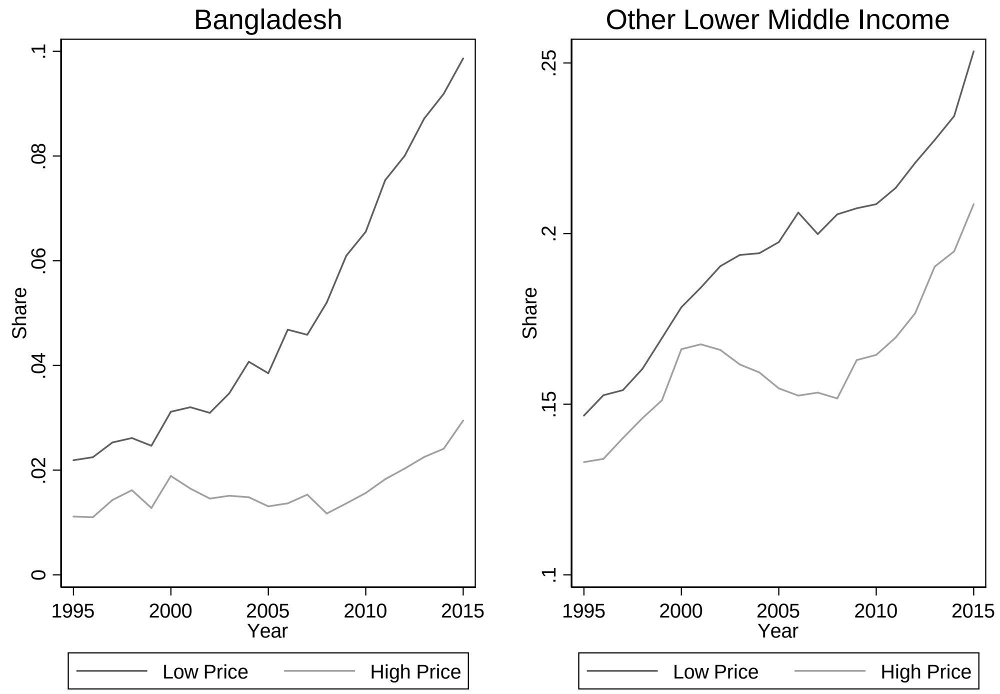


Figure 5: Trends in export shares (relative to total word exports) by garments product type. Low-price garments are products whose median unit price during 1995 to 1999 is at or below the sample median. All other garments products are classified as high priced. The second graph includes all lower-middle-income countries, excluding Bangladesh, in our data. We use the classification provided by Beck et al. (2000) to designate a country as lower middle income.

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
Notice Provided (in days)	5.55	7.28	4.62
Number of Deaths	1.49	0.52	2.01
Number of Injuries	112.68	132.92	101.84

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

Table 2: Validation of the Customs Exports Data

	(1)	(2)	(3)
Year	Customs	World Bank	Customs / World Bank
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 calculated using our customs data. In column (2) we report the aggregate annual export data as reported by the World Bank. These are based on balance of payments calculations. The correlation coefficient between the two is 0.98. In column (3) we report the ratio of the customs aggregate to the World Bank 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

	(1)	(2)
	Mean	Median
Total Number of Exporters	5,551	-
Exporters per Day	598.37	601.00
	[142.79]	
Daily Firm Exports	4.64	2.37
	[7.36]	
Number of HS6 Products	5.46	4.00
per Firm per Year	[5.04]	
Number of Destinations	5.48	4.00
per Firm per Year	[6.23]	
Number of Firm Shipment	94.75	75.00
Days per Year	[69.15]	
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. In column (2) we report the median of each variable. 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	-0.018***	-0.017***	-0.017***	-0.017***	-0.016***	-0.017***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
H_{t+1}		0.011***	0.011***	0.011***	0.010***	0.011***
		(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
H_{t-1}			0.003	0.003	0.003	0.003
			(0.003)	(0.003)	(0.003)	(0.003)
H_{t-2}			-0.000	-0.001	-0.001	-0.001
			(0.003)	(0.003)	(0.003)	(0.003)
H_{t-3}				0.003	0.003	0.003
				(0.003)	(0.003)	(0.003)
H_{t-4}				-0.002	-0.002	-0.002
				(0.003)	(0.003)	(0.003)
H_{t-5}					0.001	0.001
					(0.003)	(0.003)
H_{t-6}					0.003	0.004
					(0.003)	(0.003)
H_{t-7}						-0.001
						(0.003)
H_{t-8}						-0.004
						(0.003)
H_{t+3}						0.000
						(0.003)
H_{t+2}						-0.001
						(0.003)
Cumulative effect ($\sum H_{t+s}$)	-	-0.006	-0.003	-0.003	0.000	-0.004
P-value (H_0: $\sum H_{t+s} = 0$)	-	[0.155]	[0.584]	[0.680]	[0.988]	[0.654]
R-squared	0.003	0.003	0.003	0.003	0.003	0.003

Notes: N = 8,065,603. 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: *Hartals* and Export Shipments by *Hartal* Characteristic

	(1)	(2)	(3)	(4)
Dependent Variable	Indicator for Exporter			
Type of *Hartal*	Short-Notice	Long-Notice	Single-Day	Two to Four Day
H_t	-0.013***	-0.025***	0.013***	0.009
	(0.004)	(0.004)	(0.003)	(0.007)
H_{t+1}	0.011***	0.010**	0.003	-0.008
	(0.004)	(0.004)	(0.003)	(0.006)
H_{t-1}	0.005	0.002	0.001	-0.042***
	(0.005)	(0.004)	(0.003)	(0.009)
H_{t-2}	-0.001	-0.001	-0.000	-0.007
	(0.005)	(0.004)	(0.004)	(0.009)
H_{t-3}	-0.000	0.007*	0.002	-0.001
	(0.005)	(0.004)	(0.004)	(0.007)
H_{t-4}	-0.004	0.001	-0.003	-0.000
	(0.004)	(0.005)	(0.004)	(0.007)
H_{t-5}	-0.003	0.008**	0.002	-0.002
	(0.004)	(0.004)	(0.003)	(0.005)
H_{t-6}	0.004	0.002	0.006	0.001
	(0.003)	(0.004)	(0.004)	(0.006)
H_{t-7}				0.004
				(0.004)
H_{t-8}				-0.010
				(0.007)
H_{t+3}				-0.006
				(0.008)
H_{t+2}				-0.009
				(0.007)
Cumulative effect ($\sum H_{t+s}$)	-0.001	0.005	0.022**	-0.071***
P-value ($H_0: \sum H_{t+s} = 0$)	[0.943]	[0.619]	[0.047]	[0.002]
Observations	7,871,318	7,710,339	7,693,686	7,621,523

Notes: the dependent variable in all columns is an indicator for whether a firm exports on a given day. 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. 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: Results by Exporter Size and Robustness Checks

	(1)	(2)	(3)	(4)
Dependent Variable	Indicator for Exporter			
Estimation Method	OLS		FE	OLS
Exporters Included	Small	Large	All	
H_t	-0.010***	-0.023***	-0.016***	-0.016***
	(0.002)	(0.005)	(0.003)	(0.003)
H_{t+1}	0.007***	0.014***	0.010***	0.010***
	(0.002)	(0.005)	(0.003)	(0.003)
H_{t-1}	0.004	0.002	0.003	0.003
	(0.002)	(0.005)	(0.003)	(0.003)
H_{t-2}	-0.001	-0.001	-0.001	-0.001
	(0.002)	(0.005)	(0.003)	(0.003)
H_{t-3}	0.001	0.004	0.003	0.003
	(0.002)	(0.005)	(0.003)	(0.003)
H_{t-4}	-0.000	-0.004	-0.002	-0.002
	(0.002)	(0.005)	(0.003)	(0.003)
H_{t-5}	-0.000	0.002	0.001	0.001
	(0.002)	(0.004)	(0.003)	(0.003)
H_{t-6}	0.002	0.004	0.003	0.003
	(0.002)	(0.004)	(0.003)	(0.003)
Cumulative effect ($\sum H_{t+s}$)	0.001	-0.001	0.000	0.0002
P-value ($H_0: \sum H_{t+s} = 0$)	[0.765]	[0.904]	[0.988]	[0.988]
Observations	4,033,528	4,032,075	8,065,603	8,065,603
R-squared	0.001	0.005	0.186	0.003

Notes: the dependent variable in all columns is an indicator for whether a firm exports on a given day. In column (1) we restrict the sample to exporters with average shipment value that is below the sample median, while in column (3) we restrict the sample to the remaining exporters. In column (3) we report the estimates from a version of the baseline specification that includes firm fixed effects. In column (4) we report the estimates of the baseline specification where the standard errors have been clustered at both the firm and day level. 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) to (3) are clustered at the day level. *** p<0.01, ** p<0.05, * p<0.1

Table 7: 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	-0.034*	-0.036**	0.031***	0.032***
	(0.020)	(0.018)	(0.007)	(0.006)
H_{t+1}	0.014	0.030	-0.014	-0.004
	(0.024)	(0.022)	(0.010)	(0.007)
H_{t-1}	0.015	0.038*	-0.014	-0.010
	(0.022)	(0.021)	(0.009)	(0.007)
H_{t-2}	0.025	0.027	0.008	-0.003
	(0.019)	(0.019)	(0.008)	(0.006)
H_{t-3}	0.013	0.008	-0.004	0.004
	(0.020)	(0.018)	(0.008)	(0.005)
H_{t-4}	0.006	-0.004	0.006	0.006
	(0.020)	(0.019)	(0.008)	(0.007)
H_{t-5}	0.049**	0.030	-0.026***	-0.020***
	(0.020)	(0.019)	(0.009)	(0.007)
H_{t-6}	0.008	0.026	-0.005	0.005
	(0.019)	(0.017)	(0.008)	(0.007)
Cumulative effect ($\sum H_{t+s}$)	0.095*	0.120**	-0.018	0.010
P-value ($H_0\colon \sum H_{t+s} = 0$)	[0.094]	[0.011]	[0.369]	[0.523]
Observations	793,459	793,459	793,459	793,459
R-squared	0.009	0.221	0.013	0.187

Notes: the dependent variable in columns (1) and (2) is the natural logarithm of each firm's daily exports. 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

Table 8: Political Violence and Comparative Advantage

	(1)	(2)	(3)
Dependent Variable	Ln (Export Value)		
Political Violence × ln (Price)	-0.166***	-0.159***	
	(0.049)	(0.050)	
Political Violence × ln (Quality)			-2.098***
			(0.449)
Constant	9.616***	10.569***	9.016***
	(1.484)	(1.707)	(1.480)
Observations	100,014	77,156	100,014
R-squared	0.748	0.755	0.749

Notes: the dependent variable in all columns is the natural logarithm of a country's exports in an HS6 garments product category. In column (2), we omit the first period (1995 to 1999) from our data, which is why the sample size in this column is smaller. All regressions control for a country's human capital and its interaction with an industry's skill intensity as well as a country's capital stock and its interaction with an industry's capital intensity. All regressions also include 6-digit HS product fixed effects and country and period interaction fixed effects. Robust standard errors in parentheses are clustered at the country level. *** p<0.01, ** p<0.05, * p<0.1

Table 9: Other Sources of Comparative Advantage

	(1)	(2)	(3)	(4)	(5)
Dependent Variable			Ln (Export Value)		
Political Violence × ln (Price)	-0.102**	-0.126***	-0.167***	-0.203***	-0.172***
	(0.043)	(0.044)	(0.049)	(0.065)	(0.051)
Human Capital × ln (Price)	0.368***				
	(0.103)				
Ln (GDP per capita) × ln (Price)		0.100**			
		(0.047)			
Ln (Capital Stock) × ln (Price)			-0.008		
			(0.032)		
Contract Enforcement × ln (Price)				0.009	
				(0.006)	
Financial Development × ln (Price)					-0.000
					(0.001)
Constant	4.494***	5.949***	9.969***	11.415***	9.594***
	(1.707)	(2.058)	(2.034)	(2.017)	(1.527)
Observations	100,014	100,014	100,014	70,122	98,702
R-squared	0.749	0.748	0.748	0.761	0.748

Notes: the dependent variable in all columns is the natural logarithm of a country's exports in an HS6 garments product category. All regressions control for a country's human capital and its interaction with an industry's skill intensity as well as a country's capital stock and its interaction with an industry's capital intensity. In each of these columns, the "level" effect of the additional country characteristics are absorbed by the country and period interaction fixed effects. All regressions include 6-digit HS product fixed effects and country and period interaction fixed effects. Robust standard errors in parentheses are clustered at the country level. *** p<0.01, ** p<0.05, * p<0.1

Appendix

Table A.1: Summary Statistics and Source of Variables Used in the Comparative Advantage Regressions

Variable	Observations	Mean [Std. Dev]	Source
ln (Export Value)	100,014	6.057 [2.897]	UN Comtrade/CEPII
Political Violence	100,014	0.005 [0.909]	"Political Stability Index", World Governance Indicators
ln (Price)	100,014	3.791 [0.305]	Constructed by authors
ln (Quality)	100,014	0.049 [0.053]	Constructed by authors
Human Capital Index	100,014	2.561 [0.641]	Penn World Tables
ln (Capital Stock)	100,014	13.023 [1.875]	Penn World Tables
Skill Intensity	100,014	0.820 [0.040]	NBER-CES Database
Capital Intensity	100,014	2.789 [0.215]	NBER-CES Database
ln (GDP per capita)	100,014	9.214 [1.170]	Penn World Tables
Contract Enforcement	70,122	59.560 [13.960]	Doing Business Database
Financial Development	98,702	56.057 [45.957]	Beck et al. (2000)

Notes: this sample consists of 140 countries and 232 HS6 garments products over 5-year periods between 1995 and 2015. Export value is in thousands of constant 2011 U.S. dollars while capital stock and GDP per capita are each in millions of constant 2011 U.S. dollars. Contract enforcement is the gap in contract enforcement between each country in the sample and the country with the best contract enforcement in any year. A country's financial development as the ratio of private credit by deposit money banks and other financial institutions to GDP.