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JEL Classification: D74, N43, F51, F10

Keywords: conflict, Atlantic Trade, International Relations

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Abstract

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1 Introduction

Between the late Middle Ages and the outbreak of World War I, the number of intra-European conflicts decreased dramatically. While around one in five Europeans died due to conflict in the 15th century, about one in 100 did so in the 19th century (Morris, 2014). Scholars have proposed various explanations for this dramatic shift towards peace. For instance, Schroeder (1986) argues that the durable political settlement brought about by the Congress of Vienna after the Napoleonic Wars (1792-1815) led to a reduction in intra-European conflict. Findlay and O'Rourke (2009) explain that the technologies of the Industrial Revolution enhanced the West's comparative advantage in the use of violence and therefore diverted conflict away from Europe and towards imperial pursuits. Pinker (2011) highlights the positive effect that the Enlightenment had on peace via the spread of values of tolerance (i.e. the "humanitarian revolution"). However, given that the trend towards peace in Europe predates these events (Figure 1), the hypotheses above cannot fully account for the decline in conflict in Europe.

In this paper, we propose an alternate explanation: access to Atlantic trade. Previous efforts at examining this relationship have been constrained by a lack of historical trade data. We overcome this obstacle by using over 200 years of wheat prices (1640-1850) to construct a country- and year-level panel dataset of price pass-through between Europe and the New World. We use this measure of price pass-through as our proxy for Atlantic trade. In terms of time period covered, this is the most historically extensive dataset available to study the impact of trade on conflict onset and allows us to provide the first quantitative evidence that Atlantic trade led to a reduction in intra-European conflict onset.[1]

In theory, the impact of Atlantic trade on intra-European conflict onset is ambiguous. On the one hand, European countries that engage in Atlantic trade will have more to lose (in terms of forgone trade) if conflict between them raises their overall trade costs. The possibility of such forgone trade can deter conflict. Further, by increasing real wages (and hence a worker's outside option), Atlantic trade can increase the cost of raising an army (Findlay and Amin, 2008; Dal Bó and Dal Bó, 2011). This will also lead to lower intra-European conflict. On the other hand, when traded goods are substitutes, greater

[1] In the existing literature, the most extensive historical analysis of the trade-conflict relationship is by Barbieri (2002). She uses data from 1870 to 1992 to examine whether greater bilateral trade leads to lower probability of bilateral conflict.

trade with the New World will reduce bilateral trade between European countries. This reduction in bilateral dependence will increase the likelihood of conflict (Martin et al., 2008). Lastly, much of our sample period overlaps with the age of mercantilism, a period in which conflict was used to establish commercial dominance over imperial trade routes (Findlay and O'Rourke, 2009). Thus, greater scope for Atlantic trade can result in more conflict as European countries fight to establish imperial dominance. Ultimately, how Atlantic trade affected intra-European conflict onset is an empirical question.

To examine this relationship, we proceed in two steps. First, we use our wheat price data to estimate the degree of price pass-through between New World and European markets. A higher pass-through implies greater connectedness between these two locations and hence greater Atlantic trade. To validate the use of this price pass-through measure as a proxy for Atlantic trade, we show that it is positively correlated with actual trade during the 19th century, which is the earliest period in which trade data for our panel of European countries are available. In the second step, we examine whether two European countries' trade with the New World, as well as their bilateral trade, affect the likelihood of conflict onset between them.

To identify the causal effect of Atlantic trade on conflict onset, we rely on two sources of exogenous, weather-based shocks as instruments. First, we use annual data on tropical cyclone activity over the Atlantic Ocean. Limited meteorology and navigation technology, along with poor seaworthiness, made sailing ships during our sample period particularly vulnerable to weather-induced shipwrecks (Rappaport and Fernández-Partagás, 1997; Trouet et al., 2016). Thus, these tropical cyclones provide us with an exogenous shock to the volume of trade across the Atlantic. All else equal, this will reduce trade between Europe and the New World.[2]

While the tropical cyclone activity measure varies by year, it does not vary by European country. To induce such cross-sectional variation in our instrument, we interact annual tropical cyclone activity with wind-based sailing time between the New World and each European country in our sample using data from Pascali (2017). Prior to the common use of steamships in the 1870s, Atlantic trade was dominated by sailing vessels, which were heavily dependent on wind direction. This meant that fluctuations in wind

[2]Using data from 1500 to 1899, García-Herrera et al. (2005) show that tropical cyclone activity in the Atlantic typically began in July, peaked in September, and occurred as late as December. They also show that storms occurred throughout the year over the Atlantic. Thus, the fact that tropical cyclones occurred over a six-month period makes it unlikely that sailing ships could avoid them altogether.

patterns created exogenous changes in sailing times across the Atlantic Ocean. Note that these shipping times are predicted using wind patterns alone and are not based on any observable data. Thus, they will not be affected by navigation expertise, shipping technology or any other confounding factors.

Our identifying assumption is that the differential effect of weather shocks over the Atlantic based on wind-based sailing times only affects intra-European conflict onset via Atlantic trade. There are at least two ways in which this assumption can be violated. First, due to spatial correlation in weather, tropical cyclone activity over the Atlantic Ocean could result in greater rainfall in Europe. By altering agricultural income, these rainfall shocks can affect conflict onset independently of Atlantic trade. We address this concern by controlling for rainfall in Europe in our instrumental variable (IV) regressions. Second, adverse weather shocks may also affect naval conflicts independently of Atlantic trade. We address this by showing that our results are fully robust to excluding all naval conflicts from our sample.

A concern with our identification strategy is that year-to-year variation in tropical cyclone activity is too transitory to affect conflict. This concern is mitigated by our use of conflict onset as the dependent variable rather than conflict. By using the former, we are estimating the effect of Atlantic trade on the timing of *when* two European countries engage in conflict, rather than on the overall level of war. This is inherently a more short-run choice and will be plausibly affected by short-run volatility in weather. Nonetheless, we show that our results are robust to replacing annual tropical cyclone activity with a five-year average over the period $t = 0, -1, ..., -4$. In the latter case, we exploit longer-term variation in tropical cyclone activity to instrument Atlantic trade.

Our IV estimates confirm that greater Atlantic trade did indeed have a pacifying effect in Europe. They imply that the increase in Atlantic trade between the mid-17th century and the early 19th century lowered the probability of conflict onset by 19.22 percent from a baseline onset probability of 2.30 percent.

To validate our main result and to identify the key channels linking trade and conflict onset, we show that Atlantic trade led to an increase in real wages in Europe. We also show that Atlantic trade reduced European military sizes, which is consistent with higher wages increasing the cost of raising an army. We further illustrate that the pacifying effects of Atlantic trade were stronger for country pairs that had extensive trade links with the New World (Britain, France, Portugal, and Spain), which suggests that the possibility of forgone Atlantic trade acted as a deterrent to conflict. Finally, we rule out that our results are being driven by other plausible channels such as income and agricultural shocks, and changes in state capacity and institutions.

Our paper contributes to several strands of the literature. First, our results are related to a broad interdisciplinary literature examining the long-run causes of European pacification.[3] We contribute to this literature by being the first to quantitatively document the pacifying effects of Atlantic trade in Europe. We also contribute to a broader literature that studies the determinants of inter-state conflict (Wright, 1942; Hirshleifer, 1988; Black, 1994; Fearon, 1995) by providing a novel trade-based explanation from what is arguably the biggest trade discovery in the Early Modern period.

Second, we contribute to a large empirical and theoretical literature on the impact of bilateral trade on bilateral conflict.[4] The central question in much of this literature has been whether bilateral trade deters bilateral conflict. In contrast, our paper examines whether trade with a third country has spillover effects on the likelihood of bilateral conflict. A relatively closer paper to ours is Martin et al. (2008), who show that during the second half of the 20th century, greater multilateral trade is associated with higher probability of bilateral conflict. An advantage of our setting is that we study a period in which limited meteorology and navigation technology made trade much more vulnerable to adverse weather. This allows us to exploit exogenous weather shocks and wind-based sailing times to better identify the causal effects of trade on conflict.

Third, our paper belongs to the body of work that investigates other determinants of historical conflict. This literature has shown that historical conflict can be explained by religion (Iyigun, 2008), agricultural productivity (Iyigun et al., 2017a), climate (Iyigun et al., 2017b), a monarch's gender (Dube and Harish, 2020), and elites' intermarriage patterns (Benzell and Cooke, 2021).[5] Fourth, our work closely relates to the literature on the welfare effects of historical trade (Acemoglu et al., 2005; O'Rourke and Williamson, 2005; Bosker et al., 2013; Pascali, 2017).

[3] This literature dates back to at least Elias (1930) and includes key contributions by Chesnais (1981), Schroeder (1986), Findlay and O'Rourke (2009), Pinker (2011), and Morris (2012).

[4] Polachek (1980); Barbieri (1996); Findlay (1996); Gartzke et al. (2001); Russett and Oneal (2001); Skaperdas and Syropoulos (2001); Barbieri (2002); Gelpi and Grieco (2008); Dal Bó and Dal Bó (2011); Copeland (2015); Jackson and Nei (2015). See Barbieri (2002) and Morelli and Sonno (2017) for extensive reviews of this literature.

[5] Our paper is also related to a literature that examines the interconnectedness between historical conflict and state capacity (Besley and Persson, 2008; Gennaioli and Voth, 2015; Becker et al., 2018)

We structure the remainder of the paper as follows. In section 2, we discuss the theoretical channels through which Atlantic trade will affect intra-European conflict. In section 3, we describe our data and provide some historical background on trade and conflict during our sample period. In section 4, we describe how we calculate price pass-through and discuss our identification strategy. In section 5, we discuss our results and the mechanisms behind them. In section 6, we explore whether Atlantic trade displaced conflict to other geographic areas and report robustness checks. Lastly, in section 7, we provide a conclusion.

2 Atlantic Trade and Conflict in Europe: Mechanisms

"We enjoy at this hour an uninterrupted peace, while all the rest of Europe is either actually engaged in war, or is on the very brinks of it. Our trade is at a greater height than ever, while other countries have scarce any, thro' their own incapacity, or the nature of their government."

Daily Courant, 13 June 1734[6]

The fact that trade can deter conflict is a common, albeit controversial, view in the literature.[7] In this section, we highlight mechanisms through which Atlantic trade can either decrease or increase conflict onset in Europe. To be consistent with our empirical analysis below, we assume that conflict only occurs between European countries and not between a European country and the New World. Such conflict imposes two primary costs. First, when two European countries engage in conflict, they not only disrupt their bilateral trade (Polachek, 1980) but also their trade with the New World.[8] This forgone trade represents an opportunity cost of conflict.

Second, to engage in conflict, countries must raise an army. This results in workers being reallocated away from tradable industries (Findlay and Amin, 2008; Dal Bó and Dal Bó, 2011). The resulting loss of tradable output is another cost of conflict. Thus, both the trade-cost channel and the worker-reallocation channel represent costs of intra-

[6]Cited in Black (1984).

[7]An alternate view holds that greater trade, to the extent that it leads to asymmetric gains, can lead to greater conflict. See Barbieri (2002) for a thorough review of this literature.

[8]"During conflicts, borders are closed, transport infrastructures are destroyed, and confidence is shaken. These can affect both bilateral and multilateral trade costs" (Martin et al., 2008, p. 872).

European conflict. Both of these costs will naturally be more important for a European country that trades extensively with the New World. To put it slightly differently, a European country that does not have pre-existing trade with the New World will not be constrained by either of these costs. It follows that by making conflict more costly, greater Atlantic trade will have a pacifying effect in Europe.

There are several instances in the Early Modern period of trade being at the heart of countries' decision to engage in conflict. Indeed, the prospects of gains from trade provided a powerful incentive for weakening political leaders' willingness to use belligerent tactics. A prominent example is the government of Walpole in Britain (1721-42), who adopted a ruling strategy that both actively avoided provoking rivals and promoted international trade (Black, 1984). A clear case of such a strategy in play was the decision to not send any British troops during the War of Polish Succession (1733-5). The importance of preserving peace in the interest of trade is also demonstrated by the repeated recurrence of this idea in many of the speeches he gave to the king (Black, 1984). Another prominent example is the government of de Witt in the United Provinces (1653-1672), whose main objective was to maintain peace in order to enjoy the trading advantage gained by the Dutch (Rowen, 2015). De Witt's rule was inspired by the economist and businessman Pieter de la Court and particularly his publication *Interest van Holland*, which became the *de facto* government manifesto. This document advocated neutrality for the United Provinces in order to preserve trade and peace.[9]

While the prospect of forgone trade deterring conflict is supported by historical examples, there are other possible channels through which trade may deter conflict or indeed make conflict more likely. First, in the previous literature, trade between Europe and the New World has often been modeled using a Heckscher-Ohlin set up (O'Rourke and Williamson, 1994). In addition to the channels above, Atlantic trade based on Heckscher-Ohlin forces will also affect European wages. To understand this more clearly, suppose that the New World is relatively land abundant and commodities are relatively land intensive. This means that Atlantic trade will decrease the relative price of commodities in Europe, and via Stolper-Samuleson, will increase European wages. All else equal, higher

[9] A related example is from Turgot's influence on French government policies during his appointment as chief administrator of Limoges and as French minister of finance, trade, and public works (1761-1776). Turgot was a strong supporter of free trade and opposed war on the grounds of preserving economic growth (Groenewegen, 2012). Similarly, Gian Gastone de' Medici had the goal of maintaining trade and peace in the Grand Duchy of Florence. This was achieved by favoring the nomination of pope Clement XII, who was committed both to maintaining peace in Europe and favoring the Medici's economic and trade interests (Galluzzi, 1781).

wages will increase the cost of militarization and lower the likelihood of intra-European conflict.

Second, there are at least two reasons why Atlantic trade can make conflict more likely in Europe. As first pointed out by Martin et al. (2008), when traded products are substitutes, greater trade with other countries will lower bilateral trade between i and j. As a result, when i and j engage in conflict, the opportunity cost of forgone bilateral trade will be a less effective deterrent. Thus, all else equal, greater multilateral trade will make conflict between two countries more likely. Central to this result is the assumption that traded products are substitutes. While this assumption is consistent with empirical estimates produced using modern data, it may not be applicable during our sample period. This is because trade between the New World and Europe in the Early Modern period was often in non-competing goods, which are goods that were available in the New World but not in Europe (Findlay and O'Rourke, 2007). These goods are likely to not be substitutable with European goods or could even be complements. In these cases, greater multilateral trade need not lead to greater bilateral conflict (Martin et al., 2008).

Next, our sample period also overlaps with the Age of Mercantilism, a period in which European powers used the colonies as a source of raw materials and a market for its manufactures. Hence, conflicts during this period were motivated by a desire to maintain exclusive access to colonies. Success in such conflicts secured access to trade, which in turn helped finance greater success in conflict (Viner, 1948). This mechanism implies that greater scope for Atlantic trade will increase conflict between European countries.

To summarize, the mechanisms highlighted in the existing literature point to an ambiguous theoretical relationship between Atlantic trade and intra-European conflict. Atlantic trade will increase the cost of conflict in two ways: (a) by raising the opportunity cost (in terms of forgone trade) and (b) by raising the cost of militarization. On the other hand, Atlantic trade will make conflict in Europe more likely by (a) lowering the bilateral dependence between two European countries and (b) by increasing the payoff to conflict in terms of having exclusive access to the New World. Therefore, how Atlantic trade will impact intra-European conflict is an empirical question.[10]

[10] As we discuss in more detail below, there were rapid improvements in military technology during the latter part of the period we study, which made wars more expensive and violent. However, to the extent that the improvement in military technology was a Europe-wide phenomena, it cannot explain why country pairs that traded more with the New World would disproportionately lower conflict with each other.

3 Data

3.1 Wheat Price Data

A key challenge to empirically exploring the impact of Atlantic trade on conflict in Europe is the lack of historical trade data. The most historically extensive panel data we are aware of are that of Fouquin and Hugot (2016), which begin in 1827. As we show below, the trend towards pacification in Europe started in the mid-17th century. Thus, to explore the effect of Atlantic trade, we need trade measures from at least the 17th century. We overcome this data constraint by using wheat prices to calculate the price pass-through between Europe and the New World. Our annual wheat prices are for 12 European countries as well as four locations in the New World between 1640 and 1896. The European countries in our sample are Belgium, Britain, Florence, France, the Habsburgs, Netherlands, the Ottomans, Poland, Portugal, Prussia, Spain, and Sweden. Data on wheat prices for these countries were mainly drawn from Allen (2001) and were supplemented by other sources.[11]

The decision on which countries to include in our European sample was driven entirely by the availability of wheat price data. Further, in any given year, each of these countries were included in our sample if they were independent, i.e. they were not occupied by another country. This ensures that for the years in which they are in our data, each country had control over its decision to engage in conflict.[12] Our New World wheat prices cover both North America (Massachusetts and Pennsylvania) as well as South America (Buenos Aires and Lima) and were drawn from the Global Price and Income History Group (GPIH) website.

Using wheat as our commodity of choice to estimate price pass-through is justified on the following grounds. First, the history of Atlantic wheat trade stretches back long before the so called first era of globalization (Sharp and Weisdorf, 2013, p. 89). Wheat production and trade were prominent in the New World at least since the 18th century and constituted key colonial exports to Europe (Shepherd and Walton, 1972). Indeed,

[11] Table A.1 in the Appendix lists all cities used to construct our wheat prices, the source of the data by city, and the years for which the data are available. We were unable to find wheat prices for most countries prior to 1640.

[12] The use of a 12-country European sample could be problematic if Atlantic trade caused greater conflict between our sample countries and other European countries not in our data. Fortunately, as we show below, this is not the case. This suggests that the exclusion of other European countries from our data is not causing our results to be biased towards finding a pacifying effect.

wheat and flour exports accounted for 16.1 percent of total exports from the New World to Europe between the 1760s and the 1890s.[13] Second, wheat has been the commodity of choice to measure historical price pass-through in close to a quarter of studies in the literature (Federico, 2012). This is mainly due to wheat being a relatively homogeneous product and wheat markets being highly developed in pre-modern times in both Europe and the New World (Jacks, 2005). The latter means that wheat prices are more widely available since the Early Modern period compared to other commodities.

Third, the grain market during this period had specific characteristics that differentiated it from that of other non-import competing colonial goods such as sugar, spices and tobacco. In fact, most European governments, at least since the 18th century, adopted a series of *ad hoc* market-friendly policies including tariff reductions and the suppression of medieval regulations, which facilitated trade both within Europe and across the Atlantic (Sharp and Weisdorf, 2013; Dobado-González et al., 2012).[14] Moreover, national and international grain trade was not monopolized by a small number of companies but was instead characterized by a higher degree of competition (Dobado-González et al., 2012). These features, together with readily available data and its relative homogeneity make wheat an ideal commodity to use to measure price pass-through as a proxy for trade.[15]

3.2 Conflict Data

Our conflict data are from Brecke's Conflict Catalog, a compilation of global violent conflicts that occurred between 1400 and the present.[16] We use these data to con-

[13]The export values are based on data from Sharp and Weisdorf (2013); Lydon (2008); Wattenberg (1976) and Shepherd and Walton (1972) for 1760s to 1790s; Pitkin (1816) for 1800; and Wattenberg (1976) for 1810s-1896. We are grateful to Paul Sharp and Jacob Weisdorf for sharing their wheat trade data. Shipments of wheat and flour from the Americas to Europe began before 1700, reaching significant levels from the 1730s (Lydon, 2008, p. 5). Further, on the eve of the American Revolutionary Wars, wheat, flour, and corn filled more than 90 percent of shipping bound for Southern Europe (Lydon, 2008, p. 127).

[14]For instance, Spain gradually reduced tariffs and approved domestic free trade of grain in 1765. Tariffs on cereals declined in the 1760s also in Britain, France and Russia (Persson, 1999).

[15]We show below that our results are robust to using sugar instead of wheat to construct price pass-through during 1640 to 1768. This addresses the concern that trade in wheat across the Atlantic was relatively limited until the mid-18th century.

[16]Brecke's Conflict Catalog provides a comprehensive list of historical conflicts and has been used previously by Iyigun (2008), Besley and Reynal-Querol (2014), and Michalopoulos and Papaioannou (2016), among others. We cross-checked all conflict entries in Brecke's Catalog with those reported in Wright (1942), which is another conflict dataset used in the literature, and found a considerable overlap. All of the 726 conflicts in the Wright (1942) data during our sample period are included in our data. Brecke's Catalog includes an additional 21 conflicts that are not in the Wright (1942) data.

struct an indicator variable for conflict onset between two countries as the main dependent variable.[17] Underpinned by considerable advances in military technology and financed by higher taxation, Europe strengthened its military power globally during our sample period (Scheidel, 2019). The number of people under arms rose, standing armies were created, and larger warships encouraged a "naval race" between powers (Parker, 1996; Black, 1994). Despite this militarization, which was accompanied by the increased monopoly of the use of force by states, Europe experienced an overall decline in the number of conflicts fought on its soil between 1640 and 1896.[18] This trend is shown in Figure 1, where we illustrate the number of conflicts per year involving at least one of our European countries during 1500 to 1896.[19]

Our period of analysis begins with an extensive weakening of tensions across most of Europe, brought about by the signing of the Peace of Westphalia (1648), which put an end to one of the bloodiest conflicts of European history, the Thirty Years' War (1618-1648) (Wilson, 2009). After a short peaceful interlude, the rest of the century witnessed a series of major conflicts.[20] This phase of intra-European rivalries saw the zenith of Dutch hegemony, challenged first by England and later also by France. During the 18th century, the conflict in Western Europe was mainly between France and England: 64 out of the 126 years between 1689 and 1815 involved conflict between these two countries. The two other important actors that generated new dynamics in the European balance of power during this period were the Kingdom of Prussia and the Habsburg monarchy. The former grew in prominence due to a rapid process of militarization (Black, 1994) while the latter's strength stemmed from its substantial territorial advances in central Europe (mostly Hungary) at the expense of the Ottomans.

Despite witnessing major conflicts, the 18th century was overall more peaceful than the previous, particularly so during its second half.[21] This relatively peaceful interval was

[17]The *Catalog* records all violent conflicts with at least 32 battle deaths. It is accessible at `http://www.cgeh.nl/data`. It provides information on the number of casualties, but such information is relatively sparse. As a result, the casualty data do not feature in our analysis.

[18]While part of the literature emphasizes the process of pacification in Europe after the Napoleonic Wars (Scheidel, 2019; Hoffman, 2017), it nonetheless identifies a declining trend in European conflict from the 17^{th} century onwards, see for instance Table 2.1, p.22 in Hoffman (2017).

[19]Not only did conflict become less prevalent during 1640-1896, they also became also less violent (Morris, 2012, pp. 28-29).

[20]These include the defeat of the Ottomans by the Habsburg (1683), three Anglo-Dutch wars (1652-54; 1665-67; 1672-78), the War of Spanish Succession (1701-14); and the Great Northern War (1700-21).

[21]The key violent confrontations were the Seven Years' War (1757-63), the War of the Austrian Succession (1740-48) and the War of the Polish Succession (1733-35).

interrupted by a new age of Europe-wide warfare: the Revolutionary and Napoleonic wars of 1792-1815. Napoleon's defeat, and the peace agreements signed at the Congress of Vienna, gave way to the so called *Pax Britannica*: a long period of relative peace lasting until the outbreak of World War I. During this time, Great Britain became the uncontested global hegemon.

In addition to the temporal variation, our conflict data exhibit widespread spatial variation. This is evident in Figure A.1, where we see that while all countries, except Florence, in our sample engaged in conflict with each other, Britain, France, the Habsburg monarchy and Spain were the most actively involved in warfare. These countries participated in more than 40 conflicts between 1640 and 1896.

3.3 Other Data

To construct our instrument, we use use wind-based shipping time data from Pascali (2017) and tropical cyclone data from Mann et al. (2009). The proxy-reconstructed data from Mann et al. (2009) are based on a statistical model that uses sea surface temperatures, the El Niño/Southern Oscillation, and the North Atlantic Oscillation to estimate historical tropical cyclones over the Atlantic. They show that these reconstructed data overlap with actual tropical cyclone data during the latter part of the 19th century, which is the earliest period for which such data are available.

A concern with these model-generated cyclone data is that it was smoothed at multi-decadal periods to capture longer-term trends. As a result, the data may follow a non-stationary process. We address this by converting the annual tropical cyclone data to tropical cyclone shocks as follows:

$$CA_t = \frac{C_t - \overline{C}_T}{\overline{SD}_T} \tag{1}$$

where CA_t is the tropical cyclone anomaly in year t, $\overline{C}_T$ represents the average annual tropical cyclones during the sample period, and $\overline{SD}_T$ is the standard deviation over the same period. Thus, the variation we are using here is deviations in tropical cyclone activity in a given year from the mean. A standard augmented Dickey Fuller test rejects the null hypothesis that CA_t has a unit root at the 1% significance level.

A second concern is that the generated tropical cyclone data has a wide confidence interval, which may result in the year-to-year variation in these data to be potentially uninformative. We address this by conducting the following robustness check: we treat the tropical cyclone data as a random variable whose mean follows a Gaussian process. We then simulate 100 alternate cyclone datasets for the period 1640 to 1850 to capture the measurement error in the raw data. Figure A.3 in the Appendix shows that our simulated data form a wide interval around the data provided by Mann et al. (2009). As we discuss in more detail in section 5.2, our baseline results remain highly robust to allowing for such measurement error in the generated tropical cyclone data.

We construct a country's population using data from McEvedy et al. (1978); Bolt et al. (2018) and Iyigun (2013). While our wheat price data are available from 1640 to 1896, the data on tropical cyclones are only available until 1850. As a result, our working sample spans the period 1640 to 1850. Summary statistics for all variables used in our baseline specification are listed in Table 1.

4 Econometric Method

4.1 Estimating Price Pass-Through

In this section, we describe the method we use to estimate the degree of price transmission between two European countries as well as between a European country and the New World. The standard approach in the literature is to regress prices in one region on prices in another to obtain a time-invariant measure of price pass-through.[22] However, even for short durations, one wouldn't expect the degree of pass-through to be constant over time. Given that we use over 200 years of data, it is particularly inappropriate for us to assume that price pass-through is time invariant. Thus, to estimate time-varying measures of price pass-through, we consider the following specification of price pass-through between two countries i and j:

$$\Delta \ln P_{it} = \beta_{ij,t} \Delta \ln P_{jt} + \Delta e_{it} \tag{2}$$

[22] This approach is widely used in contexts where trade data are typically unavailable such as in historical studies as well as in the analysis of within-country trade in rural markets. See, for example, Andersson and Ljungberg (2015); Brunt and Cannon (2014); Uebele (2011); Shiue and Keller (2007); Fackler and Goodwin (2001).

where P_{it} is the price of wheat in country i and year t, P_{jt} is the price of wheat in country j in the same year, and $e^{it} \sim N(0, \sigma_e^2)$ is an error term. $\beta_{ij,t}$ is the pass-through of wheat prices from j to i and captures the extent to which shocks to prices in one region are transmitted to another. Thus, higher values imply greater price pass-through between two countries. In order to obtain time-varying measures of β_{ij}, we follow the extensive literature on estimating time-varying coefficients and set up a two-part system of equations.[23] The first part, equation (2), allows us to link observations (i.e. prices) to the coefficients to be estimated ($\beta_{ij,t}$). While the β's are not observed, in the second part we assume that they are governed by a well-defined process. More precisely, we assume that the $\beta's$ follow a random walk. That is,

$$\beta_{ij,t} = \beta_{ij,t-1} + u_{ij,t} \tag{3}$$

where $u_{ij,t} \sim N(0, \sigma_u^2)$. For ease of exposition, we will omit the ij superscript from hereon. However, note that all of our estimates of β are specific to a particular ij pair.

The procedure we use to estimate equation (2) is an extension of the algorithm in Chan and Jeliazkov (2009) and gives us a less computationally intensive way of estimating the time-varying coefficients. To see how it works, let $\bar{\beta}$ represent the vector of price pass-through coefficients to be estimated. We begin by imposing an uninformative prior, $p(\bar{\beta})$, on the distribution of these coefficients. This prior uses equation (3) to propose an initial relationship between the β's over time. Our estimation procedure then uses Bayes theorem as well as information from the data to update $\bar{\beta}$. As with all time-varying parameter models, our procedure places greater weight on nearby price observations. This means that the β's in any given year will be driven by the data points in its vicinity. This reliance on nearby observations ensures that that the β's in one year will be different from the β's in another year. It also means that each estimate of β is not based on a single observation.[24]

We illustrate the trend in price pass-through between Europe and the New World

[23] These two-part, state-space models have been widely used in other contexts in the past and date back to at least Harrison and Stevens (1971) and Cooley and Prescott (1976). To our knowledge, our paper is the first to use these methods to estimate time-varying measures of price pass-through. We provide further details on this method in the Appendix. An alternate approach to estimating price pass-through is to estimate co-integration coefficients. However, such a method will not produce time-varying coefficients and is therefore unsuitable for our purposes.

[24] As a robustness check, we also show that our main result is robust to using price gaps instead of the β's to proxy Atlantic trade.

in Figure 2.[25] As is clear from this figure, there was a marked increase in trade with the New World at the same time as there was a downward trend in intra-European conflict. Next, in Figure 3, we illustrate heterogeneity in price pass-through across the countries in our sample. While some countries clearly became more integrated (such as Britain, France, the Netherlands, Prussia, and Sweden), others became less integrated with the New World over time (for instance, the Habsburgs, the Ottomans, Portugal, and Spain). The sharp drop in price pass-through for Portugal and Spain reflects the end of colonization, as Latin America's independence marked the end of preferential trade agreements with the former colonists and the imposition of high tariffs against them (Findlay and O'Rourke, 2009; Coatsworth and Williamson, 2004).[26]

4.1.1 Validating the Pass-Through Estimates

While our estimation procedure provides us with time-varying measures of price pass-through, it is worth asking whether they are a reasonable proxy for trade. There are two limitations that are potentially relevant for us. First, if two countries suffer a common weather shock, their prices may co-move even if their level of trade is low. This is not a concern for our Atlantic trade measure as the geographic distance between Europe and the New World means that common weather shocks affecting both regions are unlikely. Second, the price pass-through between two countries could be high due to factors that are not related to trade. If so, price pass through need not be accurately capture actual trade.

We use three approaches to address this second concern. First, we compare our price pass-through measure with actual trade data in years in which both are available. We us two sources of historical trade data for this validity check: (a) data from Pascali (2017) that span the period 1845 to 1896 and (b) data from Fouquin and Hugot (2016) that span the period 1827 to 1896. Note that both datasets capture total trade between countries and not just wheat trade. We use these data to create a 13-country bilateral panel with the 12 European countries in our sample as well as the New World.[27] We then construct two

[25]To account for composition changes in our sample, we first regress our bilateral price pass-through measure on country fixed effects and year fixed effects. We then collect the residuals and plot its annual average in Figure 2.

[26]Both Prussia and Sweden have very few consecutive years of price data in the 19th century. This is why we were not able to estimate their New World price pass-through during this period.

[27]To be consistent with our price pass-through measure, we define the New World as consisting of Argentina, Peru, and the United States.

measures of trade: (a) the natural logarithm of imports from j to i and (b) imports from j to i divided by i's GDP.

In Table 2, we report the results from regressing these two trade measures on our price pass-through measure. In columns (1) and (3) we use the Pascali (2017) data while in columns (2) and (4) we use the Fouquin and Hugot (2016) data. In all four cases, the coefficient of our price pass-through measure is positive and statistically significant and confirm that our price pass-through measure is positively correlated with actual trade when both data are available. This strongly supports our choice of using price pass-through as a proxy for trade.

Second, we show that the overall trend in our price pass-through measure is consistent with the trade volume data constructed by Acemoglu et al. (2005). They construct an estimate of Atlantic trade using the number of annual average voyages-equivalent between a European country and the New World. A voyage equivalent is defined as a round-trip of a ship with a deadweight tonnage of 400 tons. Their data are available by century until 1700 and then for every 50 years until the end of the 19th century. The solid line in Figure 4 illustrates the increase in Atlantic voyages between 1600 and 1850, as captured by the Acemoglu et al. (2005) data.

To show that our price pass-through measure is capturing a similar trend in Atlantic trade, we aggregate our data by 50-year periods (1640–7000, 1700–1750,...,1800–1850) and then add it to Figure 4. As is clear from this figure, our data produce a trend that is similar to that of Acemoglu et al. (2005).[28]

Third, as discussed in further detail in section 5.1, we use the data from Fouquin and Hugot (2016) to replace the price pass-through proxy with data on imports from the New World to Europe. The results using the import data, which only span the period between 1827 and 1850, are consistent with our baseline findings and further validate our price pass-through based proxy for Atlantic trade.

[28]Given that the aggregate nature of the Acemoglu et al. (2005) data, it lacks the temporal granularity needed to be suitable for our analysis.

4.2 Econometric Specification

To estimate the effect of trade with the New World on conflict onset in Europe, we use the following econometric specification:

$$O_{ijt} = \alpha + \gamma_0 C_{ij,t-2} + \gamma_1 \beta_{ij,t-1} + \gamma_2 \beta^{NW}_{ij,t-1} + \gamma_3 X_{ijt} + \theta_{ij} + \theta_t + \varepsilon_{ijt} \tag{4}$$

where O_{ijt} is a conflict onset indicator that takes the value of one if countries i and j begin a conflict between each other. That is, $O_{ijt} = 1$ if $C_{ijt} = 1$ and $C_{ij,t-1} = 0$, where C_{ijt} is an indicator for whether two countries are in conflict in year t. We follow Esteban et al. (2012) and include a lagged-dependent variable, $C_{ij,t-2}$, to account for any persistence in conflict status.[29] $\beta_{ij,t-1}$ is the bilateral price pass-through between countries i and j, lagged by one year to avoid simultaneity. It proxies the extent of trade between these countries.

Our key variable of interest, $\beta^{NW}_{ij,t-1}$, is the extent to which countries i and j trade with the New World. To capture this combined price pass-through, we follow Martin et al. (2008) and define

$$\beta^{NW}_{ij,t-1} = 0.5 \times \left(\beta^{NW}_{i,t-1} + \beta^{NW}_{j,t-1}\right) \tag{5}$$

where β^{NW}_{i} and β^{NW}_{j} are i and j's price pass-through with both North and South America respectively.[30, 31] If greater trade with the New World did lower conflict in Europe, we expect γ_2 to be negative. We use a one-year lag of this variable to avoid simultaneity.

We also include in (4) a set of exogenous control variables, X_{ijt}, that are likely to affect the probability of conflict between i and j. These include an indicator for whether the two countries share a border and the sum of i and j's total population, which controls for

[29] Since our dependent variable is a function of both C_{ijt} and $C_{ij,t-1}$, we include a two-year lag of conflict status, $C_{ij,t-2}$, on the right-hand side.

[30] Martin et al. (2008) are interested in the effect of i and j's joint trade with the rest of the world during 1950 to 2000 on the likelihood that they will be in conflict. They define i and j's joint trade with the rest of the world as the sum of each country's trade with the rest of the world. Indeed, our econometric specification is based on the Martin et al. (2008) benchmark approach.

[31] Given that all countries in our sample imported wheat from a set of different countries (and not exclusively from the New World) and had a sizeable domestic production, β^{NW}_{ij} is not mechanically related to β_{ij}. For example, in the 18th century, Spain received significant quantities of wheat and flour from Northern Europe, Italy, the Western and Eastern Mediterranean, the Black Sea, North America, and Atlantic Africa-Barbary. Russia emerged as a key grain exporter to most European states and the Ukraine was exporting wheat to Turkey, France, Spain, and Portugal (Dobado-González et al., 2012). See also Braudel (1981) for a detailed account of intra-European wheat trade in the 15th-18th centuries.

territorial gains and losses. We use a five-year lag of population to avoid simultaneity. We include country-pair fixed effects, θ_{ij}, to capture time-invariant, country-pair-specific characteristics such as bilateral distance and whether two countries share a common language. We also include year fixed effects, θ_t, to control for Europe-wide shocks.[32] We also show below that our results are robust to interacting the country fixed effects with a year trend. Lastly, ε_{ijt} is an error term.

Before describing our results, it is worth noting two key issues with our econometric strategy. First, it might be argued that the decision to engage in conflict is a longer-term one and unlikely to be driven by short-run trade shocks. In our case this concern is mitigated by the fact that in (4), we are modeling conflict onset and not conflict occurrence. Hence, our aim is to quantify how year-to-year shocks in Atlantic trade affect a country's choice of when to start a violent confrontation. This is inherently a more short-run decision and can be driven by trade shocks.

Second, our identification of γ_2 in equation (4) is threatened by the endogeneity of β^{NW}. For instance, North (1968) points out that ocean shipping costs during our sample period were subject to both technological shocks as well as changes due to other factors. Thus, it is plausible that intra-European conflict could result in changes to shipping costs and thereby affect Atlantic trade.

We address this concern by using an instrumental variable (IV) strategy that exploits exogenous variation in trade costs. Recall that our endogenous variable, β^{NW}, is a proxy for the extent of trade between the New World and Europe. Thus, an appropriate instrument in our context must affect trade between the New World and Europe and not affect intra-European conflict through any other channels. Weather-based factors that provide exogenous variation in trade costs between Europe and the New World satisfy these requirements. We use two sources of weather-based shocks to trade costs: (a) annual tropical cyclone activity over the Atlantic Ocean and (b) shipping times between Europe and the New World due to exogenous wind patterns.

As Rappaport and Fernández-Partagás (1997) point out, limited meteorology, communication, and navigation technology along with poor seaworthiness made sailing ships during our sample period particularly vulnerable to weather-induced shipwrecks. To the

[32] Harrison and Wolf (2012) point out that the number of conflicts will be a function of the number of countries in the world. Thus, changes in the total number of countries can result in increased/decreased pacification. However, this is not a problem for our econometric analysis since the number of countries is common across all country pairs in our sample and will therefore be captured by the year fixed effects.

extent that tropical cyclones are a leading cause of such shipwrecks (Trouet et al., 2016), they provide us with an exogenous shock to the cost of transporting goods across the Atlantic. All else equal, this will reduce trade between Europe and the New World.

Indeed, historians estimate that poor weather is responsible for five percent of the vessels in the West Indies being lost due to shipwrecks; led to a 1,000 Spanish sail ships being destroyed; and to no less than a 1,000 deaths at sea per year.[33] Further, as we illustrate in Figure A.2 in the Appendix, the overall trend in Atlantic cyclone activity is positively correlated with Atlantic freight rates during the 18th and 19th centuries.[34] Finally, since these tropical cyclones occur over the Atlantic Ocean, they should not affect intra-European conflict through any channel other than Atlantic trade. We discuss potential threats to this exclusion restriction in section 5.1.

Our instrument is vulnerable to the concern that sailors could anticipate tropical cyclones and adjust their behavior accordingly. For instance, suppose Atlantic tropical cyclones always occurred during a short period of time that could be readily anticipated. If so, sailing ships could minimize their exposure to shipwrecks by avoiding Atlantic journeys during these cyclone-intensive periods. This concern is mitigated by the tendency of Atlantic tropical cyclone activity to span up to half of the year. For instance, using data from 1500 to 1899, García-Herrera et al. (2005) show that Atlantic tropical cyclone activity began in July, peaked in September, and occurred as late as December. They also show that storms occurred throughout the year over the Atlantic. Thus, while tropical cyclones peaked during the late North American summer, the fact that they occurred over a six month period made it difficult for sailing ships to avoid being damaged by tropical cyclones altogether.[35]

While Atlantic tropical cyclones provide exogenous shocks to shipping costs over the

[33]See Rappaport and Fernández-Partagás (1997) for the sources of these estimates. Further, Brzezinski et al. (2019) construct a dataset of 32 maritime disasters that affected ships destined for Spain during 1502 to 1804. Of these 32 disasters, 21 were caused by poor weather.

[34]The freight rate data are from Harley (1988) for 1741 to 1829 and North (1958) for 1830 to 1850. The Harley (1988) data are from his British series (reported in Table 9, pp. 873) and are based on shipments of coal, timber, and cotton along the UK-North America route. The North (1958) data are based on the American export freight rate index (reported in Appendix Table 2, pp. 549) and represent the freight rate of bulk commodities exported from North America to Europe.

[35]As we discuss below, we further attenuate any anticipation effects by converting our tropical cyclone data to deviations from the long-run mean (tropical cyclone shocks). Thus, our identification is coming not from the number of tropical cyclones in a year, but the deviation in these tropical cyclones from the long-term average. Given the relatively limited meteorology technology at the time, such shocks are unlikely to be anticipated.

Atlantic, they are only available by year and not by European country. To induce cross-sectional variation in our instrument, we use shipping times between Europe and the New World from Pascali (2017) that are based on exogenous changes in wind patterns.[36] Pascali (2017) points out that historical shipping times, particularly before the widespread use of steam vessels in the 1870s, were dependent on prevailing wind directions and patterns. Sailing vessels at the time could not navigate against the wind and could only reach its maximum speed when sailing downwind. Thus, unfavorable wind directions and speed could lower the sailing time between the New World and Europe and thereby affect trade. Further, because these shipping times are based on exogenous wind patterns, they are unlikely to affect intra-European conflict through any channel other than New World trade. Note that these shipping times are generated using only information on wind patterns. Importantly, they are not based on any observable data. Thus, they will not be affected by navigation expertise and any such endogenous factors.

5 Results

We present the results from estimating equation (4) in Table 3. The standards errors we report for all estimates are cluster bootstrapped at the country-pair level with 1,000 repetitions to account for the fact that both proxies for bilateral trade and Atlantic trade as well as our instrument are generated regressors.

To demonstrate how our results change with various specification choices, we start with the most parsimonious specification in column (1) and progressively add other control variables. In column (1), we focus on the relationship between bilateral conflict and bilateral trade. That is, we estimate a version of equation (4) without the Atlantic trade variable, β^{NW}. The coefficient of bilateral trade is positive and statistically insignificant. In fact, this result holds in all specifications in Table 3. Thus, our results are inconsistent with the liberal peace hypothesis that suggests that greater bilateral trade promotes greater peace between countries.[37]

In column (2) of Table 3, we include Atlantic trade and estimate (4). Recall from (5) that for any two European countries, this measure is the average of each country's price

[36]Feyrer and Sacerdote (2009) use a related strategy where they use wind patterns to instrument colonial links.

[37]In fact, whether the liberal peace hypothesis is supported by the data is intensely contested in the political science literature. See Barbieri (2002) for a thorough review of this literature.

pass-through with both North America and South America. Thus, a higher value for this measure implies that a pair of European countries traded more with the New World. The coefficient of this variable is negative and statistically significant and suggests that a pair of European countries that traded more with the New World were less likely to be in conflict with each other.

To gauge the magnitude of this effect, consider the year 1655, which is the year with the lowest average pass-through during the 17th century and the year 1830, which is the year with the highest average pass-through during the 19th century. Our results in column (2) suggest that the increase in Atlantic trade between 1655 and 1830 lowered the probability of conflict between two European countries by 0.44 percentage points. This is a 19.22 percent decline from the baseline probability of conflict onset of 2.30 percent.

In column (3), we test the robustness of our findings by including country-specific linear time trends to our baseline specification. These will control for unobservable time-varying, country-specific trends that might confound our results. As the results in column (3) indicate, we find that our main results are robust to the inclusion of these trends.

Finally, to examine whether the relationship between trade and conflict is stable over time, we divide our sample in five 50-year bins. As illustrated in Figure 5, our results are not driven by a specific time period. Importantly, these findings indicate that the relationship between trade and war we find was not driven by the onset of *Pax Britannica* (1815), a period which saw the emergence of Great Britain as a global hegemonic power (O'Brien, 1989). Lastly, the stability of our results in Figure 5 prior to the 19th century also confirms that our findings are not being confounded by the American Civil War.

5.1 Endogeneity of Atlantic Trade

As discussed above, a threat to our identification strategy is the endogeneity of Atlantic trade. We address this by using an IV strategy that exploits exogenous weather shocks to instrument Atlantic trade. As described in more detail in section 4.2 above, we use two sources of weather-based shocks: (a) annual tropical cyclone activity over the Atlantic Ocean and (b) shipping time between Europe and the New World due to exogenous wind patterns. To implement our IV strategy, we include as the instrument the following interaction:

$$CA_t \times \ln(S_{ij}),$$

where CA_t is deviations in tropical cyclone activity in a given year from its mean and is as defined in equation (1). Deviations of cyclone activity is used since they are unlikely to be anticipated by sailors. $\ln(S_{ij})$ is the natural logarithm of wind-based sailing time between the New World and European country pair ij. We provide further details on the construction of S_{ij} in the Appendix.[38]

Thus, our instrument isolates the part of each European country pair's trade with the New World that is explained purely by the interaction of exogenous changes in wind-based sailing time and annual tropical cyclone shocks. The time variation in our instruments come from Atlantic cyclone activity, CA_t, and captures the fact that greater cyclone activity results in higher trans-Atlantic trade costs. The pair-specific variation in the instruments come from the average shipping time to the New World, S_{ij}. This captures the fact that adverse weather over the Atlantic will disproportionately impact European country pairs that have longer sailing times to the New World.

We report the results of our IV estimation in Table 4.[39] In column (1) of Panel B, we show the first-stage coefficients of sailing time interacted with tropical cyclone shocks for our baseline specification (equation (4)). The coefficient of this interaction term is negative and statistically significant and confirms that the trade-reducing effect of tropical cyclones is more pronounced for European country pairs that are farther away from the New World. As is clear from the reported Kleibergen-Papp F statistic, our instrument is strong as the F statistic is comfortably above the conventional cutoff of 10.

Next, in column (1) of Panel A, we report the second-stage results. Our coefficient of interest, Atlantic trade, is negative and statistically significant and confirms that greater Atlantic trade reduces the probability that two European countries are in conflict with one another. It is worth noting that the results in Table 4 indicate that our OLS results were biased towards zero. There are two explanations for this. First, the sample period we study is one where trade and conflict were driven primarily by mercantilist concerns. Thus, not only did trade impact conflict, but conflict and violence were used to enhance trade

[38]Note that the level effect of $\ln(S_{ij})$ is captured by the country-pair fixed effects while the level effect of CA_t is captured by the year fixed effects. As a result, these two level effects are not included in our instrument set. We find that the correlation between the average annual β^{NW} and tropical cyclone shocks is -0.683, which confirms that these cyclones, on their own, have a negative effect on trade.

[39]Since our sample ends at 1850, it overlaps entirely with the "Age of Sail", which is commonly considered to be the period prior to 1871. Pascali (2017), among others, points out that after 1871, steam ships started to become the dominant mode of shipping. As steam ships are not dependent on wind patterns, the period after 1871 is one where weather shocks and wind patterns were unlikely to drive shipping costs and travel time.

by exerting commercial dominance over particular trade routes (Findlay and O'Rourke, 2009). This positive feedback effect provides one explanation for why our OLS estimates are biased towards zero.

Second, our IV estimate is a local average treatment effect (LATE) in the sense that our instrument only provides a treatment for countries that trade directly with the New World. We know that these countries will have a higher opportunity cost of conflict (in terms of forgone trade with the New World). Given this, we should expect the IV estimate to be larger in magnitude than the OLS.

In column (2), we test the robustness of our results by including country fixed effects interacted with a year trend to our baseline specification. The results in both Panels A and B continue to be robust.

In column (3), we replace our proxy for Atlantic trade that is measured using wheat price pass-through with imports from the New World to each European country in our sample. The import data are from Fouquin and Hugot (2016) and span the period 1827–1850. This is why the sample size in column (3) is smaller than the baseline. The results from using imports from the New World are consistent with our baseline findings. In Panel B, we continue to find that weather shocks adversely affect imports. Further, in column (3) of Panel A, we find that greater imports from the New World result in a lower probability of conflict onset among European countries. This result not only confirms the pacifying role of Atlantic trade, but also provides additional validation for our price pass-through proxy.

5.2 Instrument Validity

In this section, we discuss the key threats to the validity of our instruments. First, our IV strategy uses the differential effect of tropical cyclone shocks based on exogenous sailing times to identify the effect of Atlantic trade on intra-European conflict onset. It can be argued that short-term tropical cyclone shocks may be too transitory to affect conflict onset. To address this, we replace our tropical cyclone shock measure with a five-year average tropical cyclone shock over the period $t = 0, -1, ..., -4$. This average captures longer-term exposure of European countries to tropical cyclone shocks over the Atlantic. The results in column (1) of Table 5 demonstrate that our coefficient of interest remains highly robust to using longer-term tropical cyclone shocks.[40]

Second, while we instrument Atlantic trade, we do not to have an IV for bilateral trade. This is driven entirely by the lack of an appropriate instrument. Instead, we replace our default one-year lagged bilateral trade with a ten-year lag instead. Such a long lag should be sufficiently far removed from conflict onset in year t and attenuate any endogeneity concerns about bilateral trade. We report the results using this longer lag of bilateral trade in column (2) of Table 5. As these estimates confirm, our coefficient of interest remains robust.

Third, we turn to potential violations of the exclusion restriction. Our instrument will satisfy the exclusion restriction as long as Atlantic tropical cyclone shocks and wind-based shipping times only affect intra-European conflict via trade with the New World. We consider two scenarios in which this exclusion restriction might be violated. First, spatial correlation in weather may result in a correlation between tropical cyclone activity over the Atlantic and rainfall in Europe. Such rainfall shocks would then impact European income and conflict through channels other than Atlantic trade.

To tackle this concern, we use data from the National Center for Environmental Information to calculate a time-varying measure of rainfall shocks in each European country in our sample. We follow Barrios et al. (2010) and define a rainfall shock similarly to tropical cyclone anomalies as the difference between a country's rainfall in year t and its average annual rainfall over the entire sample period divided by the standard deviation of its annual rainfall over the same period. We then add both country i's and country j's annual rainfall shocks to our baseline specification. We report the results from the augmented specification in column (3) of Table 5. As these results demonstrate, our main findings remain robust.

Fourth, many of the conflicts in our sample were naval conflicts. Adverse Atlantic tropical cyclone shocks could, in principle, affect such conflicts directly and not via Atlantic trade. Rappaport and Fernández-Partagás (1997) discuss various instances during the 17th century where tropical cyclones destroyed or dispersed a European country's naval fleet and provided its adversary with a decisive advantage. To account for this exclusion restriction violation, we omit all naval conflicts from our sample in column (4) of Table 5.[41] As the results in this column demonstrate, our coefficient of interest remains

[40] In section 6.2, we show that our IV results are also robust to collapsing the data to five-year periods.

[41] We use Sondhaus (2001), Harding (2002), and Chickering et al. (2012) to identify naval conflicts in our sample. We use a conservative approach and classify any conflict that involved a navy, even if the

robust to restricting the sample to non-naval conflicts.

The robustness of our results to excluding naval wars also addresses the potentially confounding effect of piracy. We know that during our sample period, European countries encouraged - either formally or informally - the use of piracy against their rivals. Thus, one may be concerned that piracy was used as a substitute for formal warfare. However, while piracy in the Atlantic may be a substitute for formal naval warfare, it is unlikely to be a substitute for wars on European soil. Thus, the fact that our results are robust to excluding all naval wars suggests that endogenous changes in piracy are unlikely to be driving our results.

Lastly, we return to the concern that measurement error in the model-generated tropical cyclone data makes its year-to-year variation potentially uninformative. As discussed in more detail in section A.3 in the Appendix, we address this by assuming that the tropical cyclone data, $C_t = (C_1, C_2, ..., C_T)'$, for all years $t = \{1, 2, ...T\}$ are a random variable whose mean follows a Gaussian process. We then simulate 100 different tropical cyclone series, where we err on the side of caution and allow for a large degree of measurement error in the tropical cyclone data. *A priori*, we assume that the variance of the error is half the variance of the data, which ensures that the simulated data forms a wide interval around the raw data (Figure A.3). For each of these simulated series, we use it in place of the baseline tropical cylcone data and re-produce our IV estimates. On average, we find a coefficient of $\hat{\gamma}_2{}^{IV}$=-0.919, which is very similar to the baseline IV estimate of -0.967 in Table 4.

5.3 Mechanisms

Our main finding thus far is that greater trade with the New World resulted in a decline in intra-European conflict. What mechanisms can explain this result? In section 2, we discussed two ways in which Atlantic trade could result in lower intra-European conflict. First, to the extent that conflict increases trade costs, engaging in conflict will lower the value of trade with the New World. This forgone trade is an opportunity cost of conflict and will result in a lower probability of intra-European conflict. Second, if the New World is relatively land abundant, Atlantic trade will result in a decrease in the relative price of commodities and an increase real wages in Europe. In turn, this will

involvement was minor, as a naval conflict. Approximately 28 percent of the conflicts in our sample are classified as naval conflicts using this approach.

increase the cost of raising an army. For a given country, this military cost channel will also reduce the incentive to engage in conflict.

We begin by examining whether the data support the military cost channel. Note that this channel can be broken down in to two components. These are: (a) greater New World trade will increase wages in Europe and (b) greater New World trade will lower military sizes in Europe. To test these two components, we estimate the following specification:

$$M_{it} = \alpha_m + \rho_1 \beta_{i,t-1}^{NW} + \theta_i + t + \nu_{it} \tag{6}$$

where M_{it} is either the average real wage in country i and year t, the number of soldiers per capita in i, or the natural logarithm of i's total naval ships. $\beta_{i,t-1}^{NW}$ is country i's price pass-through with the New World. We also include in (6) country fixed effects, θ_i, and a year trend, t.[42] We estimate (6) using an IV approach where the instruments for β_i^{NW} are tropical cyclone shocks, CA_t, as well as its interaction with the natural logarithm of country i's wind-based sailing time to the New World, $\ln(S_i)$.

We report the results from estimating this specification in Table 6. In column (1), we construct a country's average wage from Allen (2001) and Pamuk (2000) as the dependent variable. The coefficient of interest is positive and confirms that countries with greater Atlantic trade did experience an average increase in wages.[43] In column (2), we use as the dependent variable a country's total number of soldiers per capita, while in column (3) we use the natural logarithm of a country's total number of naval ships as the dependent variable. The army size data are from Onorato et al. (2014) while the data on the number of ships are from Glete (2000). In both columns, the coefficient of interest is negative, and in column (2) it is also statistically significant. These results suggest that greater Atlantic trade lowered a country's army size and potentially its navy as well.[44]

As discussed above, both the military cost and opportunity cost channels explain the pacifying effect of Atlantic trade. To separate these two channels, we estimate a version of

[42]Given the limited number of cross-sectional units at the i and t level, our instrument becomes weak when we include both country and year fixed effects in (6). As a result, we use a year trend instead of year fixed effects.

[43]In columns (1) to (3) of Table 6, we report p values in brackets that are cluster bootstrapped at the country level. These p values are calculated using the Cameron et al. (2008) wild bootstrap procedure with 1,000 repetitions.

[44]Note that our results are not inconsistent with the widely documented militarization that Europe underwent during our sample period (Parker, 1996; Black, 1994). Our findings suggest that the rate of militarization in Europe would have been greater in a counter-factual scenario without Atlantic trade.

(4) where we interact β_{ij}^{NW} with an indicator that is one if at least one country in the pair had extensive trade links with the New World (Atlantic colonizer). We classify Britain, France, Portugal and Spain as Atlantic colonizers. To see why this interaction term is useful, consider a case where two countries jointly engage in greater Atlantic trade. According to the military cost channel, wages and hence the cost of militarization will go up in both countries. As a result, they will both reduce the size of their military and thereby neutralize the pacifying effect of Atlantic trade. In this case, the coefficient of the interaction between β_{ij}^{NW} and the Atlantic colonizer indicator will be positive.

In contrast, according to the opportunity cost channel, country pairs with greater Atlantic trade will have more to lose from a conflict between then. This will increase the incentives for pacification and result in the interaction term of interest having a negative coefficient. We report the estimated interaction coefficient in column (4) of Table 6. While imprecisely estimated, the coefficient of interest supports the opportunity cost channel.

5.4 Alternate Mechanisms

We next examine other plausible explanations for our results. It could be the case that our results are being driven by unobserved, country-specific shocks. For instance, if a European country experiences a positive agricultural productivity shock, it may provide it with the additional resources necessary to engage in conflict with other European countries. Further, by changing its demand for imports, a positive agricultural productivity shock could also affect the country's trade with the New World. Due to a lack of appropriate data during our sample period, we use two proxies for agricultural productivity. In column (1) of Table 7, we use data from Nunn and Qian (2011) to capture the total land suitable for growing Old World staples in each country in our sample. We then interact this with a year trend and add it to our baseline specification. We add separate interactions for countries i and j respectively. Next in column (2), we use data from Alfani and Ó Gráda (2017) to define a famine shock variable that is one if either country i or j experienced a famine in year t and add it to our baseline specification. In both columns, our coefficient of interest remains negative and statistically significant with a coefficient that is similar to our baseline result in Table 4.[45]

Next, in column (3), we add an indicator that is one when a colonizer in our sam-

[45]Note that the results in column (3) of Table 5 where we include rainfall shocks in Europe is an alternate way of accounting for agricultural productivity shocks that uses time-varying data.

ple ended its colonization of the New World. This captures shocks that result from the cessation of colonial links that could be correlated with both conflict and NW trade. However, as the results show, our coefficient of interest remains robust to the inclusion of this additional variable.

An alternate explanation for our main result is that during the period that we study, European countries increased their state capacity. This allowed them to better engage in trade with the New World while also affecting their ability to engage in conflict. To account for this, we obtain data on tax revenues per capita in 1650 from Pamuk and Karaman (2010). We use these data to categorize Britain, France, Netherlands, and Spain as having high state capacity and all other countries as having low state capacity. We then add an interaction between country *i*'s state capacity and a year trend and do the same or country *j* as well. As the results in column (4) demonstrate, our coefficient of interest remains robust.

Our results could also be driven by changes in institutions or the system of governance. To do so, we use data from Van Zanden et al. (2012) to categorize Britain, Netherlands, Prussia, Spain, and Sweden, as having high parliamentary activity and all other countries as having low parliamentary activity. We then add separate interactions between country *i* and *j*'s parliamentary activity and a year trend. As illustrated in column (5), our results are unaffected by the inclusion of these additional terms.

Lastly, our results could be driven by a downward trend in conflict after the Peace of Westphalia in 1648. To account for this, we restrict our sample to 1670 and onwards and re-run our baseline specification.[46] The results in column (6) confirm that our coefficient of interest remains negative and statistically significant.[47]

[46]Our choice of 1670 is driven by Figure 1, where we observe that conflicts in Europe were declining from the 1640s onwards and stabilized around the 1670s.

[47]The historical period we study precedes the "Age of Mass Migration" (1850-1914). Given that migration flows from Europe prior to that date were very small (the only major intercontinental migration had been that of black slaves from Africa to the Americas (Hatton et al., 1998)), we do not consider migration as a potential alternative mechanism explaining our results.

6 Additional Results

6.1 Displacement to Other Conflicts

Our results thus far suggest that greater Atlantic trade led to a reduction in intra-European conflict. In principle, this pacifying effect could coincide with European countries engaging in greater conflict elsewhere in the world or greater conflict with other European countries not in our sample. Indeed, the 1640 to 1850 period was also characterized by a prolonged struggle between European powers to control the resources of the rest of the world in order to gain exclusive access to new captive markets. In Africa and Asia this led to establishment of colonies and the expansion of rival empires. To examine these potential displacement effects, we first collapse our dataset to the country-year level. We then examine whether greater Atlantic trade increased a country's likelihood of other types of conflict using the following specification:

$$\tilde{O}_{it} = \alpha_c + \delta_1 \tilde{C}_{i,t-2} + \delta_2 \beta^{NW}_{i,t-1} + \theta_i + t + \mu_{it} \tag{7}$$

where $\tilde{O}_{it}$ captures the onset of various types of conflict. These are described in more detail below. We also include in (7) an indicator for conflict in year $t-2$, $\tilde{C}_{i,t-2}$, as well as country fixed effects, θ_i, and a year trend, t. We instrument β^{NW} using annual tropical cyclone shocks as well as the interaction of country i's sailing time to the New World with annual tropical cyclone shocks. Lastly, when estimating (7), we cluster the standard errors at the country level. To account for the small number of clusters, we use the Cameron et al. (2008) wild-cluster bootstrap procedure with 1,000 repetitions.

We begin our analysis by examining whether Atlantic trade led to the European countries in our sample engaging in greater conflict in the rest of the world. In column (1) of Table 8, we use as the dependent variable an indicator for whether the countries in our sample engaged in a conflict in the New World. In column (2), we use as the dependent variable an indicator for conflict outside of Europe and the New World. In both cases, the coefficient of Atlantic trade is statistically insignificant.

Next, we examine whether Atlantic trade resulted in more conflicts between our sample countries and other European countries and more civil conflicts in Europe. To the extent that this is the case, then overall conflict in Europe need not have gone down as a result of Atlantic trade. To examine this, we use as the dependent variable an indicator for conflict between our sample countries and other European countries and an indicator for civil conflict in columns (3) and (4) respectively. In both cases, the coefficient of Atlantic trade is statistically insignificant. Together with our baseline results, they suggest that Atlantic trade resulted in an overall decline in conflict in Europe.

As discussed in section 3.2, the period we study was one of rapid advancement in military technology, which resulted in wars becoming more expensive and violent. Thus, it could be the case that countries that engaged in Atlantic trade were less likely to fight wars, but any conflict was of a much larger scale. To examine whether there was a substitution towards larger-scale conflicts due to Atlantic trade, we use as a dependent variable an indicator for high-fatality conflict in column (5). We define a high-fatality conflict as one where the total fatalities was above the sample median.[48] The coefficient of Atlantic trade here is statistically insignificant.

6.2 Robustness Checks

We now subject our key result of the pacifying effects of Atlantic trade to a series of robustness checks. We begin by using an alternate proxy for trade, price gaps, instead of our default measure based on price transmission. In column (1) of Table 9, we use the absolute value of the difference in wheat prices between two countries as our measure of Atlantic trade. Our coefficient of interest remains robust. Note that a lower price gap implies greater trade between markets, which is why a positive coefficient in column (1) of Table 9 has the same interpretation as a negative coefficient in our baseline IV results in Table 4.

In column (2), we address the concern that limited wheat trade between South America and Europe during the earlier part of our sample makes wheat-based price pass-through a sub-optimal choice. To address this, we use Brazilian sugar prices to estimate price pass-through between South America and Europe between 1640 and 1768 and continue to use wheat prices to measure price pass-through between North America and Europe.[49] The results in column (2) show that our coefficient of interest is robust to using

[48]Note that our conflict data do not report fatalities for all conflicts. This is why the sample size in column (5) is smaller than the other columns in Table 8. As a robustness check, we have used the natural logarithm of total fatalities as the dependent variable and found a statistically insignificant effect of Atlantic trade.

[49]Brazilian sugar price data are not available after 1768. This should not represent a major drawback given that it was predominantly during the first 100 years of our sample that wheat trade with South America was limited in nature. Note that we were unable to obtain sugar prices for Belgium, Florence, the

sugar prices during 1640 to 1768.

In column (3), we explore the longer-term relationship between Atlantic trade and intra-European conflict by collapsing our data to five-year periods and re-estimating our baseline IV regression. As the results demonstrate, even with the five-year data, our coefficient of interest remains robust. Note that the baseline probability of conflict onset at the five-year level is approximately five times larger than the annual level. Thus, relative to their respective baseline onset probabilities, the coefficient in column (3) is similar in magnitude to our baseline coefficient in column (1) of Table 4.

Next, in column (4), we address the concern that New World trade also included trade in silver, so that greater access to New World silver may drive some of our results since our wheat prices are expressed in grams of silver. There are two mitigating factors that work to our advantage. First, according to Findlay and O'Rourke (2009), the bulk of silver trade occurred between 1500 and 1650, which largely predates our sample. Second, as Flynn (1996) and Fisher (1989) have shown, cross-border trade resulted in the impact of New World silver spreading throughout Europe. This Europe-wide effect will be captured by our year fixed effects. Nonetheless, to the extent that there may have been heterogeneous effects, they are likely to be strongest for Spain, which was the European entry point for the bulk of New World silver. To account for this, we exclude Spain from our sample in column (4). As the results demonstrate, our coefficient of interest remains robust.

Finally, in column (5) of Table 9, we examine whether our results are robust to omitting outliers. In particular, we omit price pass-through values that are above (below) the 99th (1st) percentile and re-run our baseline IV regression. As the results in this column demonstrate, our previous findings were not driven by such outliers.

7 Conclusion

The decline in intra-European conflict from the late Middle Ages to World War I has been widely documented by a large literature spanning across many disciplines. While various explanations have been put forward to explain this trend, they predominantly emphasize events that occurred during the 18th and 19th centuries. Given that the down-

Ottoman Empire, and Sweden. Hence, these countries have been omitted from column (2) of Table 9.

ward trend in intra-European conflict predates these events (Figure 1), they cannot fully explain why conflict in Europe declined starting in the late Middle Ages.

In this paper, we proposed an alternate explanation for this decline: access to Atlantic trade. Central to our finding is the use of over 200 years of wheat price data (1640-1850) to construct time-varying measures of price pass-through between Europe and the New World. These pass-through measures, which we use to proxy Atlantic trade, provides us with the most comprehensive dataset available to study the relationship between Atlantic trade and intra-European conflict. It also allows us to overcome a data constraint that has hampered past efforts at empirically examining this relationship.

Our identification strategy exploited exogenous weather shocks (tropical cyclones and wind patterns) to instrument Atlantic trade. This allowed us to provide the first causal evidence that Atlantic trade contributed to the pacification of Europe. Indeed, we found that the increase in Atlantic trade between the mid-17th century and the early 19th century lowered the probability of conflict onset in Europe by 19.22 from a baseline onset probability of 2.30 percent. To identify the mechanisms driving this result, we showed the pacifying effects of Atlantic trade were stronger for country pairs that had extensive trade links with the New World (Britain, France, the Netherlands, Portugal, and Spain), which is consistent with the idea that forgone Atlantic trade acted as a deterrent to conflict.

Rather than creating the basis for a Kantian "perpetual peace" across the globe, it is possible that Atlantic trade had negative repercussions for societies outside Europe, which did not benefit from the same pacifying effect. While our results did not find conclusive evidence of such conflict displacement, further study of these effects as well as better understanding how trade with Asia and Africa impacted conflict in these regions are fruitful avenues for future research.

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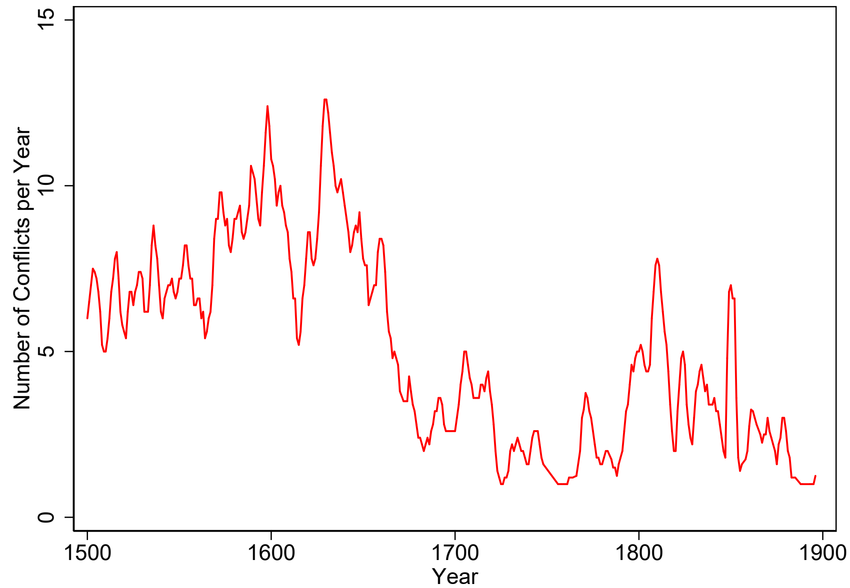
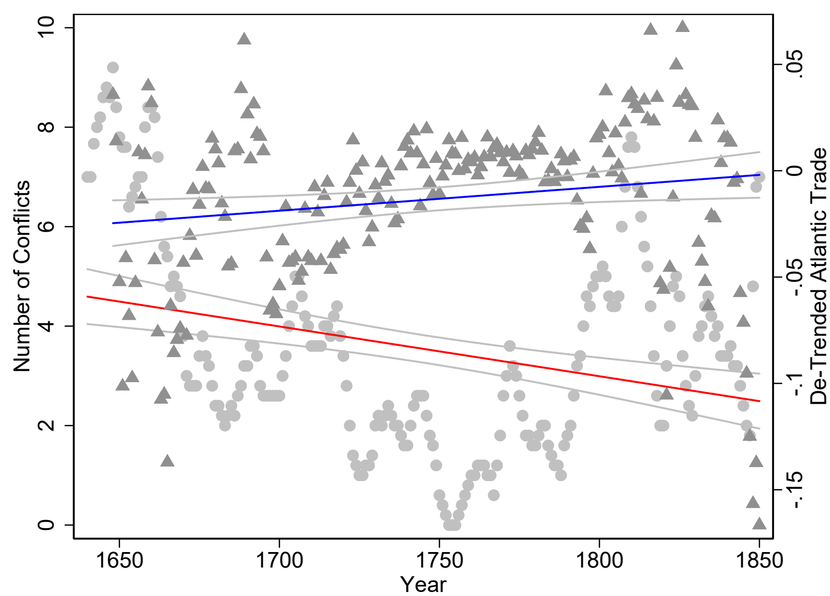
Figure 1: Trend in conflicts involving at least one of the countries in our sample.



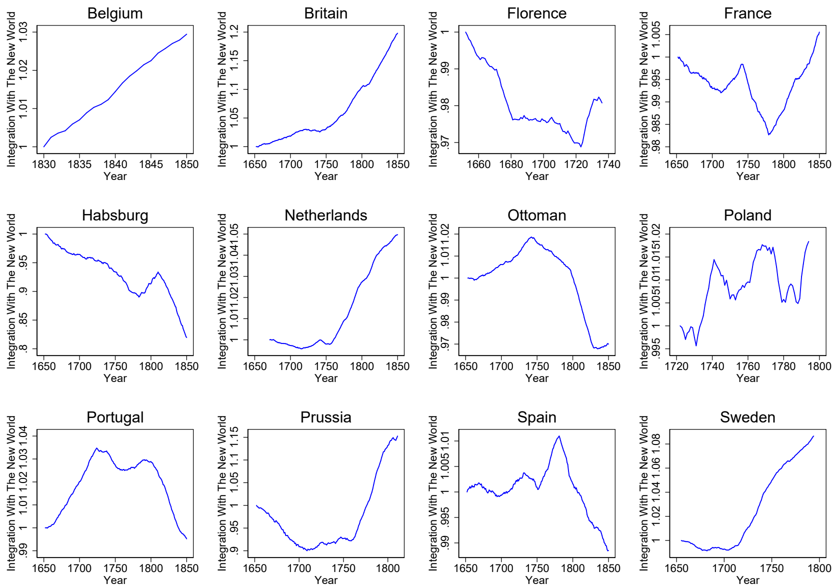
Notes: this figure plots the total number of conflicts in a year involving at least one country in our sample. The conflict data re from Brecke's Conflict Catalog.

Figure 2: Atlantic trade and intra-European conflict.



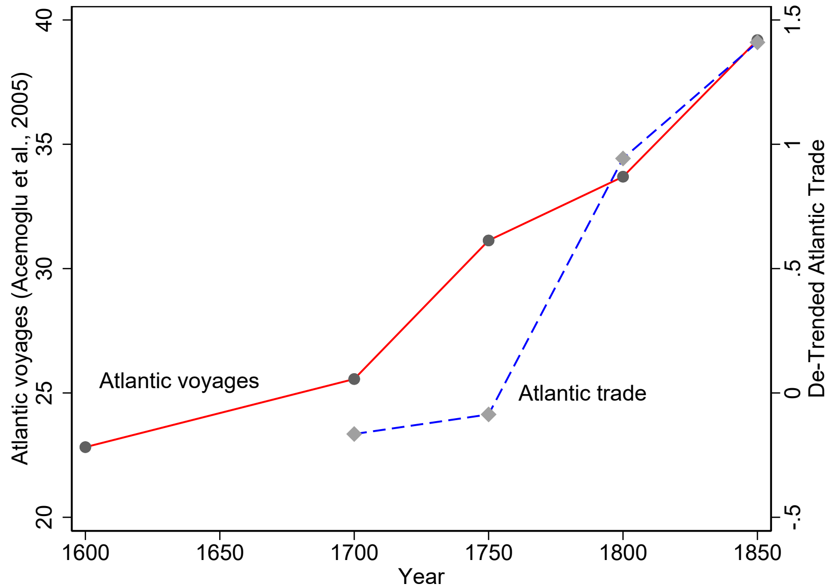
Notes: the triangles are the average annual price pass through between the New World and all European countries in our sample while the dots are the annual average of our bilateral conflict indicator.

Figure 3: New World price pass-through by country.



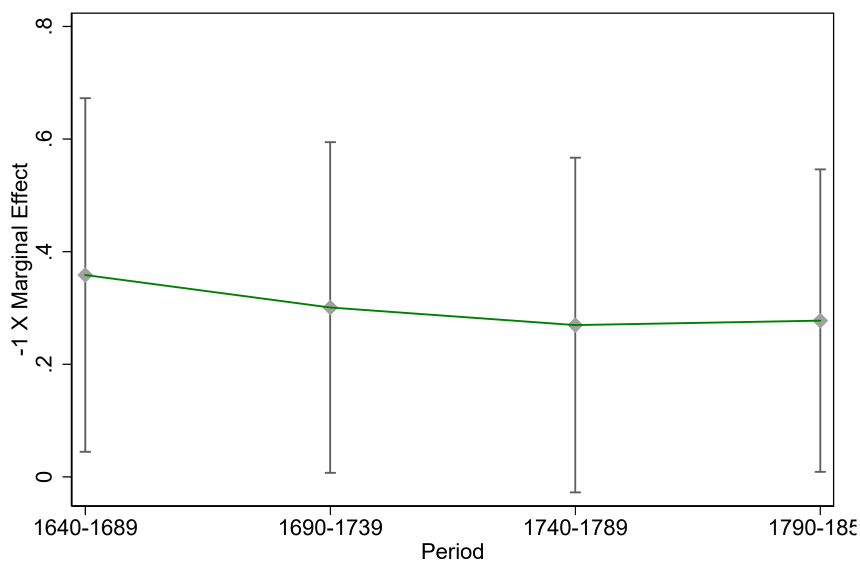
Notes: Each graph illustrates the price pass-through between a European country and the New World.

Figure 4: Atlantic voyages and Atlantic trade



Notes: the solid line is the trend in Atlantic voyages between 1600 and 1850 based on data from Acemoglu et al. (2005). These data are available by century until 1700 and then for every 50 years until the end of the 19th century. The dashed line is the average of wheat price pass-through between Europe and the New World aggregated over the periods 1640–7000, 1700–1750,...,1800–1850.

Figure 5: Atlantic trade and intra-European conflict by 50-year periods.



Notes: the dots are -1 times the effect of Atlantic trade on conflict onset in a given 50-year period while the bars represent the 95 percent confidence interval.

Table 1: Summary Statistics

	(1)	(2)
	Observations	Summary Statistics
Indicator for Conflict Onset	10,445	0.0230 (0.150)
Bilateral Trade	9,344	0.151 (0.144)
Atlantic Trade	9,906	0.133 (0.0723)
Indicator for Shared Border	10,445	0.209 (0.407)
ln(Population)	10,445	9.655 (0.787)
Tropical Cyclone Shocks	10,445	-0.098 (0.349)
ln(Sailing Time)	10,445	6.321 (0.112)
ln(Sailing Time) $\times$ Tropical Cyclone Shocks	10,445	-0.619 (2.206)

Notes: for each of the variables above, we report the sample mean and standard deviation (in brackets). All data are for the period 1640 to 1850.

Table 2: Trade and Market Integration

	(1)	(2)	(3)	(4)
Dependent variable	Ln(Imports)		Imports/GDP	
Price Pass Through	2.633***	5.317***	0.076***	0.061***
	(0.225)	(0.170)	(0.007)	(0.004)
Constant	13.788***	11.903***	0.001	-0.001
	(0.084)	(0.056)	(0.001)	(0.001)
Years Included	1845–1896	1827–1896	1845–1896	1827–1896
Observations	1,633	3,913	1,556	3,446
R-squared	0.090	0.244	0.163	0.203

Notes: the dependent variable in columns (1) and (2) is the natural logarithm of imports into a European country in our sample. In columns (3) and (4), the dependent variable is the value of imports divided by the importing country's GDP. The trade data used in columns (1) and (3) are from Pascali (2017) and span the period 1845 to 1896. The trade data used in columns (2) and (4) are from Fouquin and Hugot (2017) and span the period 1827 to 1896. All regressions include year fixed effects. The standard errors in parenthesis are robust. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 3: Atlantic Trade and Conflict Onset in Europe

	(1)	(2)	(3)
Dependent variable	Indicator for Conflict Onset		
Bilateral Trade	0.009	0.002	0.002
	(0.042)	(0.041)	(0.051)
Atlantic Trade		-0.315**	-0.302*
		(0.139)	(0.158)
Country-pair Fixed Effects	Yes	Yes	Yes
Country Fixed Effects × Year Trend	No	No	Yes
Observations	9,184	9,072	9,072
R-squared	0.169	0.170	0.174

Notes: the dependent variable in all columns is a conflict onset indicator that is one when a conflict between two countries commences and is zero otherwise. Bilateral Trade is the wheat price pass through between two countries while Atlantic Trade is the wheat price pass through between the New World and two European countries. All regressions include an indicator for conflict in year $t-2$, an indicator for whether or not the two countries share a border, the natural logarithm of both country's population lagged by five years, year fixed effects, and a constant that is not reported. The standard errors in parenthesis in all columns are cluster bootstrapped at the country-pair level with 1,000 repetitions. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 4: Endogeneity of Atlantic Trade – Instrumental Variables

	(1)	(2)	(3)
Dependent variable	Indicator for Conflict Onset		
Panel A: Second Stage			
Bilateral Trade	-0.016	-0.012	0.243
	(0.044)	(0.054)	(0.183)
Atlantic Trade	-0.967**	-0.991**	
	(0.464)	(0.474)	
Atlantic Imports			-0.346***
			(0.104)
R-squared	0.182	0.171	0.175

Dependent variable	Atlantic Trade		Atlantic Imports
Panel B: First Stage			
ln(Sailing Time) × Tropical Cyclone Shocks	-0.100***	-0.089***	-7.853***
	(0.020)	(0.019)	(2.147)
Kleibergen-Paap F statistic	29.61	23.83	14.95
Country-pair Fixed Effects	Yes	Yes	Yes
Country Fixed Effects × Year Trend	No	Yes	No
Observations	9,072	9,072	766
R-squared	0.653	0.728	0.266

Notes: in panel A column (1), we report the second-stage estimates where the dependent variable in is a conflict onset indicator that is one when a conflict between two countries commences and is zero otherwise. In panel B, we report the first-stage estimates where the dependent variable is Atlantic Trade in columns (1) and (2) and Atlantic imports in column (3). The latter uses actual import data that span the period 1827–1850, which is why the sample size is smaller. In all columns, the instrument for Atlantic Trade/Imports is ln(Sailing Time) × Tropical Cyclone Shocks. Note that the level effect of sailing time is captured by the country-pair fixed effects while the level effect of the tropical cyclone shocks is captured by the year fixed effects. The regressions in all columns include an indicator for conflict in year $t-2$, an indicator for whether or not the two countries share a border, and the natural logarithm of the sum of the two countries' population lagged by 5 years. All regressions also include year fixed effects, and a constant that is not reported. The standard errors in parenthesis in all columns are cluster bootstrapped at the country-pair level with 1,000 repetitions. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 5: Threats to Identification

	(1)	(2)	(3)	(4)
Dependent variable	Indicator for Conflict Onset			
Bilateral Trade	-0.016		-0.016	-0.032
	(0.044)		(0.044)	(0.033)
Atlantic Trade	-0.982**	-0.888**	-0.966**	-1.070***
	(0.475)	(0.449)	(0.464)	(0.269)
Bilateral Trade $(t-10)$		-0.056		
		(0.054)		
Kleibergen-Paap F statistic	27.66	28.42	29.56	27.97
Cumulative Weather Shock Instrument	Yes	No	No	No
Rainfall Shocks Included	No	No	Yes	No
Naval Conflicts Dropped	No	No	No	Yes
Observations	9,072	8,706	9,072	6,195
R-squared	0.171	0.177	0.171	0.119

Notes: the dependent variable in all columns is a conflict onset indicator that is one when a conflict between two countries commences and is zero otherwise. The instrument for Atlantic Trade is ln(Sailing Time) $\times$ Tropical Cyclone Shocks. Note that the level effect of sailing time is captured by the country-pair fixed effects while the level effect of tropical cyclone shocks is captured by the year fixed effects. In column (1), we replace annual tropical cyclone shocks with the sum of shocks over the period $t = 0$ to $t = -4$. The latter is designed to capture medium-term shocks to tropical cyclones over the Atlantic. The regressions in all columns include an indicator for conflict in year $t-2$, an indicator for whether or not the two countries share a border, and the natural logarithm of the sum of the two countries' population lagged by 5 years. All regressions also include country-pair fixed effects, year fixed effects, and a constant that is not reported. The standard errors in parenthesis in all columns are cluster bootstrapped at the country-pair level with 1,000 repetitions. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 6: Mechanisms

	(1)	(2)	(3)	(4)
Dependent Variable	Ln(Wage)	Soldiers per cap.	Ln(Navy Size)	Conflict Onset
Atlantic Trade	0.444**	-0.020***	-1.575	-0.805*
	[0.026]	[0.002]	[0.264]	(0.420)
Bilateral Trade				-0.023
				(0.044)
Atlantic Trade × Colonizer				-0.748
				(0.576)
Kleibergen-Paap F statistic	69.60	19.49	4.50	–
F statistic for Atlantic Trade	–	–	–	18.60
F statistic for Interaction Term	–	–	–	9.56
Observations	1,638	474	248	9,072
R-squared	0.724	0.590	0.843	0.172

Notes: the dependent variable in column (1) is the natural logarithm of country's annual average wage while the dependent variable in column (2) is a country's total number of soldiers per capita. The dependent variable in column (3) is the natural logarithm of a country's total number of naval ships. The regressions in columns (1) to (3) are at the country and year level, which is why the sample size in these columns is smaller. The dependent variable in column (4) is a conflict onset indicator that is one when a conflict between two countries commences and is zero otherwise. In columns (1) to (3), the instruments for Atlantic Trade are country i's ln(Sailing Time) × Tropical Cyclone Shocks and Tropical Cyclone Shocks. These regressions include an indicator for conflict in year $t-2$, country fixed effects, and a year trend while the p-values in brackets are calculated using the Cameron et al. (2008) wild bootstrap procedure with 1,000 repetitions. In column (4), the instrument for Atlantic Trade is ln(Sailing Time) × Tropical Cyclone Shocks. This regression includes all controls in the baseline specification reported in Table 4 and the standard errors in parenthesis are cluster bootstrapped at the country-pair level with 1,000 repetitions. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 7: Alternate Mechanisms

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable	Indicator for Conflict Onset					
Bilateral Trade	-0.018	-0.021	-0.014	-0.003	-0.002	-0.033
	(0.047)	(0.045)	(0.044)	(0.048)	(0.050)	(0.048)
Atlantic Trade	-1.006**	-0.990**	-1.002**	-1.088**	-0.894*	-1.024**
	(0.475)	(0.460)	(0.501)	(0.484)	(0.474)	(0.451)
Additional Controls	Staple Shocks	Famines	Colonial Links	State Capacity	Institu- tions	Westph- alia
Kleibergen-Paap F statistic	29.42	30.96	27.75	25.87	24.61	29.12
Observations	9,072	8,659	9,072	9,072	9,072	8,194
R-squared	0.171	0.177	0.171	0.172	0.172	0.181

Notes: the dependent variable in all columns is a conflict onset indicator that is one when a conflict between two countries commences and is zero otherwise. The regression in column (1) separately includes country 1 and 2's suitability for growing Old World staples interacted with a year trend. The regression in column (2) controls for whether country i or j experienced a famine in a given year as a proxy for income shocks. The regression in column (3) includes a country- and year-varying indicator for the end of colonial links between the colonizers in our sample (Britain, France, Portugal, and Spain) and the New World. The regression in column (4) includes separate interactions between an indicator for whether each country has high state capacity and a year trend. The regression in column (5) includes separate interactions between an indicator for whether each country has high parliamentary activity and a year trend. Lastly, the regression in column (6) excludes the years 1640 to 1669 to ensure that our results are not being driven by a decline in conflict after the Peace of Westphalia. In all columns, the instrument for Atlantic Trade is ln(Sailing Time) $\times$ Tropical Cyclone Shocks. All regressions include an indicator for conflict in year $t-2$, an indicator for whether or not the two countries share a border, and the natural logarithm of the sum of the two countries' population lagged by 5 years. All regressions also include country-pair fixed effects, year fixed effects, and a constant that is not reported. The standard errors in parenthesis in all columns are cluster bootstrapped at the country-pair level with 1,000 repetitions. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 8: Conflict Displacement

	(1)	(2)	(3)	(4)	(5)
Conflict Type	New World	Other NEU	Other EU	Civil	High Fatality
Atlantic Trade	-0.126	0.106	-0.013	0.249	0.769
	[0.120]	[0.618]	[0.942]	[0.248]	[0.382]
Kleibergen-Paap F statistic	83.66	83.66	83.66	83.66	7.11
Observations	2,003	2,003	2,003	2,003	967
R-squared	0.210	0.262	0.340	0.172	0.188

Notes: the dependent variable in columns (1) to (5) are indicators for conflict in the New World, outside of Europe and the New World, conflict with other European countries not in our sample, civil conflict, and high-fatality conflict respectively. High-fatality conflicts are those with total fatalities above the sample median. The instruments in all columns are country i's ln(Sailing Time) $\times$ Tropical Cyclone Shocks and Tropical Cyclone Shocks. All regressions include an indicator for conflict in year $t-2$, country fixed effects, and a year trend. The p-values in brackets are cluster bootstrapped at the country level and are calculated using the Cameron et al. (2008) wild bootstrap procedure with 1,000 repetitions. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 9: Robustness

	(1)	(2)	(3)	(4)	(5)
Dependent variable	Indicator for Conflict Onset				
	Price Gaps	Sugar Prices	5-Year Data	Without Spain	Without Outliers
Price Gap	-0.007				
	(0.006)				
Price Gap with New World	0.158***				
	(0.054)				
Bilateral Trade		0.043	-0.963*	-0.044	-0.012
		(0.055)	(0.505)	(0.059)	(0.043)
Atlantic Trade Sugar		-0.308*			
		(0.166)			
Atlantic Trade			-4.566**	-1.097***	-0.986**
			(2.146)	(0.405)	(0.477)
Kleibergen-Paap F statistic	10.39	5.61	107.50	21.73	28.78
Observations	6,123	5,019	1,844	7,250	9,007
R-squared	0.209	0.267	0.058	0.155	0.171

Notes: the dependent variable in all columns is a conflict onset indicator that is one when a conflict between two countries commences and is zero otherwise. In column (2), we use sugar prices to calculate a country's integration with South America between 1640 and 1768. In column (3), we aggregate our data to five-year periods. In column (5), we omit Atlantic Trade values that are above (below) the 99th (1st) percentile respectively. In all columns, the instrument for Atlantic Trade is ln(Sailing Time) $\times$ Tropical Cyclone Shocks. We include an indicator for conflict in year $t-2$, an indicator for whether or not the two countries share a border, the natural logarithm of the sum of the two countries' population lagged by 5 years, country-pair fixed effects, and a constant that is not reported in all regressions. We also include a five-year period fixed effect in column (3) and year fixed effects in all other columns. The standard errors in parenthesis in all columns are cluster bootstrapped at the country-pair level with 1,000 repetitions. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

A Online Appendix

A.1 Construction of the Price Data Series

A.1.1 New World

In this section, we describe the process we use to construct our wheat price data series for the New World. We constructed a separate series for North America and South America and then defined the New World as the combination of the two. Our North American price series is based on wheat prices in Massachusetts between 1640 and 1694 and wheat prices in Pennsylvania between 1720 and 1896.[1]

The raw wheat price data for Massachusetts are in grams of silver per liter while for Pennsylvania they are in grams of silver per kilogram. We convert the latter values to grams of silver per liter using the conversion of 1 liter equal to 0.772 kilograms. This conversion is provided by the Global Price and Income History Group (GPIH) website and is taken from Carter et al. (2006). By combining these two data series, we were able to construct a wheat price series for North America that spans the period 1640 to 1896. These data include prices for 198 of the 257 years during this period.

Next, we constructed a wheat price data series for South America using data from Peru and Argentina. We constructed the Argentinian price series from three sources. First, for the period 1700 to 1800, we use wheat price data from the GPIH website. These data are for Buenos Aires and include raw prices in grams of silver per kilogram. We convert these values to grams of silver per liter using the conversion 1 liter equal to 0.772 kilograms. Second, for the period 1801 to 1850, we use wheat price data from Abad et al. (2012). These data are in *reales* per *fanega*. We use the conversion provided in the raw data file of 1 *fanega* equal to 137.19 liters to first convert these data to *reales* per liter. We then use the exchange rate provided in the data of 3.030625 grams of silver per *reales* to convert the prices to grams of silver per liter.[2] Finally, for the period 1878 to 1896, we use the wheat price data provided by Francis (2014). The raw data are in U.K. pound sterling per ton. We first converted these prices to sterling per liter and then use the exchange rate provided by Clark (2005) to convert it to grams of silver per liter. The final Argentinian dataset covers the period 1700 to 1811, 1837 to 1850, and 1878 to 1896.

To construct the South American wheat price series, we supplemented the Argentinian data with pre-1700 data from Peru. These data are for Lima and cover the period 1628 to 1822, although we only use the data from 1640 to 1686.[3] The raw prices are already in grams of silver per liter. By combining the Argentinian and Peruvian data, we

[1] Both of these datasets were downloaded from the Global Price and Income History Group (GPIH) website. As of November 2017, these files were available for download here.

[2] In the raw data, this exchange rate was provided for the years 1801 to 1820. We assume that it also applied during the period 1821 to 1850.

[3] There are no wheat prices provided for the period 1687 to 1699.

were able to construct a wheat price series for South America that spans the period 1640 to 1896. These data include prices for 159 of the 257 years during this period.

A.1.2 Europe

In this section, we describe the process we use to construct our European wheat price data series. This series covers 11 countries from Europe as well as the Ottoman Empire. We describe the process we use for each country separately below.

Belgium:

The Belgian wheat price data are based on Bruges between 1830 and 1896. The prices are from (Jacks, 2005; Jacks, 2006) and are in dollars per 100 kg. We convert them in to British pounds using the exchange rate provided by Denzel (2010) and in to grams of silver using the exchange rate provided by Robert C. Allen.[4] We restrict the Belgian data to the period in which it was an independent state.

Britain:

The British wheat price data are based on prices in London between 1640 and 1896 and are provided by Robert C. Allen. The raw data are in grams of silver per liter.

Florence:

Our data for Florence cover the period 1640 to 1736 and are provided by Robert C. Allen. The raw prices are in grams of silver per liter. We restrict the data from Florence to the period in which it was an independent city state.

France:

The French wheat price data are based on prices in Paris between 1640 and 1896 and are also provided by Robert C. Allen. The raw price data are in grams of silver per liter.

Habsburg:

The Habsburg wheat price data are based on prices in Vienna between 1640 and 1896 and are also provided by Robert C. Allen. The raw price data are in grams of silver per liter.

Netherlands:

The wheat price data for Netherlands are based on prices in Amsterdam between 1640 and 1896 and are provided by Robert C. Allen. The raw data has significant gaps in the 17th century as well as between 1820 and 1866. To fill these gaps, we supplemented the baseline data with Amsterdam prices from the Allen-Unger Global Commodity Prices Dataset.[5] Both series provide wheat prices in grams of silver per liter and have a correlation coefficient between them of 0.94. We substitute any missing values in the baseline

[4]As of November 2017, the Allen dataset can be downloaded here.
[5]As of November 2017, the Allen-Unger Global Commodity Prices Dataset can be downloaded here.

data with prices from the Allen-Unger series.

Ottoman Empire

The Ottoman Empire wheat price data are based on prices in Istanbul between 1656 and 1896. These data are taken from Pamuk (2000). Due to many missing observations, we supplement these data using wheat flour prices also provided by Pamuk. The price of wheat flour is in *akches* per *kile*, where 1 *kile* is equal to 37 liters. We use this conversion to convert all wheat flour prices to *akches* per liter and then use the exchange rate provided in the raw file to convert these prices to grams of silver per liter. Before substituting wheat flour prices for wheat prices when the latter was missing, we calculated the following scaling factor:

$$S_M = \frac{\sum_t P_t^W / N_t}{\sum_t P_t^{WF} / N_t}$$

where M indexes multi-year periods 1640–1699, 1700–1749, 1750–1799,...,1850–1896. S_M is the ratio of the average price of wheat, $\sum_t P_t^W / N_t$, to the average price of wheat flour, $\sum_t P_t^{WF} / N_t$, during a period M. N_t represents the total number of years, indexed by t, during a period. The average scaling factor is 1.02 with a range between 0.84 to 1.32. Before substituting wheat flour prices for wheat prices, we multiplied the former with the appropriate period-specific scaling factor.

Poland:

The wheat price data for Poland are based on prices in Krakow between 1640 and 1794 and between 1815 and 1845. These data are also provided by Robert C. Allen. The raw data has significant gaps, which we supplemented using Krakow prices from the Allen-Unger Global Commodity Prices Dataset. Both series provide wheat prices in grams of silver per liter and have a correlation coefficient between them of 0.99. We substitute any missing values in the baseline data with prices from the Allen-Unger series.

Portugal:

The wheat price data for Portugal are based on prices in Lisbon between 1640 and 1896. These data were downloaded from the Prices, Wages, and Rents in Portugal 1300–1910 (PWR) website.[6] The raw data are in grams of silver per liter between 1640 and 1853 and in *reis* per liter from 1854 onwards. To convert the latter to grams of silver per liter, we first use the exchange rates provided by Denzel (2010) to convert the raw prices to U.K. pound sterling per liter. We then use the exchange rate provided by Clark (2005) to convert these prices to grams of silver per liter.

Prussia:

The wheat price data for Prussia are based on prices in Brunswick between 1640 and 1863. These data have been used in Jacks (2004) and Jacks (2005) and were provided by David Jacks. The raw data are in *mariengroschen* per 100 kilograms until 1800 and

[6] As of November 2017, the PWR dataset can be downloaded here.

in *reichstaler* per 100 kilograms from 1801 onwards. For the pre 1800 period, we first converted the wheat prices to *mariengroschen* per liter. We then use the exchange rates provided by Robert C. Allen in his Leipzig data series to convert the Brunswick wheat prices to grams of silver per liter.[7] Next, for the post 1800 data, we first converted the wheat prices from *reichstaler* per 100 kilograms to marks per liter using an exchange rate of 1 *reichstaler* equal to 2 mark *banco* (Denzel, 2010, p.191). We then used the exchange rates provided by Denzel (2010) to convert the raw prices to U.K. pound sterling per liter and then used the exchange rate provided by Clark (2005) to convert these prices to grams of silver per liter.

Spain:

The wheat price data for Spain are based on prices in Madrid between 1640 and 1799 and for a composite of cities from 1814 to 1884. The former series is constructed using data provided by Robert C. Allen and are in grams of silver per liter. On the other hand, the latter series is from Barquín (2001) and are in *reales* per *fanega*. We use the conversion provided by Barquín (2001) to convert these prices to *reales* per liter. We then use the exchange rates provided by Denzel (2010) to convert the raw prices to U.K. pound sterling per liter and the exchange rate provided by Clark (2005) to convert these prices to grams of silver per liter.

Sweden:

The wheat price data for Sweden are based on Östergötland from 1651 to 1735. They are from the Allen-Unger Global Commodity Prices Dataset. In the original file they are reported in *daler silvermynt* per barrel of 142.9 liters (1651-1664) and of 146.6 liters (1665-1735) and have been converted in grams of silver using the exchange rate reported in Historical Monetary and Financial Statistics for Sweden.[8] The data from 1736 to 1874 are based on Stockholm and are from the GPIH database. They are originally reported in grams of silver per kilo and have been converted to liters using the conversion of 1 liter equal to 0.772 kilograms.

A.2 Method for Estimating Market Integration

Consider the following specification of price pass through between two countries i and j:

$$\Delta \ln P_{it} = \beta_{ij,t} \Delta \ln P_{jt} + \Delta e_{it} \tag{A.1}$$

Here, the $\beta' s$ capture the pass-through of price shocks in j on to i and is our parameter

[7] In implementing this conversion we assumed that 1 *mariengroschen* is equal to 12 *pfennings*.

[8] The data can be downloaded here.

of interest.[9] We assume that the $\beta's$ follow a random walk. That is,

$$\beta_t = \beta_{t-1} + u_t \tag{A.2}$$

where $e_t \sim N(0, \sigma_e^2)$ and $u_t \sim N(0, \sigma_u^2)$. We begin by imposing the following priors:

1. $\sigma_e^2 \sim IG(v_e/2, s_e/2)$.
2. $\sigma_u^2 \sim IG(v_u/2, s_u/2)$.
3. $\beta_1 \sim N(m, h^{-1})$
4. $\alpha \sim N(m_\alpha, h_\alpha^{-1})$

We then collect all data pairs $(\Delta p_{it}, \Delta p_{jt})$ without missing values. For ease of exposition, define $y_t \equiv \Delta p_{it}$ and $x_t \equiv \Delta p_{jt}$. For any year t, if either y_t or x_t is missing, we do not include this period in the data sample.[10] For example, when $T = 5$ and y_2, y_3 and x_3 are missing, the data set only include pairs (y_t, x_t) for $t = 1, 4, 5$. Denote the non-missing values at period as $t_1, ..., t_{T_0}$.[11] We use the data that starts with the first non-missing value and ends with the last non-missing value. Hence $t_1 = 1$ and $t_{T_0} = T$ by construction. In our example, $T = 5$, $T_0 = 3$ and $t_1 = 1, t_2 = 4$ and $t_3 = 5$.

A.2.1 MCMC

Let the number of observations after discarding missing values be T_0. Our method then proceeds as follows:

1. Let $\sigma_e^2 \mid \cdot \sim IG(\overline{v}_e/2, \overline{s}_e/2)$, where $\overline{v}_e = v_e + T_0$ and $\overline{s}_e = s_e + (y - \alpha - XA\beta)'(y - \alpha - XA\beta)$.

   Here, the vector $\beta = (\beta_1, \beta_2, ..., \beta_T)$ is a $T \times 1$ vector. The $T_0 \times T$ matrix A selects the coefficient β_t with non-missing data points from the vector β. In our example above, if $T = 5$ and y_2, y_3 and x_3 are missing, then $T_0 = 3$ because $t = 2$ and $t = 3$ are treated as missing observations. Then, the matrix A is equal to

$$\begin{pmatrix} 1 & 0 & 0 & 0 & 0 \\ 0 & 0 & 0 & 1 & 0 \\ 0 & 0 & 0 & 0 & 1 \end{pmatrix}.$$

[9] For ease of exposition, we will omit the ij superscript from hereon. However, note that all of our estimates of β are specific to a particular ij pair.

[10] We can still infer β_t from the state Equation (A.2) if there is a missing value at time t. We do this using Bayes Rule.

[11] If there is no missing value, the above becomes $t_i = i$ for $i = 1, ..., T$.

$A\beta = (\beta_1, \beta_4, \beta_5)'$ is comprised of the coefficients for the observed data.The symbol X is a $T_0 \times T_0$ diagonal matrix as $diag(x_{t_1}, x_{t_2}, ..., x_{t_{T_0}})$. In our $T = 5$ example,

$$X = \begin{pmatrix} x_1 & 0 & 0 \\ 0 & x_4 & 0 \\ 0 & 0 & x_5 \end{pmatrix}.$$

The product XA is the regressor associated with β. In this example,

$$XA = \begin{pmatrix} x_1 & 0 & 0 & 0 & 0 \\ 0 & 0 & 0 & x_4 & 0 \\ 0 & 0 & 0 & 0 & x_5 \end{pmatrix}.$$

2. Next, let $\sigma_u^2 \mid \cdot \sim IG(\overline{v}_u/2, \overline{s}_u/2)$, where $\overline{v}_u = v_u + T - 1$ and $\overline{s}_u = s_u + \sum_{t=2}^{T} (\beta_t - \beta_{t-1})^2$, and

3. $\beta \mid \cdot \sim N(\overline{m}, \overline{H}^{-1})$.

We develop this algorithm based on Chan and Jeliazkov (2009) and extend it to our application with missing values. The prior for the $T \times 1$ vector β is

$$K\beta \sim N(\underline{m}, \Sigma),$$

where

$$K = \begin{pmatrix} 1 & 0 & 0 & ... & 0 & 0 \\ -1 & 1 & 0 & ... & 0 & 0 \\ 0 & -1 & 1 & ... & 0 & 0 \\ ... & ... & ... & ... & ... & ... \\ 0 & 0 & 0 & ... & 1 & 0 \\ 0 & 0 & 0 & ... & -1 & 1 \end{pmatrix},$$

$\underline{m} = (m, 0, 0, ..., 0)'$ and $\Sigma = diag(h^{-1}, \sigma_u^2, \sigma_u^2, ..., \sigma_u^2)$. One can show that

$$\beta \sim N(K^{-1}\underline{m}, K^{-1}\Sigma K^{-1'})$$

with $K^{-1}\underline{m} = m\iota$. Here ι is a $T \times 1$ vector of ones. We can simplify the above expression to

$$\beta \sim N(m\iota, H^{-1}),$$

where $H = K'\Sigma^{-1}K$. The measurement equation (A.1) provides the following likelihood:

$$y \mid \cdot \sim N(\alpha + XA\beta, \sigma_e^2 I)$$

or

$$y - \alpha \mid \cdot \sim N(XA\beta, \sigma_e^2 I),$$

where X is the previously defined $T_0 \times T_0$ diagonal matrix and A is the selection matrix. Define $Z = XA$ for ease of exposition. Standard Bayesian techniques can be used to show that the conditional posterior is

$$\beta \mid \cdot \sim N(\overline{m}, \overline{H}^{-1}),$$

where $\overline{H} = \sigma_e^{-2} Z'Z + H$ and $\overline{m} = \overline{H}^{-1}(Hm\iota + \sigma_e^{-2} Z'(y - \alpha))$. Notice that $Hm\iota = mK'\Sigma^{-1}K\iota = (hm, 0, 0, ..., 0)'$.

4. Finally, let $\alpha \mid N(\overline{m}_\alpha, \overline{h}_\alpha^{-1})$, where $\overline{h}_\alpha = h_\alpha + T_0 \sigma_e^{-2}$ and

$$\overline{m}_\alpha = \overline{h}_\alpha^{-1} \left(h_\alpha m_\alpha + \sigma_e^{-2} \sum_{s=1}^{T_0} (y_{t_s} - x_{t_s} \beta_{t_s}) \right).$$

A.3 Construction of Simulated Tropical Cyclone Data

In this section, we describe the method we use to simulate tropical cyclone data. Let $C_t = (C_1, C_2, ..., C_T)'$ denote tropical cyclones in year $t = \{1, 2, ...T\}$. We normalize C_t so that it has a zero mean and unit variance. We further assume that this vector has the following multivariate normal distribution with homoskedasticity:

$$C_t \sim N(m, \sigma^2 I_T),$$

where the distribution of the $T \times 1$ vector $m = (m_1, ..., m_T)'$ is assumed to follow a Gaussian process (GP hereafter).[12]

Time series data such as m has a very simple distributional representation under GP because the time intervals are evenly distributed. Assuming Ornstein-Uhlenbeck correlation function, the distribution of m has the following high-dimensional multivariate normal distribution:

$$m \sim N(0, \Sigma).$$

The correlation between m_i and m_j (the ith and jth elements) is $\mathrm{Corr}(m_i, m_j) = e^{-\frac{|i-j|}{L}}$, where L is the bandwidth variable to control for effective window length for the nonparametric inference. Because the data has been normalized, we assume that the covariance matrix is well captured by the the correlation matrix.

[12] A Gaussian process is a stochastic process where every finite collection of random variables has a multivariate normal distribution. It has been widely used in the machine learning literature such as in Bayesian neural networks. For further details on this process, see Williams and Rasmussen (2006) and Ebden (2015).

The parameter space includes the mean vector m, the variance σ^2 and the bandwidth L. We draw random samples from the posterior distribution of these parameters through a Markov chain Monte Carlo method (MCMC hereafter). These random samples are then used to simulate artificial values of C, denoted by $C^{(i)}$ for $i = 1, 2, ..., 100$, where each $C^{(i)}$ is a $T \times 1$ vector.

We assume informative prior with

$$\sigma^2 \sim IG(5, 2) \text{ and } L \sim G(10, 10).$$

The prior knowledge of m is implied by L. A key message is that the mean value of σ^2 is 0.5 *a priori*. It reflects the prior belief that the noise associated with the measurement error is about 50% of the magnitude of the data's variation. That is, the variance of the measurement error is half of the variance of the data. The MCMC is

1. Initialize values of (σ^2, m, L).

2. Draw $\sigma^2 \mid L, m, C$.

$$\sigma^2 \mid L, Y \sim IG\left(5 + T/2,\ 2 + \sum_{t=1}^{T}(C_t - m_t)^2/2\right)$$

3. Draw $m \mid \sigma^2, L, C$.

$$m \mid \sigma^2, L, Y \sim N(b, B),$$

   where $B = \Sigma^{-1} + \sigma^{-2}I$ and $b = B^{-1}\sigma^{-2}C$. The matrix Σ is defined as indicated by the Ornstein-Uhlenbeck correlation function.

4. Draw $L \mid \sigma^2, m, C$.

   There is no convenient form to draw L. So we resort to the Metropolis-Hastings method with a random walk. The likelihood is easy to evaluate because of normality.

5. Repeat steps 2–4 $M + G$ times. Discard the first M iterations as a burn-in sample to remove potential initial value effect.

After executing the above MCMC algorithm, the cloud of the simulated samples $\{m^{(g)}, (\sigma^2)^{(g)}, L^{(g)}\}_{g=1}^{G}$ is collected to approximate the posterior distribution $m, \sigma^2, L \mid C$. We are not interested in any parameter values, but want to draw hypothetical C's from the posterior distribution of GP to investigate any effect associated with its randomness. The following algorithm provides the details.

1. Randomly select a set $\{m^{(g)}, (\sigma^2)^{(g)}\}$ from the posterior sample.

2. Use this set of parameter to draw $^{(i)}$ from

$$C^{(i)} \sim N(m^{(g)}, (\sigma^2)^{(g)} I_T),$$

3. Repeat this process for 100 times to obtain a set of $C^{(i)}$ for $i = 1, ..., 100$.

A.4 Construction of Wind-Based Sailing Times

We obtain wind-based sailing time from Pascali (2017). These data are originally at the port level. That is, they provide the sailing time between each pair of ports in his data. To merge these data with our sample, we first assign each port in his data to its respective country. We then identify ports that are located in each of the European countries in our data as well as ports that are located in North and South America. We consider the latter to be New World ports. We exclude sailing times between ports that are located in the same country. This leaves us with a dataset of average sailing times between all New World ports and ports located in the European countries in our sample.

These sailing-time data are unavailable for Belgium, the Habsburg Empire, Poland, and Sweden, which are countries for which there aren't any recorded journeys to the New World in these data. For these countries, we assign them the following shipping time:

$$S^N \times \psi_i^N$$

where i indexes countries and N represents their nearest neighbor (by distance). S^N is the nearest neighbor's average sailing time to the New World and ψ is a scaling factor that is the ratio of i's distance to N plus N's distance to the New World divided by N's distance to the New World. Thus, these country's substituted sailing times are their nearest neighbor's sailing times scaled up by the distance between the country and the nearest neighbor.

Lastly, recall that our endogenous variable, β^{NW}, varies by European country pair ij. Therefore, to implement our IV strategy, we construct the following measure of pair-wise sailing times:

$$S_{ij} = 0.5 \times \left(S_i + S_j\right)$$

where S_i and S_j represents European country i and j's wind-based sailing time to the New World.

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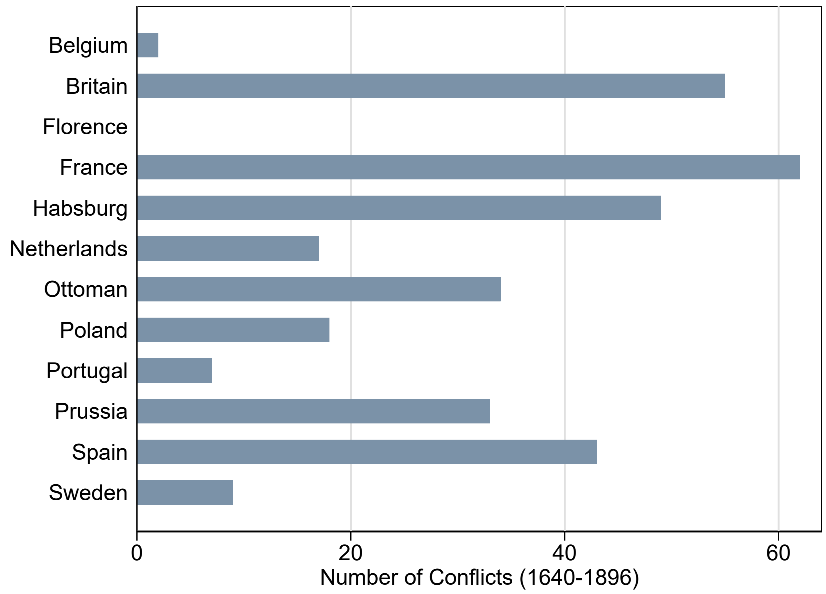
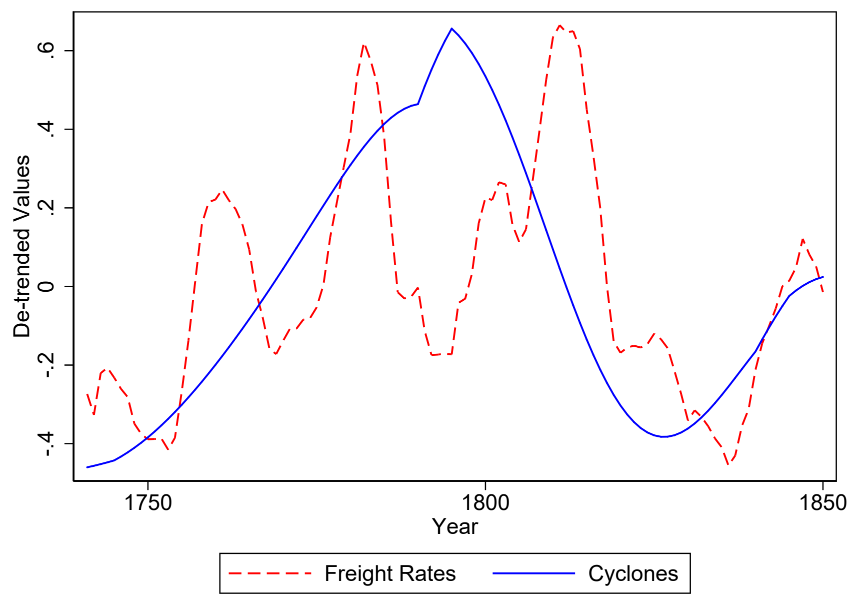
Figure A.1: Number of conflicts by country.



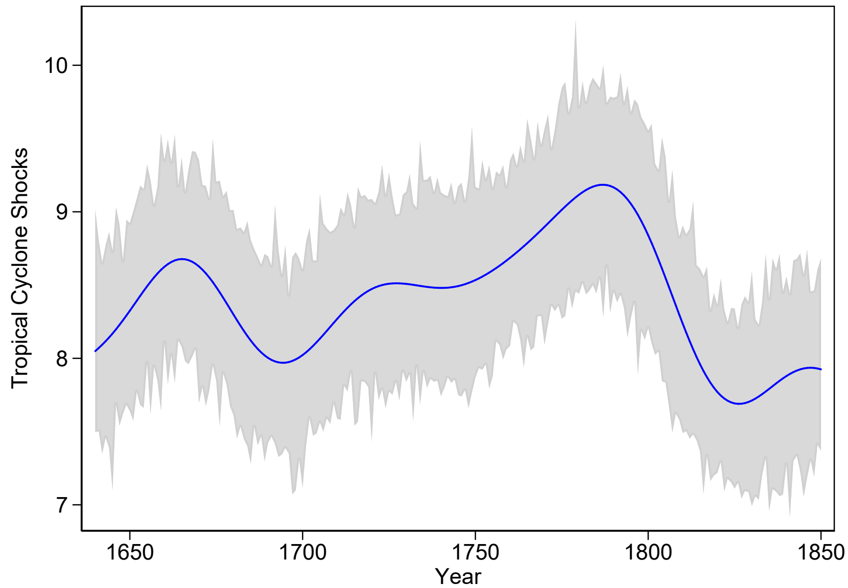
Notes: the conflict data are from Brecke's Conflict Catalog.

Figure A.2: Five-year moving average of tropical cyclone activity and Atlantic freight rates. (1740-1850).



Notes: the freight rates data are from Harley (1988) for 1741–1829 and from North (1958) for 1830–1850.

Figure A.3: Tropical cyclone data with simulated bounds (1640-1850).



Notes: the solid line is the raw data from Mann et al. (2009) while the shaded region is the bounds of the simulated tropical cyclone data.

Table A.1: Wheat Price Data Coverage

Country /Region	(1) City/State	(2) Period	(3) Number of Years	(4) Source
North America	Massachusetts	1640–1694	21	GPIH
	Pennsylvania	1720–1896	177	GPIH
South America	Lima	1640–1686	37	GPIH
	Buenos Aires	1700–1896	122	GPIH
Belgium	Bruges	1830–1896	67	Jacks (2005, 2006)
Britain	London	1640–1896	257	Allen (2001)
Florence	–	1640–1736	97	Allen (2001)
France	Paris	1640–1896	240	Allen (2001)
Habsburg	Vienna	1640–1896	241	Allen (2001)
Netherlands	Amsterdam	1640–1896	230	Allen (2001), Allen-Unger
Ottoman	Istanbul	1640–1896	154	Pamuk (2000)
Poland	Krakow	1640–1794, 1815–1845	95	Allen (2001), Allen-Unger
Portugal	Lisbon	1640–1896	248	PWR
Prussia	Brunswick	1640–1863	224	Jacks (2004, 2005)
Spain	Madrid	1640–1799	147	Allen (2001)
	Various	1814–1883	70	Barquin (2001)
Sweden	Östergötland	1651–1735	85	Allen-Unger
	Stockholm	1736–1874	46	GPIH

Notes: all wheat prices are in grams of silver per liter. GPIH refers to the Global Price and Income History Group website, ICDS refers to the *Istituto Centrale Di Statistica*, and PWR refers to the Prices, Wages, and Rents in Portugal 1300–1910 website.