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Traviss Cassidy
Mark Dincecco
Massimiliano Gaetano Onorato

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The Economic Legacy of Warfare. Evidence from European Regions

Traviss Cassidy
University of Michigan
Mark Dincecco
University of Michigan
Massimiliano Gaetano Onorato
IMT Institute of Advanced Studies Lucca

The Economic Legacy of Warfare

Evidence from European Regions*

Traviss Cassidy† Mark Dincecco‡ Massimiliano Gaetano Onorato§

March 14, 2016

Abstract

Historical warfare in Europe inflicted numerous costs on rural populations. To reduce such costs, rural populations relocated behind the relative safety of urban fortifications. We argue that war-related urbanization had positive consequences for long-run regional economic development. We geocode the locations of more than 600 conflicts in early modern Europe. We find a positive and significant relationship between historical conflict exposure and regional economic development today. Our results are robust to a wide range of econometric techniques, alternative samples, and economic outcomes. Human capital accumulation stands out as one channel through which war-related urbanization translated into regional economic development. Our results highlight the military origins of Europe's wealthy urban belt.

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†University of Michigan; tmcassid@umich.edu

‡University of Michigan; dincecco@umich.edu

§IMT Lucca; m.onorato@imtlucca.it

1 Introduction

The economic rise of Europe represents a major historical breakthrough. From 1500 to 2000, average real per capita income in Western Europe grew 15-fold (Maddison, 2010). Many scholars argue that urban centers played a fundamental role in Europe's rise (e.g., Smith, 1776, Weber, 1922, Pirenne, 1925, Bairoch, 1988, Glaeser, 2011). Today, a belt of highly urbanized regions – called the "urban belt" – forms Europe's economic backbone (Polese, 2009, p. 72).

This paper analyzes a novel source of long-run regional economic prosperity in Europe: warfare. Warfare was a fundamental part of European history (Tilly, 1992, p. 72). Parker (1996, p. 1) writes: "Hardly a decade can be found before 1815 in which at least one battle did not take place." We argue that, over the long run, greater historical warfare translated into regional economic development. Historical warfare inflicted many costs on rural populations. To reduce such costs, rural populations relocated behind the relative safety of urban fortifications. War-related urbanization, in turn, had positive economic consequences. Rural manufacturers and entrepreneurs under the threat of warfare could not only move their capital behind urban fortifications for safety, but could take advantage of urban private property rights. Furthermore, war-related urbanization could promote technological innovation and human capital accumulation. Finally, urban-based economic agglomeration effects could reduce the exchange costs for goods and labor. To the best of our knowledge, our paper is the first systematic analysis of the military origins of Europe's wealthy urban belt.

To perform this analysis, we construct a new database that links past and present. First, we identify the locations of more than 600 conflicts fought on land in early modern Europe (i.e., between 1500 and 1799). To measure regional exposure to historical warfare, we take a "market potential" approach whereby a region's conflict exposure is increasing in its geographical proximity to historical conflicts. Second, we gather modern economic data and geophysically scaled economic data at the regional level.

Our empirical analysis models regional economic activity today as a function of historical conflict exposure, controls for initial demography and local geographic features, and fixed effects by country. The results of this analysis indicate that the economic legacy of historical warfare is significant. We find that a one standard deviation increase in historical conflict exposure predicts a 17 to 20 percent average increase in current regional per capita GDP.

Our analysis controls for a wide variety of regional and national features. Still, it is possible that omitted factors that affect both historical warfare and regional economic development bias our results. To address this possibility, we use the historical presence of a young ruler (i.e., under the age of 16) to instrument for historical conflict exposure. The underlying logic is that, by virtue of governing inexperience, a young ruler is more likely than a mature ruler to 1) be vulnerable to attack by nearby states or 2) initiate conflict with nearby states. Thus, when a young ruler is on the throne, historical conflict exposure will increase in regions near the sovereign borders of the young ruler's polity. The IV analysis produces estimates that are similar in magnitude and significance to the main results. To evaluate the plausibility of our exogeneity assumption, we perform a sensitivity analysis. This analysis indicates that the IV results are robust to moderate violations of the exclusion restriction.

Our analysis accounts for different factors (e.g., initial demography, local geographic features) which could influence selection into historical conflicts. Still, both historical conflicts and the controls themselves could affect economic activity in a non-linear manner. To address this possibility, we estimate a semi-parametric treatment-effects model which allows for non-linear effects through the use of propensity scores. This analysis produces estimates of similar magnitude and significance as the main estimates. Furthermore, this analysis suggests that the relationship between historical warfare and regional economic development is in fact linear, validating our benchmark specification.

We test the robustness of our results in several other ways. As an another approach to account for omitted variable bias, we replace the country fixed effects with macro-regional (i.e., sub-national) fixed effects and re-run our main analysis. This approach produces estimates that are similar in magnitude and significance as before. Furthermore, we show that our results are robust to different conflict samples (e.g., battles only), different regional samples (e.g., exclude urban belt, exclude regions one by one), and alternative outcomes (e.g., regional population density, gross cell product).

Finally, we test for potential channels through which historical war-related urbanization can translate into long-run regional economic development. We examine three potential channels based on our conceptual framework: property rights protection, human capital accumulation, and economic agglomeration effects. The results of this analysis suggest that human capital accumulation stands out as one channel that mediates the relationship between historical war-related urbanization and regional economic development today.

Our paper contributes to the literature on the historical roots of economic development (Nunn, 2014). Scholars have linked military competition and state formation to long-run economic prosperity (Bates, 2010, Besley and Persson, 2011, O'Brien, 2011, Rosenthal and Wong, 2011, Dincecco and Prado, 2012, Voigtländer and Voth, 2013a,b, Morris, 2014). This literature tends to study the economic consequences of historical warfare for nation-states. By contrast, we analyze the economic legacy of historical warfare at the sub-national level. Europe's economic backbone is the belt of highly urbanized regions that span southern England, Belgium and the Netherlands, eastern France and western Germany, and northern Italy. In history, rural inhabitants under the threat of warfare relocated behind the relative safety of urban fortifications. Institutional development at the local level was often a historical precursor to state-level development (Stasavage, 2011, Blaydes and Paik, 2015). For all of the above reasons, it makes sense to analyze how historical warfare has influenced regional – versus national – economic development. To make our argument, we construct a new sub-national database that spans the early modern era to the present day. Our analysis of the urban belt's path from warfare to wealth provides new insights into the economic rise of Europe.

The paper proceeds as follows. Section 2 develops our conceptual framework. Section 3 describes the data. Section 4 presents the empirical strategy and main results. Section 5 performs the IV analysis. Section 6 presents the propensity score weighting results. Section 7 performs further robustness checks. Section 8 tests for transmission channels. Section 9 concludes.

2 Conceptual Framework

Our argument proceeds in two parts. First, we discuss the historical relationship between warfare and urbanization. Second, we discuss potential channels through which historical war-related urbanization can translate into regional economic development over the long run.

2.1 Warfare and Urbanization

Security was an important historical function of urban centers. Urban fortifications (outer rings of conjoined dwellings, defensive palisades or ramparts, fortified walls, bastioned traces) served at least two traditional security functions (Glaeser and Shapiro, 2002). First, urban fortifications were difficult to overcome, allowing small groups of defenders to withstand large attacks. Second, urban fortifications created a scale economy whereby the length of fortifications necessary to protect urban inhabitants fell sharply as urban populations grew.

Indeed, warfare could inflict many costs on rural populations. A first cost was property destruction by soldiers along the war march (Gutmann, 1980, pp. 32-5, Hale, 1985, pp. 182-3, 186-7, Caferro, 2008, pp. 186-7). Gutmann (1980, p. 33) writes: "Soldiers' actions read like a textbook outline of criminal practices: burning, looting, assault, rape, murder, thievery and desecration of churches." A second cost was the traditional duty of billeting soldiers in preparation for battle, in garrison or during winter, or to rest after battle's end (Gutmann, 1980, pp. 36-9, Hale, 1985, pp. 187-91, 196-7). A third cost was the loss of agricultural manpower due to the military's demands for short-term labor or recruits (Gutmann, 1980, pp. 39-41, Hale, 1985, p. 196). A final cost was rural money-raising, including the military's right of expropriation in occupied zones and its right to collect contributions in nearby zones (Gutmann, 1980, pp. 41-6, Hale, 1985, p. 185).

To reduce the costs of warfare, rural inhabitants relocated behind the relative safety of urban fortifications. Glaeser and Shapiro (2002, p. 208) call this effect the "safe harbor" effect, writing: "Indeed, the role of cities in protecting their residents against outside attackers is one of the main reasons why many cities developed over time." For example, the Dutch Revolt against Spanish rule over the latter half of the sixteenth century drove roughly 100,000 migrants north to the urban centers in the Dutch Republic (Moch, 2003, pp. 26-7, Winter, 2013, p. 406).[1] Many such urban migrants came from small villages (Verhulst, 1999, pp.

[1] Warfare could cause urban destruction, an effect that Glaeser and Shapiro (2002, p. 210) call the "target" effect. Well-known sacks include Rome in 1527, Antwerp in 1576, and Magdeburg in 1631. However, such sacks were rare (Friedrichs, 1995, p. 296). Friedrichs (1995, p. 300) writes: "No city was every completely destroyed by warfare." This observation conforms with Livi-Bacci's (2000, p. 36) general observation that, even if specific urban centers in early modern Europe saw relative decay, examples of outright disappearance (for any reason) were uncommon.

154-5).[2]

There is econometric evidence that is consistent with the safe harbor effect. Voigtländer and Voth (2013b) analyze a sample of roughly 20 countries between 1300 and 1700, finding that urbanization rates rose by more than 7 percentage points for countries that saw above-average war frequency, but rose less than 3 percentage points for countries that saw less frequent wars. Dincecco and Onorato (2016) analyze a sample of nearly 700 cities between 800 and 1800, finding that conflict exposure was associated with a 6-11 percent average increase in city populations per century.

2.2 Transmission Channels

We now describe three potential channels through which historical war-related urbanization (i.e., the safe harbor effect) can translate into long-run regional economic development.

2.2.1 Mobile Capital and Property Rights

As described above, rural populations feared the potential property loss that warfare could inflict. Rosenthal and Wong (2011, p. 105-10) argue that warfare induced an urban bias to manufacturing activity. Unlike agricultural activity, manufacturing activity was not bound to the land. Furthermore, because manufacturing capital was mobile, it was prone to thievery by soldiers marching by. Rural manufacturers thus had an incentive to move their capital behind the relative safety of urban fortifications. Indeed, urban centers were the focal point of manufacturing activity in early modern Europe (van Bavel et al., 2013, p. 385).

Beyond the relative safety of urban fortifications, urban centers could often provide manufacturers and entrepreneurs with access to local political institutions which protected private property from predatory rulers. Max Weber (1922, pp. 181-90) argues that private property rights were a defining characteristic of the historical city. Adam Smith (1776, p. 253) writes: "Order and good government, and along with them the liberty and security of individuals, were, in this manner, established in cities at a time when the occupiers of land in the

[2] In European history, there was an "urban graveyard" effect which combined high urban death rates due to epidemics with low urban birth rates (Friedrichs, 1995, pp. 130-1, Moch, 2003, pp. 44-5, Winter, 2013, p. 404). Still even if there were potential costs to urban life, there were economic and political benefits. Boone (2013, p. 234) writes: "Indeed, even in the harsher times of demographic crisis, the city continued to attract newcomers, despite the urban graveyard effect it exercised. To many, therefore, the advantages of living in a city clearly outweighed the disadvantages and inherent risks." Furthermore, infant mortality was a significant contributor to the rural-urban mortality differential (Voigtländer and Voth, 2013a, p. 780). Thus, urban migrants may not have been the main victims of the urban graveyard effect.

country were exposed to every sort of violence." De Long and Shleifer (1993), Acemoglu et al. (2005), and van Zanden et al. (2012) show evidence linking political representation with economic development in historical Europe.

Why did rulers grant local political privileges that protected private property? A standard answer revolves around warfare. Early modern governments spent large amounts on the military (Hoffman, 2015, pp. 21-2). Oftentimes, bargaining with well-off urban manufacturers and entrepreneurs was an important way for rulers to secure new funds, the "cost" of which was the granting of local political privileges (Tilly, 1975b, pp. 11, 24, Bates, 2010, pp. 44-6).

The logic of the property rights channel is as follows. Warfare played an important role in rural-urban migration through the safe harbor effect, and in particular for manufacturers and entrepreneurs. Such migrants could not only move their capital behind the relative safety of urban fortifications, but could take advantage of local political institutions that protected private property rights – the granting of which was itself related to military-related spending needs by rulers. Thus, urban migrants would have greater incentives to make growth-enhancing investments than otherwise.

2.2.2 Technological Innovation and Human Capital

A second potential channel is through technological innovation. Urban wages were higher than rural wages in early modern Europe (Voigtländer and Voth, 2013a, p. 780). Thus, urban manufacturers may have had an incentive to substitute capital for labor through technological innovation (Rosenthal and Wong, 2011, p. 105-10). Furthermore, urbanization could promote the flow of ideas (Bairoch, 1988, p. 336, Mokyr, 1995, pp. 9-10, Glaeser and Joshi-Ghani, 2015, p. xxii). For example, Mokyr (1995, p. 9) writes: "Urban areas, because of the higher frequency of human interaction, were clearinghouses for ideas and information. . . " Urbanization could also promote human capital accumulation. Glaeser and Joshi-Ghani (2015, p. xxii) write that "when the workers learn from the people around them, their human capital increases, and that makes them more productive." There may be a feedback loop between greater human capital and the ability to adopt new technology (Acemoglu, 2009, pp. 380-2). Urban centers in early modern Europe were innovation hubs (Mokyr, 1995, pp. 8-9) and places of education and learning (Friedrichs, 1995, pp. 259-60, Mokyr, 1995, pp. 10-11, van

Zanden, 2009, p. 86).[3]

The logic of the technological innovation channel runs as follows. Warfare was a catalyst for rural-urban migration via the safe harbor effect, including for relatively skilled manufacturers and entrepreneurs. Once in urban centers, such migrants could take advantage of urbanization, making technological innovation and human capital accumulation more likely than otherwise.

2.2.3 Economic Agglomeration Effects

A third potential channel is through economic agglomeration effects beyond the flow of ideas. Urbanization reduces the exchange costs for goods and labor (Glaeser and Joshi-Ghani, 2015, p. xx). For example, if an input supplier locates near a final goods producer, then both firms can increase productivity through savings on transportation costs. Similarly, urbanization promotes an efficient division of labor (Glaeser and Joshi-Ghani, 2015, p. xxi). Adam Smith (1776, p. 26) writes: "There are some sorts of industry, even of the lowest kind, which can be carried on no where but a great town." Finally, urbanization promotes thick labor markets (Glaeser and Joshi-Ghani, 2015, p. xxi). Urban centers in early modern Europe displayed diverse occupational structures (Blondé and van Damme, 2013, p. 249). Different sectors of the urban economy were subdivided into numerous specializations (Friedrichs, 1995, pp. 94-5).

The logic of the economic agglomeration channel is as follows. Rural-urban migration was an important response to warfare through the safe harbor effect, including for manufacturers and entrepreneurs. To reduce production costs, urban migrants could take advantage of economic agglomeration effects. Similarly, to find productive work, such migrants could take advantage of thick labor markets.[4]

[3] According to Mokyr (1995, pp. 14-17), local property rights promoted innovation over the short run, but reduced it over the long run as local manufacturers established entry barriers to reduce technological competition. Stasavage (2014) shows econometric evidence that supports this argument. Still, even if even if individual urban centers were captured by a local oligarchy, urban innovation for early modern Europe as a whole was important (Mokyr, 1995, pp. 17-19). Mokyr (1995, p. 19) writes: "The contribution of the totality of all European urban centers taken together, however, was enormous, even if each individual unit contributed only for a limited period of time."

[4] Ciccone (2002) finds large economic agglomeration effects in European regions today.

3 Data

3.1 Current Economic Activity

We gather data on several regional economic outcomes. First, we gather per capita GDP data at the NUTS 2 level in 2005 from Eurostat (2015).[5] To account for differences in price levels across national borders, we measure the GDP data in purchasing power standard units (PPS). Second, we gather data on three other regional (i.e., NUTS 2) economic outcomes: the share of high-technology sector employment in total employment, per capita R&D expenditures over all sectors (in PPS), and the share of economically active adults in the total population. Finally, we gather geophysically scaled economic activity data from Nordhaus (2011). These data measure the "gross cell product" (GCP) for 1-degree longitude by 1-degree latitude grid cells (i.e., approximately 100 km $\times$ 100 km) in 2005. To compute per capita GCP, we divide the gross cell product by the corresponding grid cell population.

Figure 1 maps per capita GDP by region. We shade regions according to per capita GDP by quintile, with regions in the top quintile receiving the darkest shade and regions in the bottom quintile receiving the lightest shade. Per capita GDP tends to be highest in Europe's urban belt, the highly urbanized regions that span southern England, Belgium and the Netherlands, eastern France and western Germany, and northern Italy.

3.2 Historical Warfare

We construct our historical warfare data based on Clodfelter (2002). Clodfelter's book is organized into chapters by century (i.e., from 1500 onward) and geographical zone. Within each chapter, Clodfelter categorizes military conflicts under war headings (e.g., Thirty Years' War: 1618-48). For each war heading, Clodfelter writes a multiple-paragraph entry which describes the war's details. Our unit of analysis is an individual conflict (e.g., battle). To identify the individual conflicts that comprise each war, we read through each war entry. Based on this information, we compile a list of all individual conflicts for each war. For example, according to Clodfelter, 37 individual conflicts comprise the Thirty Years' War (Table A.1). To proxy for conflict locations, we take the settlement (i.e., hamlet, village, town, city)

[5]NUTS are Eurostat's standard sub-national units of economic territory. There are three NUTS levels. NUTS 1 units correspond with major socioeconomic regions, NUTS 2 with basic regions, and NUTS 3 with small regions. GDP data are most widely available at the NUTS 2 level.

nearest to where each individual conflict took place. Our database includes all individual conflicts fought on land in Europe during the early modern era (i.e., between 1500 and 1799), which Parker (1996, p. 1) calls an "unusually belligerent" era.[6]

To measure regional exposure to historical warfare, we adapt the "market potential" measure of Harris (1954). The historical conflict exposure of region i is defined as

$$\sum_{c \in \mathcal{C}} (1 + distance_{i,c})^{-1},$$

where $distance_{i,c}$ is measured from the centroid of region i to the location of conflict c (in 100s of km). The set $\mathcal{C}$ includes all conflicts between 1500-1799. The underlying logic is that a region's exposure to a particular conflict is increasing in the conflict's proximity to the region. This measure has at least two advantages over other potential measures. First, this measure does not rely on an arbitrary cutoff distance beyond which a conflict is assumed to have no influence. Rather, it incorporates information from all conflicts. Second, the weight assigned to each conflict is bounded between zero and one, reducing the measure's sensitivity to any single conflict.[7] To facilitate the interpretation of the regression coefficients, we normalize historical conflict exposure to have a mean of zero and a variance of one.

Figure 2 maps historical conflict exposure by region (NUTS 2). Regions are shaded according to conflict exposure by quintile, with regions in the top quintile receiving the darkest shade and regions in the bottom quintile receiving the lightest shade. This figure suggests that historical conflict exposure is highest in Europe's urban belt. To complement this figure, Table 1 lists the top 35 regions ranked by historical conflict exposure. The top-ranked regions for historical conflict exposure are typically located in Belgium, the Netherlands, eastern France, western Germany, and northern Italy.

Taken together, Figure 1 and Figure 2 indicate that there is is a positive spatial correlation between historical conflict exposure and regional economic development today. However, there could be omitted confounding factors that drive the spatial variation in both historical

[6]According to Onorato et al. (2014), there was a fundamental change in the nature of warfare over the 1800s due to 1) technological improvements in transportation and communications and 2) the emergence of the mass army.

[7]If each conflict were instead weighted by $distance_{i,c}^{-1}$, then a region where a conflict location is very close to its centroid would be assigned a very large conflict exposure value, irrespective of the region's proximity to other conflicts.

warfare and current per capita GDP. To determine whether historical conflict exposure actually affects long-run regional economic development, we now undertake an econometric analysis.

4 Empirical Strategy and Main Results

4.1 Empirical Strategy

The linear specification that we estimate is

$$Y_{i,j} = \beta C_{i,j} + X_{i,j}'\phi + \mu_j + \epsilon_{i,j}, \tag{1}$$

where $Y_{i,j}$ is economic activity for region i in country j, $C_{i,j}$ is our measure of historical conflict exposure, $X_{i,j}$ is a vector of benchmark controls to be described, μ_j is the fixed effect of country j, and $\epsilon_{i,j}$ is the error term. Let $Z_j = (Z_{1,j}, \ldots, Z_{n_j,j})$ denote the vector of regional covariates Z for country j, where n_j is the number of regions in country j. Under strict exogeneity, $\mathrm{E}(\epsilon_{i,j} \mid C_j, X_j, \mu_j) = 0$, the coefficient β represents the change in average economic activity today due to a one standard deviation increase in historical conflict exposure, holding the other covariates constant.

The vector $X_{i,j}$ denotes the set of benchmark controls. We focus on "good" controls (Angrist and Pischke, 2009, p. 64) that are unlikely to be influenced by post-1500 developments. To account for initial regional demographic conditions, we proxy for regional population density in 1500.[8] To account for regional geographic features, we include binary variables that take the value 1 for regions that have primary rivers, are landlocked (i.e., no ocean or sea access), or were Roman road hubs.[9] We also account for average elevation above sea level, terrain ruggedness, and land quality.[10] Finally, we always account for average dis-

[8]We take the demographic data from Bosker et al. (2013), who truncate all city populations in 1500 of less than 5,000 inhabitants at zero. To proxy for regional population density, we aggregate city populations by region (NUTS 2) and divide by the region's area. To include all observations, we add one before taking the log of this variable.

[9]The primary rivers data are from European Environment Agency (2009), the landlockedness data are from Natural Earth (2015), and the Roman road hubs data are from Touring Club Italiano (1989).

[10]The elevation and ruggedness data are from Nunn and Puga (2012). These data are available for grid cells of roughly 1 km × 1 km. The land quality data are from Ramankutty et al. (2002). These data calculate the probability that a grid cell (of roughly 55 km × 40 km) can be cultivated based on local climate and soil conditions. For each variable, we average data values across all grid cells within each region.

tance from the region centroid to the nearest sovereign border of a historical polity over the 1500-1799 period. We include this control because proximity to historical borders may be linked with greater conflict exposure on one hand and greater interstate trade on the other. To reproduce historical sovereign borders, we digitize the maps in McEvedy (1972) for all available years in and around the early modern era (i.e., 1483, 1600, 1681, 1783). We calculate the distance between each region's centroid and the nearest border of a historical polity for each map year and then average across map years.

We take three approaches to statistical inference. First, we report standard errors robust to clustering at the country level. Second, we report the p-value of the test of β using the wild cluster bootstrap. Because there are only 22 countries in our sample, the number of clusters is small, and tests based on analytical cluster-robust standard errors can over-reject in practice. Following Cameron et al. (2008), we use the wild cluster bootstrap to achieve a test of correct size in the presence of clustering. The bootstrap is based on 10,000 replications. Third, we report the p-value of the test using Conley (1999) standard errors that allow for general forms of spatial autocorrelation of the error term. The Conley standard errors use a cutoff distance of approximately 1,500 kilometers, beyond which spatial autocorrelation is assumed to be zero.[11]

Table A.2 presents the summary statistics for the regression variables.

4.2 Main Results

Table 2 presents our estimates for the relationship between historical conflict exposure and log regional per capita GDP. Column 1 reports the results for the specification that controls for country fixed effects and average distance to the nearest polity. The estimated coefficient is positive and statistically significant at the one percent level according to all three hypothesis tests. Column 2 adds the geographic controls. The point estimate and statistical significance are virtually unchanged. Column 3 adds log population density in 1500. The point estimate falls slightly but remains large and highly statistically significant according to all three hypothesis tests.

Overall, the results in Table 2 support the argument for the economic legacy of historical warfare. The point estimates suggest that a one standard deviation increase in his-

[11] The results are very similar in magnitude and significance if we use different cutoff values.

torical conflict exposure predicts an increase in regional per capita GDP today of 17 to 20 percent. For perspective, this magnitude corresponds to the difference in per capita GDP between Tübingen (a region in the southwestern German state of Baden-Württemberg) and Schleswig-Holstein (a region in the eponymous northern German state).

5 Instrumental Variables

5.1 Young Rulers

The results in Table 2 display a positive and significant relationship between historical conflict exposure and regional economic development today. Though we control for a wide range of regional and national features, it is still possible that omitted factors that affect both historical warfare and current development bias our results. To address this possibility, we pursue an instrumental variables strategy.

To instrument for historical conflict exposure, we use the historical presence of a young ruler (i.e., under the age of 16).[12] The underlying logic is as follows. By virtue of governing inexperience, a young ruler is more likely than a mature ruler to be vulnerable to attack by nearby states which hope to exploit this weakness. Similarly, a young ruler is more likely to initiate conflict with nearby states due to his governing inexperience (e.g., to overcompensate). Overall, this logic suggests that historical conflict exposure will increase in regions near the sovereign borders of the young ruler's polity. To avoid any endogeneity concerns, we do not count any case in which the young ruler took the throne in response to the previous ruler's assassination or battle death, which could have been premeditated.

We now offer an example to illustrate this logic. King Henry II of France died of a head wound from jousting in 1559. His heir, Francis II, took the throne at the age of 15 (under the guardianship of Catherine de Medici). After surviving a coup attempt, France II died from health complications in 1560 after 17 months on the throne. He was succeeded by his 10-year old brother, Charles IX. Hale (1985, p. 18) argues that the political instability surrounding the

[12]In a somewhat similar manner, Dube and Harish (2015) and Acharya and Lee (2016) exploit historical heir data in Europe. Dube and Harish focus on the 1480-1913 period. They instrument for historical queenly rule with the presence of 1) a male first-born child and 2) a sister, finding that queens were more likely to participate in interstate wars. Acharya and Lee focus on male heirs between 1000 and 1500. They argue that a lack of heirs promoted succession disputes, which weakened state institutions and reduced the prospects for long-run economic growth. They find a negative relationship between the historical likelihood of male heirs and regional economic development today at the NUTS 1 level (i.e., major socioeconomic regions).

reigns of the young rulers Francis II and Charles IX was an important cause of the outbreak of the first French War of Religion in 1562.

5.2 IV Data

To identify the historical presence of young rulers, we gather political dynasty data from Hansen (2006). For each listed polity in early modern Europe, we record the name of each ruler, the start and end years of each reign, the cause of each death (i.e., accidental, assassination, in battle, or natural causes), and whether each ruler took the throne before the age of 16, which we define as "young." As described above, we do not count any case in which a young ruler took the throne as result of the previous ruler's assassination or battle death. There are 57 cases of young rulers according to these criteria in our sample. Out of these cases, two took the throne following accidental deaths while the others took the throne following death by natural causes.[13]

The incidence of young rulers varies at the level of the historical polity. To construct an instrument that varies at the region level (NUTS 2), we weight the historical presence of each young ruler according to the region's proximity to the young ruler's historical sovereign borders. The underlying logic is that historical exposure to the effect of young rulers is increasing in proximity to the sovereign borders of his historical polity, where conflict is most likely to occur. We again use the maps in McEvedy (1972) to identify the locations of historical polity borders.

Region i's exposure to young rulers is

$$\sum_{t=1500}^{1799} \sum_{p \in \mathcal{P}} YoungRuler_{p,t} \times (1 + distance_{i,p,t})^{-1}.$$

The set $\mathcal{P}$ contains all historical polities in Europe between 1500 and 1799. $YoungRuler_{p,t}$ equals one if a young ruler took the throne in historical polity p and year t, and zero otherwise. The variable $distance_{i,p,t}$ is measured from the centroid of region i to the nearest sovereign border of polity p in year t (in 100s of km). We measure distance in year t according to the McEvedy map that most accurately reflects the historical sovereign borders at that

[13]For robustness, we constructed a second instrument based on accidental ruler deaths. However, there were only 18 such cases in our sample. The IV estimates for the second instrument are very similar to the Table 3 estimates, but slightly less precise.

time. If multiple maps are available, then we use the most recent map drawn prior to year t. We normalize the instrument to have a mean of zero and a variance of one.

5.3 IV Results

The two-stage least squares specification that we estimate is

$$Y_{i,j} = \beta C_{i,j} + X'_{i,j}\phi + \mu_j + \epsilon_{i,j} \tag{2a}$$

$$C_{i,j} = \pi IV_{i,j} + X'_{i,j}\alpha + \eta_j + v_{i,j}, \tag{2b}$$

where $IV_{i,j}$ is historical exposure to young rulers. We include in $X_{i,j}$ the same set of benchmark controls as in the OLS analysis. The parameter of interest, β, is identified under strict exogeneity of the instrument and the control variables: $\mathrm{E}(\epsilon_{i,j} \mid IV_j, X_j, \mu_j) = 0$. Intuitively, this condition embodies two assumptions about the instrument. First, the instrument is mean independent of unobserved determinants of long-run regional economic development. Second, the instrument only affects regional economic development through historical conflict exposure, such that the instrument does not reside in the regression error of the structural equation, $\epsilon_{i,j}$.

Table 3 presents the results of the IV analysis. The top panel reports the results of the first-stage regression. The first-stage coefficient estimate is stable across different specifications of the control variables. A one standard deviation increase in historical exposure to young rulers predicts an increase in historical conflict exposure of 0.628 to 0.635 standard deviations. This coefficient is always highly significant. The bottom panel reports the results of the second-stage regression. In addition to the standard statistics, we report the p-value from the Anderson and Rubin (1949) chi-squared test of the significance of the estimated coefficient on historical conflict exposure. This test is based on the reduced-form equation and is robust to the presence of weak instruments under the assumption that the instruments are valid. We also report the Wald rk F statistic in Kleibergen and Paap (2006). This F statistic ranges from 69 to 194, indicating that the instrument is strong. The second-stage coefficient estimates are always statistically significant and quantitatively similar to the OLS estimates. The IV results suggest that a one standard deviation increase in historical conflict exposure increases regional per capita GDP today by 19 to 20 percent.

5.4 Sensitivity Analysis

The IV results rely on the assumption that the exclusion restriction holds exactly. However, our instrument may only be "plausibly exogenous" (Conley et al., 2012) in the sense that the exclusion restriction only holds approximately. Formally speaking, in the following system of equations

$$Y_{i,j} = \beta C_{i,j} + X'_{i,j}\phi + \gamma IV_{i,j} + \mu_j + \epsilon_{i,j} \tag{3a}$$

$$C_{i,j} = \pi IV_{i,j} + X'_{i,j}\alpha + \eta_j + v_{i,j}, \tag{3b}$$

we assume so far that $\gamma = 0$. Yet what if γ is near zero but not exactly equal to it? We test the robustness of our IV estimates to violations of the exclusion restriction according to Conley et al. (2012). We employ the most conservative procedure, specifying a range of values that γ can take without assuming any value is more likely than another. For a given $\delta > 0$, we construct a conservative 95 percent confidence interval for β that allows for $\gamma \in [-\delta, \delta]$.

Figure 3 presents the results of this exercise. The dotted line gives the value of the benchmark IV estimate under exogeneity as a reference point. The dashed lines represent the upper and lower bounds of the 95 percent confidence interval for β for each value of δ. We can reject the null of $\beta = 0$ at the 5 percent level for $|\delta| \leq 0.026$, meaning that our instrument would need to have a direct partial effect of more than 0.026 in order for us to conclude that β is statistically indistinguishable from zero (at the 5 percent level). To put this effect into context, it is 13 percent of the benchmark IV estimate of β (0.199) and 15 percent of the benchmark OLS estimate (0.171). Thus, the results of this exercise indicate that the IV estimates are robust to moderate violations of the exclusion restriction.

6 Propensity Score Weighting

Historical warfare may have a non-linear effect on regional economic development. Furthermore, if initial demographic conditions and regional geographic features enter into the true outcome equation in a non-linear manner, then the original linear specification may not fully account for their influence. To address such concerns, we relax the assumption that the conditional expectation of $Y_{i,j}$ is linear in $C_{i,j}$ and $X_{i,j}$. Following Cattaneo (2010), we estimate the "treatment" effects of conflict exposure in a semi-parametric environment.

Cattaneo's framework allows the treatment variable to take a finite number of values. For our sample, it is feasible to allow the treatment variable to take three values as defined by the terciles of historical conflict exposure. Regions in the first tercile have historical conflict exposure of less than -0.315, regions in the second tercile between -0.315 and 0.515, and regions in the third tercile of greater than 0.515.[14]

Following Cattaneo (2010), let $t \in \mathcal{T}$ index treatment levels (i.e., conflict exposure terciles), where $\mathcal{T} = \{1, 2, 3\}$. For $t \in \mathcal{T}$, let the random variables $Y(t)$ denote the potential economic outcomes under each treatment level. Let $T \in \mathcal{T}$ indicate which treatment level was actually received, and let $D_t = 1\{T = t\}$ indicate receipt of treatment level $t \in \mathcal{T}$. Suppose that the vector X contains observable covariates such as the controls described in Section 4.1. The key assumption for identification is

Assumption 1. For all $t \in \mathcal{T}$:

(i) $Y(t) \perp\!\!\!\perp D_t \mid X$ and

(ii) $0 < p_{min} \leq \mathrm{P}(T = t \mid X)$.

Part (i) of this assumption states that the potential economic outcomes are independent of the treatment level after conditioning on the observables in X. Part (ii) states that, for each subpopulation defined by values of X, the probability of receiving each treatment level is not too small. Following Cattaneo (2010), we exploit Assumption 1 using an inverse probability weighting scheme based on the generalized propensity score, $p_t^*(X) = \mathrm{P}(T = t \mid X)$, where we estimate the generalized propensity score by multinomial logit.

Table 4 displays the results of this analysis.[15] Column 1 uses a covariate set X that includes only average distance to the nearest polity, while column 2 adds the regional geographic controls, and column 3 adds log population density in 1500. The first row reports the average treatment effect (ATE) of being in the second tercile of historical conflict exposure as compared to being in the first. Similarly, the second row reports the ATE of being in the third tercile of historical conflict exposure as compared to being in the first. The ATE of being in the third tercile as compared to being in the second is the difference between

[14]For example, the region that includes the community of Madrid (Spain) falls into the first tercile, the region that includes greater Manchester (United Kingdom) falls into the second, and the region of Lombardy (Italy) falls into the third.

[15]The sample size falls by 13 due to the trimming of observations with small estimated propensity scores.

the second-row estimate and the first-row estimate. Median conflict exposure is -1.120 in the first tercile, 0.071 in the second tercile, and 1.034 in the third tercile. Thus, moving from the median value of conflict exposure within one tercile to the median value within the next tercile always corresponds to an increase in conflict exposure of approximately one, which is the standard deviation of conflict exposure. This fact facilitates comparison of the effects estimated in Table 4 to those in Tables 2 and 3. The ATE of being in the second tercile as compared to being in the first ranges from 0.172 (when conditioning on the full set of covariates) to 0.342. The ATE of being in the third tercile as compared to being in the second ranges from 0.231 to 0.301. All estimated treatment effects are highly statistically significant.

Overall, the results of this exercise support our main results. First, the estimated effect of historical conflict exposure is similar to the OLS and IV estimates. In particular, when conditioning on the full set of covariates, the estimates imply that a one standard deviation increase in conflict exposure roughly translates into a 17 to 23 percent increase in regional per capita GDP. The corresponding effect implied by the OLS and IV estimates ranges from 17 to 20 percent. Second, the economic effect of historical conflict exposure appears to be approximately linear. The ATE of moving from the first to the second tercile of conflict exposure (17 percent) is relatively similar to the ATE of moving from the second to the third tercile (23 percent). We interpret this result as a validation of our original linear specification.

7 Further Robustness

7.1 Region Fixed Effects

Our econometric models always include country fixed effects. However, there may be unobserved factors at the regional level that may bias our results. As another way besides the IV approach to account for omitted variable bias, we estimate the original linear specification with fixed effects at the NUTS 1 regional level (i.e., major socioeconomic regions). Our sample has 90 NUTS 1 regions.[16]

Table 5 presents the results for regional fixed effects. The point estimates are similar to the main estimates, ranging from 0.15 to 0.25. The estimated coefficients are statistically significant across all three specifications. This exercise indicates that our results are relatively

[16]For example, France has 8 domestic NUTS 1 regions (e.g., Paris Basin) and 22 domestic NUTS 2 regions.

insensitive to controlling for fixed effects at the regional (versus country) level.

7.2 Alternative Samples

7.2.1 Exclude Urban Belt

Europe's urban belt may have special features – for example, rich agricultural conditions and easy waterway access – that promoted both historical warfare and regional economic development.[17] To test whether the urban belt drives our results, we exclude the 50-plus regions that comprise this belt.[18] Figure 4 maps the urban belt regions that we exclude (dark shading).

Table 6 presents the results when we exclude the urban belt. The point estimates are similar to, and slightly smaller than, the main estimates, ranging from 0.16 to 0.19. The estimated coefficients are statistically significant across all specifications and hypothesis tests except for one (i.e., column 1, wild cluster bootstrap). This exercise indicates that some special feature of the urban belt does not drive our results.

7.2.2 Exclude Regions One by One

To test whether our results are sensitive to any outlier region, we exclude regions one by one. Figure 5 displays the results of this exercise for the most stringent main specification (i.e., column 3 of Table 2). The point estimates and confidence intervals are remarkably stable across samples. This exercise indicates that no single outlier region drives our results.

7.2.3 Exclude Capitals

To test whether capital city status influences our results, Table 7 excludes all regions that contain current sovereign capitals, of which there are nearly 30. The coefficient estimates are very similar in magnitude and significance to the main results.

[17]For example, Motamed et al. (2014) find that high agricultural potential and low transportation costs promote urbanization. Andersen et al. (2015) show that the adoption of the heavy plow in medieval Europe led to greater urbanization in regions with clay soils. Iyigun et al. (2010) find a negative relationship between the introduction of the potato in European regions suitable to its cultivation and violent conflict.

[18]Namely, we exclude all regions in southeastern England, Belgium, the Netherlands, northern Italy, and along the French-German border. Furthermore, we exclude Liechtenstein, Luxembourg, and all regions in Switzerland. However, 1) Liechtenstein and Luxembourg play no role in the fixed-effects analysis because each polity has only one NUTS 2 region and 2) Switzerland is already excluded from this analysis due to missing data for regional per capita GDP.

7.2.4 Conflict Types

Battles and sieges account for over 90 percent of historical conflicts in our sample (i.e., 350 battles and 247 sieges). To test whether conflict types influence our results, Table 8 restricts the conflict sample to battles only (top panel) or sieges only (bottom panel). The estimated coefficients remain statistically significant across all specifications. The point estimates for historical battle exposure are very similar to the main results, while the point estimates for historical siege exposure are slightly smaller. Furthermore, the estimates remain similar to the main results in magnitude and significance if we test historical conflict exposure separately for 1500-99, 1600-99, or 1700-99 (results not shown).

7.3 Alternative Outcomes

Table 9 presents the estimates for the alternative regional economic outcomes: population density, the share of high-tech employment, per capita R&D spending, and the share of economically active adults. The estimated coefficients are positive across all four outcomes above and are statistically significant according to every hypothesis test across the first three outcomes listed above.

Table 10 presents the estimates for geophysically scaled economic activity (Nordhaus, 2011). The results for historical conflict exposure are again very similar to the main results.[19]

8 Channels

The evidence shown in Sections 4 to 7 supports our argument that the economic legacy of historical warfare is significant. This section tests potential channels through which historical war-related urbanization can translate into long-run regional economic development.

Following the conceptual framework in Section 2, we focus on three potential channels for which historical data are available: property rights protection, human capital accumulation, and economic agglomeration effects. To operationalize these channels, we rely on Tabellini (2010), who constructs historical data at the "macro" regional level for eight countries.[20] To proxy for past property rights protection, we use the constraints on the executive

[19] We measure all explanatory variables in Table 10 in the same manner as for the NUTS 2 regions, except now the regional unit is a 100 km $\times$ 100 km grid cell (i.e., approximately 1-degree longitude by 1-degree latitude).

[20] Tabellini's regions are larger than NUTS 2 regions but do not correspond exactly with NUTS 1 regions. His sample countries are France, Germany (excluding East Germany), Italy, the Netherlands, Belgium, Spain,

for regional political institutions in 1850. As described in Section 2, this measure captures the intimate relationship between property rights institutions and political institutions (Acemoglu and Johnson, 2005). To proxy for past human capital accumulation, we use the literacy rate in 1880, defined as the share of the regional population who could read and write. Finally, to proxy for past economic agglomeration effects, we use the urbanization rate in 1850, defined as the share of the regional population that lived in cities with more than 30,000 inhabitants.

We estimate the equation

$$M_{t,j} = \beta C_{t,j} + X'_{t,j}\phi + \mu_j + \epsilon_{t,j}, \tag{4}$$

where $M_{t,j}$ is one of the three channels for Tabellini's region t, $C_{t,j}$ measures historical conflict exposure according to the method described in Section 3.2, $X_{t,j}$ is a vector of regional controls that mimics the set of benchmark controls in Equation 1, μ_j is the fixed effect for country j, and $\epsilon_{t,j}$ is the error term.

Table 11 presents the results of this exercise. There is a positive relationship between historical conflict exposure and each channel for the most stringent specifications (i.e., columns 3, 6, and 9). However, only the point estimate in the literacy equation is statistically significant according to all three hypothesis tests. We interpret these results with caution, because the number of regions for which Tabellini's data are available is relatively small (i.e., 65 observations) and differs from the main sample. Still, with respect to the conceptual framework, this evidence suggests that human capital accumulation stands out as one channel through which war-related urbanization translated into regional economic development over the long run.

9 Conclusion

This paper shows new evidence about the military origins of regional economic prosperity in Europe. Warfare was a fundamental feature of European history. We argue that, to reduce the costs of warfare, rural inhabitants relocated behind the relative safety of urban fortifications. War-related urbanization, in turn, had positive consequences for long-run economic development.

Portugal, and the United Kingdom.

To test our argument, we perform an empirical analysis on a new sub-national database that spans the early modern era to the present day. We show evidence that the economic legacy of historical warfare is significant. There is a positive, large, and significant relationship between historical conflict exposure and current regional economic development. This result is robust to a wide range of econometric techniques, alternative samples, and economic outcomes. Human capital accumulation stands out one channel that mediates the relationship between historical war-related urbanization and long-run regional economic development.

To the best of our knowledge, our paper represents the first systematic analysis of the military origins of Europe's wealthy urban belt. However, our results do not imply that the legacy of historical warfare is always positive. Besley and Reynal-Querol (2014), Fearon and Laitin (2014), and Dincecco et al. (2015) show evidence that the conflict legacy in Sub-Saharan Africa is negative. There is a significant relationship between historical warfare and civil conflict today. To better understand the factors that explain why the legacy of historical warfare differs across various parts of the world, further study is required.

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Table 1: Highest Historical Conflict Exposure by Region (NUTS 2)

ID	Region	Country	Conflict Exposure
BE32	Prov. Hainaut	Belgium	2.00
BE31	Prov. Brabant Wallon	Belgium	1.91
BE10	Région de Bruxelles-Capitale	Belgium	1.88
BE24	Prov. Vlaams-Brabant	Belgium	1.84
BE23	Prov. Oost-Vlaanderen	Belgium	1.80
BE35	Prov. Namur	Belgium	1.80
FR30	Nord-Pas-de-Calais	France	1.73
BE25	Prov. West-Vlaanderen	Belgium	1.72
BE21	Prov. Antwerpen	Belgium	1.70
BE22	Prov. Limburg	Belgium	1.68
BE33	Prov. Liège	Belgium	1.66
BE34	Prov. Luxembourg	Belgium	1.58
NL34	Zeeland	Netherlands	1.57
NL42	Limburg	Netherlands	1.52
DEB3	Rheinhessen-Pfalz	Germany	1.51
NL41	Noord-Brabant	Netherlands	1.50
DE12	Karlsruhe	Germany	1.47
DEB2	Trier	Germany	1.47
FR22	Picardie	France	1.46
DEA2	Köln	Germany	1.45
DEC0	Saarland	Germany	1.44
DEB1	Koblenz	Germany	1.43
DE71	Darmstadt	Germany	1.39
DEA1	Düsseldorf	Germany	1.38
DE13	Freiburg	Germany	1.35
FR42	Alsace	France	1.35
NL33	Zuid-Holland	Netherlands	1.35
DE11	Stuttgart	Germany	1.32
ITC4	Lombardia	Italy	1.31
FR41	Lorraine	France	1.29
DE72	Gießen	Germany	1.28
DE14	Tübingen	Germany	1.26
NL31	Utrecht	Netherlands	1.25
FR10	Île France	France	1.23
FR21	Champagne-Ardenne	France	1.22

Notes. Historical conflict exposure of region i is $\sum_{c \in \mathcal{C}} (1 + distance_{i,c})^{-1}$, normalized to have mean zero and variance one. The set $\mathcal{C}$ includes all conflicts between 1500-1799. $distance_{i,c}$ is the distance from the centroid of region i to the location of conflict c (in 100s of km).

Table 2: Economic Legacy of Warfare: Main Results

Dependent variable:	Log GDP per capita, 2005		
	(1)	(2)	(3)
Conflict exposure, 1500-1799	0.203 (0.058) [0.002]	0.200 (0.071) [0.011]	0.171 (0.063) [0.013]
Avg. dist. to nearest polity	Yes	Yes	Yes
Geographic controls	No	Yes	Yes
Log population density, 1500	No	No	Yes
Wild cluster bootstrap p-value	0.006	0.003	0.005
Conley p-value	0.000	0.000	0.000
R^2	0.141	0.148	0.359
Number of clusters	22	22	22
Observations	261	261	261

Notes. Estimates are obtained by ordinary least squares, using country fixed effects. Log GDP per capita is measured in purchasing power standard units (PPS). The geographic controls are primary rivers, landlockedness, Roman road hubs, elevation, ruggedness, and land quality. Robust standard errors clustered at the country level are in parentheses, followed by corresponding p-values in brackets. We report the p-values corresponding to tests of the conflict coefficient using the wild cluster bootstrap (to account for the small number of clusters) and Conley (1999) standard errors (to account for spatial autocorrelation). The wild cluster bootstrap p-values are based on 10,000 replications. The Conley standard errors use a cutoff distance of approximately 1,500 kilometers, beyond which spatial correlation is assumed to be zero.

Table 3: Economic Legacy of Warfare: IV Results

First Stage:	Conflict exposure, 1500-1799		
	(1)	(2)	(3)
Exposure to young rulers, 1500-1799	0.635	0.628	0.628
	(0.076)	(0.046)	(0.045)
	[0.000]	[0.000]	[0.000]
Avg. dist. to nearest polity	Yes	Yes	Yes
Geographic controls	No	Yes	Yes
Log population density, 1500	No	No	Yes
Conley p-value	0.000	0.000	0.000
R^2	0.770	0.778	0.782
Number of clusters	22	22	22
Observations	261	261	261

Second Stage:	Log GDP per capita, 2005		
	(1)	(2)	(3)
Conflict exposure, 1500-1799	0.199	0.195	0.199
	(0.067)	(0.092)	(0.087)
	[0.003]	[0.034]	[0.023]
Avg. dist. to nearest polity	Yes	Yes	Yes
Geographic controls	No	Yes	Yes
Log population density, 1500	No	No	Yes
Conley p-value	0.001	0.011	0.002
Anderson-Rubin p-value	0.021	0.051	0.036
Kleibergen-Paap Wald rk F statistic	69.4	185.1	193.8
Number of clusters	22	22	22
Observations	261	261	261

Notes. The first panel shows the first-stage estimates from regressing historical conflict exposure on historical exposure to young rulers. The second panel shows the second-stage estimates of the effect of historical conflict exposure on regional per capita GDP, using historical exposure to young rulers to instrument for historical conflict exposure. Log GDP per capita is measured in purchasing power standard units (PPS). The geographic controls are primary rivers, landlockedness, Roman road hubs, elevation, ruggedness, and land quality. Robust standard errors clustered at the country level are in parentheses, followed by corresponding p-values in brackets. We report the p-value corresponding to tests of the conflict coefficient using Conley (1999) standard errors (to account for spatial autocorrelation). The Conley standard errors use a cutoff distance of approximately 1,500 kilometers, beyond which spatial correlation is assumed to be zero. We report the p-value from the Anderson and Rubin (1949) test of the conflict coefficient on conflict, which is robust to the presence of weak instruments. Finally, we report the Kleibergen and Paap (2006) Wald rk F statistic for the excluded instrument.

Table 4: Economic Legacy of Warfare: Inverse-Probability Weighting

Dependent variable:	Log GDP per capita, 2005		
	(1)	(2)	(3)
ATE of 2nd vs. 1st tercile	0.293	0.342	0.172
	(0.095)	(0.093)	(0.069)
	[0.002]	[0.000]	[0.012]
ATE of 3rd vs. 1st tercile	0.594	0.639	0.403
	(0.106)	(0.113)	(0.061)
	[0.000]	[0.000]	[0.000]
ATE of 3rd vs. 2nd tercile *p*-value	0.000	0.000	0.000
Observations	248	248	248

Notes. Estimates are obtained by augmented inverse probability weighting following Cattaneo (2010). Log GDP per capita is measured in purchasing power standard units (PPS). Standard errors are in parentheses, followed by corresponding *p*-values in brackets. The first row gives the average treatment effect (ATE) of being in the second tercile of conflict exposure compared to being in the first. The second row gives the ATE of being in the third tercile of conflict exposure compared to being in the first. We report the *p*-value corresponding to the test of the ATE of being in the third tercile of conflict compared to being in the second. The propensity scores are estimated by multinomial logit using different sets of covariates. Column 1 uses average distance to the nearest polity in the multinomial logit regressions, while column 2 adds the geographic controls and column 3 adds log population density in 1500. 13 observations were trimmed from the sample due to their small estimated propensity scores.

Table 5: Economic Legacy of Warfare: Region Fixed Effects (NUTS 1)

Dependent variable:	Log GDP per capita, 2005		
	(1)	(2)	(3)
Conflict exposure, 1500-1799	0.247	0.227	0.147
	(0.094)	(0.099)	(0.087)
	[0.010]	[0.024]	[0.094]
Avg. dist. to nearest polity	Yes	Yes	Yes
Geographic controls	No	Yes	Yes
Log population density, 1500	No	No	Yes
Wild cluster bootstrap p-value	0.000	0.000	0.000
Conley p-value	0.018	0.039	0.073
R^2	0.048	0.060	0.294
Number of clusters	90	90	90
Observations	261	261	261

Notes. Estimates are obtained by ordinary least squares, using NUTS 1 region fixed effects. Log GDP per capita is measured in purchasing power standard units (PPS). The geographic controls are primary rivers, landlockedness, Roman road hubs, elevation, ruggedness, and land quality. Robust standard errors clustered at the NUTS 1 region level are in parentheses, followed by corresponding p-values in brackets. We report the p-values corresponding to tests of the conflict coefficient using the wild cluster bootstrap (to account for the small number of clusters) and Conley (1999) standard errors (to account for spatial autocorrelation). The wild cluster bootstrap p-values are based on 10,000 replications. The Conley standard errors use a cutoff distance of approximately 1,500 kilometers, beyond which spatial correlation is assumed to be zero.

Table 6: Economic Legacy of Warfare: Exclude Urban Belt

Dependent variable:	Log GDP per capita, 2005		
	(1)	(2)	(3)
Conflict exposure, 1500–1799	0.177	0.185	0.164
	(0.048)	(0.055)	(0.053)
	[0.002]	[0.003]	[0.006]
Avg. dist. to nearest polity	Yes	Yes	Yes
Geographic controls	No	Yes	Yes
Log population density, 1500	No	No	Yes
Wild cluster bootstrap p-value	0.121	0.029	0.040
Conley p-value	0.001	0.000	0.000
R^2	0.126	0.134	0.262
Number of clusters	20	20	20
Observations	205	205	205

Notes. Sample excludes all regions that are located within urban belt according to Figure 4. Estimates are obtained by ordinary least squares, using country fixed effects. Log GDP per capita is measured in purchasing power standard units (PPS). The geographic controls are primary rivers, landlockedness, Roman road hubs, elevation, ruggedness, and land quality. Robust standard errors clustered at the country level are in parentheses, followed by corresponding p-values in brackets. We report the p-values corresponding to tests of the conflict coefficient using the wild cluster bootstrap (to account for the small number of clusters) and Conley (1999) standard errors (to account for spatial autocorrelation). The wild cluster bootstrap p-values are based on 10,000 replications. The Conley standard errors use a cutoff distance of approximately 1,500 kilometers, beyond which spatial correlation is assumed to be zero.

Table 7: Economic Legacy of Warfare: Exclude Sovereign Capitals

Dependent variable:	Log GDP per capita, 2005		
	(1)	(2)	(3)
Conflict exposure, 1500–1799	0.176	0.186	0.161
	(0.070)	(0.067)	(0.063)
	[0.021]	[0.013]	[0.020]
Avg. dist. to nearest polity	Yes	Yes	Yes
Geographic controls	No	Yes	Yes
Log population density, 1500	No	No	Yes
Wild cluster bootstrap p-value	0.009	0.001	0.002
Conley p-value	0.001	0.000	0.002
R^2	0.211	0.256	0.330
Number of clusters	19	19	19
Observations	234	234	234

Notes. Sample excludes all regions that contain current sovereign capitals. Estimates are obtained by ordinary least squares, using country fixed effects. Log GDP per capita is measured in purchasing power standard units (PPS). The geographic controls are primary rivers, landlockedness, Roman road hubs, elevation, ruggedness, and land quality. Robust standard errors clustered at the country level are in parentheses, followed by corresponding p-values in brackets. We report the p-values corresponding to tests of the conflict coefficient using the wild cluster bootstrap (to account for the small number of clusters) and Conley (1999) standard errors (to account for spatial autocorrelation). The wild cluster bootstrap p-values are based on 10,000 replications. The Conley standard errors use a cutoff distance of approximately 1,500 kilometers, beyond which spatial correlation is assumed to be zero.

Table 8: Economic Legacy of Warfare: Conflict Types

Dependent variable:	Log GDP per capita, 2005		
	(1)	(2)	(3)
Battle exposure, 1500-1799	0.202	0.206	0.182
	(0.052)	(0.069)	(0.062)
	[0.001]	[0.007]	[0.008]
Avg. dist. to nearest polity	Yes	Yes	Yes
Geographic controls	No	Yes	Yes
Log population density, 1500	No	No	Yes
Wild cluster bootstrap p-value	0.009	0.005	0.009
Conley p-value	0.000	0.000	0.000
R^2	0.142	0.150	0.364
Number of clusters	22	22	22
Observations	261	261	261
Dependent variable:	Log GDP per capita, 2005		
	(1)	(2)	(3)
Siege exposure, 1500-1799	0.178	0.166	0.136
	(0.065)	(0.070)	(0.061)
	[0.012]	[0.028]	[0.038]
Avg. dist. to nearest polity	Yes	Yes	Yes
Geographic controls	No	Yes	Yes
Log population density, 1500	No	No	Yes
Wild cluster bootstrap p-value	0.011	0.006	0.009
Conley p-value	0.001	0.002	0.007
R^2	0.119	0.129	0.342
Number of clusters	22	22	22
Observations	261	261	261

Notes. Estimates are obtained by ordinary least squares, including country fixed effects. Log GDP per capita is measured in purchasing power standard units (PPS). The geographic controls are primary rivers, landlockedness, Roman road hubs, elevation, ruggedness, and land quality. Robust standard errors clustered at the country level are in parentheses, followed by corresponding p-values in brackets. We report the p-values corresponding to tests of the conflict coefficient using the wild cluster bootstrap (to account for the small number of clusters) and Conley (1999) standard errors (to account for spatial autocorrelation). The wild cluster bootstrap p-values are based on 10,000 replications. The Conley standard errors use a cutoff distance of approximately 1,500 kilometers, beyond which spatial correlation is assumed to be zero.

Table 9: Economic Legacy of Warfare: Alternative Outcomes

Dep. variable:	Log population density, 2005			High-tech employment, 2005			Log R&D spending per capita, 2005			Economically active population, 2005		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Conflict	0.857	0.762	0.692	1.468	1.050	1.018	0.705	0.586	0.553	3.019	2.420	2.354
	(0.357)	(0.236)	(0.232)	(0.248)	(0.240)	(0.232)	(0.133)	(0.226)	(0.227)	(1.282)	(1.588)	(1.587)
	[0.028]	[0.005]	[0.008]	[0.000]	[0.000]	[0.000]	[0.000]	[0.019]	[0.026]	[0.031]	[0.146]	[0.156]
Dist. to polity	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Geog. controls	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Pop. dens. 1500	No	No	Yes	No	No	Yes	No	No	Yes	No	No	Yes
Bootstrap p-val.	0.001	0.001	0.001	0.001	0.017	0.016	0.015	0.015	0.018	0.151	0.767	0.813
Conley p-value	0.004	0.001	0.004	0.000	0.000	0.000	0.000	0.000	0.001	0.001	0.031	0.039
R^2	0.093	0.272	0.386	0.113	0.154	0.164	0.108	0.137	0.175	0.103	0.140	0.150
# clusters	18	18	18	18	18	18	18	18	18	18	18	18
Observations	171	171	171	171	171	171	171	171	171	171	171	171

Notes. Estimates are obtained by ordinary least squares, including country fixed effects. Log R&D spending is measured in purchasing power standard units (PPS). The geographic controls are primary rivers, landlockedness, Roman road hubs, elevation, ruggedness, and land quality. Robust standard errors clustered at the country level are in parentheses, followed by corresponding p-values in brackets. We report the p-values corresponding to tests of the conflict coefficient using the wild cluster bootstrap (to account for the small number of clusters) and Conley (1999) standard errors (to account for spatial autocorrelation). The wild cluster bootstrap p-values are based on 10,000 replications. The Conley standard errors use a cutoff distance of approximately 1,500 kilometers, beyond which spatial correlation is assumed to be zero.

Table 10: Economic Legacy of Warfare: Gross Cell Product

Dependent variable:	Log GCP per capita, 2005		
	(1)	(2)	(3)
Conflict exposure, 1500–1799	0.182	0.188	0.181
	(0.066)	(0.058)	(0.059)
	[0.010]	[0.003]	[0.005]
Avg. dist. to nearest polity	Yes	Yes	Yes
Geographic controls	No	Yes	Yes
Log population density, 1500	No	No	Yes
Wild cluster bootstrap p-value	0.007	0.003	0.004
Conley p-value	0.000	0.000	0.000
R^2	0.130	0.181	0.199
Number of clusters	27	27	27
Observations	930	930	930

Notes. Gross cell product data are from Nordhaus (2011). The unit of observation is a 1-degree longitude by 1-degree latitude grid cell (i.e., approximately 100 km $\times$ 100 km). Estimates are obtained by ordinary least squares, using country fixed effects. The geographic controls are primary rivers, landlockedness, Roman road hubs, elevation, ruggedness, and land quality. Robust standard errors clustered at the country level are in parentheses, followed by corresponding p-values in brackets. We report the p-values corresponding to tests of the conflict coefficient using the wild cluster bootstrap (to account for the small number of clusters) and Conley (1999) standard errors (to account for spatial autocorrelation). The wild cluster bootstrap p-values are based on 10,000 replications. The Conley standard errors use a cutoff distance of approximately 1,500 kilometers, beyond which spatial correlation is assumed to be zero.

Table 11: Economic Legacy of Warfare: Channels

Dep. variable:	Literacy rate, 1880			Executive constraints, 1850			Urbanization rate, 1850		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Conflict	14.951	13.801	13.959	0.199	0.174	0.152	-0.660	5.347	4.679
	(4.396)	(2.348)	(2.527)	(0.180)	(0.184)	(0.197)	(2.428)	(3.102)	(2.811)
	[0.011]	[0.001]	[0.001]	[0.304]	[0.376]	[0.464]	[0.793]	[0.128]	[0.140]
Dist. to polity	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Geog. controls	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Pop. dens. 1500	No	No	Yes	No	No	Yes	No	No	Yes
Bootstrap p-val.	0.008	0.037	0.018	0.192	0.702	0.731	0.305	0.323	0.431
Conley p-val.	0.000	0.000	0.000	0.226	0.333	0.417	0.747	0.003	0.009
R^2	0.427	0.585	0.588	0.023	0.118	0.139	0.002	0.197	0.239
# clusters	8	8	8	8	8	8	8	8	8
Observations	65	65	65	65	65	65	65	65	65

Notes. Estimates are obtained by ordinary least squares, including country fixed effects. The geographic controls are primary rivers, landlockedness, Roman road hubs, elevation, ruggedness, and land quality. Robust standard errors clustered at the country level are in parentheses, followed by corresponding p-values in brackets. We report the p-values corresponding to tests of the conflict coefficient using the wild cluster bootstrap (to account for the small number of clusters) and Conley (1999) standard errors (to account for spatial autocorrelation). The wild cluster bootstrap p-values are based on 10,000 replications. The Conley standard errors use a cutoff distance of approximately 1,500 kilometers, beyond which spatial correlation is assumed to be zero.

Figure 1: GDP per Capita by Region (NUTS 2), 2005

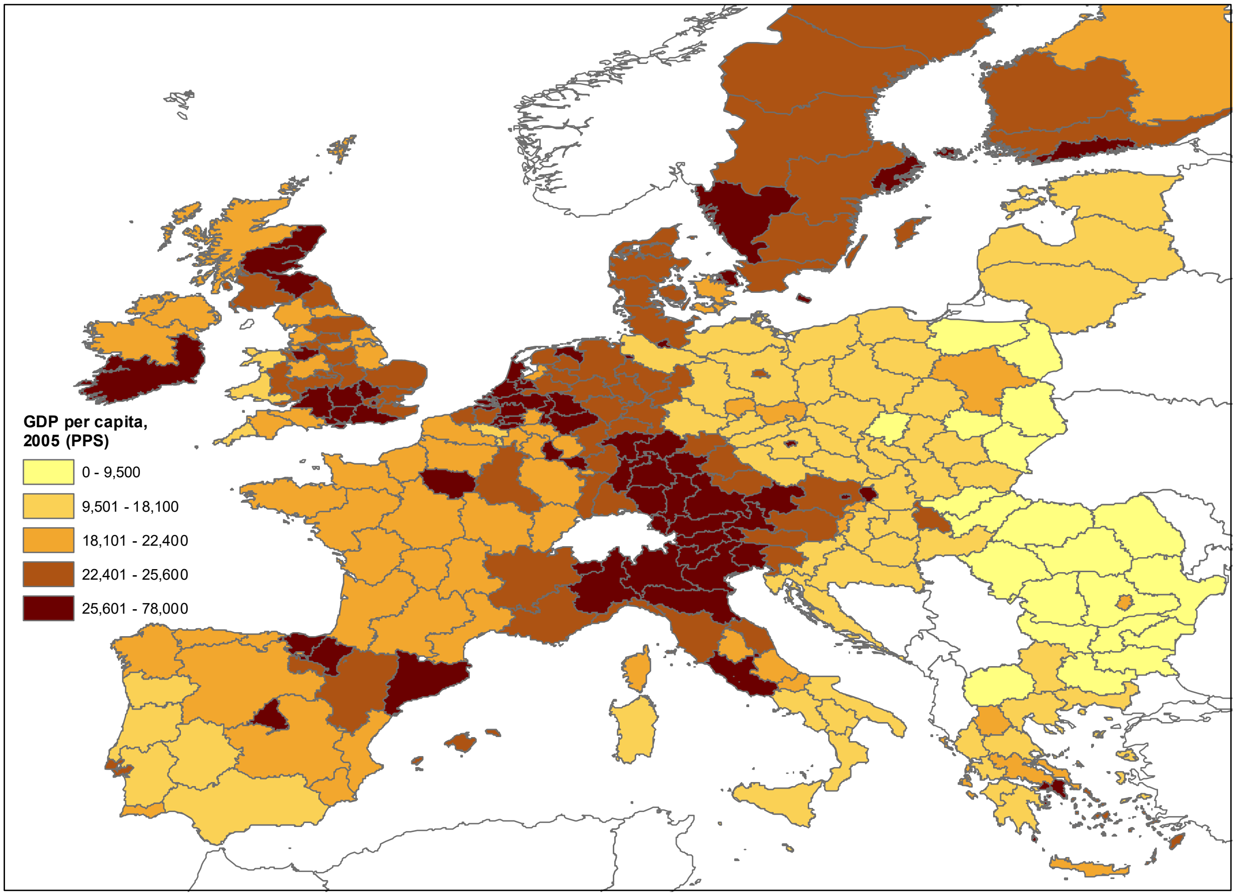


Figure 2: Historical Conflict Exposure by Region (NUTS 2), 1500-1799

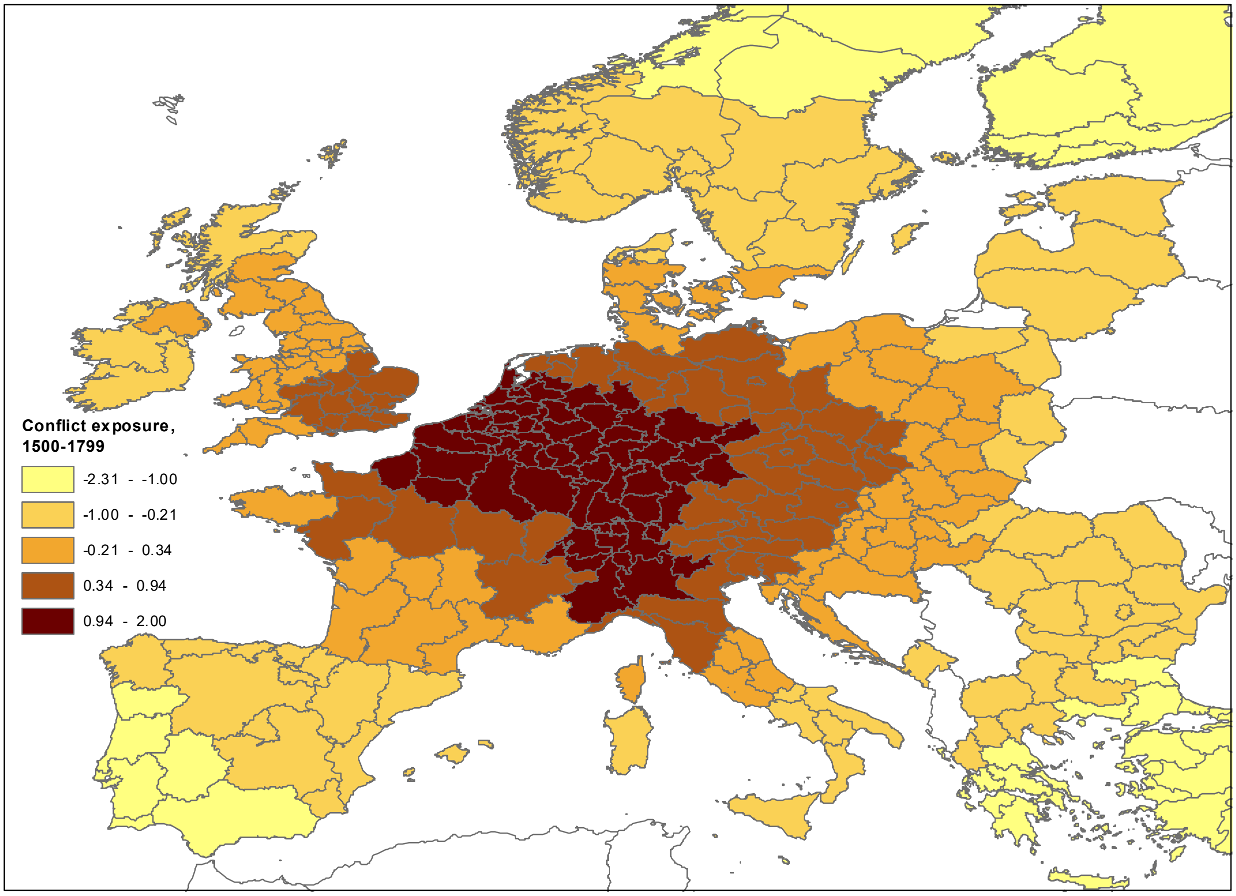
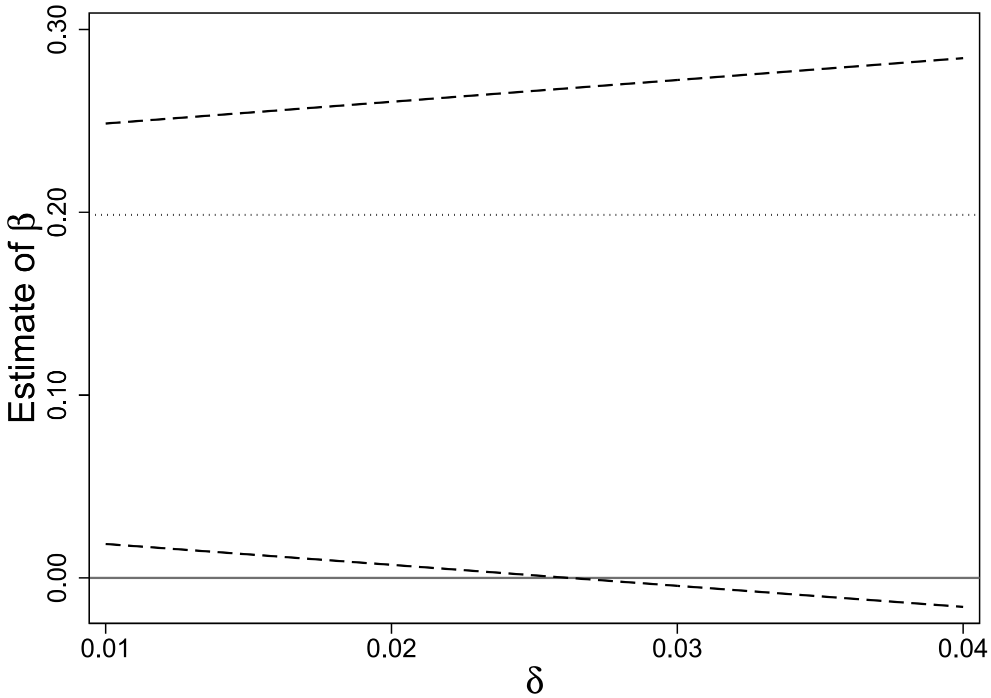


Figure 3: Conservative 95% Confidence Intervals for β under Violations of Exclusion Restriction



Notes. We plot conservative 95 percent confidence intervals for the effect on historical conflict exposure on regional per capita GDP, allowing the instrument (i.e., historical exposure to young rulers) to enter the second-stage equation directly with coefficient γ. For each δ, the dashed lines give the upper and lower bounds of the confidence interval that allows for $\gamma \in [-\delta, \delta]$. Confidence intervals are calculated according to the methods in Conley et al. (2012). The dotted line gives the benchmark IV estimate of β under exogeneity.

Figure 4: Exclude Urban Belt Regions (NUTS 2)

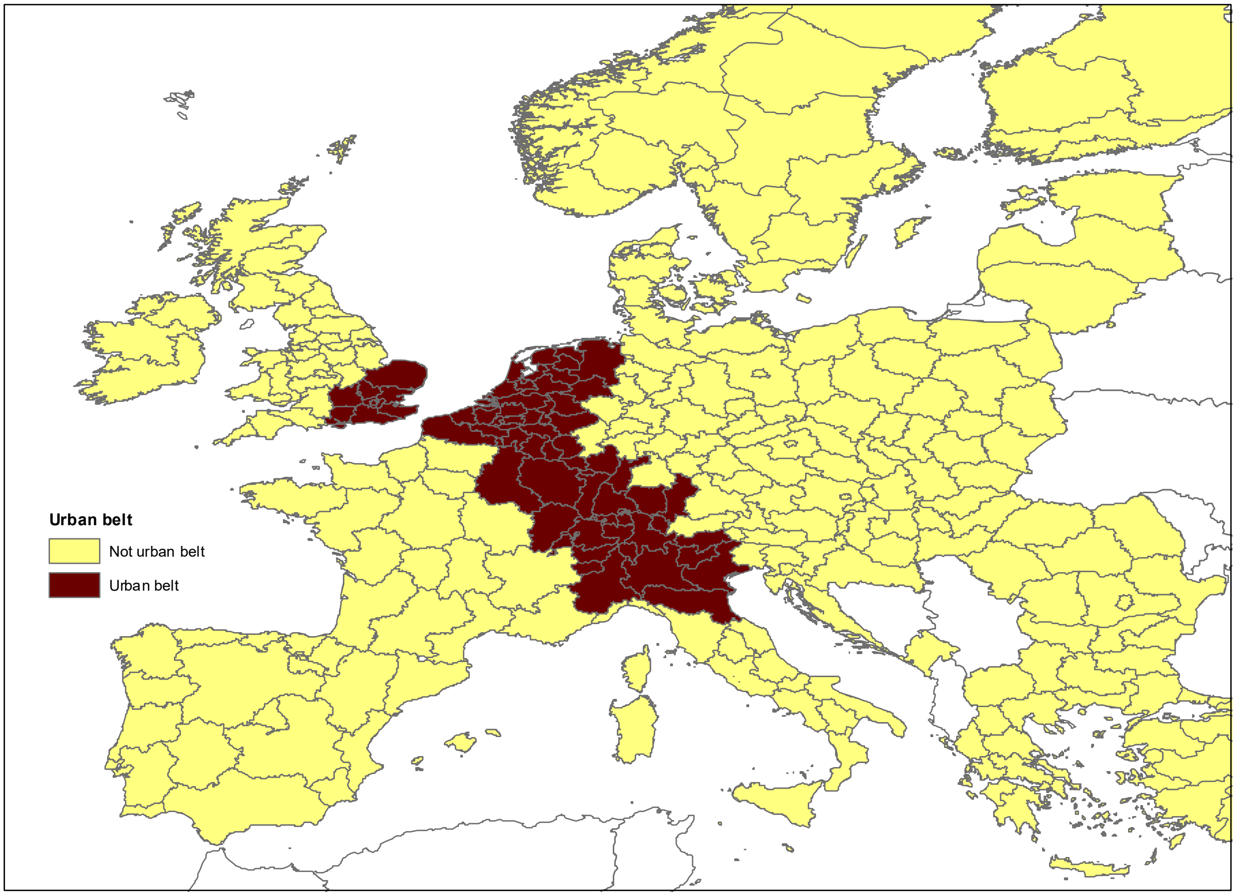
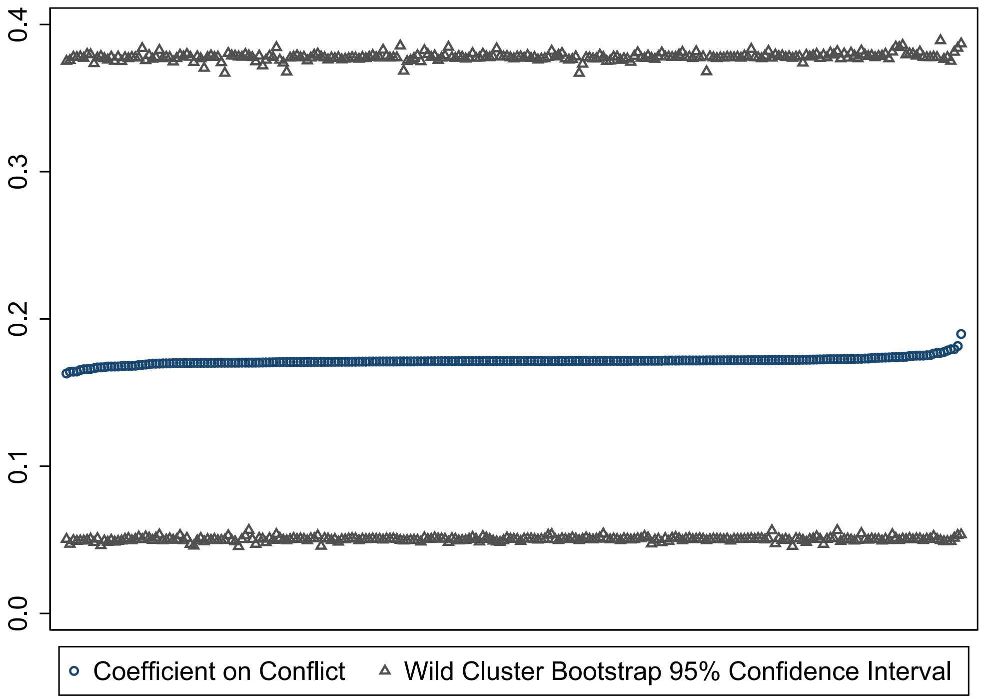


Figure 5: Robustness to Dropping Regions One by One



Notes. This figure plots the estimated coefficient on conflict exposure (ordered by magnitude) and the corresponding wild cluster bootstrap 95 percent confidence interval for each subsample formed by dropping one region. The full set of control variables is included in each regression.

Table A.1: Military Conflicts Comprising the Thirty Years' War

	Conflict Name	Year	Nearest Settlement	Country
1	Sablat	1619	Budweis	Czech Rep
2	White Hill	1620	Prague	Czech Rep
3	Fleurus	1622	Fleurus	Belgium
4	Hochst	1622	Frankfurt am Main	Germany
5	Wimpfen	1622	Bad Wimpfen	Germany
6	Stadtlohn	1623	Stadtlohn	Germany
7	Breda	1624	Breda	Netherlands
8	Bridge of Dessau	1625	Dessau	Germany
9	Lutter	1626	Lutter am Barenberge	Germany
10	Stralsund	1626	Stralsund	Germany
11	Wolgast	1628	Wolgast	Germany
12	Madgeburg	1630-1	Madgeburg	Germany
13	Breitenfeld	1631	Leipzig	Germany
14	Frankfurt (Oder)	1631	Frankfurt (Oder)	Germany
15	Werben	1631	Werben (Elbe)	Germany
16	Lützen	1632	Lützen	Germany
17	Nuremberg	1632	Nuremberg	Germany
18	River Lech	1632	Rain	Germany
19	Nordlingen	1634	Nordlingen	Germany
20	Tornavento	1636	Oleggio	Italy
21	Wittstock	1636	Wittstock	Germany
22	Breda	1637	Breda	Netherlands
23	Leucate	1637	Leucate	France
24	Breisach	1638	Breisach	Germany
25	Fuenterrabia	1638	Hondarribia	Spain
26	Rheinfelden	1638	Rheinfelden	Switzerland
27	Casale	1640	Casale Monferrato	Italy
28	2nd Breitenfeld	1642	Leipzig	Germany
29	Lérida	1642	Lérida	Spain
30	Rocroi	1643	Rocroi	France
31	Freiburg	1644	Freiburg im Breisgau	Germany
32	Allerheim	1645	Allerheim	Germany
33	Jankau	1645	Jankov	Czech Rep
34	Mergentheim	1645	Bad Mergentheim	Germany
35	Lérida	1647	Lérida	Spain
36	Lens	1648	Lens	France
37	Zusmarshausen	1648	Zusmarshausen	Germany

Source. Clodfelter (2002).

Table A.2: Summary Statistics

	Mean	Std. Dev.	Min.	Max.	Obs.
Panel A: Eurostat (2015)					
Log GDP per capita, 2005	9.91	0.42	8.54	11.26	261
Log population density, 2005	5.01	1.19	0.88	9.13	257
High-tech employment, 2005	4.09	1.78	0.89	10.49	212
Log R&D spending per capita, 2005	5.10	1.30	1.57	7.49	203
Economically active pop, 2005	59.32	5.50	44.10	78.80	254
Conflict exposure, 1500-1799	0.00	1.00	−2.88	2.07	261
Exposure to young rulers, 1500-1799	0.00	1.00	−2.57	2.29	261
Avg. distance to nearest polity	189.41	580.73	6.48	6849.00	261
Primary rivers	0.36	0.48	0.00	1.00	261
Landlocked	0.53	0.50	0.00	1.00	261
Roman road hub	0.41	0.49	0.00	1.00	261
Elevation	314.81	308.44	−2.64	2091.35	261
Terrain ruggedness	1.14	1.31	0.01	7.47	261
Land quality	0.61	0.25	0.00	0.99	261
Log population density, 1500	0.00	0.02	0.00	0.19	261
Panel B: Nordhaus (2011)					
Log GCP per capita, 2005	3.09	0.50	1.23	4.24	930
Conflict exposure, 1500-1799	0.00	1.00	−1.56	2.69	930
Exposure to young rulers, 1500-1799	0.00	1.00	−1.43	2.78	930
Avg. distance to nearest polity	177.12	134.27	5.42	560.72	930
Primary rivers	0.19	0.39	0.00	1.00	930
Landlocked	0.54	0.50	0.00	1.00	930
Roman road hub	0.22	0.41	0.00	1.00	930
Elevation	356.70	405.33	−1.27	2755.61	930
Terrain ruggedness	1.37	1.52	0.01	7.59	930
Land quality	0.53	0.33	0.00	1.00	930
Log population density, 1500	0.00	0.00	0.00	0.04	930
Panel C: Tabellini (2010)					
Literacy rate, 1880	54.84	25.94	14.60	96.50	65
Executive constraints, 1850	3.89	2.05	1.00	7.00	65
Urbanization rate, 1850	11.47	13.51	0.00	57.43	65
Conflict exposure, 1500-1799	0.00	1.00	−2.15	1.84	65
Avg. distance to nearest polity	169.24	192.50	20.51	1450.24	65
Primary rivers	0.52	0.50	0.00	1.00	65
Landlocked	0.31	0.47	0.00	1.00	65
Roman road hub	0.74	0.44	0.00	1.00	65
Elevation	350.12	261.99	0.34	947.57	65
Terrain ruggedness	1.26	1.02	0.02	3.84	65
Land quality	0.64	0.22	0.01	0.98	65
Log population density, 1500	0.01	0.02	0.00	0.19	65

Notes. See the text for variable descriptions and data sources.

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