Crisis, State Capacity, and the Rise of Autocracy

A Design-based Analysis of the Thirty Years' War*

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Abstract

Do wars enable autocracy? We examine how the Thirty Years' War (1618–48), the largest conflict in pre-modern Europe, gave rise to capable autocracies. We use planned troop movements from secret military communications to estimate the impact of town-level war exposure on the growth of fiscal and military capacity and the dismantling of parliaments. During the war, executive power increased to prevent plunder and coordinate military logistics. After the war, rulers used this capacity to consolidate autocratic rule via propaganda and repression. Pre-existing legal institutions acted as a barrier to war-induced autocracy. With parliaments eliminated, militarized absolutist regimes persisted for centuries and provided fewer public goods. Our findings highlight a dynamic trade-off in the concentration of executive power during crises.

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I Introduction

The impact of states on economic development depends not only on their capacity to implement policies, but also on the type of political regime that shapes policy objectives (Acemoglu and Robinson, 2019). While warfare is closely linked to increases in state capacity (Tilly, 1990; Besley and Persson, 2010; Gennaioli and Voth, 2015), which *type* of state emerges from war is less understood: Do wars enable autocracy?

Warfare demands rapid and centralized resource provisioning (Weber, 1978). Thus, concentrating executive power may help to organize the military effort. One perspective views such temporary dictatorship as an effective crisis response (Machiavelli, 1531; Rousseau, 1791). Another emphasizes how emergency powers can facilitate a permanent sidelining of executive constraints, paving the way for autocracy (Tocqueville, 1835; Djankov et al., 2003). From a development perspective, it is critical to understand the institutional trade-offs implied by war and to identify factors that prevent autocratization after conflict.

Documenting the regime type consequences of conflict is empirically challenging. It requires variation in conflict that is plausibly unrelated to other factors shaping state outcomes. Furthermore, characterizing state outcomes in a dynamic analysis requires detailed panel data on comparable units over long time horizons.

This paper documents the rise of capable autocracies in response to war.[1] We focus on the Thirty Years' War (1618–48), the largest conflict in pre-modern Europe and a turning point toward modern warfare based on large-scale resource mobilization (Parker, 1988). We find that an initial expansion of executive capacity to prevent wartime plundering led to persistent autocracy, unless counterbalanced by strong legal institutions. Our setting is uniquely well-suited to examine this dynamic. It offers two key sources of variation within a narrow geographic region: first, localized war exposure driven by troop movements, producing fine-grained variation in exposure; and second, large and idiosyncratic institutional variation across hundreds of sovereign states over centuries. This allows us to trace long-run development during a radical shift in state organization: the introduction of permanent taxes and standing armies, but also the widespread dismantling of parliaments.[2]

[1] We define *capable autocracies* as extractive states with a high capacity to implement policies.

[2] Brandenburg-Prussia exemplifies this transformation. Before the war, parliaments were central. However, the local institutions developed to supply troops laid the foundations for one of the most powerful autocracies in Europe. When the absolutist 'soldier king' Frederick William I took power in 1713, the army

We construct novel panel data on parliaments, ruler taxation, and military personnel for 2,230 towns in 478 states, annually for the period 1500–1789. We relate these outcomes to local exposure to the Thirty Years' War, whose large-scale troop movements posed a substantial logistical challenge for towns in the path of an army. We identify 863 towns with recorded troop presence during the war. Additionally, we gather detailed military information from archival maps, strategic briefs, and secret internal letter communications, identifying 548 realized and 307 counterfactual troop movements between strategic destinations of major military campaigns. To depict the dynamics of absolutist entrenchment, we measure local propaganda through prints and portraits, repression of town institutions, and the co-optation of local elites. We compile town-level data on pre-existing legal institutions and individual judges. Finally, we track long-run outcomes in militarization, health, and public goods provision in the nineteenth century.

Our main analysis estimates the impact of town-level troop exposure during the Thirty Years' War on executive capacity in a panel with town and year fixed effects. We test for parallel trends using event studies.[3] For troop-exposed towns, we document an increase of 67% in the propensity of its regional parliament being eliminated.[4] These towns also experienced an immediate and persistent increase in the local incidence of direct ruler taxation, at around 88% of the baseline propensity. Similarly, troop exposure accounts for a doubling of the baseline number of military personnel originating from a town. We provide a range of robustness checks to demonstrate that the effects are unlikely to be driven by omitted variables.

To address remaining endogeneity concerns, we develop a novel estimation strategy that uses detailed military information to identify ex-ante comparable towns that differed in troop exposure. Troop movements were influenced by strategic considerations: Military leaders first chose a broad campaign path. Along the path, they aimed to take fortified towns and lead their army on the fastest possible route between these strategic targets. Our

at his disposal, as well as the taxes required to finance it, were just one generation old (Clark, 2006).

[3]A balance exercise finds that predictors of troop exposure are slow-moving or time-invariant, in line with findings of conflict prediction in present-day settings (Bazzi et al., 2022). With major troop movements driven by foreign intervention of external powers, geographical considerations were key to campaign plans.

[4]Parliaments typically comprised multiple towns. Since both our treatment and all remaining variables are measured at the town level, we adopt a town-year as our primary unit of analysis. This choice entails minimal loss: the median parliamentary constituency had only seven towns. Our results remain robust under alternative aggregations (constituency-year), econometric approaches (Cox proportional hazard models at both town and constituency levels), and standard errors (allowing for arbitrary spatial correlation).

instrumental variable accounts for nonrandom deviations from campaign paths by measuring a town's distance to the nearest least-cost path connecting two fortified towns in a campaign. We then compute the average value of this instrument across many counterfactual campaign paths derived from classified military communications and recenter the original instrument to adjust for the strategic centrality of towns (Borusyak and Hull, 2023). Our findings are qualitatively unchanged when using this instrumental variable strategy.

Next, we examine how the conflict gave rise to capable autocracies. War bundles multiple channels that may have weakened executive constraints. We use detailed data and historical case studies to examine potential mechanisms. Our analysis is motivated by three questions.

First, how did executive capacity initially expand? We study two potential channels: the role of coordination in wartime governance; and the direct, violent weakening of representative institutions.[5] We first turn to coordination. During war, troops extracted local resources through plunder or taxation. Plunder increased the immediate consumption of troops but depleted future resources. Taxation as a 'stationary bandit' required coordination with the local ruler (Olson, 1993; Sánchez De La Sierra, 2020; Henn et al., 2024). We find evidence consistent with a coordination channel: our main results are driven by cases where armed forces and local rulers belonged to the same military alliance.[6] Additionally, our results are robust to directly omitting towns that were subject to destructive channels, including nearby battles or territorial annexations.

Second, how did rulers consolidate power? In treated towns, we find that rulers invested in ruler prints and portraits as symbolic capital (Bourdieu, 1977), and suppressed local institutions. Consistent with a shift in local elites' outside options, we find that a growing share of local nobles entered military service under the ruler.

Third, was the elimination of parliaments after the war inevitable? We investigate whether pre-existing legal institutions helped constrain executive overreach. In towns with a history of litigation against their ruler in the Imperial High Court (*Reichskammergericht*), we find that parliaments were not more likely to be eliminated following troop exposure. To address potential selection into litigation, we draw on an institutional rule that required

[5] A key scope condition is that the treatment posed a credible and significant threat of violence to exposed towns. Since armies almost entirely depended on local resources, upkeep was a central organizational requirement once troops had advanced on a town (Redlich, 1959).

[6] Furthermore, and consistent with this channel, we find that destructive outcomes of treatment (the loss of capital or labor) were less likely to occur with allied exposure.

litigants to consult with regional judges. Our strategy relies on the distance of a town to the closest judge before the war, using unexpected judge deaths as a plausibly exogenous source of variation in access to ex ante legal institutions.

These results suggest a dynamic trade-off between dictatorship and disorder during states of emergency (Hobbes, 1651; Tocqueville, 1835; Djankov et al., 2003). Crises increase the returns to centralized coordination, shifting capacity towards the executive. Such shifts can incentivize the executive to invest in institutions that permanently sideline executive constraints. Autocratic rule is stable when outside options available to elites, including resistance to overreach, are costly relative to the returns of supporting the executive state.

We conclude our analysis by considering the effects of the war in the long run. The gap in militarization between treated and untreated towns, which opened during the Thirty Years' War, persisted until the twentieth century. In a cross-section at the end of the nineteenth century, we find that treated towns had a higher share of military personnel, worse health outcomes (a higher share of war-disabled individuals, and lower life expectancy), and lower state-led public health provision, as indicated by the absence of state hospitals.

These findings make three contributions. First, we complement work on state capacity (Tilly, 1990; Besley and Persson, 2010; Gennaioli and Voth, 2015; Sánchez De La Sierra, 2020; Mayshar et al., 2022; Allen et al., 2023; Chambru et al., 2024) by highlighting a dynamic trade-off: capacity initially developed to prevent plunder and coordinate military logistics ultimately gave rise to autocracy.[7] This is central, as the implications of state capacity for development depend on the type of state that exercises this capacity. However, while war eliminated executive constraints, we also explore how legal institutions counterbalanced these tendencies. We thus empirically document mechanisms that pushed states off — and kept them on — the "narrow corridor" (Acemoglu and Robinson, 2019).[8]

[7] Crises, therefore, can serve to constitute and reallocate state power (Agamben, 2005). A long-standing literature in the social sciences has studied the implications of crisis dictatorship for regimes (Machiavelli, 1531; Bodin, 1576; Rossiter, 1948).

[8] By focusing on a divergence in internal state institutions, this paper complements Cantoni et al. (2024), who examine the external consolidation of states — survival, size, and compactness — in relation to fiscal capacity. In contrast, we find that the implications of the *type* of state were far-reaching, affecting political participation, militarization, and public goods provision in the long run. Furthermore, Becker et al. (2020) study how local feuds among nobles, precipitated by dynastic shocks such as the failure to produce an heir, led to increases in the size of city councils in the medieval Holy Roman Empire. Our analysis, by contrast, centers on the defining feature of participatory rule: the existence of parliaments (Carsten, 1959). Our results indicate that the predominant, lasting trend during early modernity was a shift toward autocracy, which also eroded the authority of city councils. Our dynamic analysis of mechanisms furthermore complements

Second, we address key empirical challenges in research on war (Bazzi et al., 2022). We combine information on counterfactual troop movements in secret military communications with recent econometric advances to adjust for endogeneity in troop exposure (Borusyak and Hull, 2023). Moreover, our rich data highlight a central but often overlooked mechanism of state-building during conflict: the development of logistical infrastructure for local resource mobilization, as opposed to the more visible dynamics of battles and territorial conquest.[9] We also examine the broader social consequences of war, where rulers employed militarized propaganda and coercion to consolidate power — leaving a lasting cultural legacy of conflict, an aspect that "tends to be subsumed" (Wilson, 2008, p.12) in studies of war.[10]

Finally, we investigate a critical juncture in state development. The Thirty Years' War is widely regarded as the "first state formation war" (Burkhardt, 2018) that gave rise to the modern state system. Yet despite its significance, empirical research on the long-term effects of the war remains limited.[11] We contribute to the few existing studies by providing the first comprehensive and detailed quantitative analysis of this pivotal conflict. Our findings suggest that the war not only contributed to state consolidation but also laid the groundwork for autocratic rule. In doing so, we shed light on the deep historical roots of German militarism, which had enduring implications for political stability in Europe.

a comparative literature studying the implications of initial conditions for the "bifurcation" of states after conflict (Dincecco and Wang, 2018; Kenkel and Paine, 2023; Cox et al., 2023).

[9] On this imbalance of attention, Tilly (1990, p. 81) observes that "the great seventeenth-century organizers of war involved themselves in supply as much as in battle."

[10] Notable exceptions include studies on collective memory (Tur-Prats and Valencia Caicedo, 2020), heroism (Cagé et al., 2023), and gender norms (Gupta et al., 2024) in the aftermath of war. More extensively studied are the effects of war on political mobilization following the American Revolution (Jha and Wilkinson, 2023; Ottinger and Rosenberger, 2023) and in the mass armies of the World Wars (Grosjean et al., 2023; Jha and Wilkinson, 2012; Ang and Chinoy, 2025) — a period when the institutional capacity to constrain centralized power post-conflict was much larger. In this broader context, the evolution of wealth inequality has also received substantial attention, with a focus on progressive taxation (Scheve and Stasavage, 2010), capital destruction (Heldring et al., 2022; Feigenbaum et al., 2022), or both (Piketty, 2014; Scheidel, 2018).

[11] Gierok (2023) and Schaff (2024) examine 17 towns and the town of Nördlingen, respectively, showing a large decline in civic wealth and a within-town increase in inequality following the Thirty Years' War. Our results, which document a rise of direct ruler taxation and a reallocation of power from towns to territories, are consistent with these findings. Heinz et al. (2023) study the impact of the war on crime in a nineteenth-century cross-section.

II Historical Background

II.I State Organization: Princes and Parliaments

Our study is concerned with the institutional development of states in Central Europe before and after the Thirty Years' War (1618–1648). Political organization within states rested on two pillars: the ruler and the so-called 'Estates,' representing local elites such as towns and the landed nobility.[12] The central distinction among states was between absolutist regimes, where power was concentrated in the ruler, and parliamentary systems, where representative institutions exercised significant power (Ertman, 1997).

Parliamentary Rule. Before the war, many states exhibited elements of parliamentarism, primarily through regional assemblies where Estates met with rulers to negotiate taxation and voice grievances. While less comprehensive than modern legislatures, "the assemblies of the Estates of many German principalities were indeed 'Parliaments' in the proper sense of the term, and their functions in the sixteenth, seventeenth, and eighteenth centuries were very similar to those of the English Parliament" (Carsten, 1959, p. 444).[13]

Initially formed around irregular taxation requests, parliaments took on a permanent and central role in the governance of states. Estates controlled a wide array of governing functions: they administered taxes, recruited and salaried troops, appointed officials, and presided over law and policing (Clark, 2019).[14]

A key institution supporting parliamentary rights was the Imperial High Court: it allowed subjects to pursue so-called 'subject lawsuits' against their ruler in case of overreach (Diestelkamp, 1985). Although it lacked a mechanism to directly implement its decisions, the court effectively leveraged its legitimacy and prominence for enforcement.[15]

Absolutist Rule. In contrast to parliamentarism, absolutist governance was

[12] Our analysis focuses on territorial states (prince-bishoprics and secular territories). We subsume under the term 'ruler' all specific titles of the leaders of these states (prince-bishop, prince-elector, ...).

[13] These bodies tended to encompass representatives from a broad cross-section of society next to the nobility — burghers, clerics, and sometimes peasants (Blickle, 1997; Carsten, 1959). In Bavaria, the Estates represented 5,534 noble lineages, 90 market towns, and 34 towns (Lanzinner, 1980, p. 18). Where these parliaments endured, "they preserved the spirit of constitutional government and liberty in the age of absolute monarchy" (Carsten, 1959, p. 444), became precursors to modern German state parliaments (Grube, 1957), and shaped a participatory political culture (Habermas, 1982).

[14] Appendix Figure A.1 shows a stylized illustration of parliamentary rule. A similar regularization of fiscal constitutionalism in England has been studied, for example, by Pasquet (1925) and Angelucci et al. (2022).

[15] On the efficacy of this reputation-based implementation, Schelhaß (1795) notes that "cabinets of very important imperial princes did not dare to ignore the orders of the imperial courts."

characterized by three key features: the absence of parliamentary constraints on the ruler (*'legibus absolutus'*), a fiscal apparatus directly controlled by the sovereign rather than the Estates, and the maintenance of a standing army (Anderson, 1979). Absolutist rulers financed their regimes primarily through direct taxes called *Schatzung* or *Kontribution*, levied without parliamentary approval (Ullmann, 2005).[16] Military strength, in turn, relied on a broad base of locally embedded officers, who played a central role in recruiting and commanding the standing forces (Beloff, 1954). Taken together, these features concentrated political authority in the hands of the ruler. Over the course of the seventeenth century, many states shifted towards absolutism, though this process varied widely across regions.[17]

II.II The Thirty Years' War

The Thirty Years' War broke out amidst rising religious tensions in Central Europe. In 1618, a local revolt turned into civil war between Protestants and Catholics, spreading across the Holy Roman Empire. The conflict saw the intervention of all major continental powers, and the contest for European hegemony increasingly eclipsed religious motives. Only in 1648, after decades of peace negotiation, did the violence subside (Wilson, 2009; Münkler, 2019).

Two stylized facts defined military operations during the war. First, *strategically*, the war was characterized by foreign intervention and long-range troop movements of quasi-independent military enterprisers. Second, *logistically*, the large-scale mobilization of resources necessary to sustain military forces placed unprecedented burdens on localities — at the height of the conflict, 300,000 soldiers were under arms. We illustrate these features in a case study of the Swedish campaigns of 1630–31, which showcases our underlying data.

Strategy. Strategy concerned the choice of a campaign plan. Figure I shows the campaign paths of the main Swedish army and town-level troop exposure in 1630 and 1631 on a map of parliamentary constituencies in the Holy Roman Empire.[18] Upon landing on the Baltic coast, the Swedish army under King Gustavus Adolphus (1594–1632) initiated

[16] These levies were "direct taxes, derived from the assessment of the taxpayers' income and wealth conducted by designated officials. [...] In most states, *Schatzung* and *Kontribution* were synonymous umbrella terms." Schomburg (1992, p. 331f.) The tax was collected as a fixed sum, specified separately for each town by the central ruler administration, and was not earmarked.

[17] Reflecting on the ascent of absolutism in Prussia, Frederick II noted that before the Thirty Years' War, the Estates had still been "masters of the government." In contrast, when visiting Württemberg as late as 1800, the English statesman James Fox praised the "constitutionalism" he encountered (Grube, 1957, p. 2)

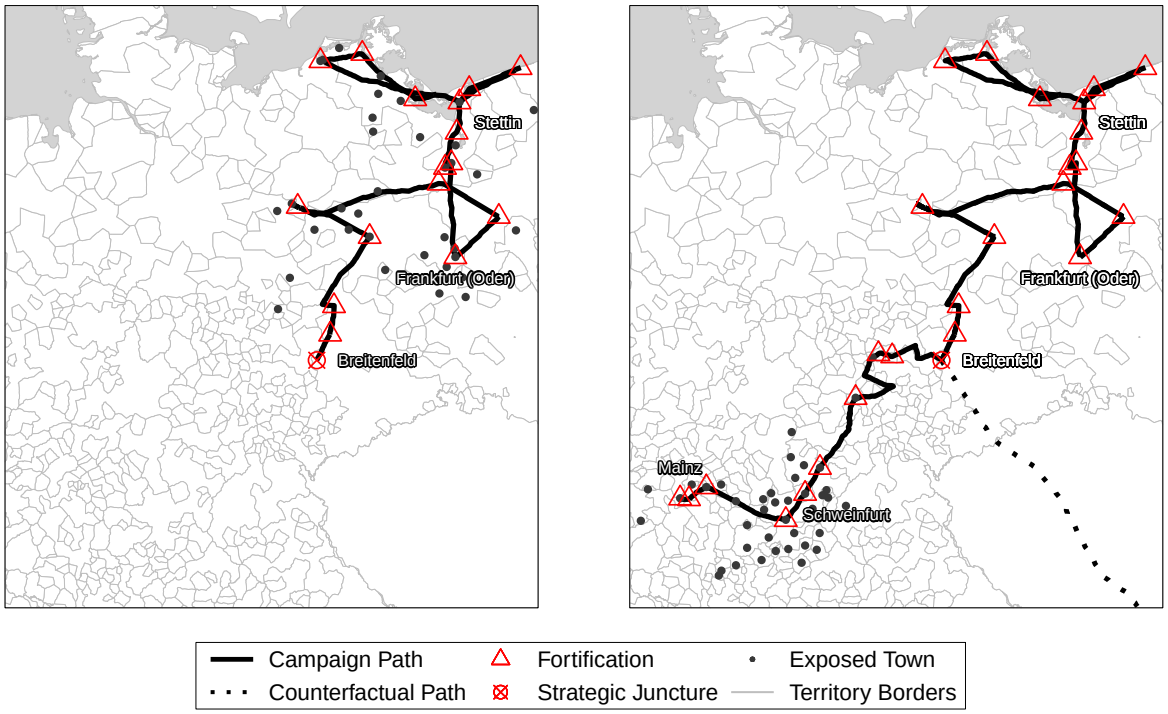
[18] We describe the data collection effort underlying the construction of these maps in Section III.II.

operations by securing fortified towns along the shoreline near Stettin (Panel A). By the autumn of 1630, Swedish forces advanced inland, targeting the key fortress of Frankfurt (Oder). Seeking a confrontation with Catholic forces, Gustavus moved westward through allied Brandenburg and Saxony with an army of 40,000 men. He won a decisive battle near Breitenfeld in September 1631.

After the victory of Breitenfeld, the Holy Roman Empire lay open. The king and his war council were divided between two possible continuations of the campaign path: a march on Vienna to directly threaten the Catholic Emperor; or march south, to weaken the Catholic prince bishops. In a narrow decision, the war council chose the latter option, and the Swedish army swept into southern Germany, taking Schweinfurt in October of 1631 and wintering in Mainz (Panel B) (Lundblad, 1826, p. 41).[19]

Figure I: The Thirty Years' War: Swedish Invasion (1630–32)

A: July 1630 – September 1631 B: September – December 1631



Note These maps show main army troop movements, fortified towns, towns exposed to troops, and a strategic juncture of the two first campaigning years (1630–31) during the Swedish invasion in the Thirty Years' War. The troop leader is Gustavus Adolphus, King of Sweden. The dotted line in Panel B indicates the counterfactual campaign path to Vienna after the Battle of Breitenfeld. The base map shows parliamentary constituency boundaries. Details on the data underlying the maps are given in Section III.II.

[19]The decision was narrow during the council, and also contested thereafter — Oxenstierna, reflecting on this moment before the senate in Stockholm twenty years later, reiterated his opinion that a march on Vienna would have been preferable (Dodge, 1895). Clausewitz (1837) echoed this opinion 200 years later.

Campaign paths, thus, were not predetermined: military leaders evaluated multiple options before deciding on the direction of movements. A substantial body of preserved primary sources stand testament to these inner workings of the war: in written instructions issued to troop leaders, "precise military orders were given regarding the march route and the quarters" (Kaiser, 1999, p. 47), frequently providing a set of options to choose from; secret letter communications and council minutes record instances in which leaders evaluated different possible routes. The availability of these source materials is unique to our setting.[20] Given the broad outline of a campaign path, the primary strategic objective of troop leaders revolved around the capture of key fortresses.[21] To move quickly between fortifications, armies aimed to take the shortest path, often relying on "handwritten lists of settlements in sequential order along known routes between major destinations" (Wilson et al., 2023, p. 1057).

Logistics. Military logistics demanded that the army could sustain itself on its route. Given the unprecedented size of troops, and the sparsity of infrastructure, such provisions had to be extracted locally in both enemy and allied territory. Military leaders aimed to avoid uncontrolled plundering to preserve troop discipline and ensure steady supplies.[22] Hence, a decentralized system of troop logistics developed, in which localities were required to pay, feed, and house soldiers, as well as to enlist recruits. For towns, the arrival of troops posed a large organizational challenge.[23] In the following, we set out to measure this troop exposure

[20]For example, in a biography of Gustavus Adolphus, Dodge (1895, p. 335) writes: "Gustavus did not, like Caesar, write commentaries at the close of his campaign, in which he could state motives which accorded with the event; he wrote as and when he thought, in the midst of the utter uncertainty of events, and he voiced his every idea. The apparent indecision was a mere habit of thinking aloud. (...) What we know of Gustavus is largely drawn from his own letters written at the moment."

[21]Military strength and control of territory lay in fortified towns, and the Thirty Years' War has been described as "the first pure fortifications war" (Menne, 1939, p. 47). For example, in a note dictated to his secretary in 1631, the Swedish king listed as a first priority for future action the "occupation of fortified places" (Wilson, 2010, p. 132).

[22]In the Swedish army, "plundering by individuals was punished by death" (Dodge, 1895, p. 78).

[23]For Tilly's occupation of the town of Coesfeld in 1623, a document of troop requirements survives. A captain was to receive four measures of wine, 20 measures of beer, 20 pounds of bread, 12 pounds of meat, two hens, half a sheep or calf per day. A uniformed lieutenant, cadet, and quartermaster were entitled to eight measures of beer, eight pounds of bread, four pounds of meat, and a quarter of a sheep or calf. A servant "in the draft" received two pounds of meat, three pounds of bread and three measures of beer per day. Horses received hay and straw, as well as a bushel of oats every three days. In addition to nutrition, the town had to supply firewood, candles, and salt to the soldiers free of charge. We include an example page of the document in Appendix Figure A.2. Adding to the organizational burden, soldiers often traveled in large cohorts, taking with them families, servants, and livestock. A document recording the quartering needs of two Catholic companies in 1648 lists 81 soldiers on horsebacks, 84 foot soldiers, 105 horses, 57 women, 48 children, 27 servants, 51 footboys, 3 maids, and 11 cows (Kraus, 2021, p. 215). Wallenstein's occupation of

and identify its impact on the organization of states.

III Data

To analyze the impact of the Thirty Years' War on state organization, we construct a dataset that integrates novel information on core state functions, detailed historical records of troop exposure, and the consolidation of autocratic governance.

III.I State Organization

Our main outcomes consist of three new measures capturing absolutist rule across 2,230 towns and their surrounding countryside within the Holy Roman Empire.[24]

To measure the erosion of parliamentarism, we map each town and its countryside to a regional parliamentary constituency. We identify 128 such constituencies through a systematic survey of regional historical works. For each town, we construct an indicator of whether its parliament was intact in a given year.[25]

To measure local fiscal capacity under absolutist rule, we draw on a comprehensive catalog of 3,885 town-level account books compiled in the *Index Librorum Civitatum* (Ranft et al., 2023). This source provides metadata on each book, including a classification and the specific years it covers.[26] We focus on the 735 account books that reference ruler-imposed taxes by name. Based on these, we construct an indicator of whether rulers levied a direct tax in a given town and year.

Local military capacity is measured using granular biographical data from the *Deutsche Biographie* (Hockerts and Lanzinner, 2022). This source contains records of 818,044 notable individuals from German-speaking regions. For our purposes, we identify 6,122 military personnel, extract information on their birthplaces, and link each to the nearest town. We

Pomerania in 1627 required the provision of food and fodder for 22,000 men.

[24]We include all towns as depicted in the *Deutsches Städtebuch* (Keyser et al., 1939-2003). This source covers all settlements in the 1937 German borders that ever obtained town status. We exclude East Prussia because it is an exclave; this choice does not affect our empirical results.

[25]In Section IV, we conduct various robustness tests to account for the fact that a typical constituency comprised multiple towns. We follow the historical literature and do not count deputation diets or other limited committee meetings as a full parliamentary meeting.

[26]The Index is an ongoing project, and some German regions have yet to be incorporated. We exclude these regions from regression analyses that rely on the Index.

classify these individuals as 'active' from the age of 20 until their death, enabling us to construct a panel of active military personnel by town and year.

We gather information on geographic characteristics of towns by measuring agricultural suitability (Fischer et al., 2021), terrain ruggedness, and distances to the coast as well as the nearest navigable river. We construct an indicator of whether a town was on a trade route. We also record the number of markets (Cantoni et al., 2020), the predominant religious denomination (Cantoni and Weigand, 2024), and the yearly mapping of towns to their rulers from 1500 to 1789 (Cantoni et al., 2019).

III.II Troop Exposure

We introduce data quantifying troop exposure across towns during the Thirty Years' War. The baseline data are collected from Keyser et al. (1939-2003) and local historical sources. We document the identity of the troops, and, where possible, the precise year of the event. Appendix Figure A.3 highlights all 863 troop-exposed towns in our data.

We then collect original data on campaign paths, strategic destinations, and counterfactual campaign paths in the most comprehensive data set of the Thirty Years' War to date. First, to construct realized campaign paths, we focus on the 18 troop leaders of the six main warring parties active in the Holy Roman Empire. We trace the movements of each leader in every year through a variety of person-specific secondary and primary sources, identifying 548 movements between locations in 55 campaigns. Appendix Table A.1 presents an overview of the warring parties, their respective troop leaders, the years of their military campaigns, and the sources consulted.[27]

Second, to identify strategic destinations, we rely on a systematic list of fortifications in the Holy Roman Empire (Klöffler, 2024). We limit this data to the 330 towns that had a permanent fortification at the outset of the Thirty Years' War.

Third, we construct counterfactual campaign paths based on strategic briefs and internal letter communications. Our main source is the collection of letters of Axel Oxenstierna, Lord High Chancellor of Sweden between 1612 and 1654, who played a pivotal role as coordinator of the battlefield during the Thirty Years' War. A large part of his correspondence,

[27] Our inclusion criterion is to consider only the highest-ranking leader of each of the six main warring factions at a given time. Furthermore, we limit our data collection to campaigns conducted within the geographical boundaries of our analysis region.

numbering 4,419 letters, has been digitized and transcribed in Riksarkivet (2024). We use GPT to translate the letters into modern English and to select those relating to campaign strategy. Based on the instructions and deliberations in these letters, we reconstruct counterfactual troop movements. Complementing Oxenstierna's letters, we consult military biographies for strategic instructions from other correspondences, which in part derive from a comparable collection of letters of the main opponent of the Swedish army: Maximilian of Bavaria and his military commanders (Bierther et al., 1907–2021). In sum, we identify 307 counterfactual troop movements. Figure I, Panel B shows the counterfactual path debated by Gustavus Adolphus at the strategic juncture of Breitenfeld.

Additionally, we collect information on war alliances: for each troop leader and each town, we code whether they were part of the Imperial-Catholic faction or the Protestant Union in a given year. Combining this information with our data on troop exposure, we construct an indicator of whether a town was exposed to allied or enemy troops.

Finally, we collect data on the accompanying effects of the war, distinguishing between variables directly tied to local troop presence and those reflecting the indirect influence of the war. First, for each exposure event, we document whether there was physical destruction, such as the burning of a city. Second, we proxy the human toll of the war. In the absence of consistent population data for our sample, we instead draw on collections of birth and baptism records from Verein für Computergenealogie (2025).[28] We precisely geolocate all three million records with non-missing birth and death years. Based on these, we identify 13,893 individuals born in 479 towns in our dataset between 1590 and 1618. For each town, we compute the mean age at death for individuals born in this pre-war cohort, and then we categorize towns into those with above- or below-median life expectancy. To capture the indirect influence of the war, we geolocate 89 landmark battles of the Thirty Years' War (Bodart, 1908), and record which towns were annexed as a consequence of the conflict.

III.III Mechanism Data

We gather data on the consolidation of absolutist rule. Two variables proxy for investments in symbolic capital through cultural representations of the ruler. First, we gather new data on printed portraits in German-speaking Europe between 1500 and 1800, including over 280,000

[28]This data was collected by a large set of genealogical associations, digitizing Church archives.

portraits in 2,000 places.[29] The source metadata allows us to identify rulers and their state officials. Second, we construct new data on printed text. We gather evidence on all known prints in German-speaking Europe between 1500 and 1800: over 863,000 publications printed in 1,000 places.[30] Using a dictionary-based approach from Just (2012), we classify print titles that mention rulers in militaristic contexts.

From Keyser et al. (1939-2003), we record the autocratic repression of local institutions, such as the appointment of a town mayor through the ruler. We construct a variable that counts the number of such instances of infringement in a given town and year.

To measure the co-optation of local elites into the state, we calculate the fraction of the nobility that is in the military in a given town and year.[31]

Furthermore, we collect town-level information on pre-existing legal institutions by drawing on data from the Imperial High Court. We observe all surviving 40,797 high court cases (Schildt and Amend-Traut, 2023), restrict our attention to the cases in which subjects pursued litigation against their ruler in the decades prior to the Thirty Years' War (between 1600 and 1618), and match litigating parties to the towns in our data.[32] To measure barriers to litigation during the same period, we compile data on all high court judges from Görtz (2024). This source provides information on the birth year, birthplace, and death year of each judge, along with the age at which they began serving on the court for a subset of cases. We identify all judges who were alive and active at any point between 1600 and 1618, and calculate the distance from each town in our dataset to the nearest such judge.

III.IV Long-Run Data

We assess the long-run effects of the war on militarization in the nineteenth and twentieth centuries. We identify the share of military personnel among the working-age population

[29] Data sourced from the "Digitaler Portraitindex druckgraphischer Bildnisse der Frühen Neuzeit." The index systematically catalogs printed portraits of rulers and other notable persons. Our data of portraits mainly encompasses print engravings (*Kupferstiche*), which were widely disseminated. The data is available at https://www.portraitindex.de.

[30] Data sourced from the 'Verzeichnis der im deutschen Sprachraum erschienenen Drucke des 16. Jahrhunderts' (VD 16), 'Verzeichnis der im deutschen Sprachraum erschienenen Drucke des 17. Jahrhunderts' (VD 17), and 'Verzeichnis der im deutschen Sprachraum erschienenen Drucke des 18. Jahrhunderts' (VD 18).

[31] In addition to the classification of a person as being in the military, the *Deutsche Biographie* also indicates nobility status for 5,036 individuals during our study period.

[32] These cases were archived centrally at the location of the Imperial High Court, so they do not exhibit place-specific selection.

from the 1895 occupation census of the German Empire (Statistisches Reichsamt, 1897). This data covers 2,145 towns, nearly the entire sample.[33] We additionally collect three measures on the human toll of militarization. We calculate the proportion of war-disabled individuals based on the occupation census. Using the birth records from Verein für Computergenealogie (2025), we pool all births between 1800 and 1914 and again divide towns into an above- and below-median life expectancy category; given the increased coverage at this later time period, this covers 703,288 individuals in 1,905 towns. Finally, we measure public health provision directly by identifying all state-run hospitals (*Landes-*, *Bezirks-*, or *Kreis-Krankenhaus*) in the 685 towns covered in a hospital survey of the late nineteenth century (Guttstadt, 1900).

IV Main Results: War and the Rise of Autocracy

IV.I Empirical Setting

We first assess the impact of the war on autocracy. Our outcomes of interest are the presence of parliaments, direct ruler taxation, and militarization. Our unit of analysis is a town.[34]

A key concern in regressing state outcomes on local troop exposure are unobserved variables that might influence both the treatment and outcomes, hence confounding the causal interpretation of the estimates. We address this concern by approaching the treatment assignment process through a design-based perspective (Athey and Imbens, 2022).

The assignment of troops to towns during the Thirty Years' War was driven by the strategic considerations of military campaigns.[35] Strategy concerned the choice of a campaign plan: Troop leaders decided on a broad campaign path. Along the path, they aimed to take fortified towns and lead their army on the fastest possible route between these strategic targets. Concerns about identification hence stem from strategic considerations, which could potentially have correlated with state organization outcomes. For example, a fortified town might have also had a better taxation infrastructure. Appendix Table A.2 allows

[33] We would like to thank Felix Kersting for kindly sharing this data.

[34] Troop exposure was local, and most variables are measured at the town level. For parliamentary constituencies, this choice entails minimal loss: the median constituency comprised seven towns. We reserve Appendix Section B.I for additional robustness checks on this aspect. Appendix Figure A.4 illustrates our data structure.

[35] We summarize the historical evidence to this end in Section II. Note that since we observe a common war shock — the entire Holy Roman Empire was enmeshed in the conflict — the treatment assignment concerns the intensive margin of war, that is, the course of individual campaign paths.

for a first assessment of these concerns. We predict troop exposure based on a number of town characteristics potentially tied to strategic considerations using a Probit model. All significant predictors are time-invariant or slow-moving.[36]

IV.II Baseline Empirical Strategy

We take a number of steps to empirically corroborate a causal link between the war and shifts in state organization. In our baseline regression specification, we estimate

$$CapableAutocracy_{it} = \beta Treated_i \times Post1618_t + \alpha_i + \alpha_t + \varepsilon_{it}. \quad (1)$$

$CapableAutocracy_{it}$ represents either an indicator of the elimination of regional parliaments for town i and year t, an indicator of the presence of direct ruler taxes, or the inverse hyperbolic sine of active military personnel. $Treated_i$ indicates whether a town was exposed to troops during the Thirty Years' War, while $Post1618_t$ is a dummy for the time period after 1618.[37] α_i and α_t are town and year-fixed effects, respectively. Importantly, the fixed effects absorb invariant characteristics, like the location of a town along a trade route, that might have influenced troop decisions.

Table I shows results of estimating equation (3). Column 1 indicates that troop exposure is associated with a significant increase in the propensity of the elimination of a regional parliament by 10.68 percentage points. This is sizeable given that overall, 16 percent of towns in our sample experience the elimination of parliaments. Similarly, troop exposure accounts for a statistically significant, substantial increase in the incidence of direct ruler

[36]Such constant factors have been identified as key predictors of conflict also in present-day settings (Bazzi et al., 2022). In an exercise predicting conflict incidents in Colombia and Indonesia, "the most predictive risk factors tend to be slow moving or time invariant. [...] Surprisingly, predictive accuracy improves little when we add time-varying factors." In our context, three influential factors emerge: First, the choice of a broad campaign path reflects the importance of foreign intervention during the war: towns in locations closer to the fringes of the Holy Roman Empire on the coast or in the south were more likely to be exposed to troops. Second, fortified towns were more likely to be treated, reflecting their strategic importance. Third, static town characteristics played a role: towns around which resources could be mobilized more easily, since they were located on a trade route, belonged to a territory with a 16th-century fiscal Chamber (Cantoni et al., 2024), or were in a less rugged area, had a significantly larger propensity of troop exposure. In contrast, local economic trends, captured by the number of markets or private and public buildings added between 1600 and 1618, are not significantly correlated with troop presence.

[37]We opt to anchor all exposure events in the year 1618. In Appendix Section B.I, we show that our results are qualitatively unchanged if we restrict our data to all towns with known timing of (non-)exposure, and consider a staggered treatment instead.

taxation at 88% of the baseline propensity. Finally, column 3 shows a doubling of the baseline inverse hyperbolic sine of military personnel originating from a town in association with the war.

Table I: Troop Exposure and Capable Autocracy

	Parliament Eliminated (1)	Ruler Taxes (2)	Military Personnel (3)
Troop Exposure	0.1068***	0.0614***	0.0277***
	(0.0147)	(0.0128)	(0.0071)
R^2	0.56	0.57	0.36
Observations	646,700	422,820	646,700
Number of Towns	2,230	1,458	2,230
Outcome Mean	0.16	0.07	0.03
Outcome Def.	(0/1)	(0/1)	(ihs)
Town FEs	✓	✓	✓
Year FEs	✓	✓	✓
Cluster	Town	Town	Town

Note This table presents results of estimating equation (3). Observations are at the town-year level, with the number of towns indicated in the table. The sample comprises 290 years. The dependent variables are (1) a binary variable of whether the parliament that represented town i has been eliminated in year t, (2) a binary variable of whether town i has a record of direct ruler taxes in year t, and (3) the inverse hyperbolic sine of military personnel born in town i active in year t. Standard errors are clustered at the town level. *, **, and *** denote significance on the 10 percent, 5 percent, and 1 percent level, respectively.

Next, we examine whether towns exposed to troops experienced differential time-varying shocks that influenced state organization prior to being treated. We estimate event study analogs of equation (3):

$$CapableAutocracy_{it} = \sum_{\tau=-7}^{8} \beta_\tau Treated_i \times RelativeDecade_{\tau(t)} + \alpha_i + \alpha_t + \varepsilon_{it} \quad (2)$$

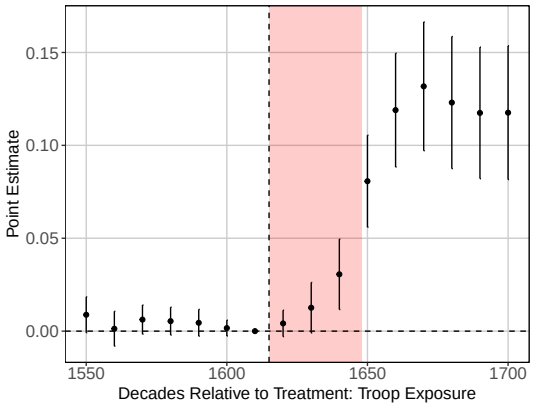
with all variables as defined above, and $RelativeDecade_t$ denoting decades until/since 1618. Results are shown in Figure II. The increases in all three outcomes are persistent and not led by pre-trends (Panels A-C). The effect of the war on parliaments (Panel A) takes hold gradually. For taxes and military personnel, the effect is immediate and grows over time (Panels B and C).[38] In sum, treated and untreated towns did not display differential trends of state organization prior to the outbreak of the war. Since the strategic considerations of armies took into account mainly constant or slow-moving town characteristics, which would

[38]Section V provides a detailed examination of the mechanisms underlying the gradual dismantling of parliaments.

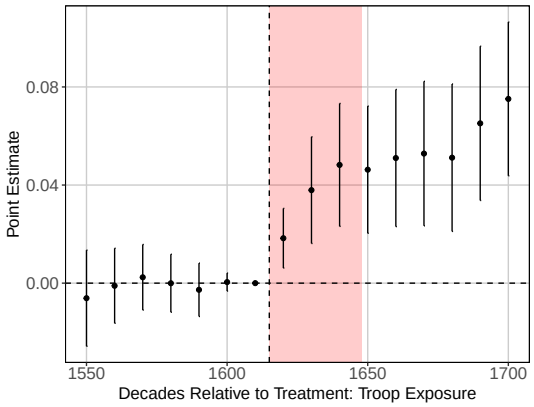
influence the level of state organization rather than relative trends, this appears plausible. We devote Appendix Section B.I to robustness checks that support this claim.

Figure II: Troop Exposure and Capable Autocracy (Event Studies)

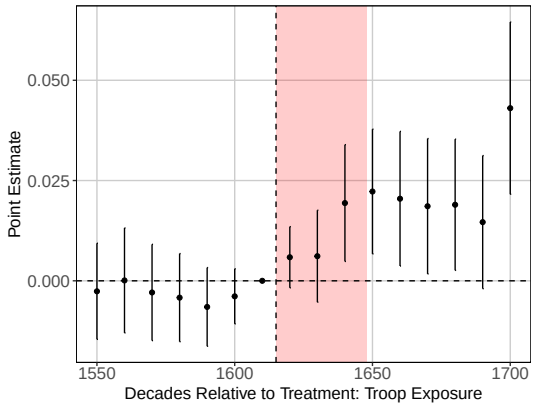
A: Parliament Eliminated



B: Ruler Taxes



C: Military Personnel



Note This plot shows results of estimating the event study regression in equation (2), with 95 percent confidence intervals. Observations are at the town-year level, with the number of towns indicated in Table I. The sample comprises 290 years. The dependent variables are (A) a binary variable of whether the parliament that represented town i has been eliminated in year t, (B) a binary variable of whether town i has records of direct ruler taxes in year t, and (C) the inverse hyperbolic sine of military personnel born in town i active in year t. The area shaded in red indicates the duration of the war. Standard errors are clustered at the town level.

IV.III Instrumental Variable

In light of the event studies presented in Figure II, a possible confounding factor would need to vary over time and across towns, coincide with the onset of the Thirty Years' War, and produce an immediate and persistent kink or discontinuity in state organization outcomes. To rule out the unlikely presence of such a confounding factor, we use detailed military

information in an instrumental variables approach.

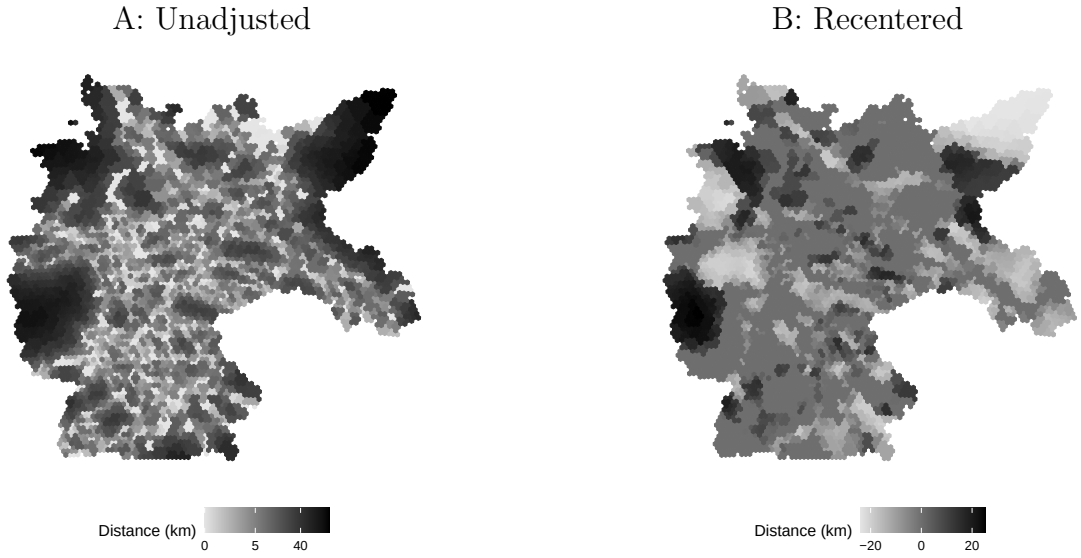
IV.III.I Baseline Instrument

Our starting point are threats to identification that emerge from campaign strategy as discussed in Section IV.I. We proceed in two steps. In a baseline instrument, we focus on the concern that non-random deviations from the fastest route between fortified towns correlate with state outcomes. Then, we account for the strategic centrality of towns by recentering the instrument. The baseline approach is similar to canonical 'least-cost path' instruments (Banerjee et al., 2020; Faber, 2014). In our context, each campaign forms a connected graph in which troop movements are edges and towns are nodes. We restrict each campaign graph to just the start and end nodes, as well as all fortified towns along the way. Second, we connect this subset of nodes using least-cost paths. Finally, we calculate the inverse hyperbolic sine of the distance of each town in our data to the closest campaign least-cost path. Figure III, Panel A, shows the spatial distribution of our instrument. This instrumental variable is relevant: the first-stage F statistic is 42, suggesting that troops mainly followed the fastest routes between targets.[39] The exclusion restriction requires that distance from the closest campaign least-cost path only influences state outcomes through troop exposure. We discuss this assumption in detail below.

In Table II, Panel A, we estimate equation (3) using the instrumental variable. Compared to the results in Table I, estimates point in the same direction and are also statistically significant at the 1% level. Also, estimated coefficients are substantively larger than their OLS counterparts. We interpret this fact through the lens of heterogeneous treatment effects. Our instrumental variable analyses focus on the central campaigns of the main armies, whereas our baseline exposure events also include smaller exposure events. As local contributions to troops were "levied on the basis of the effective [...] strength of troops" (Redlich, 1959; Ritter, 1903), we expect the estimated effect to increase in troop size. Further, under the exclusion restriction, our estimated coefficients reflect a local average treatment effect of the complier population: those towns that would not have been exposed to troops, had they not been on the least-cost path of a major campaign. This likely

[39] Appendix Figure A.5 shows, in our example of Sweden in 1630–31, that the campaign least-cost path closely tracks our more finely-grained campaign path data.

Figure III: Campaign Distance Instrument (Unadjusted and Recentered)



Note This map shows the spatial distribution of the unadjusted instrument (Panel A), and the recentered instrument, which is obtained by subtracting the expected instrument from 1,000 counterfactuals (Panel B). Appendix Figure A.6 shows the expected instrument. Details on the construction of counterfactuals are given in Section III.II and Appendix Section B.III.

reflects a set of towns that were more peripheral and hence experienced the most drastic treatment effect of integration into the central state, compared to towns that were already non-peripheral and integrated prior to troop exposure. Historical examples speak to this notion. For the small town of Kitzingen, incidentally exposed to Gustavus Adolphus on his way between the fortified towns of Nürnberg and Erfurt, "wartime contributions constituted a 1,000% increase on peacetime tax burdens" (Wilson, 2018, p. 237). Also, Wallenstein's occupation terms with backward Pomerania in 1627 generated six times the annual pre-war tax revenue. In Appendix Section B.II, we demonstrate that these instrumental variable results are maintained in various robustness checks.

IV.III.II Recentered Instrument

The causal interpretation of the results in Panel A of Figure II relies on the assumption that strategic centrality does not constitute a source of bias. The exclusion restriction might be violated for two reasons. First, our instrumental variable reflects the distance to fortified towns as strategic targets. Second, towns close to a least-cost campaign path were likely

Table II: Troop Exposure and Capable Autocracy (Instrumental Variable)

	Parliament Eliminated	Ruler Taxes	Military Personnel
	(1)	(2)	(3)
Panel A: Unadjusted Instrument			
Troop Exposure	0.5298***	0.3365***	0.3666***
	(0.132)	(0.119)	(0.097)
R^2	0.49	0.51	0.17
Panel B: Recentered Instrument			
Troop Exposure	0.6411***	0.6726**	0.4343**
	(0.240)	(0.290)	(0.219)
R^2	0.44	0.25	0.09
Number of Observations	646,700	422,820	646,700
Number of Towns	2,230	1,458	2,230
Outcome Def.	(0/1)	(ihs)	(0/1)
Town FE	✓	✓	✓
Year FE	✓	✓	✓
Cluster	Town	Town	Town

Note This table presents results of estimating equation (3), using an instrumental variable based on the distance of a town to the closest campaign least-cost path (Panel A), or the recentered instrumental variable, which is obtained by subtracting the expected instrument from 1,000 counterfactuals (Panel B). Observations are at the town-year level, with the number of towns indicated in the table. The sample comprises 290 years. The dependent variables are (1) a binary variable of whether the parliament that represented town i has been eliminated in year t, (2) a binary variable of whether town i has a record of direct ruler taxes in year t, and (3) the inverse hyperbolic sine of military personnel born in town i active in year t. Standard errors are clustered at the town level. *, **, and *** denote significance on the 10 percent, 5 percent, and 1 percent level, respectively.

more centrally located; this centrality is again picked up by the instrument.[40]

To address strategic confounders in a unified way, we aim to adjust our estimates for the expected value of the instrument given an ex-ante probability distribution over possible campaigns. This approach essentially discounts evidence from towns in central locations that would have been close to a troop movement under any circumstance (Borusyak and Hull, 2023).[41] We construct a set of counterfactual campaign paths based on internal military communications, as described in Section III.II. To obtain a probability distribution over these possible campaigns, we permute between realized and counterfactual paths at each juncture point, and thus construct a set of 1,000 different different campaign scenarios of the war. We re-compute our instrument in each of the scenarios, and then calculate the average value of the instrument across counterfactuals. This expected instrument accounts for the inherent centrality of towns within the campaigning efforts. 'Recentering' the realized instrument,

[40]Some works that employ 'least-cost path' instruments take this concern into account and assume the exclusion restriction to hold conditional on a set of distance controls (Faber, 2014). We demonstrate that our results are robust to this approach in Appendix Section B.II.

[41]Appendix Section B.III discusses our implementation of Borusyak and Hull (2023) in detail.

that is, subtracting its expected counterpart, removes this centrality.[42] Figure III, Panel B shows the recentered instrument.[43]

An example illustrates the recentering approach: the towns of Frankfurt (Oder) and Schweinfurt (depicted in Figure I) are both on a campaign path and hence have an unadjusted instrument value of zero. While Frankfurt was an important node on the way from the Baltic Coast to the interior of Germany, Schweinfurt, further south, did not have the same military centrality. Reflecting this fact, Frankfurt is visited in all counterfactuals, resulting in a recentered instrument value of zero. Schweinfurt, on the other hand, is not always visited, and its expected instrument is eight kilometers larger than the realized instrument.

In Table II, Panel B, we employ the recentered instrument. Compared to the results in Panel A, the sign and significance of coefficients are qualitatively unchanged, underlining that troop presence is causally linked to taxation, militarization, and parliament elimination. The quantitative changes of coefficients in comparison to Panel A are again informative. Now, our estimates focus on the complier population across counterfactual campaigns, hence zeroing in on the most incidentally exposed towns. The increased coefficients for taxation and parliament elimination suggest larger treatment gains for this set of towns. We devote Appendix Section B.IV to demonstrate that these instrumental variables results hold up in various robustness checks.

V Mechanism

War bundles multiple channels that may have weakened executive constraints, ultimately enabling the elimination of parliament. In this section, we use detailed data and historical case studies to examine potential mechanisms. Our analysis is structured around three key questions: Section V.I explores how executive capacity initially expanded. Section V.II then examines how rulers consolidated power permanently. Finally, Section V.III considers whether the elimination of parliaments was inevitable.

[42] This permutation-based approach follows Section 4 of Borusyak and Hull (2023).

[43] Note that recentering can affect the instrument value in both directions: some towns are exposed more than expected, and others receive less exposure than in the counterfactual scenarios.

V.I Coordination and Executive Capacity

How did executive capacity initially expand? Two broad mechanisms are plausible. On the one hand, the presence of troops may have given rise to coordination between local populations, troops, and the ruler. On the other hand, troops may have violently undermined the power of elites, thereby enhancing the relative capacity of the ruler.[44] We examine these mechanisms separately: first, the role of coordination in wartime governance; and second, the direct, violent weakening of representative institutions. We reserve Appendix Section C.I for detailed case studies and provide a stylized summary of the historical evidence below.

Our treatment is the presence of troops commanded by quasi-independent military enterprisers. Therefore, a key scope condition for potential mechanisms is that troops posed a credible and significant threat of violence to towns.

Within this scope condition, we first turn to coordination. Since armies almost entirely depended on local resources, upkeep was a central organizational requirement once troops had advanced on a town (Redlich, 1959). Armies could acquire resources through plunder or taxation: plunder maximized short-term consumption but devastated the local resource base, undermining future extraction and military discipline; taxation, by contrast, required coordination but enabled more sustainable provisioning. This suggests considerable scope for taxation to mitigate the damage caused by troop presence.[45]

Historical evidence indicates that, under these conditions, military leaders actively sought coordination with rulers.[46] A key facilitator of such coordination was the presence of a shared military alliance between the army and the ruler: this lowered the cost of communication at all levels and gave rulers clearer incentives to provision the troops. Local populations, moreover, proved willing to cede autonomy to "forestall the greater evil of military reprisals" (Wilson, 2009, p. 406).[47] Appendix Section D presents a quantitative

[44] For literature reviews distinguishing these 'demand-side' (coordination-based) and 'supply-side' explanations of state capacity, see, for example, Besley (2020), Allen et al. (2023), and Caprettini and Voth (2023).

[45] This parallels other contexts in which state intervention cannot prevent external shocks but can mitigate their consequences — for example, by adopting irrigation systems in response to shifting river patterns (Allen et al., 2023).

[46] Coordinating taxes with local rulers had two key advantages. First, centralized intervention was important, as effective provisioning depended on the surrounding countryside of the town. Second, rulers could serve as quicker and more legitimate intermediaries between civilian and military authorities than parliamentary bodies, which faced high transaction costs due to long planning horizons and the need to coordinate across stakeholders (Kraus, 2021).

[47] Overall, the unprecedented size and mobility of early modern armies left little room for effective resistance

framework that formalizes these incentives: we embed the trade-off between dictatorship and disorder during crises (Djankov et al., 2003) in theories of state-building under roving and stationary bandits (Olson, 1993; Sánchez De La Sierra, 2020; Henn et al., 2024).

Our empirical analysis tests the two main hypotheses emerging from this framework. First, troop exposure events should be significantly less likely to coincide with destruction when coordination was facilitated. Second, executive capacity should be more likely to emerge where coordination between troops and local rulers was easier. We operationalize the notion of easier coordination by coding whether troops and local rulers belonged to the same military alliance in a given treatment instance.[48] Appendix Table A.3 shows that, across the full sample of treatment events, the destruction of capital and labor — measured by whether a town was destroyed due to troop presence or experienced above-median population losses during the war, respectively — was less likely when the troops administering the treatment were allied with the local rulers.

We then augment equation (3) by interacting the treatment variable with this coordination indicator. Table III presents results. The coefficient on the interaction term is positive, statistically significant, and sizeable across all three outcome measures.[49]

Next, we consider the possibility that troop exposure expanded executive capacity through destruction, directly shifting the local balance of power. In addition to suffering significant capital and labor supply shocks, some towns were located close to battles or annexed. This may have weakened the bargaining position of local elites.

To empirically assess this alternative mechanism, Appendix Table A.5 presents estimates from restricted samples that exclude troop exposure events likely to involve destructive channels. Specifically, we omit towns that suffered capital destruction or above-median population losses, were located within 50 kilometers of major battles, or were annexed after the war. The results remain robust: the estimated coefficients are similar in magnitude and remain statistically significant relative to the baseline in Table I.[50]

once forces were en route (Guthrie, 2002, p. 160).

[48] We distinguish between the two primary sides in the conflict: Catholic- and Protestant-aligned forces, with coding details provided in Section III.II.

[49] These results remain robust when excluding exposure events involving troops operating in their domestic territories, as shown in Appendix Table A.4. This suggests, in line with the historical evidence, that the strategic 'overstaffing' of armies to weaken local elites was not primarily at play.

[50] Given the reduction of our sample by 92% due to the sparsity of population records, the direct tax coefficient in Panel B, column 2 becomes marginally insignificant but remains sizeable.

Table III: Troop Exposure and Coordination

	Parliament Eliminated (1)	Ruler Taxes (2)	Military Personnel (3)
Troop Exposure	0.0762***	0.0291*	0.0004
	(0.0204)	(0.0169)	(0.0061)
Troop Exposure × Alliance	0.0516**	0.0525**	0.0461***
	(0.0245)	(0.0220)	(0.0116)
R^2	0.56	0.57	0.37
Observations	646,700	422,820	646,700
Number of Towns	2,230	1,458	2,230
Outcome Mean	0.16	0.07	0.03
Outcome Def.	(0/1)	(0/1)	(ihs)
Town FEs	✓	✓	✓
Year FEs	✓	✓	✓
Cluster	Town	Town	Town

Note This table presents results of estimating equation (3). Observations are at the town-year level, with the number of towns indicated in the table. The sample comprises 290 years. The dependent variables are (1) a binary variable of whether the parliament that represented town i has been eliminated in year t, (2) a binary variable of whether town i has a record of direct ruler taxes in year t, and (3) the inverse hyperbolic sine of military personnel born in town i active in year t. Standard errors are clustered at the town level. *, **, and *** denote significance on the 10 percent, 5 percent, and 1 percent level, respectively.

Taken together, these findings support the relevance of a coordination-based mechanism in the expansion of executive capacity, operating against the backdrop of a credible threat of violence. This interpretation is consistent with qualitative accounts of the Thirty Years' War. Hintze (1906) argues that the rise of military officials responsible for provisioning troops laid the institutional groundwork for absolutism in Prussia. In a broader account of autocratic state formation, Fraenkel (1941) highlights the Thirty Years' War as a critical moment in the emergence of extraordinary commissarial authority, showing how the logistical demands of military coordination gave rise to a 'dual state' that bypassed parliamentary institutions.

V.II The Consolidation of Autocracy

Rulers retained the local recruitment and taxation infrastructure resulting from the war even after troops had moved on, permanently bypassing Estate consent. How did this consolidation of autocracy occur following the initial expansion in executive capacity?

Both bottom-up and top-down mechanisms are plausible. The war-induced crisis may have shifted preferences and collective psychology toward favoring strong central authority (Alsan et al., 2023). Moreover, rulers may have used their capacity to invest in repression,

altering elite outside options to stabilize their rule (Acemoglu and Robinson, 2006). We examine the relevance of both channels; Appendix Section C.II provides historical case studies.

Evidence of a psychological shift among subjects is scarce due to the temporal distance of the historical episode. The few surviving accounts suggest that the experience of emergency (*Not*) during the war prompted subjects to infer a broader necessity (*Notwendigkeit*) for centralized authority (Kraus, 2021). A larger body of surviving evidence for this rationale is closely tied to state influence.[51] Rulers themselves invoked the maxim that 'necessity knows no law' and employed both visual and written propaganda to legitimize their claims to authority.

Beyond persuasion, rulers also intervened directly, forcibly dissolving parliaments and co-opting local institutions (Press, 1991). This consolidation of executive power, combined with the erosion of autonomous elite authority, made "the court, administrative, and military appointments increasingly attractive to the local nobles" (Wilson, 2009, p. 359), further weakening the role of parliaments.

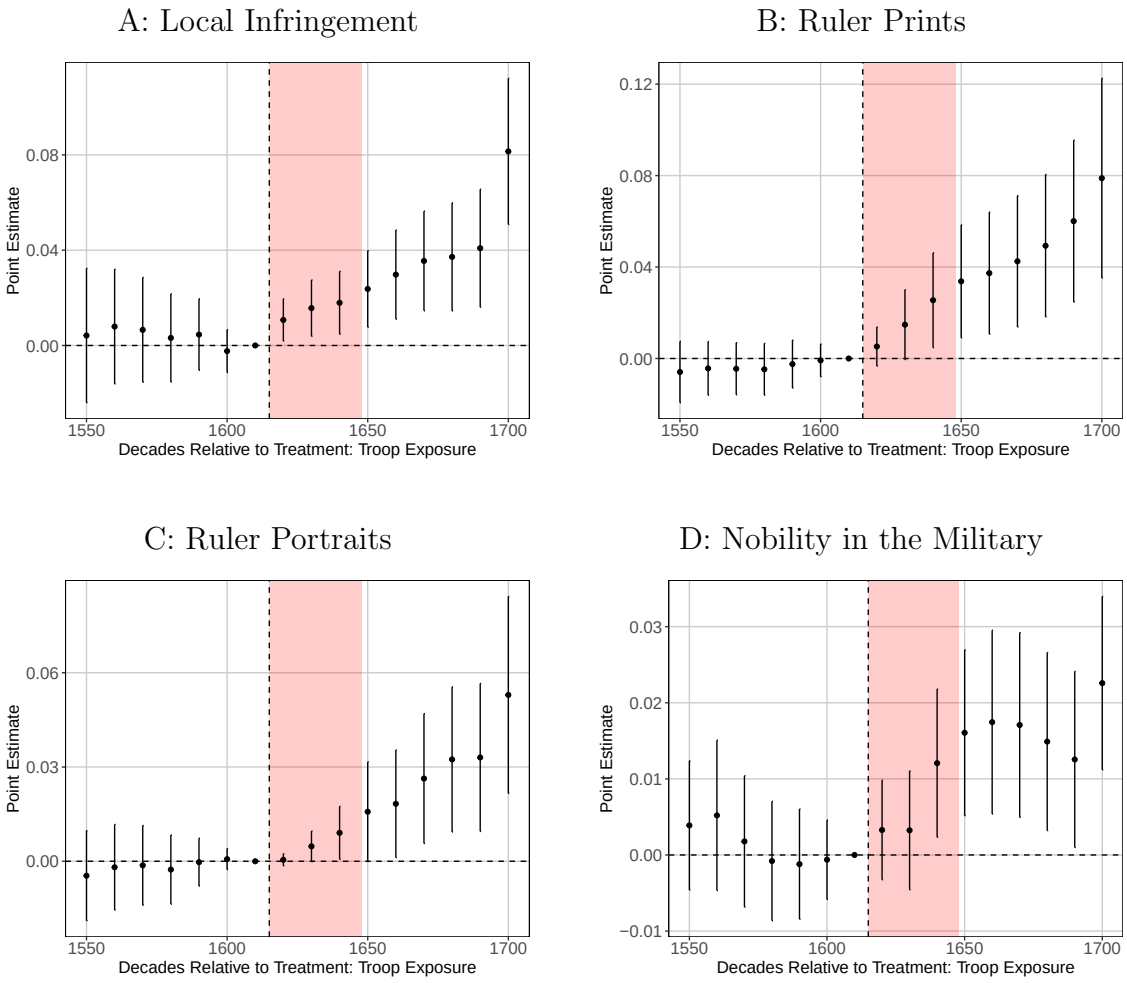
We evaluate these mechanisms empirically, within the limitations inherent in early modern data. We re-estimate equation (3) using four outcomes: the presence of printed ruler portraits; militaristic prints; instances of repression, proxied through local infringements by rulers; and elite co-optation, proxied by the share of nobles in military service. Appendix Section C.III illustrates examples from our data. Table IV presents the results. Troop exposure is significantly associated with all four outcomes. Figure IV shows that these effects emerged immediately and were not led by pre-trends. Taken together, support the interpretation that both bottom-up legitimation and top-down enforcement mechanisms contributed to autocratic stabilization.

V.III Legal Resistance and Parliamentary Survival

Was the elimination of parliaments inevitable? Against the pressures that encouraged elites to join the autocratic state stood the costs of resisting executive overreach. A principal

[51] Writing in 1672, the political philosopher Samuel Pufendorf —employed at the Swedish and later the Prussian court— "distilled from the memory of civil war a powerful rationale for the extension of state authority. Against the 'libertas' of the Estates, Pufendorf asserted the 'necessitas' of the state." (Clark, 2006, p. 36)

Figure IV: Troop Exposure and the Consolidation of Autocracy (Event Studies)



Note This plot shows results of estimating the event study regression in equation (2), with 95 percent confidence intervals. Observations are at the town-year level, with the number of towns indicated in Table I. The sample comprises 290 years. The dependent variables are (A) the inverse hyperbolic sine of the instances of repression of local institutions in town i and year t, (B) the inverse hyperbolic sine of the number of ruler prints with a militaristic title from town i and year t, (C) the inverse hyperbolic sine of the number of portraits of the ruler state in i in year t, and (D) the share of nobility from town i in year t that is also in the military. Standard errors are clustered at the town level.

Table IV: Troop Exposure and the Consolidation of Autocracy

	Ideology		Repression	Co-optation
	Ruler Prints	Ruler Portraits	Infringement Count	Nobility in the Military
	(1)	(2)	(3)	(4)
Troop Exposure	0.0540***	0.0347***	0.0593***	0.0122***
	(0.0181)	(0.0131)	(0.0150)	(0.0036)
R^2	0.57	0.51	0.61	0.21
Observations	646,700	646,700	646,700	646,700
Number of Towns	2,230	2,230	2,230	2,230
Outcome Mean	0.05	0.02	0.14	0.01
Outcome Def.	(ihs)	(ihs)	(ihs)	(0/1)
Town FEs	✓	✓	✓	✓
Year FEs	✓	✓	✓	✓
Cluster	Town	Town	Town	Town

Note This table presents results of estimating equation (3). Observations are at the town-year level, with the number of towns indicated in the table. The sample comprises 290 years. The dependent variables are (1) the inverse hyperbolic sine of the instances of repression of local institutions in town i and year t, (2) the inverse hyperbolic sine of the number of ruler prints with a militaristic title from town i and year t, (3) the inverse hyperbolic sine of the number of portraits of the ruler state in i in year t, and (4) the share of nobility from town i in year t that is also in the military. Standard errors are clustered at the town level. *, **, and *** denote significance on the 10 percent, 5 percent, and 1 percent level, respectively.

mechanism through which subjects could contest their rulers was the Imperial High Court of the Holy Roman Empire, which "provided support to the estates against their rulers and strengthened their determination for self-assertion" (Bahlcke, 2012, p. 49). Especially for parliaments that had strengthened their position relative to their rulers before the war, the court served as a "brake on princely absolutism" (Press, 1994).[52]

To examine this channel, we measure the prior legal resistance of towns. Specifically, we construct an indicator of whether a town was involved in at least one instance of litigation against its ruler during the pre-war period, 1600–1618. We then re-estimate equation (3), fully interacting the treatment variable with this indicator. Table V presents the results. Towns with a history of litigation in the Imperial High Court were significantly less likely to experience the discontinuation of parliament — by an amount that offsets the baseline coefficient (column 1). This effect remains robust when including controls (column 2).

[52]For example, "the Counts of Isenburg, Oettingen, Reuß, Schlitz-Görtz, and numerous other territorial princes were unequivocally instructed by the Imperial High Court not to levy taxes arbitrarily" (Bahlcke, 2012, p. 49). The state of Hesse-Darmstadt offers another illustration of how legal resistance could constrain permanent state transformation. During the war, the landgrave built a sizeable standing army, which he used to enforce monthly ruler taxes. In 1647, several towns petitioned the Imperial High Court, objecting that the Estates were "assessed for taxation and forbidden to meet on their own initiative"(Carsten, 1959, p. 160).

A potential concern is that the ability to organize a lawsuit in the Imperial High Court may correlate with unobserved town characteristics that also influenced a ruler's capacity to dismantle parliamentary institutions. To address this possible selection into litigation, we use a plausibly exogenous source of variation in access to the court. Specifically, we draw on geographic constraints introduced by the court directive (*Reichskammergerichtsordnung*) of 1548/55, which mandated that high judges (so-called assessors) be drawn from the same or an adjacent region as the litigant (Jahns, 2010). This requirement created variation in litigation access based on the local availability of high court judges.

When a judge died, the replacement involved a lengthy administrative process, which meant that some towns temporarily lost access to nearby judicial representation due to chance events. We use this feature to construct the distance to the nearest judge in the time period 1600–1618, recentered based on unexpected judge deaths. This recentered measure captures plausibly exogenous variation in access to litigation.[53] We then use this instrument to predict the litigation indicator.[54] We binarize the fitted value from this regression for comparability with the baseline indicator. Columns 3 and 4 in Table V present the results of including this interaction term in our analysis. The estimates remain qualitatively similar compared to the baseline interaction.

V.IV Autocracy in the Long Run

Our results point to a critical juncture (Acemoglu and Robinson, 2013; Callen et al., 2024): the war created a window of opportunity for absolutism, and in the absence of effective resistance, this new institutional equilibrium endured for centuries. To document its long-run persistence, we revisit our data on military biographies from the *Deutsche Biographie*, which provides consistent coverage up to 1900. Following the approach in Section IV, we construct an indicator variable that equals one if there is at least one notable military figure from city i who is alive and over the age of 20 in year t.

[53] To construct a counterfactual pool of judges who could have been alive during this period, we simulate lifespans using the canonical Gompertz (1825) distribution, calibrated to match the observed number of living judges in 1600–1618. For each birth decade of judges, we compute the median life expectancy. In 1,000 iterations, we randomly assign judge deaths and compute the distance from each town to the nearest counterfactual judge. We then recenter the observed town-level distance using this expected value to estimate the decline in legal access caused by judge mortality (Borusyak and Hull, 2023).

[54] This recentered distance strongly predicts litigation. The associated first-stage regression yields an F-statistic of 16.

Table V: Troop Exposure and Parliament Elimination (Litigation)

	Parliament Eliminated			
	(1)	(2)	(3)	(4)
Troop Exposure	0.1250***	0.1191***	0.1192***	0.1147***
	(0.0161)	(0.0156)	(0.0160)	(0.0154)
Troop Exposure × Litigation	-0.0949**	-0.1049***		
	(0.0387)	(0.0366)		
Troop Exposure × Litigation (Fitted)			-0.0803**	-0.0805**
			(0.0407)	(0.0387)
R^2	0.56	0.59	0.56	0.58
Observations	646,700	646,700	646,700	646,700
Number of Towns	2,230	2,230	2,230	2,230
Outcome Mean	0.1624	0.1624	0.1624	0.1624
Town FEs	✓	✓	✓	✓
Year FEs	✓	✓	✓	✓
Controls		✓		✓
Cluster	Town	Town	Town	Town

Note This table presents results of estimating equation (3), with an additional interaction term if a resistance mechanism is present. The mechanism is defined as an indicator of whether the town litigated against its ruler in the Imperial High Court in the 1600–1618 period (columns 1 and 2), or the binarized fitted value from predicting this indicator, using the recentered distance of a town to the closest Imperial High Court judge in the same time period (columns 3 and 4). The sample comprises 290 years. The dependent variable is a binary variable of whether the parliament that represented town i has been eliminated in year t. Standard errors are clustered at the town level. *, **, and *** denote significance on the 10 percent, 5 percent, and 1 percent level, respectively.

Panel A of Figure V compares treated and untreated towns. Prior to 1618, both groups followed a similar, low-level trajectory. However, after the war, militarization in treated towns steadily increased, with roughly 30% of them producing notable military figures by the end of the nineteenth century. Panel B focuses exclusively on treated towns, this time distinguishing between towns that did and did not experience the elimination of parliament. Here too, we see a growing divergence in militarization beginning in the mid-seventeenth century, which persisted for centuries.

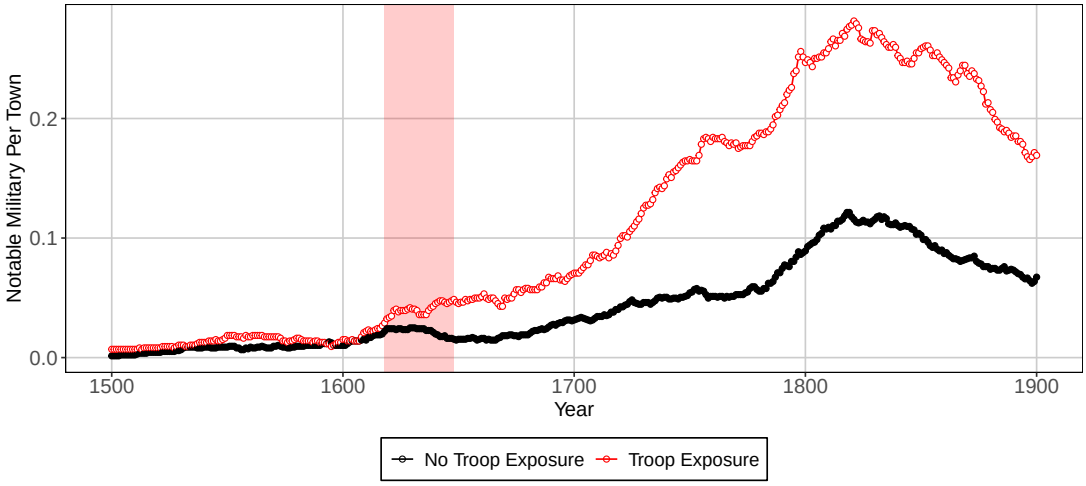
We further explore the implication of this long-run militarization in a nineteenth-century cross-section of towns. We estimate

$$Autocracy_i = \beta Treated_i + X'\gamma + \varepsilon_{it}, \tag{3}$$

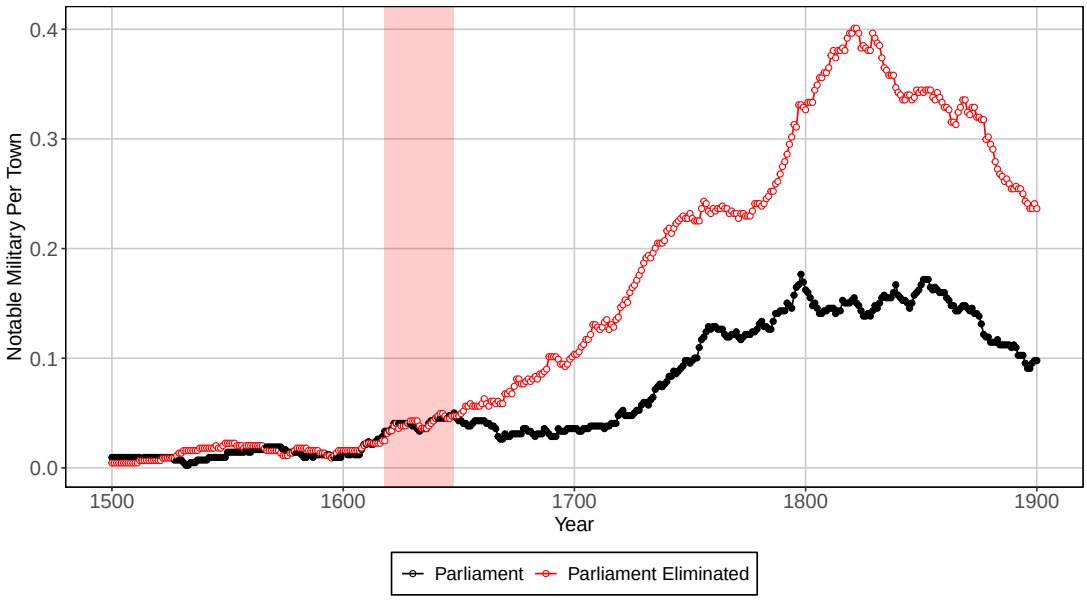
where $Autocracy_i$ is the share of military personnel among the working-age population in 1895, the share of war-disabled individuals in the same year, an indicator of whether

Figure V: Troop Exposure and Long-Run Militarization

A: Militarization By Troop Exposure



B: Militarization By Parliament Survival, Conditional on Exposure



Note The plot shows the average number of military personnel as coded from the *Deutsche Biographie* from 1500 to 1900, separately for groups of towns. Panel A distinguishes treated and untreated towns. Panel B separates towns by the elimination of parliament, in a sample of treated towns. Details on the construction of the data are given in Section III.

town inhabitants had below-median life expectancy in the 1800–1914 period, or an indicator of whether a town had a state-run hospital in the late nineteenth century. X bundles all pre-1618 geographic and economic covariates, and we use Conley standard errors with a cutoff of 100km. Table VI shows results: treated towns had more military personnel, more war-disabled individuals, lower life expectancy, and fewer state-run hospitals.

Table VI: Troop Exposure and Militarization in the Nineteenth Century

	Share Military	Share Invalid	Below-Median Life Exp.	State Hospital
	(1)	(2)	(3)	(4)
Troop Exposure	0.0995**	0.1512**	0.0664**	-0.0443***
	(0.0421)	(0.0595)	(0.0323)	(0.0064)
Observations	2,145	2,145	1,905	685
R^2	0.06	0.09	0.02	0.05
Outcome Def.	(std.)	(std.)	(0/1)	(0/1)
Outcome Mean	0.00	0.00	0.50	0.12
Controls	✓	✓	✓	✓
Standard Errors	100 km	100 km	100 km	100 km

Note This table presents results of estimating equation (3), Standard errors are indicated in the table *, **, and *** denote significance on the 10 percent, 5 percent, and 1 percent level, respectively.

VI Conclusion

In this paper, we document the emergence of capable autocracies in response to the Thirty Years' War (1618–1648), the largest conflict in pre-modern Europe. Using planned troop movements from secret military communications, we estimate how town-level war exposure led to the growth of fiscal and military capacity while dismantling parliaments. We find that the coordination of military logistics shifted authority toward the executive, and that legal institutions acted as a constraint on war-induced autocracy.

These results suggest a dynamic trade-off between dictatorship and disorder during states of emergency (Hobbes, 1651; Tocqueville, 1835; Djankov et al., 2003). Crises increase the returns to centralized coordination, shifting capacity towards the executive. Such shifts can incentivize the executive to invest in institutions that permanently sideline executive constraints. Autocratic rule is stable when outside options available to elites, including resistance to overreach, are costly relative to the returns of supporting the executive state.

The Thirty Years' War marked a turning point in militarization and the consolidation of executive power in German-speaking Europe. Absolutism entrenched a culture that increasingly valued command and loyalty over openness and deliberation. Historians have argued that this militaristic legacy hindered the adoption of liberal-democratic values and ultimately turned into the most potent threat to the European order (Carsten, 1958; Rosenberg, 1958). Moreover, the expansion of executive powers during the war set a precedent for future autocratic consolidation. In 1933, the emergence of militarized executive officials under dictatorship drew on a legacy of 'exceptional rule' rooted in early modern absolutism (Hachtmann and Süß, 2006).

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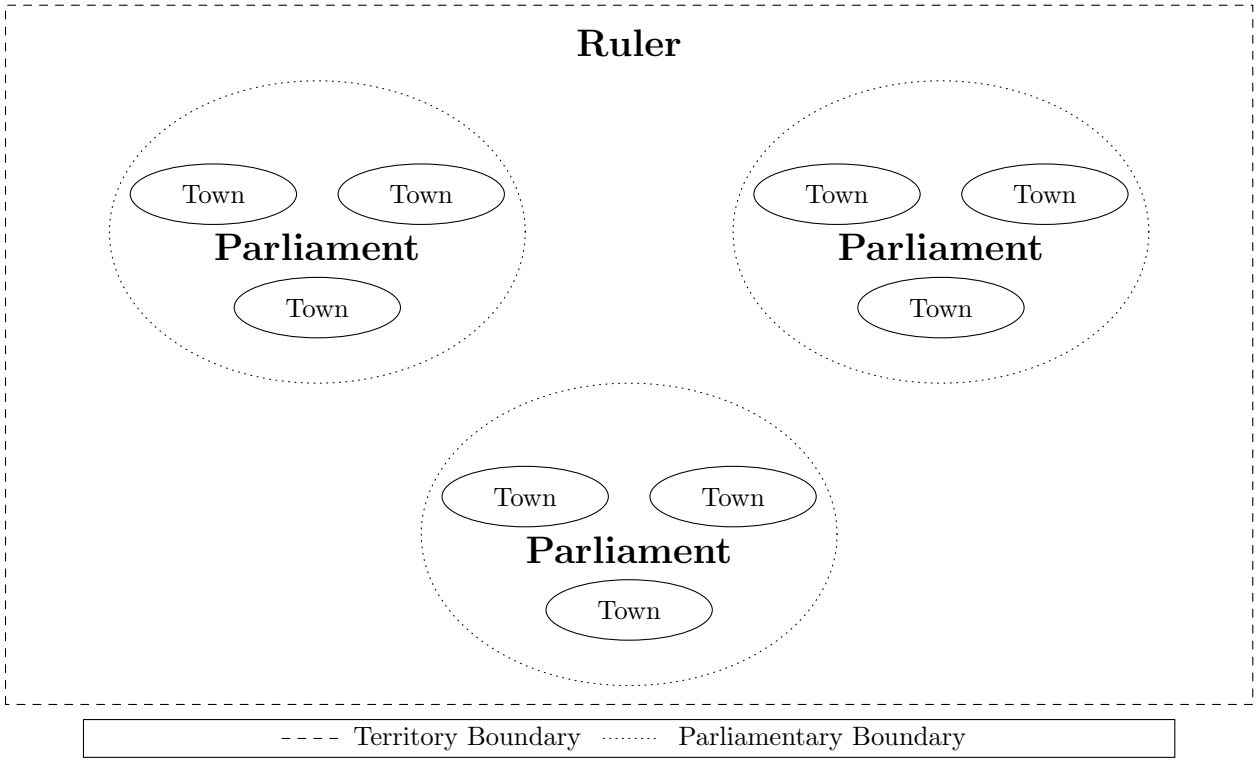
Supplementary Appendix: For Online Publication

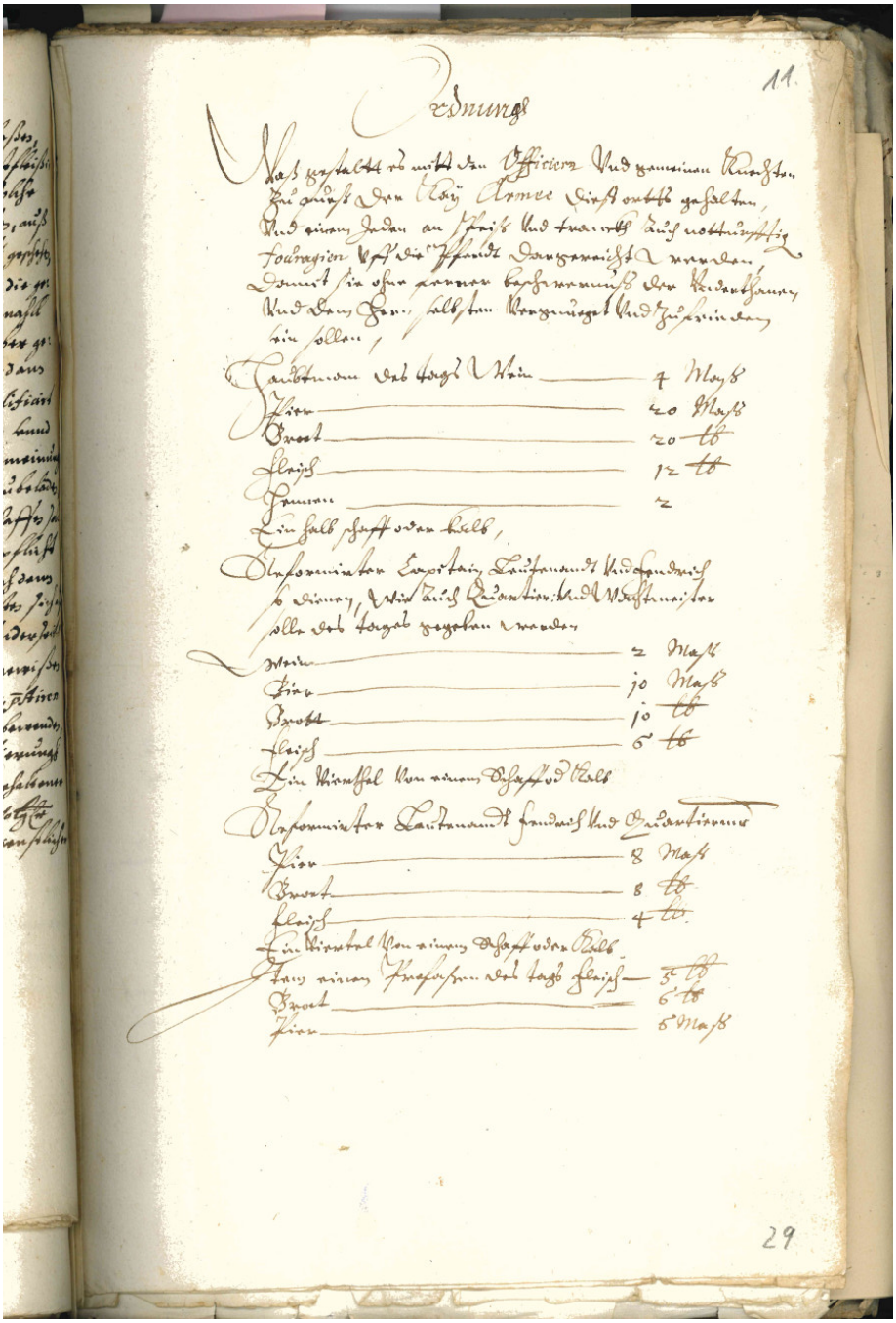
A Tables and Figures

Figure A.1: Stylized Parliamentary Rule Structure



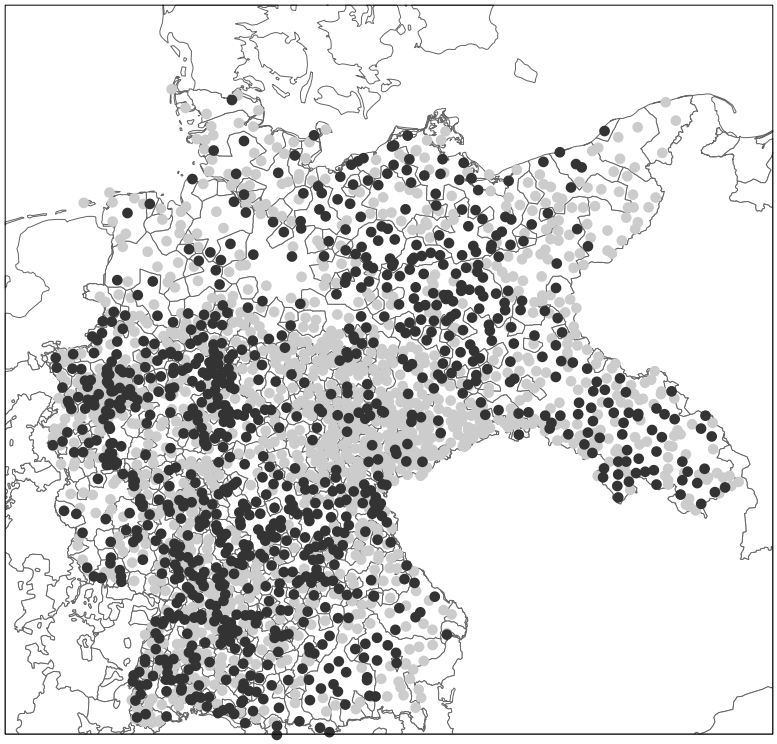
Note This plot shows a stylized overview over the structure of parliamentary rule. Towns belong to parliamentary regions. A ruler might ruler over multiple parliamentary regions.

Figure A.2: Tilly's Troop Requirements of the Town of Coesfeld, 1623



Note Example page of Tilly's troop requirements of the town of Coesfeld, located in the Bishopric of Münster, in 1623. It lists nutrition demands separately for different military ranks.

Figure A.3: Troop Exposure of Towns



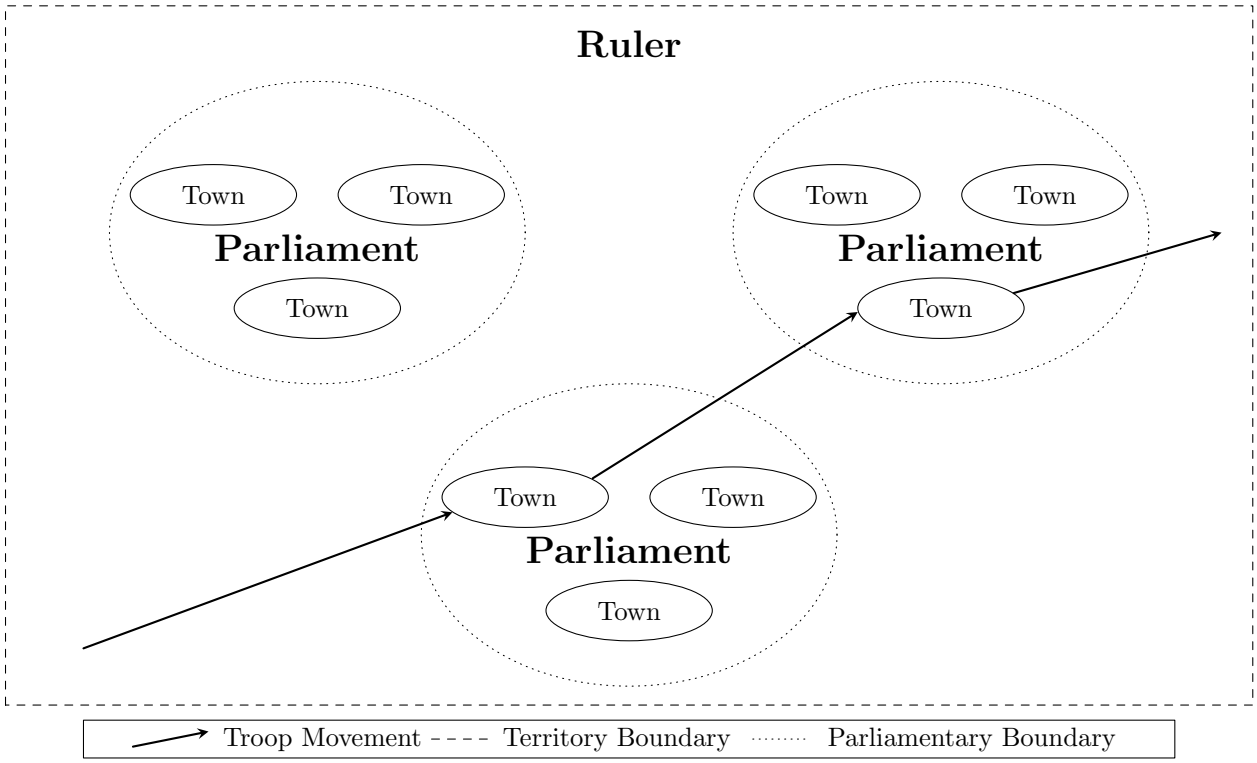
Note This map illustrates the troop exposure data. Each point is the location of a town in our data. Highlighted points are towns that have an associated troop exposure event. The base map shows parliamentary constituencies in the Holy Roman Empire.

Table A.1: Troop Leaders

War Party	Troop Leader	Campaign Years	Main Sources
Catholic League	Tilly	1618-1632	Kaiser (1999); Schmidt-Brentano (2022)
Catholic League	Johann von Aldringen	1632-1634	Brohm (1882); Schmidt-Brentano (2022)
Denmark	Christian IV	1625-1629	Lockhart (2014); Opel (1872); Schmidt-Brentano (2022)
France	Bernard of Saxe-Weimar	1635-1639	Droysen (1885); Schmidt-Brentano (2022)
France	Guébriant	1639-1643	Schmidt-Brentano (2022)
France	Turenne	1643-1648	Neuber (1869); Schmidt-Brentano (2022)
Imperial	Wallenstein	1625-1630; 1632-1633	Schmidt-Brentano (2022); Von Ranke (1869)
Imperial	Pappenheim	1632	Stadler (1991); Schmidt-Brentano (2022)
Imperial	Matthias Gallas	1634-1639; 1643-1645; 1646-1647	Höbelt (2016); Schmidt-Brentano (2022)
Imperial	Leopold Wilhelm	1639-1643; 1645-1646	Schmidt-Brentano (2022)
Imperial	Holzappel	1647-1648	Höfer (1997); Schmidt-Brentano (2022)
Protestant	Mansfeld	1618-1626	Gindley (1884); Anonymous (1622); Schmidt-Brentano (2022); Wedgwood (1969)
Sweden	Gustavus Adolphus	1630-1632	Dodge (1895); Roberts (1958); Geijer (1845)
Sweden	Gustav Horn	1632-1634	Geijer (1845); Lorentzen (1894)
Sweden	Herman Wrangel	1634-1636	Geijer (1845); Lorentzen (1894)
Sweden	Johan Banér	1637-1641	Geijer (1845); Schmidt-Brentano (2022)
Sweden	Lennart Torstensson	1641-1644	Merian (1700); Geijer (1845); Lorentzen (1894)
Sweden	Carl Gustaf Wrangel	1644-1648	Merian (1700); Steckzén (1920); Geijer (1845); Lorentzen (1894)

Note This table shows the war parties, the main troop leaders, their campaigning years in service of the war party, and the sources we consult to reconstruct their movements. Details on the construction of the data are given in Section III.II.

Figure A.4: Stylized Parliamentary Rule Structure and Troop Movements



Note: This plot complements Appendix Figure A.1 with troop movements. Troop movements, decided on by independent military leaders affiliated with a warring party, exposed individual towns to soldiers.

Table A.2: Troop Exposure (Balance)

	Troop Exposure				
	(1)	(2)	(3)	(4)	(5)
Distance Coast (std.)	-0.1239**				-0.1434**
	(0.0574)				(0.0589)
Latitude	-0.1055***				-0.1525***
	(0.0368)				(0.0394)
Longitude	-0.0120				0.0059
	(0.0106)				(0.0117)
Fortified in 1618		0.3298***			0.2224***
		(0.0750)			(0.0778)
On Trade Route in 1618			0.5543***		0.5360***
			(0.0574)		(0.0589)
Fiscal Chamber in 1618			0.1352**		0.1415**
			(0.0560)		(0.0565)
Ruggedness (std.)			-0.0119		-0.0839**
			(0.0299)		(0.0380)
Distance River (std.)			-0.0505*		-0.0160
			(0.0298)		(0.0331)
Agricultural Suitability (std.)			0.0065		0.0428
			(0.0302)		(0.0316)
Markets 1600-1618 (std.)				0.0240	0.0259
				(0.0259)	(0.0255)
Public Construction 1600-1618 (std.)				0.0318	0.0236
				(0.0266)	(0.0270)
Private Construction 1600-1618 (std.)				0.0272	0.0157
				(0.0271)	(0.0259)
Observations	2,230	2,230	2,230	2,230	2,230

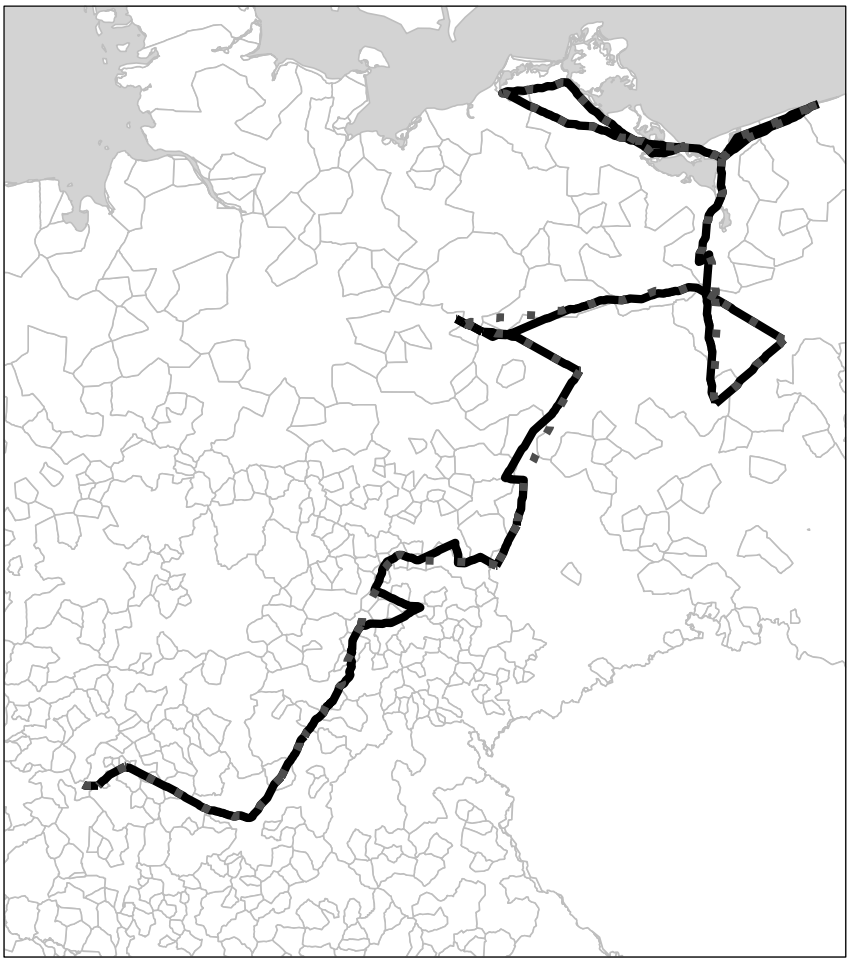
Note This table presents results of estimating the equation $Treated_i = X_i\gamma + \varepsilon_i$, with X_i denoting covariates, in a Probit model. Observations are at the town level, with the number of towns listed in the table. The dependent variable is an indicator of whether a town was exposed to troops during the Thirty Years' War. Standard errors are robust. *, **, and *** denote significance on the 10 percent, 5 percent, and 1 percent level, respectively.

Table A.3: Allied Troop Exposure and Destruction

	Destruction	Below-Median Life Exp.
	(1)	(2)
Allied Exposure	-0.0587**	-0.1780***
	(0.0251)	(0.0564)
Observations	863	181
R^2	0.05	0.19
Outcome Def.	(0/1)	(0/1)
Outcome Mean	0.22	0.49
Controls	✓	✓
Standard Errors	100 km	100 km

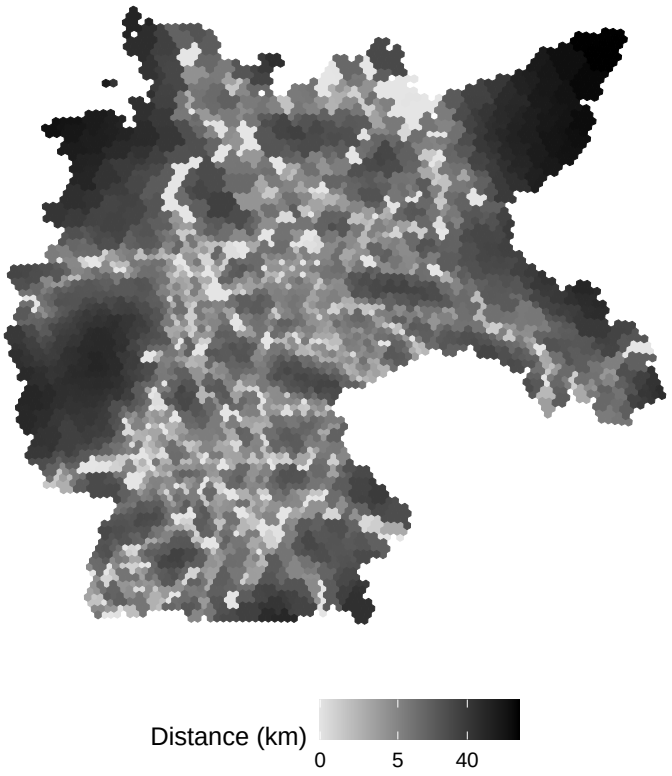
Note This table presents results from estimating $Destruction_i = \beta SameAlliance_i + \varepsilon_i$ in a sample of towns with $TroopExposure_i = 1$, where $Destruction_i$ and $SameAlliance_i$ are described in the main text. Observations are at the town level, with the number of towns indicated in the table. Standard errors are indicated in the table. *, **, and *** denote significance on the 10 percent, 5 percent, and 1 percent level, respectively.

Figure A.5: Least-Cost Campaign Path vs. Actual Campaign Path



Note This map shows an illustrative example of the least-cost path between fortified towns, compared to the actual campaign path, in the context of the campaigns depicted in Figure I. Details on the construction of least-cost paths and campaign paths are given in Section III.II.

Figure A.6: Instrument (Expected)



Note This map shows the spatial distribution of the average instrument from 1,000 counterfactuals. Details on the construction of counterfactuals are given in Section III.II and Appendix Section B.III.

Table A.4: Troop Exposure and Coordination (Subset)

	Parliament Eliminated (1)	Ruler Taxes (2)	Military Personnel (3)
Troop Exposure	0.0708***	0.0319*	0.0026
	(0.0208)	(0.0174)	(0.0061)
Troop Exposure × Alliance	0.0503*	0.0527**	0.0443***
	(0.0261)	(0.0229)	(0.0123)
R^2	0.57	0.58	0.37
Observations	622,340	413,250	622,340
Number of Towns	2,146	1,425	2,146
Outcome Mean	0.16	0.07	0.03
Outcome Def.	(0/1)	(0/1)	(ihs)
Town FEs	✓	✓	✓
Year FEs	✓	✓	✓
Cluster	Town	Town	Town

Note This table presents results of estimating equation (3), augmented with an indicator of whether the troop exposure event occurred in the same military alliance. The sample drops towns where military and local population were under the same territory. Observations are at the town-year level, with the number of towns indicated in the table. The sample comprises 290 years. The dependent variables are (1) a binary variable of whether the parliament that represented town i has been eliminated in year t, (2) a binary variable of whether town i has a record of direct ruler taxes in year t, and (3) the inverse hyperbolic sine of military personnel born in town i active in year t. Standard errors are clustered at the town level. *, **, and *** denote significance on the 10 percent, 5 percent, and 1 percent level, respectively.

Table A.5: Troop Exposure and Destruction (Subsets)

	(1)	(2)	(3)	(4)
Panel A: Parliament Eliminated				
Troop Exposure	0.0819***	0.1096**	0.1042***	0.1229***
	(0.016)	(0.045)	(0.020)	(0.018)
R^2	0.56	0.56	0.56	0.59
Observations	578,840	67,570	356,990	478,790
Number of Towns	1,996	233	1,231	1,651
Panel B: Direct Taxes				
Troop Exposure	0.0728***	0.0445	0.0831***	0.0795***
	(0.015)	(0.042)	(0.018)	(0.015)
R^2	0.58	0.48	0.56	0.58
Observations	385,700	52,200	248,530	324,510
Number of Towns	1,330	180	857	1,119
Panel C: Military Personnel				
Troop Exposure	0.0363***	0.0350*	0.0219**	0.0207***
	(0.009)	(0.021)	(0.010)	(0.008)
R^2	0.36	0.25	0.37	0.38
Observations	578,840	67,570	356,990	478,790
Number of Towns	1,996	233	1,231	1,651
Town FEs	✓	✓	✓	✓
Year FEs	✓	✓	✓	✓
Cluster	Town	Town	Town	Town
Not Destroyed	✓			
Above-Median Life Exp.		✓		
No Close Battle (50 km)			✓	
No Annexation				✓

Note This table presents results of estimating equation (3), focusing on subsets of the data as indicated in the table. Observations are at the town-year level, with the number of towns indicated in the table. The sample comprises 290 years. The dependent variables are (A) a binary variable of whether the parliament that represented town i has been eliminated in year t. (B) a binary variable of whether town i has records of direct ruler taxes in year t, (C) the inverse hyperbolic sine of military personnel born in town i active in year t, and Standard errors are clustered at the town level. *, **, and *** denote significance on the 10 percent, 5 percent, and 1 percent level, respectively.

B Robustness Checks

B.I Baseline Empirical Strategy Robustness

Section IV.II suggests a causal link between the war and shifts in state organization. We take a number of steps to demonstrate the robustness of this link. First, we directly account for a broad range of potential strategic confounders in our baseline estimates. In Appendix Table B.1, we flexibly control for geographic and economic covariates, such as the presence of a fortification or the presence of a trade route in 1618.[1] Our results are robust to accounting for these town characteristics, which suggests that time-varying unobserved shocks do not confound our estimation.

Second, we demonstrate that our results hold up when considering variation *within* states: since our treatment and outcomes are determined at the local level (cf. Appendix Figure A.4), and since some towns change territory over time, Appendix Table B.2 additionally includes territory fixed effects. This does not qualitatively affect our results. Also, we note that our results are not driven solely by Prussia or by any other single territory, as shown in Appendix Figure B.1.

Third, we present a number of strategies to construct more comparable control groups. Taking a holistic approach to military targeting, we include all of our economic and geographic town-level covariates to estimate treatment propensity scores via Probit, which are then used for nearest-neighbor matching. Appendix Table B.3 shows that the matching is effective in establishing covariate balance across treated and untreated towns.[2] In Appendix Table B.4, we demonstrate that our results are robust to estimating equation (3) in this matched sample. The method of calculating propensity scores does not drive our results: results are qualitatively unchanged if we use full instead of nearest-neighbor matching (Appendix Table B.5), or draw on Euclidean distance in the nearest-neighbor match (Appendix Table B.6). To account for targeting more directly, Section II suggests that the primary military objective was to take fortified towns, while the exposure of non-fortified towns was more incidental. In Appendix Table B.7, we repeat our main analysis using just the sample of non-fortified towns, which does not affect our results. In the same spirit, and to the same effect, Appendix Table B.8 excludes from our sample the places featured in maps compiled by the Swedish military during the Thirty Years War.[3]

Fourth, we examine the robustness of our results to the measurement of our outcomes. Appendix Table B.9 shows the relationship between troop exposure and parliament elimination obtains when we aggregate our panel to the constituency-year level, considering

[1] Parallel to the balance exercises in Appendix Table A.2, our covariates are distance to the coast (std.), latitude, longitude, an indicator of whether a town was fortified in 1618, an indicator of whether a town was on a trade route in 1618, an indicator of whether a town was in the jurisdiction of a fiscal Chamber in 1618, ruggedness (std.), distance to the closest navigable river (std.), agricultural suitability (std.), the number of markets added in 1600-1618 (std.), and public and private construction events 1600-1618 (std.), all interacted with $Post1618_{it}$.

[2] Using the standardized mean difference as a metric, Appendix Figure B.2 further underlines covariate balance in the matched sample.

[3] We extract this information from Riksarkivet (2024). Swedish military success and planning crucially hinged on maps of strategic targets. During his advance into central Germany, the Swedish king wrote to Stockholm on July 2, 1631: "All our maps stop here," and included orders to send the best map makers from Sweden to the German mainland in order to collect and make maps of target towns (Gäfvert, 1998, p. 309).

the share of treated towns in a constituency as our treatment. In Appendix Table B.10, we consider a Cox proportional Hazard model at the town level, and in Appendix Table B.11 at the constituency level. Appendix Table B.12 shows robustness to various restrictive standard errors at the town level, and Appendix Table B.13 at the constituency level. Appendix Tables B.14 and B.15, show restrictive standard errors for the remaining two outcomes — direct ruler taxes and military personnel. Additionally, we consider the use of the inverse hyperbolic sine of military personnel. Appendix Table B.16 implements robustness checks suggested by Chen and Roth (2024). This does not affect the statistical significance of our results.

Finally, we consider as treatment the staggered timing of the first town-level exposure event, instead of the interaction with $Post1618_t$. We omit from our sample all treated towns for which we do not know the exact exposure year and re-estimate equation (3) instead with the dummy $Exposure_{it}$ if town i has had troop exposure in any $t' \geq t$. Appendix Table B.17 shows that our results are robust to this specification.[4]

Table B.1: Troop Exposure and Capable Autocracy (Controls)

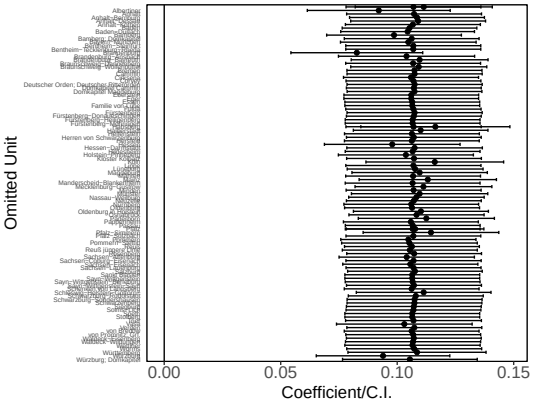
	Parliament Eliminated (1)	Ruler Taxes (2)	Military Personnel (3)
Troop Exposure	0.1022***	0.0492***	0.0192***
	(0.0143)	(0.0131)	(0.0065)
R^2	0.58	0.58	0.37
Observations	646,700	422,820	646,700
Number of Towns	2,230	1,458	2,230
Outcome Mean	0.16	0.07	0.03
Outcome Def.	(0/1)	(0/1)	(ihs)
Town FEs	✓	✓	✓
Year FEs	✓	✓	✓
Cluster	Town	Town	Town
Controls	✓	✓	✓

Note This table presents results of estimating equation (3), including controls. Observations are at the town-year level, with the number of towns indicated in the table. The sample comprises 290 years. The dependent variables are (1) a binary variable of whether the parliament that represented town i has been eliminated in year t, (2) a binary variable of whether town i has a record of direct ruler taxes in year t, and (3) the inverse hyperbolic sine of military personnel born in town i active in year t. Controls are distance to the coast (std.), latitude, longitude, an indicator of whether a town was fortified in 1618, an indicator of whether a town was on a trade route in 1618, an indicator of whether a town was in the jurisdiction of a fiscal Chamber in 1618, ruggedness (std.), distance to the closest navigable river (std.), agricultural suitability (std.), the number of markets added in 1600-1618 (std.), and public and private construction events 1600-1618 (std.), all interacted with $Post1618_{it}$. Standard errors are clustered at the town level. *, **, and *** denote significance on the 10 percent, 5 percent, and 1 percent level, respectively.

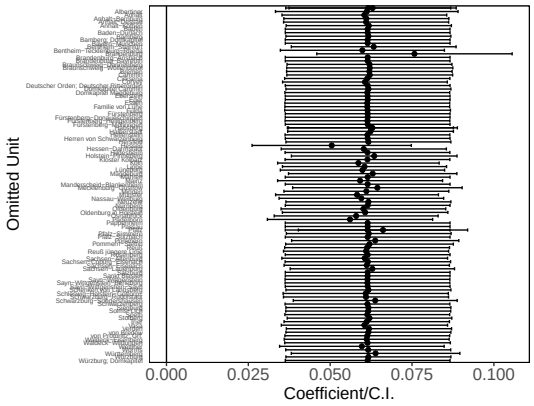
[4]Our results also hold when we instead use methodologies that are robust to heterogeneous treatment effects (De Chaisemartin and d'Haultfœuille, 2020).

Figure B.1: Troop Exposure and Capable Autocracy (Leave-Out Plots)

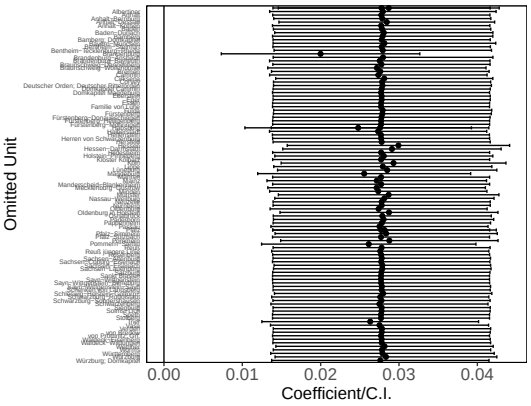
A: Parliament Eliminated



B: Ruler Taxes



B: Military Personnel



Note This plot shows results of estimating equation (3), with 95 percent confidence intervals, leaving out one territory that ever convened a parliament at a time. Observations are at the town-year level. The full sample comprises 290 years and 2,230 towns. The dependent variables are (A) a binary variable of whether the parliament that represented town i has been eliminated in year t, (B) a binary variable of whether town i has records of direct ruler taxes in year t, and (C) the inverse hyperbolic sine of military personnel born in town i active in year t. Standard errors are clustered at the town level.

Table B.2: Troop Exposure and Capable Autocracy (Territory Fixed Effects)

	Parliament Eliminated (1)	Ruler Taxes (2)	Military Personnel (3)
Troop Exposure	0.0911***	0.0638***	0.0325***
	(0.0132)	(0.0129)	(0.0071)
R^2	0.68	0.59	0.38
Observations	646,700	422,820	646,700
Number of Towns	2,230	1,458	2,230
Outcome Mean	0.16	0.07	0.03
Outcome Def.	(0/1)	(0/1)	(ihs)
Town FEs	✓	✓	✓
Year FEs	✓	✓	✓
Cluster	Town	Town	Town
Territory FEs	✓	✓	✓

Note This table presents results of estimating equation (3), including territory fixed effects. Observations are at the town-year level, with the number of towns indicated in the table. The sample comprises 290 years. The dependent variables are (1) a binary variable of whether the parliament that represented town i has been eliminated in year t, (2) a binary variable of whether town i has a record of direct ruler taxes in year t, (3) the inverse hyperbolic sine of military personnel born in town i active in year t. Standard errors are clustered at the town level. *, **, and *** denote significance on the 10 percent, 5 percent, and 1 percent level, respectively.

Table B.3: Troop Exposure (Balance, Matched Sample)

	Troop Exposure				
	(1)	(2)	(3)	(4)	(5)
Distance Coast (std.)	-0.0567				-0.0581
	(0.0636)				(0.0653)
Latitude	-0.0606				-0.0592
	(0.0411)				(0.0436)
Longitude	0.0083				0.0135
	(0.0121)				(0.0132)
Fortified in 1618		0.1256			0.1486*
		(0.0795)			(0.0822)
On Trade Route in 1618			-0.0278		-0.0367
			(0.0689)		(0.0703)
Fiscal Chamber in 1618			0.0474		0.0610
			(0.0616)		(0.0621)
Ruggedness (std.)			0.0301		0.0131
			(0.0333)		(0.0436)
Distance River (std.)			-0.0135		-0.0118
			(0.0328)		(0.0366)
Agricultural Suitability (std.)			-0.0258		-0.0139
			(0.0326)		(0.0347)
Markets 1600-1618 (std.)				0.0256	0.0268
				(0.0285)	(0.0280)
Public Construction 1600-1618 (std.)				0.0193	0.0182
				(0.0292)	(0.0295)
Private Construction 1600-1618 (std.)				0.0036	-0.0003
				(0.0281)	(0.0287)
Observations	1,726	1,726	1,726	1,726	1,726

Note This table presents results of estimating the equation $Treated_i = X_i\beta + \varepsilon_i$, with X_i denoting covariates, in a Probit model. Observations are at the town level. The sample is a matched sample as described in Appendix Section B.I. The number of towns is indicated in the table. The dependent variable is an indicator of whether a town was exposed to troops during the Thirty Years' War. Standard errors are robust. *, **, and *** denote significance on the 10 percent, 5 percent, and 1 percent level, respectively.

Figure B.2: Troop Exposure (Balance, Full Sample and Matched Sample)

Note This graph shows the balance of pre-war observables with respect to the treatment, in the full (white dots) and matched (black dots) sample. The matched sample is obtained via Probit nearest-neighbor matching using distance to the coast (std.), latitude, longitude, an indicator of whether a town was fortified in 1618, an indicator of whether a town was on a trade route in 1618, an indicator of whether a town was in the jurisdiction of a fiscal Chamber in 1618, ruggedness (std.), distance to the closest navigable river (std.), agricultural suitability (std.), the number of markets added in 1600-1618 (std.), and public and private construction events 1600-1618 (std.).

Table B.4: Troop Exposure and Capable Autocracy (Nearest-Neighbor Matching)

	Parliament Eliminated (1)	Ruler Taxes (2)	Military Personnel (3)
Troop Exposure	0.1061***	0.0553***	0.0237***
	(0.0163)	(0.0138)	(0.0077)
R^2	0.57	0.55	0.36
Observations	500,540	322,190	500,540
Matched Sample	✓	✓	✓
Number of Towns	1,726	1,111	1,726
Outcome Mean	0.17	0.07	0.03
Outcome Def.	(0/1)	(0/1)	(ihs)
Town FEs	✓	✓	✓
Year FEs	✓	✓	✓
Cluster	Town	Town	Town

Note This table presents results of estimating equation (3), in a matched sample. Observations are at the town-year level, with the number of towns indicated in the table. The sample comprises 290 years. The dependent variables are (1) a binary variable of whether the parliament that represented town i has been eliminated in year t, (2) a binary variable of whether town i has a record of direct ruler taxes in year t, and (3) the inverse hyperbolic sine of military personnel born in town i active in year t. The matched sample is obtained via Probit distance nearest-neighbor matching using all town covariates in Table B.3. Standard errors are clustered at the town level. *, **, and *** denote significance on the 10 percent, 5 percent, and 1 percent level, respectively.

Table B.5: Troop Exposure and Capable Autocracy (Full Matching)

	Parliament Eliminated (1)	Ruler Taxes (2)	Military Personnel (3)
Troop Exposure	0.1123***	0.0520***	0.0281***
	(0.0189)	(0.0152)	(0.0075)
R^2	0.57	0.59	0.35
Observations	646,700	422,820	646,700
Matched Sample	✓	✓	✓
Number of Towns	2,230	1,458	2,230
Outcome Mean	0.16	0.07	0.03
Outcome Def.	(0/1)	(0/1)	(ihs)
Town FEs	✓	✓	✓
Year FEs	✓	✓	✓
Cluster	Town	Town	Town

Note This table presents results of estimating equation (3), in a matched sample. Observations are at the town-year level, with the number of towns indicated in the table. The sample comprises 290 years. The dependent variables are (1) a binary variable of whether the parliament that represented town i has been eliminated in year t. (2) a binary variable of whether town i has a record of direct ruler taxes in year t, and (3) the inverse hyperbolic sine of military personnel born in town i active in year t. The matched sample is obtained via Probit distance full matching using all covariates in Table B.3. Standard errors are clustered at the town level. *, **, and *** denote significance on the 10 percent, 5 percent, and 1 percent level, respectively.

Table B.6: Troop Exposure and Capable Autocracy (Euclidean Distance Matching)

	Parliament Eliminated (1)	Ruler Taxes (2)	Military Personnel (3)
Troop Exposure	0.0955***	0.0502***	0.0282***
	(0.0163)	(0.0142)	(0.0075)
R^2	0.57	0.57	0.38
Observations	500,540	300,730	500,540
Matched Sample	✓	✓	✓
Number of Towns	1,726	1,037	1,726
Outcome Mean	0.18	0.08	0.03
Outcome Def.	(0/1)	(0/1)	(ihs)
Town FEs	✓	✓	✓
Year FEs	✓	✓	✓
Cluster	Town	Town	Town

Note This table presents results of estimating equation (3), in a matched sample. Observations are at the town-year level, with the number of towns indicated in the table. The sample comprises 290 years. The dependent variables are (1) a binary variable of whether the parliament that represented town i has been eliminated in year t, (2) a binary variable of whether town i has a record of direct ruler taxes in year t, and (3) the inverse hyperbolic sine of military personnel born in town i active in year t. The matched sample is obtained via Euclidean distance nearest-neighbor matching using all covariates in Table B.3. Standard errors are clustered at the town level. *, **, and *** denote significance on the 10 percent, 5 percent, and 1 percent level, respectively.

Table B.7: Troop Exposure and Capable Autocracy (Non-Fortified)

	Parliament Eliminated (1)	Ruler Taxes (2)	Military Personnel (3)
Troop Exposure	0.1067***	0.0561***	0.0138**
	(0.0162)	(0.0132)	(0.0063)
R^2	0.56	0.58	0.32
Observations	551,000	353,220	551,000
Number of Towns	1,900	1,218	1,900
Outcome Mean	0.16	0.06	0.01
Outcome Def.	(0/1)	(0/1)	(ihs)
Town FEs	✓	✓	✓
Year FEs	✓	✓	✓
Cluster	Town	Town	Town

Note This table presents results of estimating equation (3), in a sample that omits fortified towns as of 1618. Observations are at the town-year level, with the number of towns indicated in the table. The sample comprises 290 years. The dependent variables are (1) a binary variable of whether the parliament that represented town i has been eliminated in year t, (2) a binary variable of whether town i has a record of direct ruler taxes in year t, and (3) the inverse hyperbolic sine of military personnel born in town i active in year t. Standard errors are clustered at the town level. *, **, and *** denote significance on the 10 percent, 5 percent, and 1 percent level, respectively.

Table B.8: Troop Exposure and Capable Autocracy (Not in Swedish Plans)

	Parliament Eliminated (1)	Ruler Taxes (2)	Military Personnel (3)
Troop Exposure	0.1079***	0.0674***	0.0160**
	(0.0153)	(0.0136)	(0.0062)
R^2	0.56	0.58	0.33
Observations	602,910	390,340	602,910
Number of Towns	2,079	1,346	2,079
Outcome Mean	0.16	0.07	0.02
Outcome Def.	(0/1)	(0/1)	(ihs)
Town FEs	✓	✓	✓
Year FEs	✓	✓	✓
Cluster	Town	Town	Town

Note This table presents results of estimating equation (3), in a sample that omits towns featured in Swedish military plans. Observations are at the town-year level, with the number of towns indicated in the table. The sample comprises 290 years. The dependent variables are (1) a binary variable of whether the parliament that represented town i has been eliminated in year t, (2) a binary variable of whether town i has a record of direct ruler taxes in year t, and (3) the inverse hyperbolic sine of military personnel born in town i active in year t. Standard errors are clustered at the town level. *, **, and *** denote significance on the 10 percent, 5 percent, and 1 percent level, respectively.

Table B.9: Troop Exposure and Parliament Elimination (Constituencies)

	Parliament Eliminated (1)	(2)
Troop Exposure	0.3943***	0.3499***
	(0.1057)	(0.1045)
R^2	0.58	0.61
Observations	37,120	37,120
Number of Constituencies	128	128
Outcome Mean	0.16	0.16
Constituency FEs	✓	✓
Year FEs	✓	✓
Controls		✓

Note This table presents results of estimating equation (3), at the level of parliamentary constituencies. The dependent variable is a binary variable of whether the parliament in constituency j has been eliminated in year t. The independent variable is the share of towns in the parliamentary constituency j that were exposed to troops during the Thirty Years' War. Observations are at the constituency-year level, with the number of constituencies indicated in the table. The sample comprises 290 years. Standard errors are clustered at the level of constituencies. *, **, and *** denote significance on the 10 percent, 5 percent, and 1 percent level, respectively.

Table B.10: Troop Exposure and Parliament Elimination (Cox Hazard Model)

	Parliament Eliminated			
	(1)	(2)	(3)	(4)
Troop Exposure	0.488***	0.555***	0.555***	0.555***
	(0.0648)	(0.0613)	(0.0613)	(0.185)
Observations	542,637	542,637	542,637	542,637
Number of Towns	2230	2230	2230	2230
Controls		✓	✓	✓
Standard Errors	Robust	Robust	Town	Constituency

Note This table presents results of estimating equation (3), using a Cox Hazard model. Observations are at the town-year level, with the number of towns indicated in the table. The sample comprises 290 years. The dependent variable is a binary variable of whether the parliament that represented town i has been eliminated in year t. Standard errors are clustered at the level of towns. *, **, and *** denote significance on the 10 percent, 5 percent, and 1 percent level, respectively.

Table B.11: Troop Exposure and Parliament Elimination (Cox Hazard Model, Constituencies)

	Parliament Eliminated		
	(1)	(2)	(3)
Troop Exposure	1.355***	1.589***	1.589***
	(0.436)	(0.453)	(0.453)
Observations	44,142	44,142	44,142
Number of Constituencies	172	172	172
Controls		✓	✓
Standard Errors	Robust	Robust	Constituency

Note This table presents results of estimating equation (3), using a Cox Hazard model at the level of parliamentary constituencies. The dependent variable is a binary variable of whether the parliament in constituency j has been eliminated in year t. The independent variable is the share of towns in the parliamentary constituency j that were exposed to troops during the Thirty Years' War. Observations are at the constituency-year level, with the number of constituencies indicated in the table. The sample comprises 290 years. Standard errors are clustered at the level of constituencies. *, **, and *** denote significance on the 10 percent, 5 percent, and 1 percent level, respectively.

Table B.12: Troop Exposure and Parliament Elimination (Standard Errors)

	Parliament Eliminated				
	(1)	(2)	(3)	(4)	(5)
Troop Exposure	0.1068***	0.1068***	0.1068***	0.1068***	0.1068***
	(0.0147)	(0.0357)	(0.0300)	(0.0394)	(0.0312)
Standard-Errors	City	Constituency	50km	100km	200km
R^2	0.56	0.56	0.56	0.56	0.56
Observations	646,700	646,700	646,700	646,700	646,700
Number of Towns	2,230	2,230	2,230	2,230	2,230
Outcome Mean	0.16	0.16	0.16	0.16	0.16
Town FEs	✓	✓	✓	✓	✓
Year FEs	✓	✓	✓	✓	✓

Note This table presents results of estimating equation (3), using different standard errors. Observations are at the town-year level, with the number of towns indicated in the table. The sample comprises 290 years. The dependent variable is a binary variable of whether the parliament that represented town i has been eliminated in year t. Standard errors are clustered at the level of (1) towns, (2) constituencies, or Conley standard errors with a cutoff of (3) 50km, (4) 100km, or (5) 200km. *, **, and *** denote significance on the 10 percent, 5 percent, and 1 percent level, respectively.

Table B.13: Troop Exposure and Parliament Elimination (Constituencies, Standard Errors)

	Parliament Eliminated			
	(1)	(2)	(3)	(4)
Troop Exposure	0.3943***	0.3943***	0.3943***	0.3943***
	(0.1057)	(0.1106)	(0.1094)	(0.1508)
Standard-Errors	Constituency	50km	100km	200km
R^2	0.58	0.58	0.58	0.58
Observations	37,120	37,120	37,120	37,120
Number of Constituencies	128	128	128	128
Outcome Mean	0.16	0.16	0.16	0.16
Constituency FEs	✓	✓	✓	✓
Year FEs	✓	✓	✓	✓

Note This table presents results of estimating equation (3), at the level of parliamentary constituencies. The dependent variable is a binary variable of whether the parliament in constituency j has been eliminated in year t. The independent variable is the share of towns in the parliamentary constituency j that were exposed to troops during the Thirty Years' War. Observations are at the constituency-year level, with the number of constituencies indicated in the table. The sample comprises 290 years. Standard errors are clustered at the level of (1) constituencies, or Conley standard errors with a cutoff of (2) 50km, (3) 100km, or (4) 200km. *, **, and *** denote significance on the 10 percent, 5 percent, and 1 percent level, respectively.

Table B.14: Troop Exposure and Ruler Taxes (Standard Errors)

	Ruler Taxes				
	(1)	(2)	(3)	(4)	(5)
Troop Exposure	0.0614***	0.0614***	0.0614***	0.0614**	0.0614**
	(0.0128)	(0.0171)	(0.0211)	(0.0287)	(0.0304)
Standard-Errors	City	Constituency	50km	100km	200km
R^2	0.57	0.57	0.57	0.57	0.57
Observations	422,820	422,820	422,820	422,820	422,820
Number of Towns	1,458	1,458	1,458	1,458	1,458
Outcome Mean	0.07	0.07	0.07	0.07	0.07
Town FEs	✓	✓	✓	✓	✓
Year FEs	✓	✓	✓	✓	✓

Note This table presents results of estimating equation (3), using different standard errors. Observations are at the town-year level, with the number of towns indicated in the table. The sample comprises 290 years. The dependent variable is a binary variable of whether town i has a record of direct ruler taxes in year t. Standard errors are clustered at the level of (1) towns, (2) constituencies, or Conley standard errors with distance cutoffs as indicated in columns (3-5). *, **, and *** denote significance on the 10 percent, 5 percent, and 1 percent level, respectively.

Table B.15: Troop Exposure and Military Personnel (Standard Errors)

	Military Personnel				
	(1)	(2)	(3)	(4)	(5)
Troop Exposure	0.0277***	0.0277***	0.0277***	0.0277***	0.0277**
	(0.0071)	(0.0083)	(0.0076)	(0.0085)	(0.0109)
Standard-Errors	City	Constituency	50km	100km	200km
R^2	0.36	0.36	0.36	0.36	0.36
Observations	646,700	646,700	646,700	646,700	646,700
Number of Towns	2,230	2,230	2,230	2,230	2,230
Outcome Mean	0.03	0.03	0.03	0.03	0.03
Town FEs	✓	✓	✓	✓	✓
Year FEs	✓	✓	✓	✓	✓

Note This table presents results of estimating equation (3), using different standard errors. Observations are at the town-year level, with the number of towns indicated in the table. The sample comprises 290 years. The dependent variable is the inverse hyperbolic sine of military personnel born in town i active in year t, and Standard errors are clustered at the level of (1) towns, (2) constituencies, or Conley standard errors with distance cutoffs as indicated in columns (3-5). *, **, and *** denote significance on the 10 percent, 5 percent, and 1 percent level, respectively.

Table B.16: Troop Exposure and Military Personnel (ihs Robustness)

	Military Personnel					
	(1)	(2)	(3)	(4)	(5)	(6)
Troop Exposure	0.0277***	0.0127***	0.0146***	0.0316***	0.0693***	0.0188***
	(0.0071)	(0.0042)	(0.0044)	(0.0080)	(0.0173)	(0.0049)
R^2	0.36	0.32	0.34	0.36	0.34	0.31
Observations	646,700	646,700	646,700	646,700	646,700	646,700
Number of Towns	2,230	2,230	2,230	2,230	2,230	2,230
Outcome Mean	0.0262	0.0065	-0.0911	-0.9701	-2.923	0.0233
Town FEs	✓	✓	✓	✓	✓	✓
Year FEs	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓
Cluster	Town	Town	Town	Town	Town	Town
LHS Definition	ihs	x=0	x=0.1	x=1	x=3	any

Note This table presents results of estimating equation (3), using different outcome definitions. Observations are at the town-year level, with the number of towns indicated in the table. The sample comprises 290 years. The dependent variable is the military personnel born in town i active in year t. Variations in the transformation of the dependent variable are as noted in the column description. *, **, and *** denote significance on the 10 percent, 5 percent, and 1 percent level, respectively.

Table B.17: Troop Exposure and Capable Autocracy (Exact Timing)

	Parliament Eliminated	Ruler Taxes	Military Personnel
	(1)	(2)	(3)
Troop Exposure	0.1104***	0.0613***	0.0277***
	(0.0153)	(0.0128)	(0.0071)
R^2	0.56	0.57	0.36
Observations	646,700	422,820	646,700
Number of Towns	2,230	1,458	2,230
Outcome Mean	0.16	0.07	0.03
Outcome Def.	(0/1)	(0/1)	(ihs)
Town FEs	✓	✓	✓
Year FEs	✓	✓	✓
Cluster	Town	Town	Town

Note This table presents results of estimating equation (3), in sample that includes only towns with precisely dated troop exposure (or no troop exposure). The independent variable is an indicator that takes the value of 1 after the first exposure event in a town has been documented. Observations are at the town-year level, with the number of towns indicated in the table. The sample comprises 290 years. The dependent variables are (1) a binary variable of whether the parliament that represented town i has been eliminated in year t, (2) a binary variable of whether town i has a record of direct ruler taxes in year t, and (3) the inverse hyperbolic sine of military personnel born in town i active in year t. Standard errors are clustered at the town level. *, **, and *** denote significance on the 10 percent, 5 percent, and 1 percent level, respectively.

B.II Unadjusted Instrument Robustness

This section is concerned with the robustness of the findings in Panel A of Table II. In Appendix Table B.18, we demonstrate that our results are robust to including flexible controls.[5]

Second, we demonstrate that our results hold up when considering variation within territories: Appendix Table B.19 additionally includes territory fixed effects. This does not qualitatively affect our results.

Appendix Table B.20 leans further into the interpretation of the instrument as identifying incidentally exposed towns: We narrow the sample to exclude all fortified towns, and again, results are robust.

Table B.18: Troop Exposure and Capable Autocracy (Unadjusted Instrument, Controls)

	Parliament Eliminated (1)	Ruler Taxes (2)	Military Personnel (3)
Troop Exposure	0.5350***	0.3365***	0.3655***
	(0.1330)	(0.1193)	(0.0971)
R^2	0.49	0.51	0.17
Observations	646,700	422,820	646,700
IV	✓	✓	✓
Number of Towns	2,230	1,458	2,230
Outcome Mean	0.16	0.07	0.03
Outcome Def.	(0/1)	(0/1)	(ihs)
Town FEs	✓	✓	✓
Year FEs	✓	✓	✓
Cluster	Town	Town	Town
Controls	✓	✓	✓

Note This table presents results of estimating equation (3), using an instrumental variable based on the distance of a town to the closest campaign least-cost path. Observations are at the town-year level, with the number of towns indicated in the table. The sample comprises 290 years. The dependent variables are (1) a binary variable of whether the parliament that represented town i has been eliminated in year t, (2) a binary variable of whether town i has a record of direct ruler taxes in year t, and (3) the inverse hyperbolic sine of military personnel born in town i active in year t. Controls are distance to the coast (std.), latitude, longitude, an indicator of whether a town was fortified in 1618, an indicator of whether a town was on a trade route in 1618, an indicator of whether a town was in the jurisdiction of a fiscal Chamber in 1618, ruggedness (std.), distance to the closest navigable river (std.), agricultural suitability (std.), the number of markets added in 1600-1618 (std.), and public and private construction events 1600-1618 (std.), all interacted with $Post1618_{it}$. Standard errors are clustered at the town level. *, **, and *** denote significance on the 10 percent, 5 percent, and 1 percent level, respectively.

[5] Controls are the same as in Appendix Table B.1.

Table B.19: Troop Exposure and Capable Autocracy (Unadjusted Instrument, Territory Fixed Effects)

	Parliament Eliminated (1)	Ruler Taxes (2)	Military Personnel (3)
Troop Exposure	0.3474***	0.2826***	0.3480***
	(0.1029)	(0.0997)	(0.0882)
R^2	0.65	0.55	0.22
Observations	646,700	422,820	646,700
IV	✓	✓	✓
Number of Towns	2,230	1,458	2,230
Outcome Mean	0.16	0.07	0.03
Outcome Def.	(0/1)	(0/1)	(ihs)
Town FEs	✓	✓	✓
Year FEs	✓	✓	✓
Cluster	Town	Town	Town
Territory FEs	✓	✓	✓

Note This table presents results of estimating equation (3), using an instrumental variable based on the distance of a town to the closest campaign least-cost path and including territory fixed effects. Observations are at the town-year level, with the number of towns indicated in the table. The sample comprises 290 years. The dependent variables are (1) a binary variable of whether the parliament that represented town i has been eliminated in year t, (2) a binary variable of whether town i has a record of direct ruler taxes in year t, and (3) the inverse hyperbolic sine of military personnel born in town i active in year t. Standard errors are clustered at the town level. *, **, and *** denote significance on the 10 percent, 5 percent, and 1 percent level, respectively.

Table B.20: Troop Exposure and Capable Autocracy (Unadjusted Instrument, Excluding Fortified Towns)

	Parliament Eliminated (1)	Ruler Taxes (2)	Military Personnel (3)
Troop Exposure	0.7023***	0.3023**	0.2016**
	(0.2138)	(0.1206)	(0.0877)
R^2	0.41	0.52	0.22
Observations	551,000	353,220	551,000
IV	✓	✓	✓
Number of Towns	1,900	1,218	1,900
Outcome Mean	0.16	0.06	0.01
Outcome Def.	(0/1)	(0/1)	(ihs)
Town FEs	✓	✓	✓
Year FEs	✓	✓	✓
Cluster	Town	Town	Town

Note This table presents results of estimating equation (3), using an instrumental variable based on the distance of a town to the closest campaign least-cost path. The data are subset to exclude fortified towns. Observations are at the town-year level, with the number of towns indicated in the table. The sample comprises 290 years. The dependent variables are (1) a binary variable of whether the parliament that represented town i has been eliminated in year t, (2) a binary variable of whether town i has a record of direct ruler taxes in year t, and (3) the inverse hyperbolic sine of military personnel born in town i active in year t. Standard errors are clustered at the town level. *, **, and *** denote significance on the 10 percent, 5 percent, and 1 percent level, respectively.

B.III Borusyak and Hull (2023)

Notation

As our baseline causal model, consider a simplified (time-invariant) version of regression equation (3):

$$y_i = \beta x_i + \varepsilon_i$$

with $i = 1, \ldots, N$ for the N towns in the Holy Roman Empire, $x_i = TroopPresence_i$ an indicator of the presence of troops in town i during the Thirty Years' War, and $y_i = RulerTaxes_i$ an indicator of the levying of ruler taxes. We aim to estimate β.[6]

Our instrument is, for each town, the inverse hyperbolic sine of the distance to the closest campaign least-cost path. This instrument is computed according to a known, deterministic function:

$$z_i = \tau\left(CampaignPaths; Loc_i\right)$$

with components:

- $CampaignPaths = (Realized_k)_{k=1}^{K}$: a vector of all realized and counterfactual campaigns, with $Realized_k$ an indicator of whether campaign k was realized.
- Loc_i: the geographic loction of town i.
- $\tau\left(CampaignPaths, Loc_i\right)$: a function that proceeds in four steps.
    1. For each realized campaign (i.e. each nonzero element in $CampaignPaths$), calculate least-cost paths that connect fortified towns.
    2. Collect all realized campaigns in one spatial line object.
    3. Calculate the minimum distance of town i to this spatial line object.
    4. Take the inverse hyperbolic sine of this distance.

In the notation of Borusyak and Hull (2023), the function $\tau\left(CampaignPaths; Loc_i\right)$ can thus be written as $f\left(g; w_i\right)$, with

- $f() = \tau()$ a common function,
- $g = CampaignPaths$ a vector of shocks, and
- $w_i = Loc_i$ a unit-specific measure of exposure.

The dimension of g is $K = |R| + |C|$ with R the set of realized campaigns and C the set of counterfactual campaigns. We describe the construction of these sets in Section III.II.

[6] Extending the argument to our other outcomes, or to a panel setting with $t = 1500, \ldots, 1789$, is straightforward.

Assumptions

Drawing on this notation, we discuss the three key assumptions in Borusyak and Hull (2023):

- Assumption 1 (Shock exogeneity): $g \perp \varepsilon | w$.

  This assumption states that there is an as-good-as-random component to the decision of troop leaders to take one route but not the other and that this decision is orthogonal to town unobservables conditional on town locations. Motivated by the historical evidence in Section II.II, which highlights that campaign paths were not deterministic and that geographical determinants were key to the strategy of invading armies, this appears plausible.[7]

- Assumption 2. (Known assignment process): $G(g|w)$ is known in the support of w.

  Analogous to the approach in Borusyak and Hull (2023), Section 4, we apply a clustered permutation of campaign paths: At each juncture, we assume $G(g|w)$ to be uniform over all possible continuations of the campaign path.[8]

- Assumption 3. (Weak mutual dependence): $\mathbb{E}_{P_N}\left[\frac{1}{N^2}\sum_{i,j}|\mathrm{Cov}_{P_N}\left[\tilde{z}_i, \tilde{z}_j \mid w\right]|\right] \to 0$.

  This requires the shocks to induce 'enough' cross-sectional variation in the recentered instrument. Our setting has a large number of shocks that traverse different geographic regions and hence impact different sets of towns. The impact of any finite set of shocks on the covariance of the recentered instrument is hence small, and mutual dependence low.

If these three assumptions are satisfied, estimates using the recentered instrument are consistent for β (Proposition 1).

Implementation Algorithm

We use the fact that $z_i = \tau\left(CampaignPaths; Loc_i\right)$ together with Assumption 2 to obtain counterfactual values of the instrument.

Our algorithm proceeds in three steps:

1. **Shock Distribution.** We use $G(g|w)$ to obtain different realizations of the vector $g = CampaignPaths$. We sample campaigns uniformly at random from the junctures at which possible campaign routes branch off. We repeat this process 1,000 times.

2. **Instrument Function.** We plug each realization of $CampaignPaths$ into $\tau()$ to compute the instrument. For each campaign that has $Realized_k = 1$ in the counterfactual, we fit a least-cost path between the start node, end node, and fortified town, as described in Section III.II. We connect these points using least-cost paths and calculate the minimum distance of each town to the closest least-cost path.

3. **Expected Instrument.** We average the resulting instrument across each realization of $CampaignPaths$ to obtain the expected instrument of each town.

[7] Strictly speaking, from a 'design-based' perspective (considering the sample to be fixed and the course of the war, to be stochastic), Assumption 1 is satisfied by default (Borusyak and Hull, 2023).

[8] In Borusyak and Hull (2023), this permutation is applied within clusters of similar planned lines in the Chinese high-speed rail network. We discuss the robustness of our results to this assumption below.

B.IV Recentered Instrument Robustness

Overall Robustness

This section is concerned with the robustness of the findings in Panel B of Table II. In Appendix Table B.21, we demonstrate that our results are robust to including flexible controls.[9]

Second, we demonstrate that our results hold up when considering variation within territories: Appendix Table B.22 additionally includes territory fixed effects. This does not qualitatively affect our results.

Appendix Table B.23 shows that results are robust when narrowing the sample to exclude all fortified towns.

Table B.21: Troop Exposure and Capable Autocracy (Recentered Instrument, Controls)

	Parliament Eliminated (1)	Ruler Taxes (2)	Military Personnel (3)
Troop Exposure	0.6426***	0.6738**	0.4342**
	(0.2408)	(0.2933)	(0.2192)
R^2	0.44	0.25	0.09
Observations	646,700	422,820	646,700
Recentered IV	✓	✓	✓
Number of Towns	2,230	1,458	2,230
Outcome Mean	0.16	0.07	0.03
Outcome Def.	(0/1)	(0/1)	(ihs)
Town FEs	✓	✓	✓
Year FEs	✓	✓	✓
Cluster	Town	Town	Town
Controls	✓	✓	✓

Note This table presents results of estimating equation (3), using a recentered instrumental variable based on the distance of a town to the closest campaign least-cost path. Observations are at the town-year level, with the number of towns indicated in the table. The sample comprises 290 years. The dependent variables are (1) a binary variable of whether the parliament that represented town i has been eliminated in year t, (2) a binary variable of whether town i has a record of direct ruler taxes in year t, and (3) the inverse hyperbolic sine of military personnel born in town i active in year t. Controls are distance to the coast (std.), latitude, longitude, an indicator of whether a town was fortified in 1618, an indicator of whether a town was on a trade route in 1618, an indicator of whether a town was in the jurisdiction of a fiscal Chamber in 1618, ruggedness (std.), distance to the closest navigable river (std.), agricultural suitability (std.), the number of markets added in 1600-1618 (std.), and public and private construction events 1600-1618 (std.), all interacted with $Post1618_{it}$. Standard errors are clustered at the town level. *, **, and *** denote significance on the 10 percent, 5 percent, and 1 percent level, respectively.

[9] Controls are the same as in Appendix Table B.1.

Table B.22: Troop Exposure and Capable Autocracy (Recentered Instrument, Territory Fixed Effects)

	Parliament Eliminated (1)	Ruler Taxes (2)	Military Personnel (3)
Troop Exposure	0.5776***	0.6355**	0.4165**
	(0.2130)	(0.2665)	(0.2086)
R^2	0.59	0.33	0.15
Observations	646,700	422,820	646,700
Recentered IV	✓	✓	✓
Number of Towns	2,230	1,458	2,230
Outcome Mean	0.16	0.07	0.03
Outcome Def.	(0/1)	(0/1)	(ihs)
Town FEs	✓	✓	✓
Year FEs	✓	✓	✓
Cluster	Town	Town	Town
Territory FEs	✓	✓	✓

Note This table presents results of estimating equation (3), using an instrumental variable based on the distance of a town to the closest campaign least-cost path and including territory fixed effects. Observations are at the town-year level, with the number of towns indicated in the table. The sample comprises 290 years. The dependent variables are (1) a binary variable of whether the parliament that represented town i has been eliminated in year t, (2) a binary variable of whether town i has a record of direct ruler taxes in year t, and (3) the inverse hyperbolic sine of military personnel born in town i active in year t. Standard errors are clustered at the town level. *, **, and *** denote significance on the 10 percent, 5 percent, and 1 percent level, respectively.

Table B.23: Troop Exposure and Capable Autocracy (Recentered Instrument, Excluding Fortified Towns)

	Parliament Eliminated (1)	Ruler Taxes (2)	Military Personnel (3)
Troop Exposure	0.6660**	0.5960***	0.2362**
	(0.3116)	(0.1893)	(0.0817)
R^2	0.43	0.29	0.17
Observations	551,000	353,220	551,000
Recentered IV	✓	✓	✓
Number of Towns	1,900	1,218	1,900
Outcome Mean	0.16	0.06	0.01
Outcome Def.	(0/1)	(0/1)	(ihs)
Town FEs	✓	✓	✓
Year FEs	✓	✓	✓
Cluster	Town	Town	Town

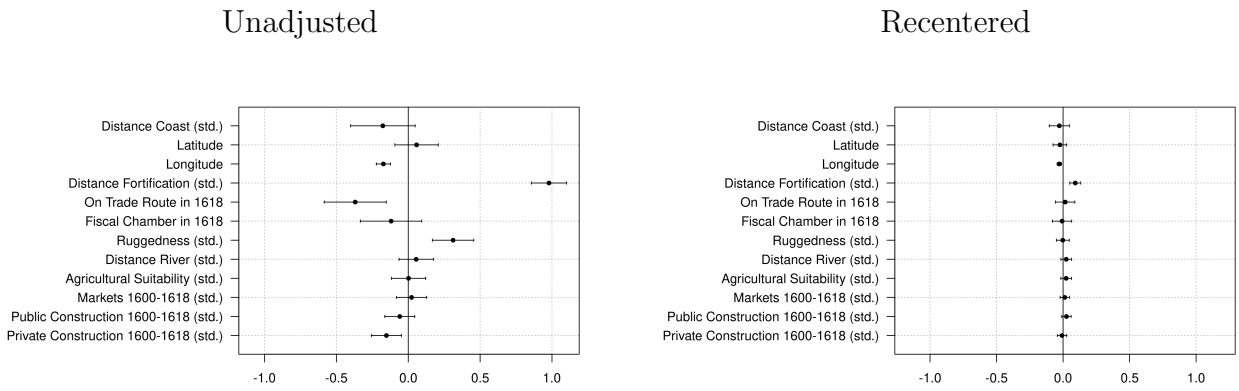
Note This table presents results of estimating equation (3), using an instrumental variable based on the distance of a town to the closest campaign least-cost path. The data are subset to exclude fortified towns. Observations are at the town-year level, with the number of towns indicated in the table. The sample comprises 290 years. The dependent variables are (1) a binary variable of whether the parliament that represented town i has been eliminated in year t, (2) a binary variable of whether town i has a record of direct ruler taxes in year t, and (3) the inverse hyperbolic sine of military personnel born in town i active in year t. Standard errors are clustered at the town level. *, **, and *** denote significance on the 10 percent, 5 percent, and 1 percent level, respectively.

Specification Tests

We follow Section 4 in Borusyak and Hull (2023) to conduct specification tests. In Appendix Figure B.3, we show that recentering the instrument drastically reduces the imbalance in covariates across treatment and control groups, conditional on the instrument. We regress the unadjusted (left figure) and recentered (right figure) instrument on pre-war observables as listed in the respective figure.

Additionally, the expected instrument is not predictive of the recentered instrument: in a regression of the recentered instrument on the expected instrument, the R^2 is 0.01. (In contrast, regressing the unadjusted instrument on the expected instrument yields an R^2 of 0.92.)

Figure B.3: Balance of Observables with Respect to the Instrument (Unadjusted and Recentered)



Note The figure shows estimated coefficients and 95% confidence intervals from a regression of the unadjusted (left figure) and recentered (right figure) instrument on pre-war observables.

Robustness to Assignment Process

Our data collection attempts to identify possible choices between campaign paths that were narrow from the perspective of contemporaries and military historians. Nevertheless, the context requires us to make assumptions about $G(g|w)$. In our baseline specification, we apply a clustered permutation of campaign paths: At each juncture, we assume $G(g|w)$ to be uniform over all possible continuations of the campaign path. Appendix Table B.24 shows results from an instrumental variables regression using the unadjusted instrument, including the expected treatment as an exogenous control. Results are very similar to Panel B of Table II.

Next, we show robustness to the choice of $G(g, w)$. In line with a suggestion in Borusyak and Hull (2023), we control for an alternative expected instrument. Instead of assuming a uniform probability of the choice of campaign path at each juncture node, we estimate the probability of realizing a campaign path based on campaign observables, via maximum likelihood estimation in 10-fold cross-validation. Appendix Table B.25 shows results when including this alternative expected instrument; results are robust.

Table B.24: Troop Exposure and Capable Autocracy (Expected Instrument)

	Parliament Eliminated (1)	Ruler Taxes (2)	Military Personnel (3)
Troop Exposure	0.6782**	0.7605**	0.4569*
	(0.3139)	(0.3779)	(0.2441)
R^2	0.43	0.16	0.06
Observations	646,700	422,820	646,700
IV	✓	✓	✓
Number of Towns	2,230	1,458	2,230
Outcome Mean	0.16	0.07	0.03
Outcome Def.	(0/1)	(0/1)	(ihs)
Town FEs	✓	✓	✓
Year FEs	✓	✓	✓
Cluster	Town	Town	Town
Controls	✓	✓	✓

Note This table presents results of estimating equation (3), using an instrumental variable based on the distance of a town to the closest campaign least-cost path. The expected instrument is included as a control. Observations are at the town-year level, with the number of towns indicated in the table. The sample comprises 290 years. The dependent variables are (1) a binary variable of whether the parliament that represented town i has been eliminated in year t, (2) a binary variable of whether town i has a record of direct ruler taxes in year t, and (3) the inverse hyperbolic sine of military personnel born in town i active in year t. Standard errors are clustered at the town level. *, **, and *** denote significance on the 10 percent, 5 percent, and 1 percent level, respectively.

Table B.25: Troop Exposure and Capable Autocracy (Alternative Expected Instrument)

	Parliament Eliminated (1)	Ruler Taxes (2)	Military Personnel (3)
Troop Exposure	0.6350**	0.7748**	0.4590**
	(0.2995)	(0.3889)	(0.2335)
R^2	0.45	0.14	0.05
Observations	646,700	422,820	646,700
IV	✓	✓	✓
Number of Towns	2,230	1,458	2,230
Outcome Mean	0.16	0.07	0.03
Outcome Def.	(0/1)	(0/1)	(ihs)
Town FEs	✓	✓	✓
Year FEs	✓	✓	✓
Cluster	Town	Town	Town
Controls	✓	✓	✓

Note This table presents results of estimating equation (3), using an instrumental variable based on the distance of a town to the closest campaign least-cost path. The expected instrument is calculated through estimating the probability of realizing a campaign path based on campaign observables, via maximum likelihood estimation in 10-fold cross-validation. Observations are at the town-year level, with the number of towns indicated in the table. The sample comprises 290 years. The dependent variables are (1) a binary variable of whether the parliament that represented town i has been eliminated in year t, (2) a binary variable of whether town i has a record of direct ruler taxes in year t, and (3) the inverse hyperbolic sine of military personnel born in town i active in year t. Standard errors are clustered at the town level. *, **, and *** denote significance on the 10 percent, 5 percent, and 1 percent level, respectively.

C Historical Case Studies

This appendix provides historical case studies to support the mechanisms discussed in Sections V.I and V.II of the main text. We repeat every sentence of the main text in italics and provide relevant case studies below the sentence. Section C.I addresses the initial coordination and development of executive capacity during the war. Section C.II examines the persistence of these changes post-war. Section C.III details examples from data sources on prints, portraits, and infringement.

C.I Initial Coordination and Executive Capacity

Troops posed a credible and significant threat of violence to towns.
Troops advancing in the Upper Palatinate threatened to burn civilian houses unless they were supplied: "Thus, the quartered guests of Paul Brunner (in the Upper Palatinate) threatened around 1632 to set his house on fire if he did not provide them with adequate provisions" (Kraus, 2021, p. 273). Otherwise, soldiers resorted to violent self-provisioning: "When the monthly pay failed to arrive in 1631, the soldiers ran into the villages and forcibly provided for themselves." (Kraus, 2021, p. 273)

Since armies almost entirely depended on local resources, upkeep was a central organizational requirement once troops had advanced on a town.
War provisioning had to be local, as "centralized war financing was hardly feasible in practice" (Kraus, 2021, p. 217). By 1635, "the imperial-league army was supplied 98% from local contributions" (Kraus, 2021, p. 218). See also the footnote at the close of Section II.II for the large list of troop supplies required during exposure.

Armies could acquire resources through plunder or taxation: plunder maximized short-term consumption but devastated the local resource base, undermining future extraction and military discipline.
In Hesse, "even before the coming of troops, there were many villages who were on the margins of subsistence." (Theibault, 1995, p. 141) Fearing the erosion of discipline, "plundering by individuals was punished by death" in the Swedish army (Dodge, 1895, p. 78) and also under Wallenstein.

Taxation, by contrast, required coordination but enabled more sustainable provisioning. This suggests considerable scope for taxation to mitigate the damage caused by troop presence.
In the Swedish army, "Gustavus explicitly ordered his generals to follow Wallenstein's example of systematic contributions drawn through local tax structures, rather than ad hoc demands that caused widespread devastation." (Wilson, 2010, p. 245). Kraus (2021, p. 452) notes military leaders aimed to not devastate "the economic strength and stability of quartered regions."

Under these conditions, military leaders actively sought coordination with rulers.
Tilly, for example, strictly "ordered that commanders cooperate with territorial administrators in keeping a weekly record of which soldiers were quartered in which households" (Theibault, 1995, p. 141). During Wallenstein's campaign in Pomerania (1627), he urged the duke to provide supplies, implicitly threatening violence: "In order

to preserve better discipline and to prevent the complete ruin of the country, we amicably request that Your Grace makes arrangements to provide the troops with the necessary sustenance." (Wilson, 2010, p. 107)

Coordinating taxes with local rulers had two key advantages. First, centralized intervention was important, as effective provisioning depended on the surrounding countryside of the town.
In the town of Neumarkt, ruler-led coordination between the town and countryside was key: "In 1646, the war commissariat sent a provisions commissioner to Neumarkt, who was to request the necessary provisions from the relevant and nearby estates and then distribute them accordingly." (Kraus, 2021, p. 221) Also in Auerbach, centralized distribution was managed by a ruler official: "The Auerbach provisions office distributed in the year 1621 127,050 loaves of bread and 1,095 buckets of beer [...] The distribution was apparently handled personally by the *Kastner*." (Kraus, 2021, p. 222).

Second, rulers could serve as quicker and more legitimate intermediaries between civilian and military authorities than parliamentary bodies, which faced high transaction costs due to long planning horizons and the need to coordinate across stakeholders.
On ruler legitimacy, Kraus (2021, p. 324) notes that "princely officials were generally held in high regard not only by the local population but also by the military." Thus, "the different levels of the hierarchy within the army negotiated with the corresponding levels of the administrative hierarchy within the territory." (Theibault, 1995, p. 138) Civilian complaints speak to the inadequacy of the legislature in comparison: "As early as 1631, however, the government had considered stripping the magistrate of responsibility for distributing provisions and managing their payment. These deliberations were likewise prompted by complaints that civic officials 'had no respect from the soldiers and were unable to collect payment'." (Kraus, 2021, p. 227)

A key facilitator of such coordination was the presence of a shared military alliance between the army and the ruler: this lowered the cost of communication at all levels and gave rulers clearer incentives to provision the troops.
Kraus (2021, p. 451) notes, in the context of territories allied with the Catholic League, that "local administrators, in particular, often shared the same social background and habitus as the League's officer corps, allowing them to interact with officers on equal footing." In the vast majority of cases, thus, "successful military enterprise rested on a direct relationship with a ruler." (Parrott, 2017, p. 78)

Local populations, moreover, proved willing to cede autonomy to forestall the greater evil of military reprisals.
Kraus (2021, p. 458) notes that "the shared goal — managing the war — motivated subjects to accept, and in some cases even explicitly demand, greater influence of the princely administration in their daily lives." Thus, "over time, the influence of local administrators on urban war governance increased. By 1639, the city even complained that Kastner Rexrada, despite being commissioned by the government, 'refused to take responsibility for the contribution on behalf of the city'." (Kraus, 2021, p. 227) Local officials gave some legitimacy to the contributions: "For example, the allocation of military quarters was carried out by local offices based on constantly updated reports on population figures,

available housing capacity, and related data. Likewise, when setting the contribution rates for each billeting, the Kastner assessed the individual economic situation of the subjects. In addition, subjects could appeal to the government through the formal process of petition (*Supplikationsweg*)." (Kraus, 2021, p. 446) Thus, "the terms of the provisions ordinance were burdensome for the villagers, but they were as clear and fairly apportioned." (Theibault, 1995, p. 141)

Overall, the unprecedented size and mobility of early modern armies left little room for effective resistance once forces were en route.
Of villages in the Werra region in Hesse, Theibault (1995, p. 142) notes that "active resistance was the hardest to justify [...] the balance of forces was so unequal that such attacks were suicidal." Thus, civilians did not resist: "there is no evidence that the villagers of the Werra ever tried to fend off occupation by force [...] most villagers adopted the option of passive acceptance." (Theibault, 1995, p. 143) Instead of defense, surrender was common: after the Batte of Nördlingen in 1634, "almost all Protestant garrisons in south Germany surrendered" (Parker, 1997, p. 348).

Taken together, these findings support the relevance of a coordination-based mechanism in the expansion of executive capacity.
Given the violent scope conditions of the context, coordination with the ruler benefited all parties: "For the city, it provided an ally with greater authority over the military [...] and enforcement means such as military execution. The government thereby gained insight and intervention possibilities in municipal administration, and the military [...] obtained a court of appeal." (Kraus, 2021, p. 228). Examining the alternative hypothesis of destruction, Theibault (1995, p. 143) notes that "the primary objective of the soldiers was to find provisions, not to destroy property in the occupied territory." Similarly, Press (1988, p. 280) notes that the war "above all provoked autonomous action by the princes," rather than working through destruction.

The logistical demands of military coordination gave rise to a 'dual state' that bypassed parliamentary institutions.
Hintze (1910) highlights how, in Prussia, initially temporary war commissariates became central to enhancing executive power and circumventing estates. In Neumarkt in 1646, a provisioning commissar managed resource allocation (Kraus, 2021, p. 221), and by 1649, a *Kastner* was tasked with contribution assessments, ending municipal autonomy (Kraus, 2021, p. 246). "The entire tax-structure, civil service and local administration of the Great Elector came into being as technical sub-departments of the *Generalkriegskommissariat* [...] The Prussian bureaucracy, in other words, was born as an offshoot of the Army." (Anderson, 1979) Hintze (1910, p. 242) notes that "the Prussian administrative organization of the 18th century had its characteristic core in the commissariates," which stood "at the center of the great monarchical reforms that created the modern state."

C.II The Consolidation of Autocracy

Rulers retained the local recruitment and taxation infrastructure resulting from the war even after troops had moved on, permanently bypassing Estate consent.

"Occupying troops had imposed their own framework of collecting taxes [...] Those taxes continued after the war, as the central administration adopted the mechanisms and terminology of the troops."(Theibault, 1995, p. 195) Wilson (2010, p. 810) notes that, "The growth of military taxation eroded the Estates' role, since the burdens were often imposed without consultation. Many territorial rulers continued these taxes after 1648." In Prussia, taxes were levied unilaterally (Carsten, 1958, p. 181). Additionally, wartime administrative practices persisted: separate registers tracked arrears (Theibault, 1995, p. 195), and in Abterode, the "contribution collector" role became a permanent fixture (Theibault, 1995, p. 196).

The few surviving accounts suggest that the experience of emergency (Not) during the war prompted subjects to infer a broader necessity (Notwendigkeit) for centralized authority.
Theibault (1995, p. 195) notes that "occupying forces imposed contributions justified by 'emergency (Notdurft)' during the war," and that "the confusion and desperation underscored the necessity of some kind of protection and thus provided a basis for the revival of central rule after the war." The experience of violence drove "the acceptance of new security structures," including standing armies (Rohrschneider and Tischer, 2018; Bahlcke, 2012, p. 27). Overall, 'necessity' became an "argument to legitimize change" (Wilson et al., 2023).

A larger body of surviving evidence for this rationale is closely tied to state influence.
Samuel Pufendorf argued in 1672 that the state's purpose was "that men, by means of mutual cooperation and assistance, be safe against the harms and injuries they can and commonly do inflict on one another," asserting the sovereign's right to "force individual citizens to contribute so much of their own goods as the assumption of those expenses is deemed to require." (Pufendorf, 1672)

Rulers themselves invoked the maxim that 'necessity knows no law' and employed both visual and written propaganda to legitimize their claims to authority.
Pamphlets were key to propaganda: "The principal task of the pamphlet now consisted in the evaluation and interpretation of current events, as well as in the dissemination of religious and political propaganda." (Harms, 1985, p. 141) The court shaped public narratives through printed poratraits and pamphlets, as Bauer (1997, p. 191) notes: "Courtly publicity was primarily constituted through the production, distribution, and reception of specific printed media." Bavarian propaganda, for instance, celebrated alleged successes like the 1703 Neuburg campaign despite it having failed in reality (Arndt, 2013, p. 41). Arndt (2013, p. 374) observes that the absolutist Wittelsbach court bluntly acknowledged publishing prints "to whitewash its undertakings."

Beyond persuasion, rulers also intervened directly, forcibly dissolving parliaments and co-opting local institutions.
Military force was used to suppress parliamentary opposition in Cleves-Mark, for example (Press, 1991, p. 324). In 1667, the Elector of Brandenburg and ruler of Cleves-Mark famously wrote: "I have become convinced that I owe the preservation of my position (...) to God, and next to God, to my army" (Fay, 1917, p. 772f).

This consolidation of executive power, combined with the erosion of autonomous elite authority, made the court, administrative, and military appointments increasingly attractive to the local nobles.

In Brandenburg, Frederick William's approach of selective favors to the nobility fractured the Estates, as Carsten (1958) notes: "A party came into being which supported his policy, and the Estates were no longer united in their opposition." This division was evident when fourteen Cleves noblemen, including five councilors, two officials, and one army officer, protested a deputation they had not approved, citing its derogation of their master. Similarly, a pro-electoral noble faction, with seven of twelve protesters being councilors, officers, or officials, opposed another unconsented deputation, leading to rival assemblies as "the Estates were now split beyond repair" (Carsten, 1958).

C.III Data Sources

We describe here examples of the portrait, print, and local infringement data introduced in Section III.III.

C.III.I Portraits

Panel A of Appendix Figure C.1 shows a 1701 portrait of Frederick I, King of Prussia, in armour with the subtext 'strong and wise king'. Panel B shows a 1679 portrait of Maximilian Emanuel, Duke of Bavaria, also in armour. In the bottom is a poem that celebrates his military victories.

C.III.II Prints

Examples of classified titles in our data include:

- "The King of Prussia and his soldiers' conversation with God, along with a triumphal ode of the King of Prussia, 1759" [Original: "des koenigs in preussen und seiner kriegs-knechte gespraech mit gott, samt einer triumpfs-ode des koenigs in pressen"]
- "Victory-crowned weapons of the imperial, electoral, Bavarian, and other allied imperial peoples, as they were led before, during, and after the glorious victory obtained on August 12, 1687, against the hereditary enemy of Christianity at Mohács, under the high command of the two incomparable heroes of Bavaria and Lorraine" [Original: "sieg-bekroente waffen der kayserlich- chur- bayrisch- und anderer aliirten reichs-voelckerwie solche vor/ in/ und nach der den 12. augusti dieses 1687. jahrs. wieder den erb-feind christl. namens bei mohatz ruehmlichst erhaltenen victori unter hohem commando der beeden unvergleichlichen heroen von bayern und lothringen ... gefuehrt worden."]
- "Brief exposition of the reasons why His Electoral Highness of Brandenburg, etc., was compelled to take up defensive arms against Electoral Cologne and Münster, 1673" [Original: "kurtze fuerstellung. aus was ursachen se. churfl. durchl. zu brandenburg/ in preussen/ zu magdeburg/ guelich/ cleve/ bergen/ stettin/ pommern/ [et]c. hertzog/ [et]c. [et]c. unuembgaenglich bewogen worden/ wider chur coelln und muenster die defensions-waffen zu ergreiffen."]

Figure C.1: Examples: Portrait Printed Graphics

A: Frederick I

B: Maximilian Emanuel

Note Panel A: https://www.portraitindex.de/bilder/zoom/sbb-wadzeck-000226. Panel B: https://www.portraitindex.de/documents/obj/33423445.

C.III.III Infringement

We measure autocratic repression of local institutions using data from (Keyser et al., 1939-2003), a comprehensive historical encyclopedia chronicling events in German towns. Our variable counts the number of infringement instances per town and year, focusing on cases such as the direct appointment of town mayors by rulers or other interventions in local governance that eroded local autonomy
Examples in our data include:

- In Borken (Westfalen), in 1624: " the territorial lord revoked all rights, freedoms, and privileges of the city due to its recalcitrance and appointed the city authorities himself" [Original: "Infolge der religioesen Wirren zu Ende 16. Jh. Kaempfe mit dem Landesherrn, der 1624 der Stadt wegen ihrer Widersetzlichkeit alle Rechte, Freiheiten und Priv. nahm, selbst die Stadtobrigkeit ernannte;"]

- In Moeckern, in 1710: "the king regulated the municipal administration after the ruler's office had intervened in the appointment of council positions" [Original: "1710 Regulierung des rathaeusl. Wesens durch Kg., nachdem Amt Eingriffe in die Besetzung der Ratsstellen vorgenommen hatte."]

- In Freystadt (Bayern), in 1662: "council elections were only allowed in the presence of the lord's chief official, indicating a restriction on urban self-governance" [Original: "1662 Ratswahl nur in Anwesenheit des herrschaftlichen Oberamtmannes."]

D Framework

Players, Timing, and Choices

There are three players $i \in \{\text{town}, \text{military}, \text{ruler}\}$. There are two periods, $t \in \{1, 2\}$, and the state of the world in each period is either war (w) or no war ($\bar{w}$): $s_t \in \{w, \bar{w}\}$. In period $t = 1$, war occurs with certainty ($s_1 = w$). In period $t = 2$, war continues with probability p, i.e. $s_2 = w$ with probability p, and $s_2 = \bar{w}$ with probability $1 - p$.
Each player selects a governance regime $g \in \{d, \ell, e\}$, representing:

- d: disorder (plundering),
- ℓ: legislature (parliamentary rule),
- e: executive (autocratic rule).

In equilibrium, all players coordinate on the same governance regime g. We describe payoffs and then describe the equilibrium.

Payoffs

Towns

Towns have an income y_t and must bear costs associated with provisioning toops during war. In $t = 1$, these costs are:

- Disorder: Δ_d
- Legislature: Δ_ℓ
- Executive: Δ_e

Costs are ordered as follows:

$$\Delta_d > \Delta_\ell > \Delta_e$$

reflecting that disorder is most damaging and the legislature incurs higher transaction costs than the executive.
In $t = 2$, town income depends on the initial governance choice whether the war continued:

- If $g_1 = d$, towns are depleted: $y_2 = 0$ (otherwise $y_1 = y_2 = y$).
- If $g_1 = \ell$ and war continues ($s_2 = w$), extraction continues with cost Δ_ℓ.
- If $g_1 = e$, extraction occurs regardless of the state of the world (lock-in of autocracy), and cost Δ_e is incurred.

Disorder is strictly dominated. Towns choose between ℓ and e by solving:

$$\max_{E=\mathbb{1}(g=e)} \quad y - (1 - E) \cdot \Delta_\ell - E \cdot \Delta_e + [y - p \cdot (1 - E) \cdot \Delta_\ell - E \cdot \Delta_e]$$

Simplifying the above, the town chooses $g = e$ (executive) if:

$$\frac{1 + p}{2} \Delta_\ell > \Delta_e.$$

That is, the executive is chosen when the legislature is relatively inefficient relative to the autocracy, and the probability of continued war is sufficiently high.

Military

The military receives:

- Δ_d if disorder is chosen ($g = d$) through immediate plunder, but no future gain.
- Δ_e if the executive or the legislature extract (i.e. the executive extracts and transfers to the military without loss).

The military prefers to avoid plundering (i.e., prefers $g \in \{\ell, e\}$ over $g = d$) if:

$$\frac{1}{1+p}\Delta_d < \Delta_e$$

When indifferent between the executive and the legislature, the military prefers the executive due to the ruler's legitimacy with soldiers.

Ruler

The ruler receives:

- 0 during wartime,
- Δ_e in peacetime if $g = e$ (executive governance).

Thus, the ruler strictly prefers $g = e$.

Equilibrium

All players coordinate on $g = e$ (executive governance) if the following condition holds:

$$\frac{1+p}{2}\Delta_\ell > \Delta_e > \frac{1}{1+p}\Delta_d$$

This equilibrium condition ensures that:

- Towns prefer the executive over the legislature,
- Military prefers delegation to the executive over disorder,
- The ruler prefers the executive, as it generates peacetime rents.

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