How Autocracies Form: State Capacity, Absolutism, and the Thirty Years' War

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Abstract

This study documents the emergence of capable autocracies in response to violent conflict. In novel data, we examine how the Thirty Years' War (1618–48), the largest conflict on European soil before World War I, facilitated the consolidation of *ex ante* weak states at the expense of early forms of parliament. War exposure gave rise to local needs for centralized intervention, easing constraints on the ruler. We leverage exogenous variation in troop movements to estimate this initial fiscal-military expansion and the unraveling of parliaments it enabled. Places with high capacity for collective action could insulate against ruler overreach. Absolutist regimes, once established, saw accelerated state growth, while at the same time undoing local democratic institutions. These changes persisted for centuries after the war. Our findings shed light on the expansion of states in the face of crisis, highlighting a potential trade-off between the short-run and long-run optimality of governance structures.

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1 Introduction

The maxim that "necessity has no law", *necessitas non habet legem*, is frequently invoked during times of crisis. Citizen become substantially more accepting of drastic measures in the face of exceptional circumstances. This naturally has political consequences, strengthening authorian tendencies (Djankov et al., 2003; Alsan et al., 2023). Nowhere does this tradeoff between dictatorship and disorder emerge more vividly than in the case of war, when regimes mobilize human and material resources on a large scale by means of centralized intervention to enter into armed struggle. An abundant literature links the emergence of capable states to the occurrence of violent conflict (Tilly, 1990; Besley and Persson, 2011; Gennaioli and Voth, 2015). But, viewed through the lens of crisis response, the dynamic implications of warfare are less clear: Why do states continue to expand even after the arms have been laid down? And what type of regime takes hold in the consolidating state?

Empirically documenting the dynamic consequences of conflict for state building is challenging. Warfare is endogenous, and a lack of suitable cross-country comparison groups hinders tracing outcomes over long enough time horizons. The description of mechanisms is obstructed by information quality: existing data largely collapses conflict incidence to a point in time and space, and reduces state outcomes to proxies of fiscal capacity or parliamentary strength.[1]

In this paper, we trace the impact of the Thirty Years' War (1618–48), the largest conflict on European soil before World War I, on state capacity and participatory institutions in the territories of the Holy Roman Empire. We collect novel data on taxation, militarization, legislation, and parliaments at the local and regional level for 2,390 towns in 213 polities, yearly for the time period 1500–1789. In addition, our data depicts the war in great detail, connecting troop movements, military leaders, and battles to 1,715 town-level war exposure events. This allows us to employ a novel identification strategy, considering war exposure as a composite treatment in part generated by exogenous variation in troop movements.

The setting is uniquely suitable to follow the consolidation and transformation of states over centuries. It comprises hundreds of sovereign political entities within a narrow

[1]The "inability to link sequences of actions and reactions between actors across space" is a limitation even in contemporary data on conflict, which "often does not record connections between related events, and thus cannot capture the spatial dynamics that typically characterize conflict escalation." (Blair and Sambanis, 2020, p. 1908).

geographical range, and it represents a drastic shift in state organization: the period saw the establishment of permanent taxes, standing armies, and standardized legislation.[2]

Our argument departs from a simple fact: maintaining an army is at the heart of the war effort. The necessity to enlist, pay, feed, and house soldiers created scope for, and acceptance of, centralized government intervention. For military leaders, communicating with the central ruler of a territory was less costly than coordinating troop upkeep with parliament. This expanded the violent and bureaucratic capacity of the ruler, allowing him to enforce taxation even after the war had ended.[3] Sidestepping parliamentary constraints this way, the influence of deliberative institutions was reduced further, unraveling the political equilibrium toward an autocracy also in places not directly affected by the war.

In our empirical analysis, we find an immediate and persistent increase in taxation and militarization for places that were exposed to troops during the Thirty Years' War; these places also became considerably less likely to be called to parliament. We provide historical evidence, and a range of robustness checks, that this effect was not driven by troops being drawn to places that were already on a path of state consolidation. We leverage detailed information on the war to construct control groups comparable in the ex ante targeting propensity: for example, we compare only places listed as potential targets in military plans. In another exercise, we rely on facilitating battles, which enabled the winning army to advance, and hence acted as mediators of town-level war exposure. We construct counterfactual realizations of the war in which battles could have ended differently, changing the set of exposed towns. Adjusting our estimates for the "expected treatment" (Borusyak and Hull, 2023) confirms the baseline empirical findings.

Turning to mechanisms, we first show that troop upkeep was the key driver behind the effects of war exposure, rather than violent attacks or ruler turnover. We provide historical evidence that the war asymmetrically impacted communication costs for parliament,

[2]For example, the standing army at the disposal of absolutist Prussian "soldier king" Frederick William I (1688–1740), as well as the permanent taxes required to finance it, were just one generation old when he took office in 1713 (Clark, 2006).

[3]The Thirty Years' War provides an ideal context to study the consequences of warfare because its impact was highly localized: It pioneered troop finances that were expropriated wherever the army was passing through, so-called "contributions" (Redlich, 1959). External means of war finance, like loans, were not yet available to rulers due to commitment problems (Queralt, 2019). Furthermore, the contributions system did not differentiate between allies and enemies: occupying armies cooperated closely with domestic bureaucracies to ensure troop upkeep. Rulers continued these institutions after the occupation had ended. We expand on this point when discussing mechanisms in Section 5.

and that military leaders as a result preferentially corresponded with rulers to mobilize resources. In turn, we show that the expansion of violent capacity came at the expense of elites: in places affected by the war, the nobility joined the military, being absorbed into the nascent absolutist state. Culturally, militaristic prints and portraits proliferated. We show how this increased ruler capacity was used to ultimately cast aside parliaments. Speaking to the importance of preventive checks, we demonstrate that places with high capacity for collective action were better able to mobilize resources during the war, while at the same time insulating against ruler overreach.

We then proceed to examine the entrenchment of absolutist states. Once parliaments were eliminated, rulers increased legislative activity, instituted standing armies, and proceeded to crack down on local self-governance, thus expanding their reach to initially untreated towns within the same parliamentary constituency.

Finally, we consider the long-term, ambivalent consequences of the war: in a cross-section of states in the 19th century, absolutist territories had higher tax ratios, but delayed the introduction of a constitution.

Research on the nexus between states and warfare dates back as far as Hobbes (1651) and was particularly advanced in works by Hintze (1906), Finer (1975), Brewer (1989), and Tilly (1990). We expand on recent theoretical and empirical contributions (Besley and Persson, 2008; Gennaioli and Voth, 2015; Dincecco and Onorato, 2016; Becker et al., 2020) by employing a wide array of outcome measures that go beyond fiscal capacity and opening up the "black box" of the conduct of war and its financing.[4] We furthermore complement research on the local economic effects of conflict (Feigenbaum et al., 2022; Gierok, 2023) with a granular analysis of state organization outcomes.[5]

A range of works considers the fate of representative institutions within consolidating states (Karaman and Pamuk, 2013; Angelucci et al., 2022; Desierto and Koyama, 2022; Kenkel and Paine, 2023; Cox et al., 2023), and the role of elites in state-building more generally (Bai et al., 2022; Garfias and Sellars, 2022; Acemoglu and Robinson, 2023). We underpin these studies by documenting the dynamic consequences of war on the power

[4]The "bellicist" perspective on state-making is not without its critics: for example, Grzymala-Busse (2020) assigns a substantial role to the Medieval Church. For the purposes of our study, we take the institutional context at the start of the 16th century as given, applying equally to treated and untreated places in our data.

[5]In data on civic wealth, public revenues, public expenditure and debt levels for 17 towns in the Holy Roman Empire, Gierok (2023) documents that the Thirty Years' War led to large city-level increases in expenses related to war finance. Our paper complements these descriptive findings by showing quasi-experimental evidence that the phenomenon was present throughout the Empire, and by tracing its implications for militarization and state organization in the long run.

balance within the state.

Our setting furthermore speaks to research on democracy and autocracy (Olson, 1993; Glaeser and Shleifer, 2002; Djankov et al., 2003). We demonstrate how the unraveling of parliaments pushed emergent absolutist states off the "narrow corridor" (Acemoglu and Robinson, 2019). Counter to conventional accounts (Huntington, 1991; Acemoglu and Robinson, 2006), our setting demonstrates that autocratic regimes can be strengthened by crises; more generally, we relate to works on the expansion of states in crises (Higgs, 1987; Allen et al., 2023), where our findings highlight a potential trade-off between the short-run and long-run optimality of governance structures.

The remainder of the paper is organized as follows. In Section 2, we introduce the political organization of the states in the Holy Roman Empire, and give an overview over the Thirty Years' War. In Section 3, we describe the wide-ranging data collection. Our main empirical findings on the emergence of absolutism in response to warfare are presented in Section 4. Section 5 discusses the mechanisms behind state expansion and parliamentary unraveling. In Section 6, we examine the entrenchment of absolutist states, before turning to the long shadow of the war in Section 7. Section 8 concludes.

2 Historical Background

2.1 Princes and Parliaments: State Organization in the Holy Roman Empire

The Holy Roman Empire emerged from the Middle Ages as a loose confederation of quasi-sovereign, but weak, constituent territories with low fiscal, military, and legal capacity: taxes were extraordinary affairs; armies were raised ad hoc through mercenaries; and jurisprudence largely relied on traditional laws (Whaley, 2012).[6]

Within these territories, political organization rested on two pillars: the ruler and the so-called "Estates", mostly comprised of the local nobility, who owned agricultural lands and the economic surplus thence generated. Ensuring the orderly working of the rural economy required government functions, like the enforcement of property rights in the face of conflict. Due to communication costs and asymmetric information, governance was hard to organize collectively among the coalition of landowners. This gave a role to rulers, who, initially as *primus inter pares*, had developed a comparative advantage at

[6]Territories can be classified as either prince-bishoprics, secular states, and city states. We focus on the first two types, which governed a large share of the landmass, omitting city states from our analysis.

supplying centralized state functions. To do so, they needed to salary bureaucrats and soldiers, which required revenue. In the absence of other financing options, income had to stem from the agricultural surplus. From the perspective of the sovereign, two paths were available for accessing this revenue: coordination with the landed elite, or extraction through the bureaucracy and military.

European states in the Late Middle Ages were characterized by the fact that elites were too weak to organize governance themselves, and rulers were too weak to extract revenue themselves. This gave rise to early forms of parliaments, so-called "diets". In these assemblies, the Estates bargained with the ruler, supplying taxes in return for protection (Carsten, 1959). In addition to the landed nobility, the Estates usually assembled city representatives, the clergy, and sometimes peasants; however the nobility was usually the dominant group.[7] As bureaucracies and armies became more capital-intensive over the course of the 15th and 16th centuries, parliaments took on a central role in the governance of territories.[8]

Nevertheless, parliamentary power was brittle. It rested on the inability of the ruler to organize or coerce revenue without consulting the local elites, and it required a stable elite coalition. The institutional organization of parliaments had this concern at its core. A number of measures were taken to avoid a "ratchet effect" by granting too much power to the ruler: taxes were always limited in size and duration; small, one-off grants ensured the continued need to consult parliament. Spending was monitored tightly, and Estates closely guarded their right of tax approval. For similar reasons, Estates also insisted on ancillary privileges, like the consultation in law-making.[9] Additionally, parliaments were organized to ensure that the elite coalition remained intact: diets always required the joint summoning of the Estates, barring attempts at "divide and conquer" by consulting them in separate committees. Crucially, these measures, while ensuring political participation

[7]For our purposes, the clergy, as landowners, faced the same incentives and constraints as the nobility. towns were similar, too: while urban capital was more mobile than that of agricultural producers, burghers nevertheless required protection of trade routes and the enforcement of contracts at court to produce a surplus, and while density aided organization within city limits, coordination across towns was also difficult.

[8]These parliaments of course represented a substantially smaller share of the population than parliaments in contemporary democracies. Nevertheless, their electorate was sizeable: In Bavaria, 5,534 noble lineages, 90 market towns, and 34 towns were represented by delegates (Lanzinner, 1980, p. 18). In places where where diets did survive, they were the seed institutions of parliaments during later expansions of the vote (Grube, 1957).

[9]Concerns about the gradual loss of rights also manifested in ceremonial acts; for example, rulers were usually required to sign off an "affirmation of parliamentary privilege" at the beginning of each bargaining session.

for some, kept state capacity low through requiring governments to operate on a basis of exceptional revenue requests (Cox et al., 2023). By the onset of our period of analysis in 1500, nearly all large territorial entities in Central Europe had developed parliaments; very small territories had similar, albeit informal, arrangements.

2.2 The Thirty Years' War

In this institutional context, war broke out in 1618. Throughout Central Europe, religious tensions had been rising since an uneasy peace between Protestants and Catholics at Augsburg in 1555. A local revolt of Protestant Estates in Bohemia hence quickly spiraled into civil war that first swept over the Western Holy Roman Empire in the early 1620s. Soon, the conflict provoked the intervention of all major continental powers, and religious motives formed a mere background to the contest for European hegemony. The spiral of violence, which concentrated in the Holy Roman Empire, could only be broken in May of 1648 after decades of complex negotiations (Münkler, 2019).

Over these thirty years, fighting sometimes reduced to a simmer, only to flare up again in violent episodes, during which armies of either side traversed much of the Holy Roman Empire in the pursuit of larger strategic goals. We sketch the arguably most consequential episode here, the first Swedish intervention of 1630–1632. Figure 1 illustrates stylized troop movements and facilitating battles of the campaign.[10]

Provoked by the occupation of the German Baltic Coast by Catholic troops in 1628, Sweden under King Gustavus Adolphus (1594–1632) began to prepare for armed intervention. In July 1630, the king and his troops took Pomerania and parts of Mecklenburg without encountering considerable resistance. Advancing down the east bank of the Oder river into the mainland of the Holy Roman Empire, a first consequential battle with the Catholic army was fought at Frankfurt (Oder) on April 13, 1631 (Panel A). Gustavus won, tightening his influence over Brandenburg and allowing him to advance into Saxony. Here, he defeated the Imperial forces again near the town of Breitenfeld (Panel B) and swept into Southern Germany. In the spring of 1632, the Catholic army regrouped and unsuccessfully attempted to halt the Swedish advance into Bavaria at Rain am Lech (Panel C). As Swedish troops flooded Bavaria, their supply lines were now extremely stretched, and the army was at danger of being encircled. They hence moved North again, suffering a first setback at Nürnberg in September 1632 (Panel D), which allowed the Catholics

[10] Section 3.2 details the construction of the data underlying these maps.

to take back parts of Southern Germany. Hurrying further north, Gustavus was killed in battle in November and his troops scattered. Nevertheless, Sweden maintained active operations on the continent until the Peace of Westphalia sixteen years later.

This episode more broadly reflects the military logic of the Thirty Years' War: Battles mediated local war exposure. Following a battle, the party that emerged victorious determined much of the activity, while the losing side needed to collect scattered regiments and regain troop strength. The next confrontation would only occur once both sides perceived a reasonable chance of winning. For armies, battle outcomes would hence open up or block off entire regions. Nevertheless, some areas were lightly guarded at times, like Pomerania and Mecklenburg in the fall of 1630, and thus proved easy targets.

As thousands of soldiers were swept across the map by battle fortunes, the life of local communities in the Holy Roman Empire was deeply impacted by the presence of soldiers. Territories large and small had to deal with troops passing through their lands: once an army had advanced into an area, or was granted passage through a territory, the towns and villages in its way were highly vulnerable. In the following, we set out to measure this war exposure, and delineate and quantify its impact on governance structures.

3 Data

We depict the historical setting presented in Section 2 through a range of novel, detailed data on the baseline organization of the state, war exposure, and medium- to long-term measures of state expansion.

3.1 Baseline State Organization

Our base data covers the 2,390 towns in the Holy Roman Empire, as depicted in the *Deutsches Städtebuch* (Keyser et al., 1939-2003).[11]

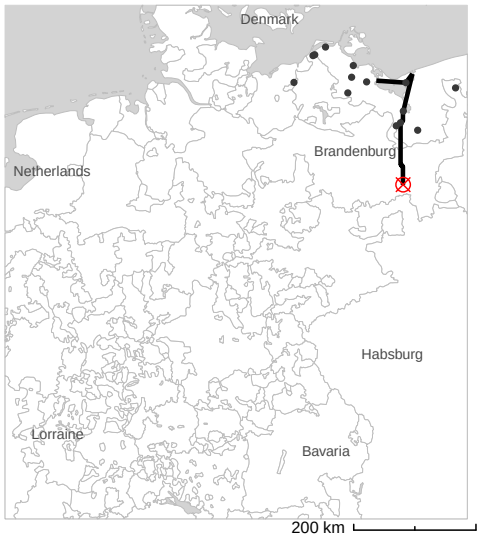
Onto this base, we add 3,885 town-level record books from the *Index Librorum Civitatum* (Ranft et al., 2023). For a given town, this source lists all available medieval and early modern account books, together with a categorization and the date range they cover. We restrict the records to direct taxes levied by the territories to which the town belonged.[12]

[11] This source covers all places in the 1937 German borders that at some point in their history were a town.

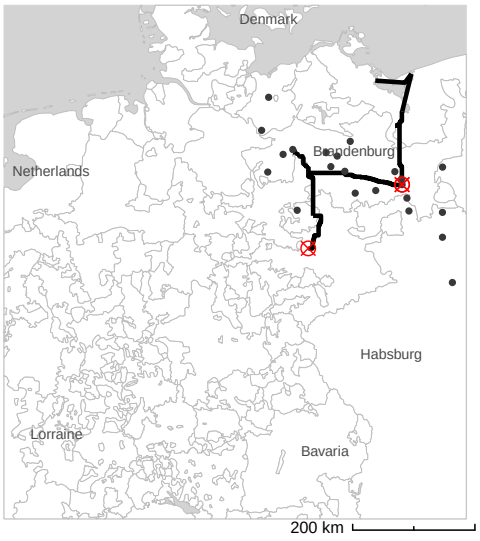
[12] Note that the *Index* is an ongoing effort, and not all German regions have been covered so far. We exclude those regions from all regression analyses that rely on the *Index*.

Figure 1: The Thirty Years' War: Swedish Intervention (1630–32)

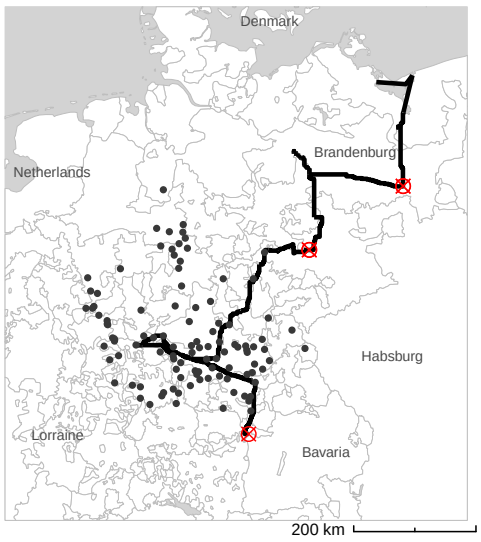
A: 1630–31



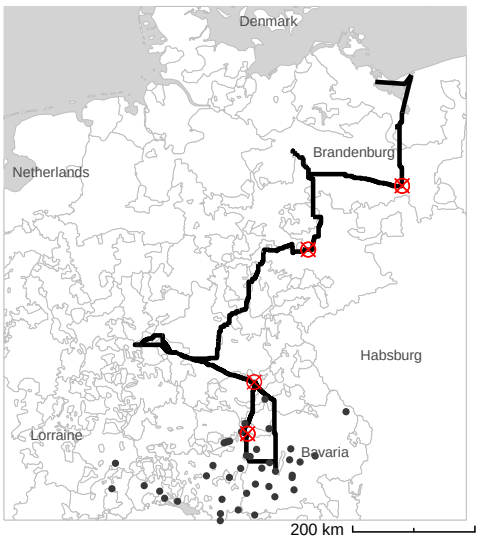
B: 1631



C: 1631–32



D: 1632



Note The maps show facilitating battles (red crossed dots), troop leader movements (black line), and affected towns (black dots) of each phase of the initial Swedish intervention in the Thirty Years' War (1630–32). The troop leader is Gustavus Adolphus, King of Sweden. Facilitating battles are: Frankfurt (Oder) (Panel A), Breitenfeld (Panel B), Rain am Lech (Panel C), and Nürnberg/Alte Veste (Panel D). Base map shows territories in the Holy Roman Empire in 1600 from Nüssli (2014). Details on the data underlying the maps are given in Section 3.2.

For measures of military capacity, we record the time period of activity for 3,456 territory regiments from a complete list of early modern armies (Tessin, 1986).

For more granular information on military capacity, we refer to the *Deutsche Biographie* (Hockerts and Lanzinner, 2022). This resource provides information of 730,000 notable individuals in the German-speaking lands, including birth and death year, professions, and residential locations over the life span. We assign individuals to the town closest to their birth place, identify military personnel, and classify them as "active" from age 20 to their death. We also record their nobility status.

We consult historical works to construct a complete list of the 62 parliaments which were active in our area of analysis in the 16th century, and, where applicable, we record the year in which they were last convened. Note that parliamentary constituencies were tied to regions, and one ruler might have needed to coordinate with multiple parliaments.[13] For each parliament, we identify the towns in our data that it represented. We also record the composition of the Estates — whether the nobility, clergy, towns, and peasantry were represented in parliament.

To capture geographic town characteristics, we furthermore measure agricultural suitability (FAO, 2002), terrain ruggedness, as well as distances to the coast, the closest navigable river, and the border of the Holy Roman Empire. We include data on whether a town was fortified, its distance from the closest trade route, its number of markets, as well as the predominant religious denomination.

3.2 War Exposure

Our base data on local troop burden comes from Cantoni and Weigand (2021).[14] For each town, the source provides a brief comment, extracted from Keyser et al. (1939-2003), describing each separate war exposure event; it also records the year of occurrence. We narrow the data to events during the Thirty Years' War. For undated entries, we consult town-level histories to assign a precise year to the event. In total, 918 towns experienced 1,715 war exposure events, with over 50% of places burdened by troops on multiple occasions.

[13]The close ties of Estates to distinct geographic areas is reflected by the fact they were also known as the *Landschaft*, literally translated as "landscape".

[14]This data was previously used in Cantoni et al. (2023). We also take the yearly mapping of towns to their rulers between 1500 and 1789 from Cantoni et al. (2019). We omit East Prussia from the data because it is an exclave; our empirical results are unaffected by this choice.

Appendix Figure A.1 highlights all war-exposed towns in our data.

We embed this information on town-level exposure in the broader context of the war. Bodart (1908) provides a list of 89 landmark battles during the time period of the Thirty Years' War, which records the exact time and place, involved conflict parties and military leaders, and troop strengths. 60 such battles in Central Europe had a direct consequence for the area of the Holy Roman Empire. We identify all troop leaders that are mentioned more than once in these battles, of which there are 13, and consult historical sources to trace their movements across the map as accurately as possible, allowing us to map stylized troop movements for each point in time during the war.

With this information, we assign town-level events, where applicable, to the battle that facilitated the winning army to pass through the town. For example, turning back to Figure 1, all Swedish movements in Brandenburg in 1631 (Panel B) had the Battle at Frankfurt (Oder) as "facilitating battle", while the advance into Bavaria in 1632 (Panel D) was a result of the Battle of Rain.

3.3 Advanced State Organization

A final component of our data collection is concerned with the medium- and long-term changes to state capacity and deliberative institutions.

To measure legal capacity, we collect data from the *Repertorium der Policeyordnungen der Frühen Neuzeit* (Härter and Stolleis, 2021), which includes 693,000 ordinances, categorized by topic, and specifies the places to which they applied, which we map to the towns in our data. For innovations to military capacity, we identify the timing of the introduction of all 19 territorial standing armies in the Holy Roman Empire during our period of analysis. Local political participation is measured through the presence of an appointed (instead of elected) town council; this data that was previously used in Bosshart and Dittmar (2023).

To measure broader militarization and the composition of the military, we count the number of noble individuals in a given town and year as indicated in the *Deutsche Biographie*. This source also classifies whether a person was active in the military. We use this information to calculate the fraction of the nobility that is in the military. We furthermore consider print data from *VD16-18*, the "Union Catalogue of Books Printed in German Speaking Countries". This source lists the title, as well as the year and place of publication, for more than 630,000 early modern prints held in libraries across Germany.[15]

[15]During the time period we study, printing was highly decentralized and hence occurred close to the place

Drawing on a dictionary of military-related terms from the time period, we identify those prints that have military-related language in the title. From the *Digital Portrait Index of Early Modernity*, a comprehensive portrait database of around 300,000 portraits from that period, we extract the places and years of portraits. This source also indicates whether the depicted person has a military role. We thus construct the number of military portraits originating from a town and year.

To assess the long-term impact of the war on territories, we document the timing of the introduction of first constitution for the states of the German Confederation in the 19th century (Hartung, 1921); and, from a 1800 state handbook of the Holy Roman Empire (Varrentrapp, 1800), we collect data on state revenue and population for each territory. This is, to the best of our knowledge, the earliest comprehensive population and revenue survey of German states.

4 Main Empirical Results: The Emergence of Absolutism

4.1 Taxation, Militarization, Parliament Elimination

We first assess the impact of the war on the organization of the state as it emerged in the late Middle Ages. Our outcomes of interest are direct taxation, militarization, and the presence of parliaments. Since war exposure was very local, as are all of the outcomes, we consider the town as a unit of analysis.[16]

Our baseline regression specification is

$$\begin{aligned} Consolidation_{ijt} &= \beta Treated_{ij} \times Post1618_t \\ &\quad + \alpha_i + \alpha_j + \alpha_t + \varepsilon_{ijt} \end{aligned} \tag{1}$$

$Consolidation_{ijt}$ represents either an indicator of the presence of territory-level direct taxes, the log number of active military personnel, or an indicator of the elimination of parliaments in town i, territory j, and year t. $Treated_{ij}$ indicates whether a town was exposed to the Thirty Years' War at least once, while $Post1618_t$ is a dummy for the time

of distribution.

[16] In Section 6, we demonstrate how these local shifts, through the elimination of parliaments, influenced territory-wide outcomes, leading to the entrenchment of absolutism.

period after 1618.[17] α_i, α_j, and α_t are town, territory, and year fixed effects, respectively.

Table 1 shows estimation results. Column 1 shows that war exposure is associated with a statistically significant, substantial increase in the presence of direct taxes of 87% of the baseline probability. Similarly, war exposure accounts for a doubling of the baseline inverse hyperbolic sine of military personnel originating from a town (column 2). Finally, column 3 indicates a significant increase in the propensity of parliament elimination by 7 percentage points in association with the war. This is sizeable given that overall, 12 percent of towns in our sample experience the elimination of parliaments.

Table 1: War Exposure and Absolutism

	Direct Taxes (1)	Military Personnel (2)	Parliament Eliminated (3)
War Exposure	0.0543***	0.0282***	0.0700***
	(0.0115)	(0.0074)	(0.0138)
R^2	0.56800	0.38022	0.49415
Observations	476,180	653,660	653,660
Outcome Mean	0.0621	0.0281	0.1222
Outcome Def.	(0/1)	(ihs)	(0/1)
Town FEs	✓	✓	✓
Territory FEs	✓	✓	✓
Year FEs	✓	✓	✓
Cluster	Town	Town	Town

Note Table presents results of estimating equation (1). Observations are at the town-year level. The sample comprises 290 years and 2,390 towns. The dependent variables are (1) a binary variable whether town i has a record of direct territory taxes in year t, (2) the inverse hyperbolic sine of military personnel born in town i active in year t, and (3) a binary variable 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.

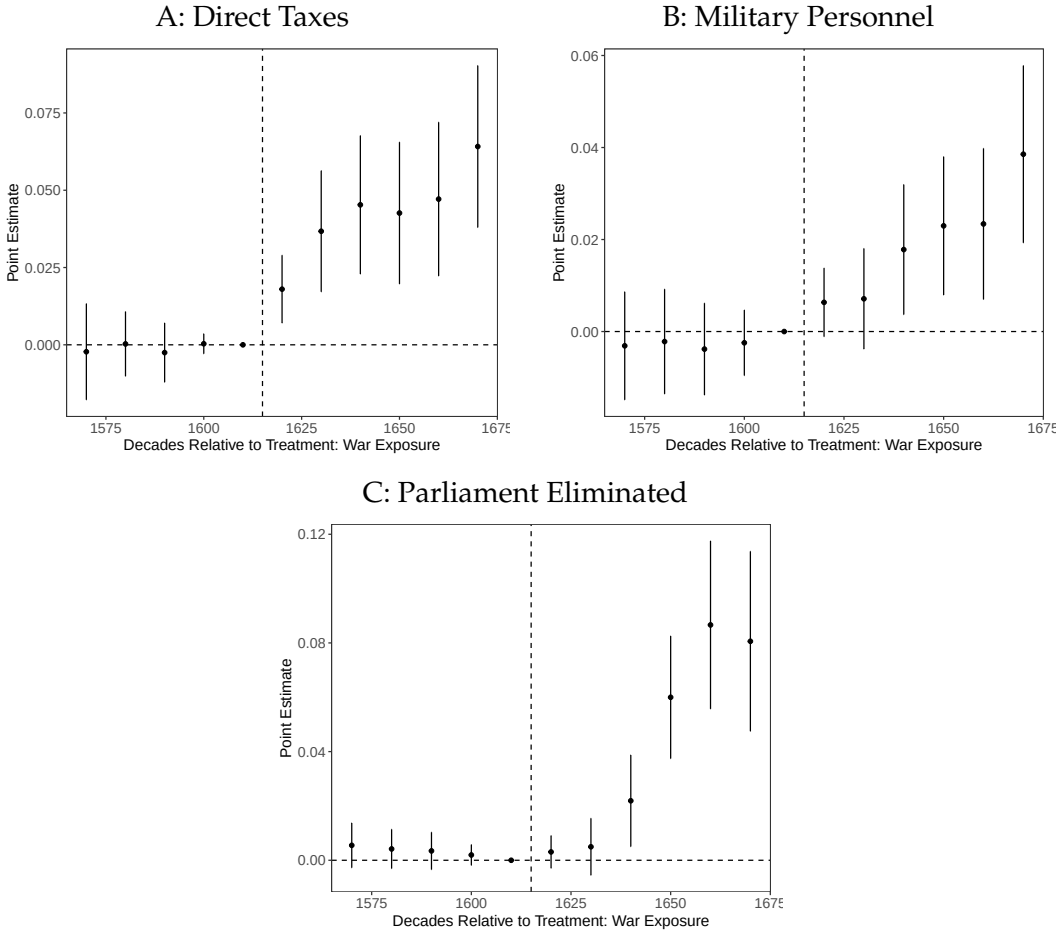
To examine the dynamics of these finding, we estimate event study analogues of equation (1):

$$\begin{aligned} Consolidation_{ijt} &= \sum_{\tau=-5}^{5} \beta_\tau Treated_{ij} \times RelativeDecade_{\tau(t)} \\ &\quad + \alpha_i + \alpha_j + \alpha_t + \varepsilon_{ijt} \end{aligned} \tag{2}$$

[17] For ease of exposition, we opt to anchor all exposure events in the year 1618. Our results are qualitatively unchanged if we instead consider the staggered timing of the first exposure event for each town.

with all variables as defined above, and $RelativeDecade_t$ denoting decades until/since 1618. Results are shown in Figure 2. The increases in all three outcomes are persistent and not led by pre-trends (Panels A-C). For taxes and military personnel, the effect is immediate (Panels A and B), while the effect of the war on parliaments (Panel C) only takes hold gradually. We devote Section 5 to examine the gradual unraveling of parliaments in more detail.

Figure 2: War Exposure and Absolutism (Event Studies)



Note The plot shows results of estimating the event study regression in equation (2), with 95 percent confidence intervals. Observations are at the town-year level. The sample comprises 290 years and 2,390 towns. The dependent variables are (A) a binary variable whether town i has records of direct territory taxes in year t, (B) the inverse hyperbolic sine of military personnel born in town i active in year t, and (C) a binary variable whether the parliament that represented town i has been eliminated in year t. Standard errors are clustered at the town level.

4.2 Identification: Historical Evidence

A concern is that these results could be driven by omitted variables: troops might have traversed places that were already on a path toward state consolidation. Such targeting might have occurred explicitly, if military strategy favored places with high (anticipated) state growth, or implicitly, if logistical concerns were correlated with growth, for example because troops marched along central trade routes.

Historical evidence strongly suggests that the former concern was not at play, the war being "waged almost entirely without a larger strategic purpose", since logistical concerns made it "virtually impossible to sustain an ambitious and strategically meaningful campaign plan" (Parrott, 2011, p. 132ff.). Instead, soldiers followed the "call of their stomachs" (Van Creveld, 1977, p. 10).

To address the concerns that logistics might confound our results, we first consider the abundance of historical evidence that directly ties the expansion of princely administrations to the war: absent other means of war finance, troops subsisted by expropriating direct taxes, so-called "contributions" wherever they went. These taxes were raised in both allied and enemy territories, and they were substantial compared to previous levels of taxation. For the town of Kitzingen, "wartime contributions constituted a 1000% increase on peacetime tax burdens" (Wilson, 2018, p. 237). Wallenstein's occupation terms with Pomerania in 1627 generated six times the annual pre-war tax revenue.[18] These direct taxes emerged as the leading source of territorial income overall, during and after the war. A very similar mechanism holds for militarization. Mustering took place in the same manner as war finance: the cost was shifted to inhabitants of the areas in which troops resided, who, aided by local officials, had to supply soldiers. Key intermediaries were local "military enterprisers", who commanded individual regiments and often happily switched sides when war fortunes turned. Finally, turning to the outcome of parliament elimination, the historical literature posits that the "clear winner of the Thirty Years' War was the territorial state, embarking on the path to absolutism" (Press, 1988, p. 266). To illustrate the mechanisms behind these claims in the context of our framework, we provide wide-ranging historical and empirical evidence in Section 5.

[18] Additionally, troops needed to be fed and sheltered, with the burden of organization also falling on local officials. 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 (J. Kraus, 2021, p. 215).

4.3 Identification: Robustness

We take a number of steps to empirically support the causal link between war and shifts in state organization. Our rich data allows us to account for a broad range of potential strategic and logistical confounders. First, we note that our analyses include town-, territory-, and year fixed effects, hence absorbing invariant characteristics that might have influenced troop decisions, such as the baseline wealth of a town.[19] Furthermore, our event study graphs in Figure 2 show that attacked towns did not have differential trends of consolidation prior to the outbreak of the war. Our results are robust to accounting for a wide range of town characteristics that might confound our estimation: in Appendix Table A.1 we flexibly control for geographic and economic covariates, such as the distance to navigable rivers, the presence of a fortification, or the number of markets held in 1618.[20] To address potential spatial correlation, Appendix Tables A.2, A.3, and A.4 include, for each outcome, standard errors clustered at different regional levels, and Conley standard errors for distance cutoffs of 25, 50, and 100 kilometers, which does not affect results. We complement our findings on militarization by showing robustness to two alternative measures, constructed from separate data sources: in Appendix Figure A.2, we once measure militarization through Wikipedia biographies of military personnel (Panel A), and once with territory-level active regiments (Panel B). Patterns are broadly similar to our baseline measure.

4.4 Identification: Comparable Control Groups and Expected Treatment

We next present a number of strategies to construct more comparable control groups. First, we focus on towns that could have plausibly been exposed to the war but were not. Taking a holistic approach to military targeting, we include all of our economic and geographic covariates to construct treatment propensity scores, which are then used for nearest neighbor matching. Results in Appendix Table A.5 are robust.

[19] Note that these time-invariant factors have been identified as key predictors of conflict incidence in modern settings. Bazzi et al. (2022) note: "The most predictive risk factors tend to be slow moving or time invariant. [...] Surprisingly, predictive accuracy improves little when we add time-varying factors, including economic output, government finance, communication infrastructure, natural disasters, elections, and fluctuations in rainfall, temperature, commodity prices, drug production, and U.S. military activity."

[20] Specifically, our controls are agricultural suitability, ruggedness, distance to the coast or navigable river, distance to the border of the Holy Roman Empire; distance to the closest trade route, the existence of fortification, the number of markets; and whether a place was Protestant, all measured in 1618 and interacted with $Post1618_{ijt}$.

To leverage military objectives more directly, we note that the primary goal of warring factions was to take fortified towns, while the exposure of non-fortified towns was more incidental (Guthrie, 2002, p. 160). In Appendix Table A.6, we repeat our main analysis using just the sample of non-fortified towns, which does not affect our results. More restrictively, we draw on the fact that Swedish military success and planning in the invasion of 1630–32, as described in Section 2.2, crucially hinged on maps of strategic targets.[21] We collect information on the universe of 17th century military plans held in the National Archives of Sweden, covering 263 places within the Holy Roman Empire. We show the results of estimating equation (1) just within these places in Appendix Table A.7. Our results remain qualitatively unchanged, with the coefficient on taxation losing significance due to the loss in power associated with reducing our sample by 89%.

In a final exercise, we leverage detailed information about the war to account for the ex ante propensity of each treated unit to be targeted. Our aim is to adjust our estimates for the *expected treatment* of each town (Borusyak and Hull, 2023).[22] During the course of the war, battles served as critical junctures: they allowed the successful army to advance, stalling the defeated troops until they gathered enough strength to engage in battle again. Hence, battles mediated local war exposure. For example, we can trace the five times at which a substantial troop burden weighed on Frankfurt (Oder) to facilitating battles in which the advancing army succeeded, and would have not taken the town otherwise.[23] The Bavarian city of Oberstdorf, on the other hand, was only exposed to the war once, as a result of the Battle at Rain in 1632, and it would have been spared from exposure if the facilitating battle had ended differently. Crucially, battle outcomes were all but deterministic (A. Kraus, 1990, p. 191). This implies that a component of town-level war exposure was random and hence exogenous to local conditions. To construct the expected treatment, we measure town war exposure under different counterfactual realizations of the war, as influenced by this randomness. Our approach is conservative in that it only perturbs the partial equilibrium, removing exposure events for treated towns if a facili-

[21] While Sweden had carefully planned the initial landing on the German Baltic coast, they advanced faster than their terrain knowledge: On July 2, 1631, the Swedish King Gustav II Adolf wrote from Jerichow in northern Saxony to one of his military commanders, Johan Banér: "all our maps stop here", including orders to send the best map makers from Sweden to the German mainland (Gäfvert, 1998, p. 309).

[22] We devote Appendix Section B to an extensive discussion of Borusyak and Hull (2023) in the context of our empirical setting, reserving a brief summary for this section.

[23] Frankfurt (Oder) was exposed to Wallenstein in 1626 and 1627 after he had won the Battle at Dessau Bridge; in 1628 after the Imperial army had conquered Wolgast, in 1631 it was conquered by Gustavus Adolphus, and in 1643 the Swedes passed through after the Battle of Wittstock.

tating battle ends differently in the counterfactual, but leaving the succession of battles unaffected.

The resulting simulations accord with the notion that counterfactual non-exposure to the war requires all relevant battles to have ended differently. Hence, Frankfurt (Oder), which has five facilitating battles, is reached in 94% of the war counterfactuals, whereas Oberstdorf, with one facilitating battle, is affected by the war only in 53% of cases. Appendix Figure A.3 shows the spatial distribution of the simulation-based average counterfactual treatment.

Appendix Table A.8 shows results when we control for the expected treatment. Compared to the results in Table 1, coefficients are qualitatively unchanged, underlining that troop presence is causally linked to taxation, militarization, and parliament elimination.

5 Mechanism: Warfare and Ruler Capacity

We now examine the mechanisms through which war exposure led to an immediate and permanent increase in taxation and militarization, and an eventual decline of deliberative institutions. Our starting point is that the local mobilization of resources created scope for, and acceptance of, bureaucratic infrastructure. For military leaders, communicating with the central ruler of a territory was less costly than coordinating troop upkeep with parliament. This expanded the violent and bureaucratic capacity of the ruler vis-a-vis the elites, allowing him to independently project fiscal and military power. Sidestepping parliamentary constraints this way gradually rendered participatory institutions irrelevant. We proceed to trace each step of this argument, from the immediate state reaction to its persistence and the consequences for parliaments.

First, we demonstrate that troop upkeep was the key driver behind the effects of war exposure on fiscal-military expansion. We omit from our sample war exposure events that were bundled with other potential channels. Appendix Tables A.9, A.10, and A.11 demonstrate that, for each outcome, the effect of the war was not driven by wartime destruction or violence, by places that changed ruler as a result of the war, by towns that encountered extreme religious repression during the counter-reformation, or by places that experienced especially heavy population losses with the plague. Our results are also not driven by Prussia alone, and, in fact, by no other single territory (Appendix Figure A.4). Instead, military leaders, who were overwhelmingly private contractors, relied

on local officials and fiscal infrastructure to expropriate material and human resources in an orderly manner. They attempted to avoid uncoordinated plundering, which sacrificed troop morale, discipline and strength (Wilson, 2018, p. 235). The local bureaucracy had an incentive to comply to "forestall the greater evil of military reprisals" (Wilson, 2009, p. 406).

Next, we provide historical evidence that the war asymmetrically impacted communication costs for parliament, and that military leaders as a result preferentially corresponded with central rulers to mobilize resources. Since the proliferation of mercenaries and the monetarization of feudal aid toward the end of the Middle Ages, rulers were traditionally closer to the military class than members of parliament. They thus served as a natural point of contact. For example, as the Imperial Army leader Albrecht von Wallenstein was advancing into Pomerania in 1627/8, he sent a letter to the duke, stating that "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). This tendency was reinforced by the fact that parliaments became much harder to organize during wartime. In the duchy of Cleves-Mark, the diet of 1642 had to be postponed because roads were deemed unsafe, and many landlords were unwilling to abandon their estates for the time of the diet. Hence, "successful military enterprise rested on a direct relationship with a ruler and with his authority to award tax revenues", with the aim of "long-term integration with the state's authority." (Parrott, 2017, p. 78).

We show empirical evidence that the resulting expansion of violent capacity came at the expense of elites. We estimate equation (1) with three outcomes related to the military. To measure the absorption of the nobility into the absolutist state, we calculate the fraction of the nobility that is employed in the military in a given town and year. To measure militaristic culture, we consider the number of prints which have military language in the title, as well as the number of portraits that depict a military person, at the town-year level.[24] Table 2 shows results. Consistent with the historical evidence, the share of the nobility with a position in the military increased in places exposed to the war. Also, military prints and portraits increased.

Similar to military capacity, taxation institutions were retained after the occupying army had withdrawn: for instance, after reclaiming the town of Hameln in 1633, the Duke

[24] A detailed description of the data is given in Section 3.3.

Table 2: War Exposure and Militarization

	Military Prints (1)	Military Portraits (2)	Nobility in the Military (3)
War Exposure	0.0142**	0.0019**	0.0120***
	(0.0065)	(0.0008)	(0.0038)
R^2	0.39148	0.04902	0.22656
Observations	653,660	653,660	653,660
Outcome Mean	0.0207	0.0020	0.0162
Outcome Def.	(ihs)	(ihs)	(share)
Town FEs	✓	✓	✓
Territory FEs	✓	✓	✓
Year FEs	✓	✓	✓
Cluster	Town	Town	Town

Note Table presents results of estimating equation (1). Observations are at the town-year level. The sample comprises 290 years and 2,390 towns. The dependent variables are (1) the share of nobility from town i in year t that is also in the military, (2) the inverse hyperbolic sine of the number of prints with a militaristic title from town i and year t, and (3) the inverse hyperbolic sine of the number of portraits of military personnel in i 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.

of Brunswick increased the local war tax, which had been instituted by the expelled army (Gelderblom, 2016). The same holds true at the end of the war: Wilson (2018, p. 237) notes that "the new taxes and commissioners were often retained, albeit in modified form, after 1648, as in Brandenburg"; also small territories were "compelled to put their administration on new footing" (Wilson, 2009, p. 406).

This increased capacity was used to ultimately cast aside parliaments. The most salient examples are those in which military strength was directly put to use to break Estate resistance, as it occurred for example in Cleves-Mark (Press, 1991, p. 324). We discuss the gradual extension of taxation infrastructure in an extended case study of the state of Bavaria in Appendix Section C.

Based on these case studies, a key part of the mechanism is the absence of institutional checks that would prevent this self-reinforcing tendency of parliamentary erosion. These checks were not uniformly absent across the territories of the Holy Roman Empire at the outset of the war. Specifically, places differed in their capacity to organize collectively. We measure this concept by drawing on a unique institutional feature: so-called "subject lawsuits" (Diestelkamp, 1985). In 1495, the Imperial High Court had opened and with it an avenue for subjects of territories to pursue litigation against their ruler by means

of referring to the Emperor's court structure. We draw on data of the universe of 40,797 surviving court cases from Schildt and Amend-Traut (2023).[25] We match conflict parties to the locations in our data and identify all towns that were involved in at least one instance of litigation against their territorial ruler prior to the outbreak of the Thirty Years' War. Then, we re-estimate equation (1) with an additional interaction term of treatment with that indicator. We expect this mechanism to increase the capacity of places to mobilize resources while at the same time limiting the scope of ruler overreach. Table 3 shows results: the interaction term is positive and sizeable for taxation and militarization in columns 1 and 2, but negative in column 3 with a magnitude that cancels out the baseline coefficient. These findings affirm the notion that the capacity of the ruler relative to his subjects was the channel through which the war affected state outcomes.

Table 3: War Exposure and Absolutism (Collective Action)

	Direct Taxes (1)	Military Personnel (2)	Parliament Eliminated (3)
War Exposure	0.0263**	0.0231**	0.1077***
	(0.0119)	(0.0078)	(0.0165)
War Exposure × Mechanism	0.0789***	0.0139*	-0.1045***
	(0.0231)	(0.0155)	(0.0229)
R^2	0.57008	0.38033	0.49634
Observations	476,180	653,660	653,660
City FEs	✓	✓	✓
Territory FEs	✓	✓	✓
Year FEs	✓	✓	✓
Controls	✓	✓	✓
Cluster	Town	Town	Town

Note Table presents results of estimating equation (1), with an additional interaction term if a mediation mechanism is present. The mediation mechanism is defined as an indicator of whether the town had seen any previous litigation against the ruler in the Imperial High Court. Observations are at the town-year level. The sample comprises 290 years and 2,390 towns. The dependent variables are (1) a binary variable whether town i has a record of direct territory taxes in year t, (2) the inverse hyperbolic sine of military personnel born in town i active in year t, and (3) a binary variable whether the parliament that represented town i has been eliminated in year t. The sample is obtained via Probit nearest neighbor matching using agricultural suitability, ruggedness, distance to the coast or navigable river, distance to the border of the Holy Roman Empire; distance to the closest trade route, the existence of fortification, the number of markets; and whether a place was Protestant, all measured in 1618. Standard errors are clustered at the town level. *, **, and *** denote significance on the 10 percent, 5 percent, and 1 percent level, respectively.

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

6 The Absolutist State

In this section, we examine the entrenchment of absolutism in more detail. We demonstrate that it was precisely the elimination of parliaments that gave way to further state expansion, propagating the effect to initially untreated towns within the same parliamentary constituency.

The power balance in a participatory regime is maintained through limitation of ruler capacity and protection of the parliamentary coalition. The primary means of ensuring this balance in early modern diets was control of taxation; but parliaments also controlled legislation, vetoed permanent armies, and protected local institutions from overreach. Eliminating parliaments would hence allow a ruler to expand into these domains, and crucially do so in the entire parliamentary constituency, including places that had not been directly exposed to the war.

We measure state expansion through the inverse hyperbolic sine of legislative acts that apply to a town-year cell, and by an indicator of the presence of a standing army. To measure local institutions, we define a "local autocracy" score variable that is 1 if there is a record of a town council appointed by the prince, -1 if the council is elected, and 0 otherwise. To address the notion that the elimination of parliaments propagated the local effects of war exposure, we estimate equation (1) once as a "reduced form" with war exposure as the treatment, and contrast this with results obtained from using parliamentary elimination as the treatment variable.[26]

Table 4 shows results. Panel A considers the reduced form, Panel B the direct effect of absolutism. In column 1, we examine legislation volume. War exposure is associated with an increase in the ihs of legislative acts of the size of the baseline mean, significant at the 5% level. The propensity to have a standing army is 5.8 percentage points higher in war-exposed places (column 2), and the town autocracy score is 0.017 higher compared to a mean of -0.05.

Looking at the association with parliamentary elimination directly in Panel B, we see that the effects point in the same direction, but are considerably more pronounced: For legislation volume, the association is 4.5 times larger compared to war exposure, and significant at the 1% level. Similarly, the effect is 6.5 times larger for standing armies, and

[26] We consider all outcomes at the town level. Legislative events usually applied to the entire territory but were sometimes targeted to specific places. Standing armies were instituted at the territory level. Town councils varied at the town level. Parliaments represented sub-territorial regions. To account for the coarser level at which our treatment is administered, we cluster standard errors at the territory level by default.

marginally larger for the town autocracy score.

Table 4: State Consolidation After Parliaments

	Legislation	Standing Army	Town Autocracy
	(1)	(2)	(3)
Panel A: War Exposure			
War Exposure	0.2311**	0.0588*	0.0177*
	(0.116)	(0.034)	(0.009)
R^2	0.66	0.54	0.82
Panel B: Absolutism			
Eliminates Parliament	1.0618***	0.3810***	0.0204**
	(0.404)	(0.104)	(0.008)
R^2	0.68	0.58	0.82
Number of Observations	653,660	653,660	653,660
Town FE	✓	✓	✓
Territory FE	✓	✓	✓
Year FE	✓	✓	✓
Outcome Mean	1.02	0.23	-0.05
Outcome Def.	(ihs)	(0/1)	(-1/0/1)
Cluster	Territory	Territory	Territory

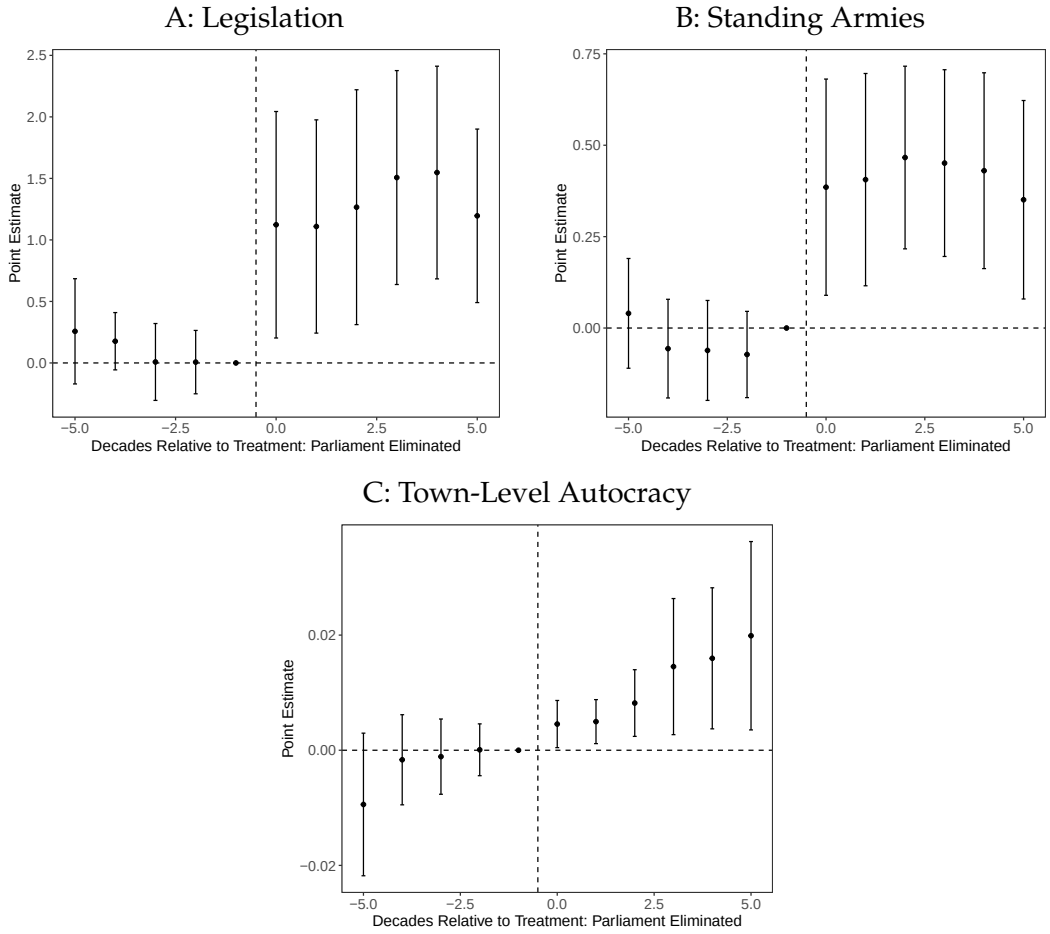
Note Table presents results of estimating equation (1) (Panel A), or its analogue with the elimination of parliaments as treatment (Panel B). Observations are at the town-year level. The sample comprises 290 years and 2,390 towns. The dependent variables are (1) the inverse hyperbolic sine of the number of legislative acts that apply to town i in year t, (2) indicator of the presence of a standing army town i in year t, and (3) a score variable whether the council in town i was appointed by the prince (1) or elected (-1) in year t. Standard errors are clustered at the territory level. *, **, and *** denote significance on the 10 percent, 5 percent, and 1 percent level, respectively.

The event studies in Figure 3, which are analogues of equation 2 with the elimination of parliament as a treatment variable, confirm that the state consolidation timing coincides with the elimination of parliament.[27]

These results hence suggest further expansion of the state once parliaments are eliminated, and they are also consistent with the notion of propagation of the local effects of the war to places that were spared from troop presence. To interpret the difference in coefficients between Panels A and B as only reflective of propagation, we require parliamentary elimination to be uniquely tied to the war. In support of this perspective, Appendix Fig-

[27] In Panel C, the leftmost, pooled coefficient relating to events more than 50 years prior to treatment is not centered at zero, and the standard errors are substantially larger. This likely stems from data quality issues in the early 16th century rather than from an omitted variable: in Appendix Figure A.5, we show the same graph but with ten pre- and post-periods. Again, only the leftmost, pooled pre-period coefficient, which refers to the decades of 1500–1518, is not centered at zero.

Figure 3: State Consolidation After Parliaments (Event Studies)



Note The plot shows results of estimating the event study regression in equation (1), with 95 percent confidence intervals. Observations are at the town-year level. The sample comprises 290 years and 2,390 towns. The dependent variables are (A) the inverse hyperbolic sine of the number of legislative acts that apply to town i in year t, (B) indicator of the presence of a standing army town i in year t, and (C) a score variable whether the council in town i was appointed by the prince (1) or elected (-1) in year t. Standard errors are clustered at the territory level. Appendix Figure A.5 shows Panel C with ten pre- and post-periods.

ure A.6 shows that the emergence of absolutist states was entirely confined to the time period 1618–1710. Leaning into this interpretation in Appendix Table A.12, using (recentered) war exposure as an instrument for parliamentary elimination, leaves our results qualitatively unchanged.

7 The Long Shadow of the War

We conclude our empirical results with a survey of absolutism in the 19th century. In a cross-section, we consider two sides of the state performance medal: capacity, as measured through per capita tax revenue in 1800 (the earliest year for which this data is available), and participation, as measured through the timing of the introduction of the first constitution.[28] We estimate

$$StatePerformance_j = \beta Absolutist_j + Controls_j + \varepsilon_j \tag{3}$$

where $StatePerformance_j$ denotes either per capita tax revenue, or the year of constitution adoption; $Absolutist_j$ is an indicator whether the state has previously eliminated parliament, and $Controls_j$ are territory-level aggregates of the geographic and economic controls described in Section 4.3.[29]

Results are presented in Table 5. Panel A demonstrates that revenue per capita was higher in absolutist states: on average, absolutist states levied 1.6 thalers more than non-absolutist states. The inclusion of controls and territory size quartile fixed effects leaves the effect qualitatively unchanged, as does considering Conley standard errors. Panel B shows that absolutist states took, on average more than a decade longer to adopt a constitution in the 19th century. Again, the results are qualitatively unaffected by the inclusion of controls. Appendix Figure A.7 shows that no single territory is driving these patterns.

[28] In 1815, the founding document of the German Confederation endorsed that its member states adopt a constitution, in the light of increasing popular demands for broader political participation. The timing of the introduction, however, varied widely: the Mecklenburg states had already ratified a constitution in 1755; Prussia resisted a constitution until 1850.

[29] We omit states from our sample that never convened a parliament.

Table 5: Absolutism in the 19th Century: State Performance

	State Revenue (p.c.)				
	(1)	(2)	(3)	(4)	(5)
Absolutist	1.941**	1.588*	1.557*	1.557*	1.557*
	(0.7694)	(0.9183)	(0.8358)	(0.7996)	(0.8482)
Standard-Errors	Heteroskedasticity-robust			50km	100km
R^2	0.16906	0.21663	0.33464	0.33464	0.33464
Observations	36	36	36	36	36
Outcome Mean	4.980	4.980	4.980	4.980	4.980
Controls		✓	✓	✓	✓
Size Qt. fixed effects			✓	✓	✓
	Date of First Constitution				
	(1)	(2)	(3)	(4)	(5)
Absolutist	13.54*	11.62	14.96*	14.96**	14.96***
	(6.934)	(7.571)	(8.014)	(6.858)	(3.245)
Standard-Errors	Heteroskedasticity-robust			50km	100km
R^2	0.08252	0.42304	0.48031	0.48031	0.48031
Observations	35	35	35	35	35
Outcome Mean	1,826.7	1,826.7	1,826.7	1,826.7	1,826.7
Controls		✓	✓	✓	✓
Size Qt. fixed effects			✓	✓	✓

Note Table presents results of estimating equation (3). Observations are at the town-year level. The sample comprises 35 (36) territories. The dependent variables are (A) per capita tax revenue in territory *j* in year 1800, (B) the year of constitution adoption in territory *j*. Standard errors are clustered at the territory level. *, **, and *** denote significance on the 10 percent, 5 percent, and 1 percent level, respectively.

8 Conclusion

In this paper we document the emergence of capable autocracies in response to violent conflict. In novel data, we show how the Thirty Years' War (1618–48), the largest conflict on European soil before World War I, facilitated the consolidation of *ex ante* weak states at the expense of early forms of parliament. War exposure gave rise to local needs for centralized intervention, easing constraints on the ruler. We leverage exogenous variation in troop movements to estimate this initial fiscal-military expansion and the unraveling of parliaments it enabled. Once established, absolutist regimes saw accelerated state growth, while at the same time undoing local democratic institutions. These changes persisted for centuries after the war.

The phenomena studied in this setting did not go unnoticed by contemporaries. In

England, the second half of the 17th century saw fierce debates on the ratchet effects of a standing army — John Trenchard's work "An Argument, Shewing that a Standing Army is Inconsistent with a Free Government" (1697) is exemplary in this respect. The United States constitution, framed nearly a century later, requires military funds to be limited to two years at a time as a preventive check. Even more closely related to our proposed mechanism, the Third Amendment to the Constitution prohibits the quartering of soldiers in private homes without the consent of the owner. In England, it was exactly an attempt at this practice under martial law that gave rise to the Petition of Right of 1628, a key factor in the revolution to follow. The tensions induced by troop quartering, and their implications for government outcomes, remain acute in contexts of insurgent and paramilitary groups around the world today. Further work will shed light on the nexus between regime type and external threat in contemporary settings.

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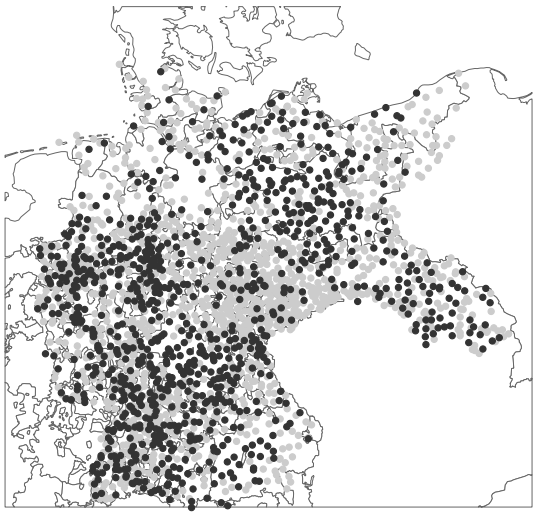
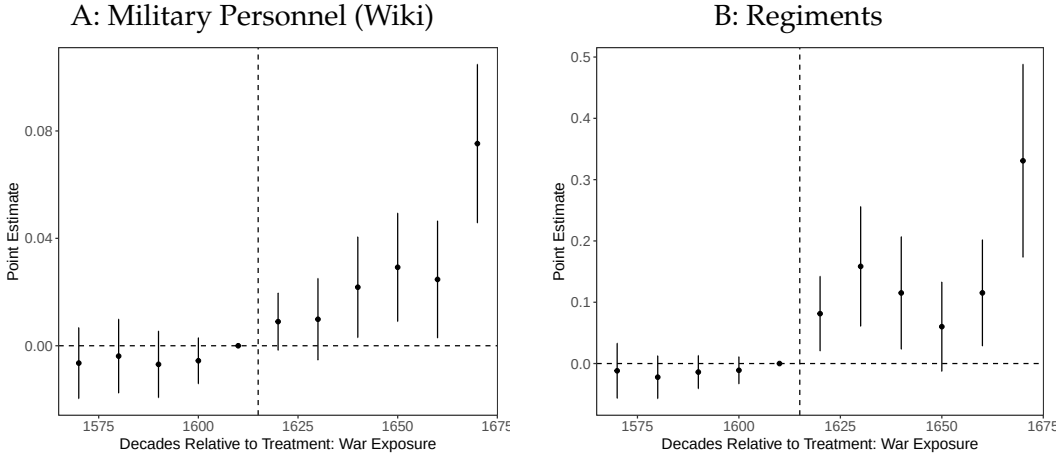
Supplementary Appendix: For Online Publication
A Tables and Figures

Figure A.1: Locations of War-Exposed towns



Note The map illustrates the war exposure data from Cantoni and Weigand (2021). Each point is the location of a city in our data. Highlighted points are towns that have at least one associated war exposure event. Base map shows territories in the Holy Roman Empire in 1600 from Nüssli (2014).

Figure A.2: Alternative Measures of Militarization Event Studies



Note The plot shows results of estimating (A) the event study regression in equation (1), and (B) its territory-level analogue, with 95 percent confidence intervals. Observations are at the town-year level in Panel A and at the territory-year level in Panel B. The sample comprises 290 years, 213 territories, and 2,390 towns. The dependent variables are (A the inverse hyperbolic sine of military personnel born in town i active in year t, according to Wikipedia data, and (B) the inverse hyperbolic sine of regiments active in territory j in year t.

The territory-level event study specification is

$$Consolidation_{jt} = \sum_{\tau=-5}^{5} \beta_\tau Treated_j \times RelativeDecade_{\tau(t)} + \alpha_j + \alpha_t + \varepsilon_{jt}$$

Where $Treated_j$ is a binary indicator of town-level exposure volume larger than 50% of the number of towns in territory j.

Standard errors are clustered at the town level (Panel A), and at the territory level (Panel B).

Table A.1: War Exposure and Absolutism (Controls)

	Direct Taxes (1)	Military Personnel (2)	Parliament Eliminated (3)
War Exposure	0.0432***	0.0196***	0.0579***
	(0.0113)	(0.0070)	(0.0139)
R^2	0.57880	0.38765	0.50538
Observations	476,180	653,660	653,660
Outcome Mean	0.0621	0.0281	0.1222
Outcome Def.	(0/1)	(ihs)	(0/1)
Town FEs	✓	✓	✓
Territory FEs	✓	✓	✓
Year FEs	✓	✓	✓
Cluster	Town	Town	Town
Controls	✓	✓	✓

Note Table presents results of estimating equation (1), including controls. Observations are at the town-year level. The sample comprises 290 years and 2,390 towns. The dependent variables are (1) a binary variable whether town i has a record of direct territory taxes in year t, (2) the inverse hyperbolic sine of military personnel born in town i active in year t, and (3) a binary variable whether the parliament that represented town i has been eliminated in year t. Controls are agricultural suitability, ruggedness, distance to the coast or navigable river, distance to the border of the Holy Roman Empire; distance to the closest trade route, the existence of fortification, the number of markets; and whether a place was Protestant, all measured in 1618 and interacted with $Post1618_{ijt}$. Standard errors are clustered at the town level. *, **, and *** denote significance on the 10 percent, 5 percent, and 1 percent level, respectively.

Table A.2: War Exposure and Direct Taxes (Standard Errors)

	Direct Taxes					
	(1)	(2)	(3)	(4)	(5)	(6)
War Exposure	0.0543***	0.0543***	0.0543**	0.0543***	0.0543**	0.0543*
	(0.0115)	(0.0178)	(0.0249)	(0.0191)	(0.0261)	(0.0283)
Standard-Errors	city_id	terr_id_1618	region_id	50km	100km	200km
R^2	0.56800	0.56800	0.56800	0.56800	0.56800	0.56800
Observations	476,180	476,180	476,180	476,180	476,180	476,180
Outcome Mean	0.0621	0.0621	0.0621	0.0621	0.0621	0.0621
Town FEs	✓	✓	✓	✓	✓	✓
Territory FEs	✓	✓	✓	✓	✓	✓
Year FEs	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓

Note Table presents results of estimating equation (1), using different standard errors. Observations are at the town-year level. The sample comprises 290 years and 2,390 towns. The dependent variables is a binary variable whether town i has a record of direct territory taxes in year t. Standard errors are clustered at the level of (1) towns, (2) territories, (3) regions, or Conley standard errors with a cutoff of (4) 50km, (5) 100km, or (6) 200km. *, **, and *** denote significance on the 10 percent, 5 percent, and 1 percent level, respectively.

Table A.3: War Exposure and Military Personnel (Standard Errors)

	Military Personnel					
	(1)	(2)	(3)	(4)	(5)	(6)
War Exposure	0.0282***	0.0282***	0.0282***	0.0282***	0.0282***	0.0282***
	(0.0074)	(0.0095)	(0.0096)	(0.0080)	(0.0089)	(0.0108)
Standard-Errors	city_id	terr_id_1618	region_id	50km	100km	200km
R^2	0.38022	0.38022	0.38022	0.38022	0.38022	0.38022
Observations	653,660	653,660	653,660	653,660	653,660	653,660
Outcome Mean	0.0281	0.0281	0.0281	0.0281	0.0281	0.0281
Town FEs	✓	✓	✓	✓	✓	✓
Territory FEs	✓	✓	✓	✓	✓	✓
Year FEs	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓

Note Table presents results of estimating equation (1), using different standard errors. Observations are at the town-year level. The sample comprises 290 years and 2,390 towns. 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) territories, (3) regions, or Conley standard errors with a cutoff of (4) 50km, (5) 100km, or (6) 200km. *, **, and *** denote significance on the 10 percent, 5 percent, and 1 percent level, respectively.

Table A.4: War Exposure and Parliament Elimination (Standard Errors)

	Parliament Eliminated					
	(1)	(2)	(3)	(4)	(5)	(6)
War Exposure	0.0700***	0.0700**	0.0700*	0.0700**	0.0700*	0.0700***
	(0.0138)	(0.0320)	(0.0354)	(0.0281)	(0.0361)	(0.0242)
Standard-Errors	city_id	terr_id_1618	region_id	50km	100km	200km
R^2	0.49415	0.49415	0.49415	0.49415	0.49415	0.49415
Observations	653,660	653,660	653,660	653,660	653,660	653,660
Outcome Mean	0.1222	0.1222	0.1222	0.1222	0.1222	0.1222
Town FEs	✓	✓	✓	✓	✓	✓
Territory FEs	✓	✓	✓	✓	✓	✓
Year FEs	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓

Note Table presents results of estimating equation (1), using different standard errors. Observations are at the town-year level. The sample comprises 290 years and 2,390 towns. The dependent variables is a binary variable whether the parliament that represented town i has been eliminated in year t. Standard errors are clustered at the level of (1) towns, (2) territories, (3) regions, or Conley standard errors with a cutoff of (4) 50km, (5) 100km, or (6) 200km. *, **, and *** denote significance on the 10 percent, 5 percent, and 1 percent level, respectively.

Table A.5: War Exposure and Absolutism (Matching)

	Direct Taxes (1)	Military Personnel (2)	Parliament Eliminated (3)
War Exposure	0.0460***	0.0258***	0.0640***
	(0.0155)	(0.0086)	(0.0171)
R^2	0.53161	0.37065	0.50562
Observations	288,260	412,090	412,090
Matched Sample	✓	✓	✓
Outcome Mean	0.0754	0.0348	0.1329
Outcome Def.	(0/1)	(ihs)	(0/1)
Town FEs	✓	✓	✓
Territory FEs	✓	✓	✓
Year FEs	✓	✓	✓
Cluster	Town	Town	Town

Note Table presents results of estimating equation (1), in a matched sample. Observations are at the town-year level. The sample comprises 290 years and 2,390 towns. The dependent variables are (1) a binary variable whether town i has a record of direct territory taxes in year t, (2) the inverse hyperbolic sine of military personnel born in town i active in year t, and (3) a binary variable whether the parliament that represented town i has been eliminated in year t. The sample is obtained via Probit nearest neighbor matching using agricultural suitability, ruggedness, distance to the coast or navigable river, distance to the border of the Holy Roman Empire; distance to the closest trade route, the existence of fortification, the number of markets; and whether a place was Protestant, all measured in 1618. Standard errors are clustered at the town level. *, **, and *** denote significance on the 10 percent, 5 percent, and 1 percent level, respectively.

Table A.6: War Exposure and Absolutism (Non-Fortified)

	Direct Taxes (1)	Military Personnel (2)	Parliament Eliminated (3)
War Exposure	0.0516***	0.0185***	0.0740***
	(0.0114)	(0.0067)	(0.0143)
R^2	0.56877	0.35055	0.49530
Observations	454,140	624,660	624,660
Outcome Mean	0.0579	0.0217	0.1235
Outcome Def.	(0/1)	(ihs)	(0/1)
Town FEs	✓	✓	✓
Territory FEs	✓	✓	✓
Year FEs	✓	✓	✓
Cluster	Town	Town	Town

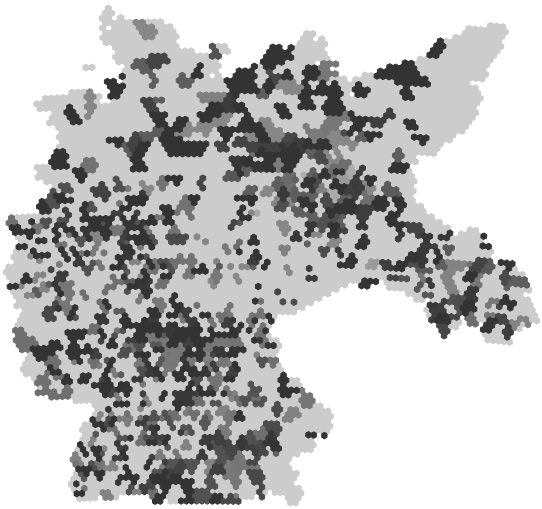
Note Table presents results of estimating equation (1), in a matched sample. Observations are at the town-year level. The sample comprises 290 years and 2,154 towns. The dependent variables are (1) a binary variable whether town *i* has a record of direct territory taxes in year *t*, (2) the inverse hyperbolic sine of military personnel born in town *i* active in year *t*, and (3) a binary variable 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.

Table A.7: War Exposure and Absolutism (Swedish Plans)

	Direct Taxes (1)	Military Personnel (2)	Parliament Eliminated (3)
War Exposure	0.0270	0.0958**	0.0778*
	(0.0324)	(0.0479)	(0.0419)
R^2	0.51246	0.44653	0.50658
Observations	55,100	67,570	67,570
Outcome Mean	0.0912	0.1525	0.1237
Outcome Def.	(0/1)	(ihs)	(0/1)
Town FEs	✓	✓	✓
Territory FEs	✓	✓	✓
Year FEs	✓	✓	✓
Cluster	Town	Town	Town

Note Table presents results of estimating equation (1), in a matched sample. Observations are at the town-year level. The sample comprises 290 years and 263 towns. The dependent variables are (1) a binary variable whether town *i* has a record of direct territory taxes in year *t*, (2) the inverse hyperbolic sine of military personnel born in town *i* active in year *t*, and (3) a binary variable 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.

Figure A.3: Average Simulated Counterfactual Treatment



Note The map shows the distribution of the average simulated counterfactual treatment, from 1,000 simulations.

Table A.8: War Exposure and Absolutism (Expected Treatment)

	Direct Taxes (1)	Military Personnel (2)	Parliament Eliminated (3)
War Exposure	0.0760**	0.0803***	0.1402***
	(0.0362)	(0.0297)	(0.0438)
R^2	0.56806	0.38075	0.49450
Observations	476,180	653,660	653,660
Outcome Mean	0.0621	0.0281	0.1222
Outcome Def.	(0/1)	(ihs)	(0/1)
Town FEs	✓	✓	✓
Territory FEs	✓	✓	✓
Year FEs	✓	✓	✓
Cluster	Town	Town	Town
Controls	✓	✓	✓

Note Table presents results of estimating equation (1), controlling for the "expected treatment". Observations are at the town-year level. The sample comprises 290 years and 2,390 towns. The dependent variables are (1) a binary variable whether town i has a record of direct territory taxes in year t, (2) the inverse hyperbolic sine of military personnel born in town i active in year t, and (3) a binary variable 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.

Table A.9: War Exposure and Direct Taxes (Subsets)

	Direct Taxes					
	(1)	(2)	(3)	(4)	(5)	(6)
War Exposure	0.0543***	0.0804***	0.0461***	0.0841***	0.0569***	0.0511***
	(0.0115)	(0.0152)	(0.0120)	(0.0180)	(0.0143)	(0.0129)
R^2	0.56800	0.58259	0.55194	0.62329	0.57423	0.56879
Observations	476,180	327,990	425,140	253,750	414,120	368,300
Outcome Mean	0.0621	0.0735	0.0561	0.0609	0.0598	0.0626
Town FEs	✓	✓	✓	✓	✓	✓
Territory FEs	✓	✓	✓	✓	✓	✓
Year FEs	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓
Cluster	Town	Town	Town	Town	Town	Town
Stable Territory		✓				
Not Counterreformed			✓			
No Plague				✓		
Undestroyed					✓	
Ever Convened Parliament						✓

Note Table presents results of estimating equation (1), focusing on subsets of the data. Observations are at the town-year level. The sample comprises 290 years and 2,390 towns. The dependent variables is a binary variable whether town *i* has a record of direct territory taxes in year *t*. We omit places that experienced a rule change as a result of the Thirty Years' War (1), counter-reformation (2), the plague (3), destruction (4), and places that did not convene formal parliaments to begin with (5). Standard errors are clustered at the town level. *, **, and *** denote significance on the 10 percent, 5 percent, and 1 percent level, respectively.

Table A.10: War Exposure and Military Personnel (Subsets)

	Military Personnel					
	(1)	(2)	(3)	(4)	(5)	(6)
War Exposure	0.0282***	0.0212***	0.0274***	0.0049	0.0424***	0.0351***
	(0.0074)	(0.0081)	(0.0082)	(0.0053)	(0.0103)	(0.0092)
R^2	0.38022	0.39917	0.39165	0.20067	0.38260	0.37941
Observations	653,660	485,170	570,140	333,790	560,280	476,760
Outcome Mean	0.0281	0.0250	0.0276	0.0117	0.0289	0.0309
Town FEs	✓	✓	✓	✓	✓	✓
Territory FEs	✓	✓	✓	✓	✓	✓
Year FEs	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓
Cluster	Town	Town	Town	Town	Town	Town
Stable Territory		✓				
Not Counterreformed			✓			
No Plague				✓		
Undestroyed					✓	
Ever Convened Parliament						✓

Note Table presents results of estimating equation (1), focusing on subsets of the data. Observations are at the town-year level. The sample comprises 290 years and 2,390 towns. The dependent variable is the inverse hyperbolic sine of military personnel born in town *i* active in year *t*, and We omit places that experienced a rule change as a result of the Thirty Years' War (1), counter-reformation (2), the plague (3), destruction (4), and places that did not convene formal parliaments to begin with (5). Standard errors are clustered at the town level. *, **, and *** denote significance on the 10 percent, 5 percent, and 1 percent level, respectively.

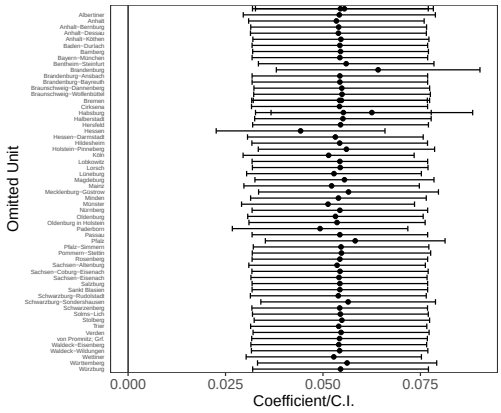
Table A.11: War Exposure and Parliament Elimination (Subsets)

	Parliament Eliminated					
	(1)	(2)	(3)	(4)	(5)	(6)
War Exposure	0.0700***	0.0996***	0.0707***	0.0502**	0.0343**	0.0816***
	(0.0138)	(0.0171)	(0.0149)	(0.0202)	(0.0158)	(0.0171)
R^2	0.49415	0.52099	0.49373	0.48046	0.48157	0.53501
Observations	653,660	485,170	570,140	333,790	560,280	476,760
Outcome Mean	0.1222	0.1256	0.1183	0.1098	0.1120	0.1676
Town FEs	✓	✓	✓	✓	✓	✓
Territory FEs	✓	✓	✓	✓	✓	✓
Year FEs	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓
Cluster	Town	Town	Town	Town	Town	Town
Stable Territory		✓				
Not Counterreformed			✓			
No Plague				✓		
Undestroyed					✓	
Ever Convened Parliament						✓

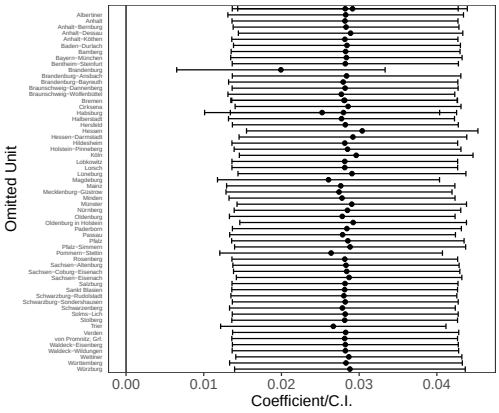
Note Table presents results of estimating equation (1), focusing on subsets of the data. Observations are at the town-year level. The sample comprises 290 years and 2,390 towns. The dependent variables is a binary variable whether the parliament that represented town *i* has been eliminated in year *t*. We omit places that experienced a rule change as a result of the Thirty Years' War (1), counter-reformation (2), the plague (3), destruction (4), and places that did not convene formal parliaments to begin with (5). Standard errors are clustered at the town level. *, **, and *** denote significance on the 10 percent, 5 percent, and 1 percent level, respectively.

Figure A.4: War Exposure and Absolutism (Leave-Out Plots)

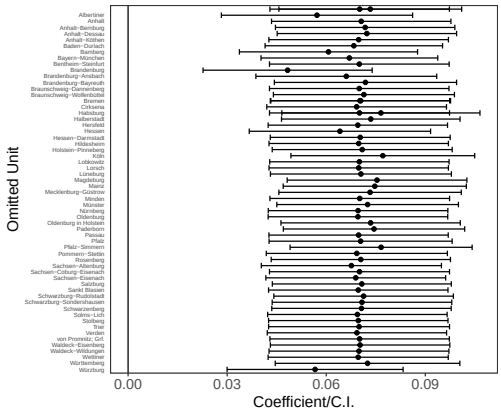
A: Taxation Growth



B: Militarization

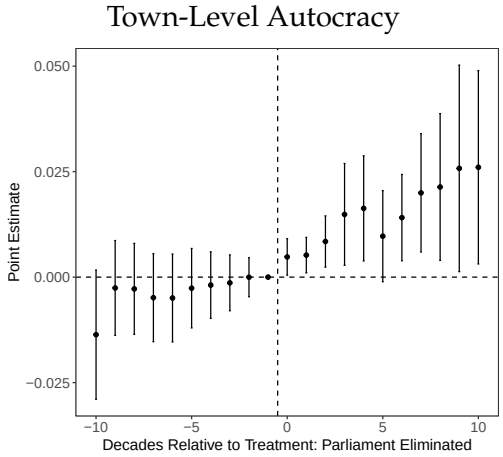


C: Deliberative Institutions



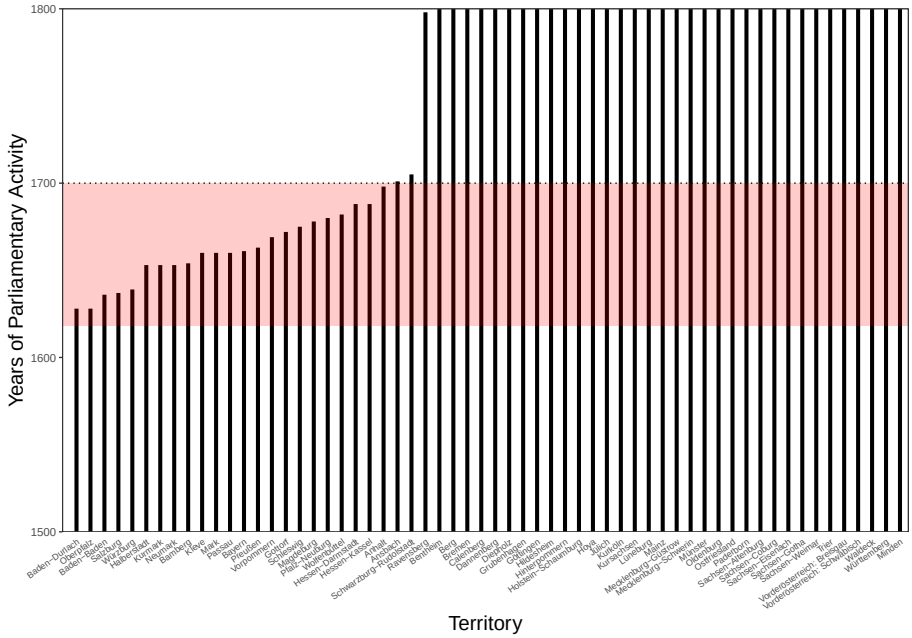
Note The plot shows results of estimating equation (1), with 95 percent confidence intervals, leaving out one territory that convened a parliament in 1500 at a time. Observations are at the town-year level. The sample comprises 290 years and 2,390 towns. The dependent variables are (A) a binary variable whether town i has records of direct territory taxes in year t, (B) the inverse hyperbolic sine of military personnel born in town i active in year t, and (C) a binary variable whether the parliament that represented town i has been eliminated in year t. Standard errors are clustered at the town level.

Figure A.5: Local Autocracy After Parliaments (Long Event Study)



Note The plot shows results of estimating the event study regression in equation (1), with 95 percent confidence intervals and ten pre- and post-periods. Observations are at the town-year level. The sample comprises 290 years and 2,390 towns. The dependent variable is a score variable whether the council in town i was appointed by the prince (1) or elected (-1) in year t. Standard errors are clustered at the territory level.

Figure A.6: Timing of the Elimination of Parliaments



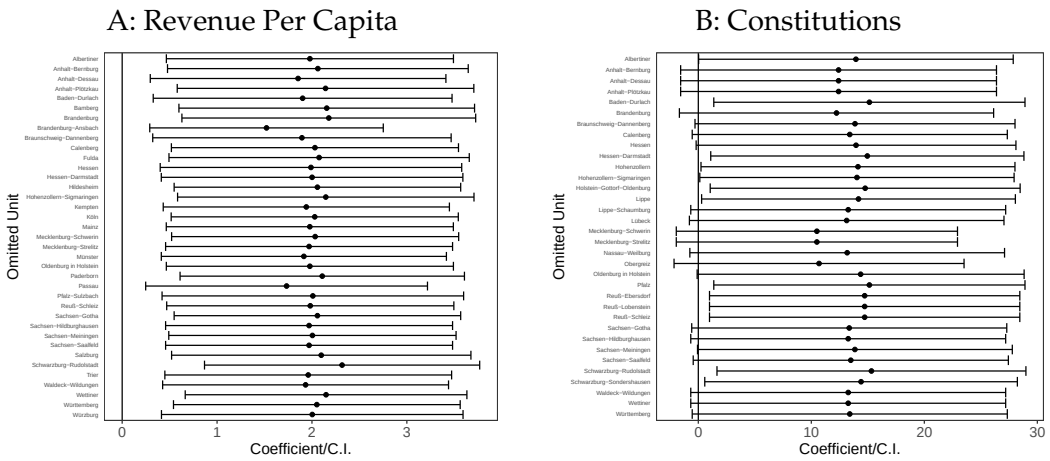
Note The graph shows the time range during which parliaments existed in the territories of the Holy Roman Empire, for each territory that ever convened parliament separately.

Table A.12: State Consolidation After Parliaments (IV)

	Legislation Volume (1)	Standing Army (2)	Town Autocracy (3)
Parliament Eliminated	2.865***	0.7774**	0.3646**
	(1.011)	(0.3175)	(0.1544)
R^2	0.60905	0.53514	0.76999
Observations	653,660	653,660	653,660
Recentered IV	✓	✓	✓
Outcome Mean	1.018	0.2304	-0.0536
Outcome Def.	(ihs)	(0/1)	(-1/0/1)
Town FEs	✓	✓	✓
Territory FEs	✓	✓	✓
Year FEs	✓	✓	✓
Cluster	Territory	Territory	Territory

Note Table presents results of estimating equation (1) with parliament elimination as treatment, using recentered war exposure as an instrumental variable Observations are at the town-year level. The sample comprises 290 years and 2,390 towns. The dependent variables are (1) the inverse hyperbolic sine of the number of legislative acts that apply to town i in year t, (2) indicator of the presence of a standing army town i in year t, and (3) a score variable whether the council in town i was appointed by the prince (1) or elected (-1) in year t. Standard errors are clustered at the territory level. *, **, and *** denote significance on the 10 percent, 5 percent, and 1 percent level, respectively.

Figure A.7: Absolutism in the 19th Century (Leave-Out Plots)



Note The plot shows results of estimating equation (3), with 95 percent confidence intervals, leaving out one territory in the sample at a time. Observations are at the territory level. The sample comprises 35 and 36 territories. The dependent variables are (A) per capita tax revenue in territory j in year 1800, (B) the year of constitution adoption in territory j. Standard errors are clustered at the territory level.

B Borusyak and Hull (2023)

We approach our setting in a design-based framework. The thought experiment hence fixes the sample — all 2,390 towns in the Holy Roman Empire are observed — and considers the treatment assignment, that is, the course of the war, to be stochastic.

From this perspective, targeting implies that a place would have been exposed to the war in many counterfactual realizations of the treatment. Intuitively, we want to discount evidence from these frequently-visited places in our empirical approach.

This starting point provides natural bounds on the informativeness of the estimates in Table 1: If the course of the war was non-stochastic, that is, it could *only* have happened like it did, we should discount every observation, and the OLS coefficients are not informative of the treatment effect without further assumptions. If, on the other hand, the war would have exposed an entirely different set of towns every time, so every unit had the exact same ex ante propensity to be treated, then the results in Table 1 are unbiased.

As much of the historical evidence highlights, neither of these extremes seems realistic: the war was, by no means, deterministic; at the same time, some places were much more likely to be affected by the war than others. Intuitively, we would hence like to adjust our estimates for the ex ante propensity of each unit to be targeted, leveraging the contingencies that the war created for identification. We draw on Borusyak and Hull (2023) to operationalize this notion.

The approach requires two components: first, a treatment that is assigned through a known function of exogenous shocks and predetermined variables; second, a known distribution from which the shocks are drawn. Then, a researcher can generate counterfactual shock realizations from that distribution and plug these into the treatment function to compute a counterfactual treatment vector. Averaging over all of these counterfactual treatments, one obtains a summary measure of non-randomness in shock exposure for each unit, the *expected treatment*. To obtain a consistent estimate, it is sufficient to include the expected treatment as a control in the empirical specification.

In our context, the expected treatment is the fraction of times that a given place would have experienced local war exposure under many different realizations of the war. Our approach aims to stay close to the actual course of the war, and give a lower bound on the randomness inherent to treatment assignment. We hence obtain a conservative control function: The simulated average treatment vector will be uniformly closer to the realized treatment vector than the true expected treatment vector. Specifically, we consider the outcome of facilitating battles to be the only shocks to local war exposure.

Our algorithm proceeds in three steps:

1. **Treatment Assignment Function**. We link each of the 1,715 town-level war exposure events to a facilitating battle, as described in Section 3.2.

2. **Shock Distribution.** We estimate battle win probabilities in a logistic regression based on a range of geography- and army-specific battle-level observables. We fol-

low Borusyak and Hull (2023), Appendix C.5, to fit the probabilities such that the recentered treatment is balanced on observables.[1] We then draw counterfactual battle outcomes from a Bernoulli distribution governed by the estimated probabilities, and delete all town exposure events that would not have happened, according to the counterfactual battle results.

3. **Expected Exposure.** We aggregate this set of counterfactual exposure events to the town level to obtain the set of counterfactually treated towns. We repeat this process 1,000 times to calculate the expected treatment of each town.

2

[1] We aim for balance along all of our town characteristics mentioned in Section 4.3: agricultural suitability, ruggedness, distance to the coast or navigable river, distance to the border of the Holy Roman Empire; distance to the closest trade route, the existence of fortification, the number of markets; and whether a place was Protestant, all measured in 1618. Our battle covariates are absolute troop strengths of both sides, relative troop strenght, the distance of a battle field from coasts, rivers, and borders, battle latitude, longitude, and altitude. Our results are qualitatively robust to instead fitting a logistic model based on these variables to maximize the MLE objective function, and they are also robust to instead using relative troop strength at the outset of the battle as a sufficient statistic for the predictable component of battle outcomes. They are furthermore robust to only considering battles which saw the death of a military leader, or those that were "close" ex ante (i.e. had a troop strength asymmetry of below 60-40).

[2] Viewed from this perspective, the war is a collection of shift-share settings: each battle is a shock that affects towns according to the battle outcome, and according to the town's vulnerability. The shock-generating function has a random component.

C Parliamentary Unraveling: Bavaria Case Study

To illustrate how this initial shift in power balance unraveled parliamentary participation, we present the well-documented case of the Duchy of Bavaria.

Bavaria, at the outset of the war, was a parliamentary state in which taxation was heavily guarded. Early attempts at raising war revenue were blocked by the Estates with reference to the fact that this form of taxation was "not possible or feasible in these lands"; five years into the war, this position was reiterated, referring to the war tax as a "new, unheard-of, and never-before practiced method in the lands of Your Serene Highness" (Kummer, 2005, p. 102, 112).

As the war increasingly inflicted the Bavarian lands, the contributions system wrested war financing from the hands of the Estates. Bavarian local account books seamlessly transitioned from recording Estate taxes in 1630 to recording military contributions after the Swedish invasion from 1632 (Kraus, 2021, p. 321). Following the Swedish expulsion, the duke's position hence improved considerably, justifying his financial leeway with reference to "the preservation of the God-given lands and people in the duchy, our beloved fatherland"; the Estates, on the other hand, had not been summoned in a diet since the outbreak of the war and were reduced to unsuccessful pleas to "soon return to the traditional way of governance" (Kummer, 2005, p. 108, 113).

The continuation of the war accelerated this tendency; in 1634, Maximilian, for the first time, collected a tax through his own administrative apparatus, without prior consent of the Estates (Burger, 2012, p. 40); the same occurred in 1647. By the end of the war, he continued and refined the contributions system (Kraus, 2021, p. 246).

At the same time, new positions in the expanding state bureaucracy and military opened up. This naturally changed the incentive, especially of poorer nobility, to exit the nobility coalition in favor of the emergent absolutist state. Rulers were acutely aware of this; in Bavaria, duke Maximilian did not employ violence against the nobility, but instead "left the Bavarian Estates intact, relying on economic pressures that made court, administrative and military appointments increasingly attractive to the local nobles" (Wilson, 2009, p. 359).

His successor, Ferdinand Maria, convened one last diet in 1661, before entirely relegating the residual role of the Estates to a permanent commission.