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Warfare and Economic Inequality: Evidence from Preindustrial Germany (c. 1400-1800)

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ABSTRACT

What was the impact of military conflict on economic inequality? I argue that ordinary military conflicts increased local economic inequality. Warfare raised the financial needs of communities in preindustrial times, leading to more resource extraction from the population. This resource extraction happened via inequality-promoting channels, such as regressive taxation. Only in truly major wars might inequality-reducing destruction outweigh inequality-promoting extraction and reduce inequality. To test this argument I construct a novel panel dataset combining information about economic inequality in 75 localities, and more than 700 conflicts over four centuries. I find that the many ordinary conflicts — paradigmatic of life in the preindustrial world — were continuous reinforcers of economic inequality. I confirm that the Thirty Years' War was indeed a great equaliser, but this was an exception and not the rule. Rising inequality is an underappreciated negative externality in times of conflict.

1. Introduction

It has become a historic truism that wars reduce economic inequality. Whether this negative relationship is due to the destruction of capital, demographic decline, the confiscation of wealth of the rich, plundering, state collapse or decline of trade and commerce, this empirical regularity is thought to hold throughout history (for studies on equalising wars see van Zanden 1995, Piketty 2014, Scheve and Stasavage 2016, Milanovic 2016, Scheidel 2017, Alfani, Gierok and Schaff 2022).

This paper examines the accuracy of the "wars are great equalisers"-hypothesis for wars in the preindustrial period. It argues that the conventional view has focused mostly on major wars, and therefore overlooks that warfare had two countervailing effects: multifaceted destruction could reduce inequality, but inegalitarian resource extraction could also increase inequality. Because this second factor often outweighed the limited destruction of *ordinary* conflicts, most wars between major political authorities in the preindustrial era actually led to higher economic inequality. This was the result of political authorities being induced by the threat of

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war to extract economic resources. Extraction happened via inequality-promoting channels, most importantly regressive taxation, and credit. Only in truly major wars could destruction outweigh extraction and reduce inequality. The Holy Roman Empire – Germany, for short[1] – is the ideal testing ground for this hypothesis. Germany was a cockpit of preindustrial warfare. It saw many ordinary wars, but was also the area where a truly major war took place, the Thirty Years' War (1618-48). Moreover, Germany was historically a very diverse entity: economically, geographically and institutionally. It was politically and in terms of security provision a highly decentralised area. I will account for this diversity and make use of it to document the countervailing effects of warfare on inequality.

To test my hypothesis I assemble a new body of evidence. I employ data on the distribution of wealth in 75 urban and rural communities, based on information on the wealth of about 417,000 households, collected from archival tax records and secondary sources. Wealth inequality data are combined with information about more than 700 battles and sieges between important political actors to examine conflict and inequality in Germany 1400 to 1800. To shed light on the mechanisms I also gather data about the local presence of military garrisons and military construction activity. Following the literature, my first econometric strategy exploits the plausibly exogenous occurrence of battle action, conditional on a rich set of controls. I complement this reduced-form exercise with a flexible difference-in-differences (or event study) research design for two case studies, to support the common trends assumption and to establish a more robust causal relationship. I combine this empirical analysis with a historical account of what actually happened in places that experienced warfare in preindustrial Germany, and I rationalise this historical account in a theoretical framework about the two countervailing effects of conflicts on inequality.

To address concerns about endogeneity I employ several strategies. Most importantly I limit the analysis to conflicts between important political actors — analogous to interstate wars in modern times — because these wars were more likely to be exogenous events for individual communities. I employ a conflict measure that indicates whether a community experienced conflict in its surroundings, not whether it was actually attacked, thus reducing the potential of the treatment being correlated with economic outcomes. I also review the reasons historians have identified for the outbreak of each war in the dataset, and find that almost all wars broke out for reasons that were unlikely to be related to local inequality. I confirm the main results in two case studies, of the Schmalkaldic War (1546-47) and the Palatine Succession War (1688-97), using a flexible difference-in-differences set up.

I find a strong and positive relationship running from warfare to economic inequality. During ordinary conflicts wealth shares shifted from the lower and middle classes of society to the rich. As a consequence, economic inequality increased. This strong statistical relationship is also economically relevant. Specifically, a one-standard deviation increase in conflict exposure is associated with an increase in the Gini coefficient by 0.21 to 0.22 standard deviations. This result is robust to several checks. Most importantly, I show that the effect of warfare is qualitatively preserved across a range of different threshold distances, suggesting that selection into treatment is unlikely to drive the results. Moreover, the main result is supported by the flexible difference-in-differences estimates. The positive conflict-inequality relationship found during ordinary wars was significantly different from what is found during the time of the Thirty Years' War. That war was the exception and not the rule, simply because it was exceptionally destructive in several ways and lasted for thirty years. It is therefore no surprise that this war had a different effect on inequality compared to ordinary wars.

My conceptual framework suggests that the seemingly puzzling empirical results are due to two countervailing effects of conflicts on inequality: egalitarian destruction, and inequality-promoting extraction, mostly via regressive taxation. Usually the latter factor outweighed the former one to increase inequality. This interpretation is consistent with several additional results. I document that localities in close proximity to a conflict actually experienced inequality decline, but that a positive warfare-inequality relationship emerged gradually at greater distances from battle action. This pattern suggests that ordinary wars' direct destructive effect mattered little for inequality, because war-related destruction was geographically limited, causing comparatively little total destruction. Instead, because the indirect extractive inequality-promoting effect of warfare affected a larger geographical area, it overall outweighed the destructive inequality-reducing effect. Additionally, the pattern in the shift of wealth shares, away from the poor and towards the rich, is characteristic of regressive taxation. Moreover, wars were associated with a higher increase of inequality in the second half of the seventeenth and eighteenth centuries compared to previous periods. This later period was the time when the "Military Revolution" was in full swing, which made warfare immensely costly and increased political authorities' need to extract resources. I also find that places that were exposed to conflicts repeatedly experienced a higher inequality increase, presumably because authorities had to extract resources from the population repeatedly via inequality-promoting channels.

The most suggestive evidence indicative of the hypothesised mechanism — costly wars inducing authorities to inequality-promoting extraction — comes from data on local defence infrastructure. I find that the setting up of garrisons, central to communities' protection, were associated with higher inequality, as was the construction of buildings with military purpose, such as barracks, arsenals or foundries for weapons.

The paper makes three principal contributions. First, it contributes to the literature on economic inequality. Recent research has revealed a striking empirical pattern: contrary to a conventional "Kuznetsian view" (see Kuznets 1955), inequality across Europe did not start to grow with the beginning of industrialisation, but increased almost constantly since about the sixteenth century. In other words, much of the high levels of inequality observed in the early twentieth century might actually have preindustrial roots (Alfani 2021). Yet little is known about why inequality reached high levels before industrialisation began. The expansion of markets and the economy (van Zanden 1995, van Bavel 2016), demographic growth (Milanovic 2016, Pfister 2020), exclusive

[1] The geographical area this paper is interested in, is the Holy Roman Empire of the German Nation, and more precisely its inner part that was subject to the imperial constitution (see Whaley 2012). Geographically this area corresponds roughly to modern Germany and Austria. As a shorthand, I will refer to this area either as "Germany" or "the Empire".

political institutions (Alfani and Ryckbosch 2016, Minns et al. 2020), and ideological factors have been proposed as major causes of preindustrial inequality growth (Piketty 2020). Major warfare has commonly been considered a factor that reduced economic inequality (for example, Scheidel 2017).[2] This paper empirically shows that the focus on big wars might be misleading when we want to understand the general relationship between warfare and inequality. My findings suggest that we need to consider the indirect effects of conflicts (see Glick and Taylor 2010), especially the many ordinary ones, because they generated a negative externality in the form of increasing economic inequality. Conflicts might thus be a significant part of the explanation for why economic inequality was high already when industrialisation was about to start. This is in line with Alfani and Di Tullio (2019), who suggest that the rise of the fiscal state might explain preindustrial inequality growth. The results are also in line with research showing that even a conflict as disruptive as the U.S. Civil War could not diminish the concentration of wealth in the hands of elites in the medium run (Ager et al. 2021). More broadly the paper is in line with Ogilvie (2021) and Acemoglu et al. (2005) who point to extractive institutions as general causes of economic inequality in history.

Second, the paper also provides a historically plausible theoretical framework of why warfare — arguably a defining characteristic of preindustrial life (Tilly 1992) — might increase inequality. The argument builds on Alfani and Di Tullio (2019; see also Alfani 2015, 2021), who argue that the early modern Military Revolution and the way European polities taxed their subjects to pay for it increased economic inequality. My conceptual contribution is to extend that line of thought, by synthesising it with the idea that wars can be destructive and inequality-reducing. My framework suggests that what mattered for the impact of a war on inequality was whether the inequality-promoting extractive effect or the inequality-reducing destructive effect dominated. I also provide suggestive empirical evidence for this hypothesis.

Finally, the paper also extends the literature on the nexus between warfare, state formation and fiscal capacity (Tilly 1992, Gennaioli and Voth 2014, Hoffman 2015, Dincecco 2015, Becker et al. 2018). I suggest that warfare not only increased fiscal capacity and stimulated the formation of economically beneficial, property rights-protecting states. When political elites reacted to the risk of warfare with resource extraction, they also made preindustrial society more unequal.

The next section outlines the conceptual framework and provides historical background information. Section 3 describes the data. Section 4 presents the empirical strategy, the main results of the paper, investigates the robustness of these results and provides direct evidence on defence infrastructure and extraction, to shed light on the hypothesised mechanisms. Section 5 provides additional case study evidence, and Section 6 concludes.

2. Conflicts and Economic Inequality: Historical Evidence and Theoretical Framework

This section presents a framework suggesting that military conflicts often promoted economic inequality in preindustrial times. I first describe what happened historically when localities were facing the risk of being involved in a conflict, and why most ordinary wars might have had a different effect on inequality than those major wars that the "great equaliser"-accounts have mostly focused on (see van Zanden 1995, Piketty 2014, Scheve and Stasavage 2016, Scheidel 2017, Alfani, Gierok and Schaff 2022). To fix ideas conceptually I then provide a simple formal representation of the two countervailing effects of warfare on inequality — destruction and extraction. (More historical background information is provided in the Appendix.)

2.1. Historical Evidence: Extraction, Destruction and Inequality

In preindustrial Germany, three types of conflicts existed. First, there were feuds, a small form of conflict, usually fought among two opposing individuals. Second, there were bottom-up revolts by peasants or townsmen. Third, there were conflicts between important political actors — analogous to "interstate wars" in modern times — such as imperial estates and authorities of comparable or higher rank like the King of France or Italian states (see Kroener 2013).[3] I focus on the third category of conflicts because these were more likely to be exogenous events from the perspective of an individual locality and hence less likely to be correlated with local inequality. The focus is thus on conflicts that were entirely different from those civil wars analysed in studies interested in the contribution of inequality to the outbreak of social conflict today (see Blattman and Miguel 2010, Baten and Mumme 2013).

It is well known that exposure to warfare increased political elites' need to extract financial resources in preindustrial times. This was true at the level of centralised polities or "states", but also at the local level (see Kamen 1984, Parker 1996, Karaman and Pamuk 2013). For individual towns this resource need derived from their fundamental task to maintain peace and provide protection for their inhabitants.[4] In Germany, the preindustrial period was still characterised by the absence of monopolies of force in the hands of consolidated states, and so towns had a central role in providing protection locally (Volckart 2002, Isenmann 2014).[5] The capacity to protect inhabitants gave preindustrial towns a "safe harbour" character (Dincecco and Onorato 2016).

[2] Note that Milanovic (2016: 56) and Scheidel (2017: 213) explicitly allow for the possibility that wars did not decrease inequality under certain circumstances.

[3] Imperial estates were those political authorities in Germany that represented their polities (territories and imperial cities) at the *Reichstag* (imperial diet).

[4] I use the terms "community", "locality" and "town" interchangeably.

[5] German towns — territorial and imperial cities — had a comparatively high degree of administrative and fiscal autonomy from their *de jure* overlords. Since most territories could not establish themselves administratively until the end of the early modern period, communities maintained considerable fiscal and military autonomy (Schilling and Ehrenpreis 2015).

Incidents of warfare in the surrounding area increased the *risk* for communities of being attacked — for instance, in a subsequent battle of the same war or potentially a future war (see the Appendix for historical examples) — even if war did not actually come to town. This risk increased the demand for protection. It made the extended use of costly defence infrastructure necessary, in terms of fortification, equipment and buildings, and personnel (for example, stronger walls, more weapons, extended arsenals and more military forces). For instance, the political elites of Nuremberg decided to reinforce the town's fortification just after the War of the Swabian League (1376-77) and reportedly in expectation of an upcoming war between princes and cities, which eventually came about with the War of the Cities (1387-94) (Isenmann 2014: 101).[6] The more military technology advanced during the "Military Revolution", the more costly it became for communities to adapt to it (Parker 1996, Kroener 2013).

In order to obtain the necessary funds local political authorities had to extract economic resources from their inhabitants and from the surrounding area. There were two major channels to obtain resources: taxation and credit (see again the Appendix for historical examples). Both were conducive to increasing economic inequality. First, the most important channel to extract resources for defence was most likely taxation (Fouquet 1989). Three types of taxes were levied: wealth taxes, consumption taxes and extraordinary war taxes (for example, *Nürnberger Hussitensteuer*). Such defence-related taxation was invariably inequality promoting, because premodern tax systems functioned radically differently compared to their modern counterparts. In contrast to modern states (see Scheve and Stasavage 2016), tax systems in preindustrial times were not *progressive*, but outright *regressive*: "the poor were taxed proportionally more than the rich" and so "post-tax inequality was higher than pre-tax inequality" (Alfani and Di Tullio 2019: 147).

Wealth taxes were at best inequality-neutral, because they were levied proportionally, asking the same share from rich and poor households.[7] Still, parts of the riches of the Church and the nobility were exempted, making these taxes weigh most likely more on lower segments. However, the regressive character of preindustrial tax systems principally came from consumption taxes, and tax systems at the time relied much more on these indirect taxes than on direct taxation of wealth or income. Consumption taxes — "the most unsocial tax imaginable" (Boelcke 1971: 171; my translation) — were regressive because they weighed disproportionally on the poor strata. They spent a larger share of their budgets on basic goods compared to rich people, and therefore paid proportionally higher taxes (Winnige 1996, Alfani and Di Tullio 2019: 149).[8] Extraordinary war taxes, the third type of taxes, were potentially the most regressive ones. They were often levied as poll tax, setting a fixed amount for everybody, regardless of personal economic strength. By construction, these taxes took an increasing share of income or wealth the poorer the taxpayer was. Yet poll taxes were appealing to authorities, especially in wartime when funds were needed urgently, because they required low administrative effort (Isenmann 2014).[9] Sometimes, high taxes even pressured poor people into selling their real estate to large landowners (Hoffman 1996).

Credit was the second principle mode to finance war-related expenses. Warfare increased the demand for credit substantially (Stasavage 2011). However, credit only postponed regressive taxation, because eventually the borrowed money had to be paid back with interest.[10] Moreover, creditors were usually local elites, for instance rich merchants, guild masters or officials. Since interest rates were relatively high in preindustrial times — between three and ten percent (see Chilosi et al. 2018: 647) — rich lenders could accumulate even more wealth when warfare increased political authorities' demand for credit. Poor people could not make such profitable investments in public debt (Schulze 1995: 271, Alfani and Di Tullio 2019).

Beyond taxation and credit, other extraction processes might have increased inequality during warfare, such as war profiteering. Wars increased the demand for defence-related goods and services, such as fortification, weapons or mercenaries. These *expenses* favoured producers and traders of military equipment and services, which were usually members of the socio-economic elite (Scheve and Stasavage 2016, Schulze 1995, Alfani and Di Tullio 2019).

Are there any reasons to believe that the effect of warfare on wealth inequality was not just shortlived, but actually persisted, potentially even for several decades? This question is particularly important because the empirical analysis below covers relatively long periods of 25 years. There are at least two mechanisms, a mechanical and an institutional one, that suggest that the effect was most likely "sluggish", that is, semi-permanent. First, wealth is a *stock* — not a flow like income — that turns around slowly. The distribution of wealth therefore adjusts in longer cycles than income. Second, there are also reasons to believe that the financial need caused by warfare stayed high after the actual conflict for at least some time, necessitating continued resource extraction. One would expect that increases in regressive taxation were to some extent sticky, for example because the financial need caused by warfare was initially covered by credit, which was then paid back over an extended period. Additionally, it is likely that new

[6] Another example of a community experiencing warfare in its surrounding repeatedly in a relatively short period and improving its defence infrastructure is Speyer: it was exposed to battle action during the Palatine Succession War in 1688, and again to two battles of the War of the Spanish Succession in 1703 and 1713. In between, the town improved its fortification (Keyser 1964: 386, Dincecco and Onorato 2016). More systematic empirical evidence suggesting that it was rational for political elites to consider past conflicts to assess risk, which then influenced the decision to expand local defence infrastructure, comes from Table 2. It shows that almost three quarters of the community-period observations in my dataset experienced warfare not just once, but outright twice in the same period between 1400 and 1600.

[7] For example, in 1530 Rostock the tax rate was uniformly 10 percent, and in 1676 Nördlingen 3.5 percent (Staude 1912: 154, Friedrichs 1979: 158).

[8] Consumption taxes substantially increased prices of basic goods, such as beer or grain, by about 10 to 30 percent (Isenmann 2014: 525). Given that urban middle class households spent approximately 70 percent of their budget on foodstuffs, taxation could be a heavy burden on them and lower classes (Dirlmeier 1978: 420).

[9] A poll tax was for example levied in Straubing during the War of the Spanish Succession (Keim 1957).

[10] Debt was such an important means of finance that towns earmarked 60 percent and more of their revenues for servicing debt in the late early modern period (Chilosi et al. 2018: 639-640).

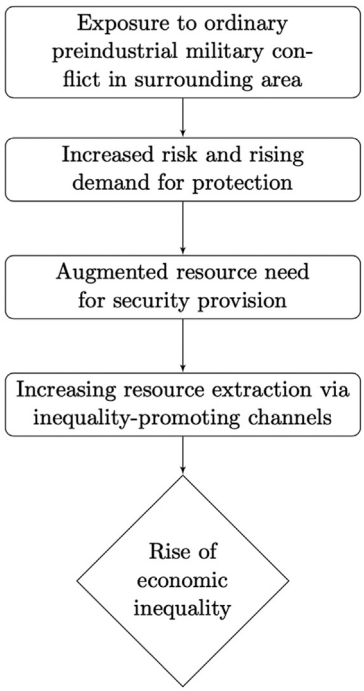

Fig. 1. Extractive Effect of Warfare on Economic Inequality

defence infrastructure embedded maintenance costs, deriving for example from the need to maintain fortification or to store military equipment (Hohrath 1996: 308, Isenmann 2014: 454-456).[11]

One might be wondering whether the inequality-promoting effect of resource extraction by public authorities might have been offset by social spending. This is unlikely, because social spending was marginal for local governments. A typical town government in preindustrial Germany spent around 29 to 41 percent for security and external affairs but only 0 to 2 percent for welfare, such as health and poor relief (Isenmann 2014: 519-521; see also Alfani and Di Tullio 2019: 165-169).

Fig. 1 puts the historical argument schematically together, and summarises why one would expect that conflict-induced resource extraction led *ceteris paribus* to a redistribution of economic resources from poorer people to socio-economic elites.

Of course, inequality-promoting extraction was not the only consequence of warfare. There was also destruction: demolition of physical capital leading to economic decline and rent reduction, expropriation and confiscation, or population decline (often intensified by plague). These forces pushed towards lower inequality. Many studies have focused on that destructiveness and the major wars that were characterised by it (van Zanden 1995, Piketty 2014, Milanovic 2016, Scheidel 2017, Alfani et al. 2022). I argue that for most preindustrial wars, the extractive effect was presumably larger than the destructive effect and so wars constantly increased economic inequality. Only in truly major wars destruction might outweigh extraction and reduce inequality, but this was an exception and not the rule. For preindustrial Europe, the only war for which actual evidence of a significant inequality reduction exists is the Thirty Years' War in Germany. That was indeed a textbook case of destructiveness, which due to the long duration of the war spilled over all of Germany (see Alfani, Gierok and Schaff 2022).

The exceptional destructiveness of the Thirty Years' War is mirrored in its extraordinarily high number of people killed during that period compared with other preindustrial wars (see Table 1). A few other conflicts saw high casualties too, such as the War of Spanish Succession, but only in the case of the Thirty Years' War deaths were almost completely concentrated on one country: Germany (Wilson 2009: 787). Other conflicts apparently caused relatively small destruction, most likely because they were geographically more limited, without the spill-over effects characteristic of the Thirty Years' War.

Before estimating the relationship between warfare and inequality more systematically, I summarise the two countervailing effects of conflicts in a simple theoretical framework.

[11] Warfare often triggered the acquisition of weapons, which had to be stored and maintained. For example, Überlingen reacted to the threats of the Swabian War (1499) with the acquisition of new canons and built an arsenal for storing these. This arsenal then required to hire a highly skilled weapon master (Koberg 1975). More historical detail is provided in the Appendix.

Table 1
Wars in Germany and Their Casualties

Date	War	Casualties	Casualties p.a.
1377-1389	Swabian League of Cities War	3,250	250
1385-1388	Sempach War	175	44
1419-1434	Hussite Wars	23,400	1,463
1462	Bavarian War	2,000	2,000
1522-1523	Knight's Revolt	2,000	1,000
1524-1525	Peasant's War	145,000	72,500
1546-1547	Schmalkaldic War	8,000	4,000
1569-1583	Münster Rebellion	6,000	400
1618-1648	Thirty Years' War	8,000,000	258,065
1688-1697	Palatine Succession War	580,000	58,000
1701-1714	War of Spanish Succession	1,205,000	86,071
1733-1735	War of Polish Succession	85,000	28,333
1740-1748	War of Austrian Succession	330,270	36,697
1756-1763	Seven Years' War	988,000	123,500
1778-1779	War of Bavarian Succession	300	150
1792-1797	War of First Coalition	330,800	55,133
1798-1801	War of Second Coalition	444,270	111,068

Notes: Data from Brecke (1999). The figures do not distinguish the precise causes of deaths because of warfare, such as battle action, plague diffused by armies or famine caused by damaged crops, stolen food reserves and stolen herds. Casualties p.a. indicate the average number of deaths per year.

2.2. Theoretical Framework: Personal Wealth Accumulation During Warfare

To rationalise the previous historical account and illustrate the countervailing effects of warfare on economic inequality, I adapt the personal wealth framework of Piketty and Zucman (2014) to my case. The main aim of this static framework is to state likely directions of the destructive and extractive effect on inequality, to highlight the logic of the following empirical analysis.

We are interested in the development of average household wealth W between two periods t and $t+1$: $W_t \rightarrow W_{t+1}$. Without warfare the accumulation process of W can simply be described as:

$$W_{t+1} = \left[S + (1+r) \times W_t\right] \tag{1}$$

S are total savings net of depreciation (where $S = s \times Y$, s being the savings rate and Y income) and r is the interest rate. Two amendments are necessary to describe the development of wealth inequality during warfare. First, we differentiate between the wealth of high-wealth holders W^H and low-wealth holders W^L, and their corresponding savings are S^H and S^L. These two groups are representative of rich and poor households in society.

Second, we need to consider that warfare introduced two additional parameters in the process: extraction and destruction. The risk of being involved in a war increased the demand for protection of communities, and resource extraction. er is the extraction rate by which W was reduced. However, a characteristic feature of resource extraction in preindustrial communities, in particular of taxation, was its regressive character (see Alfani and Di Tullio 2019). While regressive taxation hit the poor more heavily than the rich, rich individuals even benefited disproportionately from the war-time expansion of credit by public authorities. They might have also benefited from poor strata selling their real estate to large landowners, or from the way extracted resources were spent by authorities. All this allowed the rich to accumulate more wealth, reducing their effective extraction rate. We differentiate therefore between the extraction rate of high- and low wealth holders and assume $er^L > er^H$.

But military conflicts also caused destruction, which could reduce inequality through the demolition of capital, expropriation, population or economic decline (see Scheidel 2017). w represents the wealth share reduction because of destruction. Poor strata experienced less wealth reduction due to destruction compared to elites. A poorer person simply has less capital goods that could be demolished or plundered, and propertyless people had, by definition, nothing. Instead, rich people have a lot that can be destroyed or plundered, and they were often the main prey for armies sacking towns. Additionally, if a war became so immensely expensive that governments defaulted on their debt the rich lost substantial parts of the money they typically lent to governments, increasing the destructive effect for them (see Schmidt 2018: 632). The poor did not suffer such losses. Somewhat paradoxically poor people potentially even benefited from increased mortality during warfare. Increased mortality brought about lower prices for foodstuffs and real estate, and higher wages due to the scarcity of labour, facilitating poor peoples' property accumulation (van Zanden 1995, Pfister 2020). In other words, while extraction was regressive, destruction was progressive. We differentiate therefore again between high- and low-wealth holders' destruction rate and assume $w^H > w^L$.

Through simple re-arrangement we can derive a function of wealth accumulation for high- and low-wealth owners during warfare:

$$W^H_{t+1} = \left(1 - er^H\right) \times \left(1 - w^H\right) \times \left[S^H + (1+r) \times W_t\right] \tag{2}$$

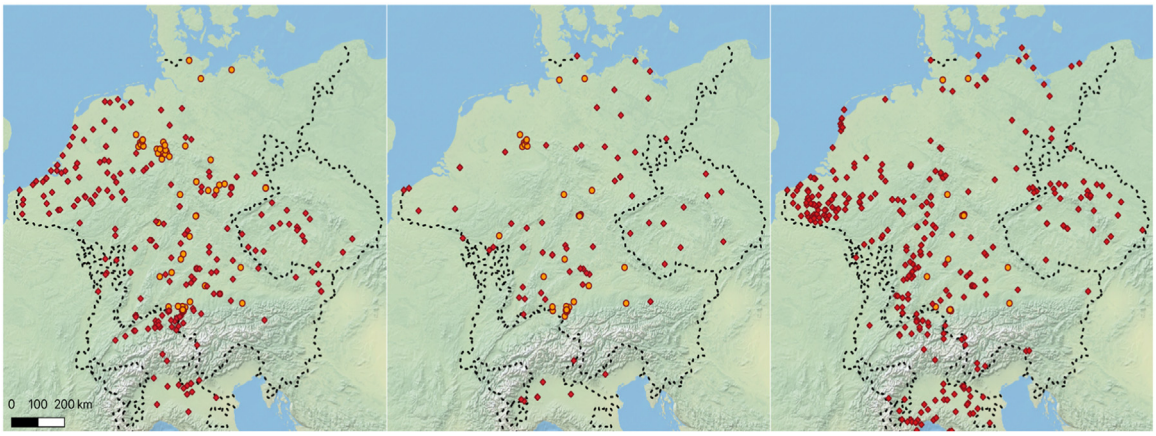

Fig. 2. Spatial Distribution of Localities and Conflicts (3 Periods)
Notes: Localities included in the analysis depicted in yellow and conflicts depicted in red. The three maps from left to right show conflicts that happened before (1375-1617), during (1618-1648) and after the the Thirty Years' War (1649-1800). Borders of the inner and wider Holy Roman Empire around 1545 from Chilosi et al. (2018).

and

$$W_{t+1}^{L} = \left(1 - er^{L}\right) \times \left(1 - w^{L}\right) \times \left[S^{L} + (1 + r) \times W_{t}\right] \tag{3}$$

If we assume that all variables in the square brackets are fixed during warfare, then Equations 2 and 3 suggest that after a military conflict in $t + 1$ the change in wealth possessed by high- and low-wealth holders depended on the magnitudes of the extraction rates (er^H and er^L) and the rates of destruction (w^H and w^L). These parameters determined the wealth differentials between rich and poor caused by warfare. If one takes a basic inequality measure, such as the ratio between rich and poor households' wealth share (see Piketty 2020) — W^H/W^L — then it is straightforward to see how the extraction and destruction rate determined wealth inequality. Equations 2 and 3 also suggest that depending on whether the conflict was small or large, the importance of extraction and destruction shifts, and with them inequality. Low-destruction conflicts had a relatively higher *er* but a relatively lower *w*, which could exacerbate inequality. But in a major war destruction could become so large that it offsets the extractive effect and reduce inequality. (A simple simulation of these predictions is reported in the Appendix.) This framework is of course highly stylised, and we are not going to test it as such. However, the empirical results below are consistent with the framework.

3. Data

Fig. 2 provides an overview of the localities (yellow dots) for which inequality data are available, and the conflicts (red diamonds) that happened during the period of analysis. They are the main variables of the following analysis.[12]

I use data on community-level economic inequality from Schaff (2022a), which is an extended dataset of Alfani et al. (2022), using the full available distributions. The Appendix provides a detailed discussion of how the dataset has been constructed and the sources. The dataset records Gini coefficients and wealth percentiles in steps of 25 years. It covers rural and urban communities within the borders of the inner part of the Holy Roman Empire. It contains well known larger, urban communities such as Munich or Frankfurt a.M., but also lesser known, smaller, rural communities like Deuchelried and Blomberg. The data are based on registers of local property tax registers that were levied on households of citizens and non-citizens. The tax base was the totality of mobile and immobile assets, such as real estate, crops, animals, cash money, loans, household goods and other property, sometimes even labour income. The exact composition of the tax base differed slightly from community to community, but real estate was the main asset class everywhere, and by far the most important component of national wealth in preindustrial economies. Subject to the limitations discussed below, such tax registers give a fairly accurate picture of household-level wealth inequality. These data refer most closely to the distribution of wealth (or capitalised income). Yet trends in wealth inequality are the best, and usually the only proxy for trends in income inequality in preindustrial, predominantly agricultural economies, like Germany (Alfani 2015). Note that interventions which might affect the income distribution immediately, such as taxation, likely affect the wealth distribution with some time lag.

Potentially, the local wealth distributions obtained from tax registers are biased towards the middle. Parts of the patrimony of the very top (for example nobles, clergy, officials and other privileged groups) of the distributions are sometimes but not always missing because of tax exemptions.[13]

[12] All data and replication files can be accessed on openicpsr (https://doi.org/10.3886/E183421V1). See Schaff 2022b.

[13] One can only tentatively approximate the size of the excluded groups. The nobility and clergy made up between approximately 1.5 and 0.5 percent of the total population in early modern Germany (Saalfeld 1980: 480). Most excluded households were probably part of these groups, but not all members of clergy and nobility were excluded from taxation.

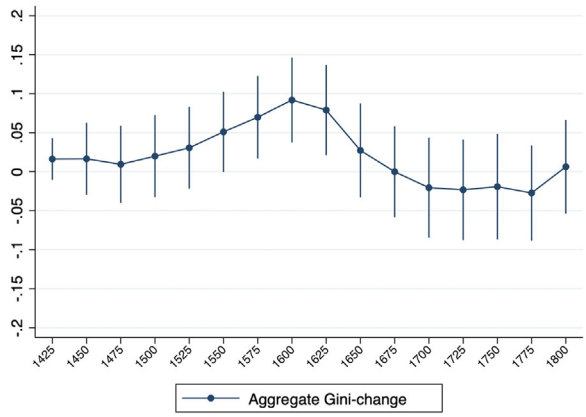

Fig. 3. Economic Inequality in German Communities (Aggregate Gini-Change), c. 1400-1800
Notes: The trend line of Gini change has been obtained regressing an unbalanced panel of Gini coefficients of the 75 communities included in the analysis on a full set of locality fixed effects and year dummies (reference year 1400). The coefficients on the year dummies represent aggregate Gini change (see Alfani et al. (2022). Confidence intervals indicate significance at the 95-percent level.

Note that German tax registers have been found to cover poor households very well (Alfani et al. 2022).[14] The missing parts of the distribution are likely to bias my estimates of the impact of ordinary conflicts downwards.[15] The results are therefore to be considered lower-bound estimates.

The dataset has a panel structure and includes 75 places for which at least two data points are available. Fig. 3 reports the aggregate inequality trends for these places, which are very similar to other European areas, exception made for the period of the Thirty Years' War (Alfani et al. 2022).

The selection of places into the dataset depended on the availability and survival of archival tax documents since the time of their creation until today. This suggests that attrition is most likely random in this unbalanced panel and uncorrelated with the error term of the model. One might be concerned that survival of tax registers is negatively correlated with exposure to warfare. While this possibility cannot be entirely excluded, it is historically not very likely, because communities were usually quick to levy taxes again, even after the most destructive wars. For example, Nördlingen was exposed to outright two major battles during the Thirty Years War, the "First-" and "Second Battle of Nördlingen" (1634, 1645). Yet there exist tax registers for the years 1636 and 1646 in the city archive. From 1400 to 1800 the dataset contains 504 observations. This is substantially more than recently published studies of the determinants of inequality in the preindustrial period have.[16] Note that the seemingly small number of observations refers to wealth distributions in a given locality and year. Obtaining one wealth distribution requires lots of information: wealth of every household in a locality. In total, the dataset is based on wealth information of about 417,000 households.

For constructing the treatment variable I have assembled data on military conflicts in which at least one imperial estate or authorities of similar rank participated. There are obvious endogeneity concerns, which will be addressed in the next section. I have upgraded the conflict database of Dincecco and Onorato (2016) using several secondary sources to cover more specifically Germany. The construction of the dataset is explained in detail in the Appendix. It contains 709 conflicts (battles and sieges), making it the most comprehensive one for preindustrial Germany. For all conflicts the year and closest settlement were recorded, so I could geo-reference them with high precision. It is likely that the dataset does not record all conflicts — a general problem when studying preindustrial warfare (Dincecco and Onorato 2016) — but it is reasonable to assume that it includes the most important conflicts that historical research has recorded. For example, for the Thirty Years' War the dataset includes 83 individual conflicts.

As main variable of interest I construct a dummy that equals one if there was at least one conflict between two inequality measurement points within a distance of 200 kilometres from a locality. This is in line with previous research on the topic (see Besley and Reynal-Querol 2014, Dincecco and Onorato 2016). Using a dummy indicator has the advantage of reducing the potential for error in the measurement of conflict frequency (Dincecco and Onorato 2016). In order to address concerns about the reliability of this simple measure, I perform a robustness check in the Appendix with alternative categorical variables. The 200 kilometres threshold has been chosen by means of iteration and is in line with historical research (see below).

Fig. 4 provides an overview of the amount of conflicts and to what extent communities were affected by them. The left frame shows the number of conflicts per period. The two major spikes are the Thirty Years' War in the seventeenth century and the French Revolutionary and Napoleonic Wars at the end of the eighteenth century (for a similar pattern see Dincecco and Onorato 2016). The latter one comprises a seemingly exceptional number of conflicts but many of them actually happened at the margins of the

[14] See the Appendix for more discussion of the coverage of poor strata in the tax registers, and for alternative estimates where propertyless households have been dropped from the wealth distributions, which does not substantially change the results of the analysis.

[15] It is likely that, for instance, highly paid town magistrates benefited from inegalitarian resource extraction during warfare. Since these groups were sometimes missing from the wealth tax data, I might underestimate the true impact of conflicts on economic inequality.

[16] For example, Milanovic (2018) has 41 observations in his cross-country study of premodern societies.

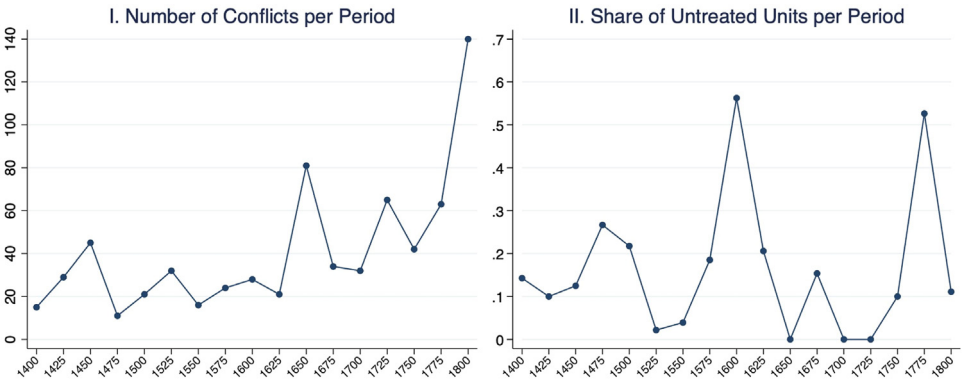

Fig. 4. Conflicts and Treatment Status (200km), c. 1400-1800

Notes: In the left frame the number of conflicts is counted over the period previous of year t = 1400, 1425, ..., 1800. The number of conflicts refers to all conflicts counted in the respective period in the wider Holy Roman Empire. The right frame shows the share of units that did not experience at least one conflict within a radius of 200 kilometres in a given period.

Table 2
Summary Statistics of Main Variables

Variables	1400-1600			1625-1650			1675-1800		
	N	Mean	Std.dev	N	Mean	Std.dev	N	Mean	Std.dev
Gini	308	0.567	0.155	70	0.568	0.151	126	0.549	0.145
First Decile Share	308	0.816	0.899	70	0.649	0.896	126	0.839	0.920
Second Decile Share	308	1.692	1.378	70	1.478	1.415	126	1.845	1.469
Third Decile Share	308	2.533	1.730	70	2.355	1.819	126	2.980	1.875
Fourth Decile Share	308	3.523	2.183	70	3.419	2.086	126	4.047	2.166
Fifth Decile Share	308	4.747	2.602	70	4.970	2.435	126	5.203	2.377
Sixth Decile Share	308	6.291	3.018	70	6.568	2.911	126	6.506	2.509
Seventh Decile Share	308	8.327	3.285	70	8.616	3.255	126	8.320	2.763
Eighth Decile Share	308	11.68	3.762	70	11.81	3.323	126	10.95	2.725
Ninth Decile Share	308	17.35	3.948	70	17.73	2.744	126	16.47	2.474
Tenth Decile Share	308	43.09	16.68	70	42.40	16.18	126	42.84	14.66
Bottom 50% Share	308	13.31	7.958	70	12.87	7.818	126	14.91	7.753
Conflict Exposure	308	0.799	0.402	70	0.900	0.302	126	0.857	0.351
Repeated Conf. Exp.	308	0.727	0.446	70	0.857	0.352	126	0.802	0.400

Notes: The Tenth Decile Share is equivalent to the Top 10% wealth share of the population. The Bottom 50% share is the sum of deciles one to five. Conflict exposure means that a community was exposed to at least one conflict, and repeated conflict exposure means that a community was exposed to at least two conflicts.

Empire, especially in the Low Countries, Italy and Bohemia, as can be seen in Fig. 2. The right frame of Fig. 4 shows the share of communities that were not exposed to warfare in a given period, that is, that were not treated (for information on treated units, see Table 2 below). This share fluctuated considerably. In some periods, especially those of intense warfare, such as during the Thirty Years' War or during the four major succession wars in the seventeenth and eighteenth centuries (see Table 1), all communities in the panel experienced at least one conflict. This is little surprising given the relatively wide threshold radius of 200 kilometres.

In substance, this analysis is limited to information about where and when a conflict (battle or siege) took place. The treatment indicator is thus imperfect, a proxy for the totality of military events during a war. It is not possible to measure more precisely and systematically the intensity of single conflicts, for example with the number of casualties. One might also ask whether the effect of a conflict on inequality differed when a polity was one of the belligerents. Unfortunately, systematic information about victors and losers for every battle is simply beyond our current historical knowledge, not to speak of alliances. In the Appendix I provide case-study evidence suggesting that being among the losers might have implied a differential impact of warfare on economic inequality. Similarly, in conflicts that were part of a larger war, troop movements and raids might have had effects similar to an actual battle. Because of a lack of systematic data, troop movements and raids, too, cannot be considered.[17]

[17] Note that if there was indirect exposure to militant events through troop movements and raids which my conflict data do not adequately capture, a locality would appear as non-exposed while in reality it was exposed. If the hypothesis of this paper is correct that militant events overall promoted inequality, then the omission of militant events such as troop movements and raids would most likely lead to an underestimation of the true effect of wars, but not to an overestimation.

Ultimately, given the hypothesis that wars increased resource extraction and inequality, it would be insightful to have information about actual resource extraction in communities, such as data on taxes and public loans. Unfortunately, such information, too, is to my best knowledge not systematically available. This motivates the reduced-form approach, which investigates the direct relationship between warfare and inequality. However, to shed some light on the possible mediators connecting warfare and inequality, I gather evidence on communities' defence infrastructure from the *Deutsches Städtebuch* for a subset of the database. First, I record the presence of military garrisons in a community. Second, I assemble information on construction of buildings or sites with a military purpose, such as barracks, arsenals, gunmen houses (*Schützenhaus*), stables for war horses (*Marstall*), firing ranges (*Schiesshaus*) or foundries for weapons.[18]

Table 2 reports summary statistics for the inequality and conflict exposure indicators employed in the analysis. There are slightly more observations for the first two centuries of the analysis than for the latter two. The figures indicate, for example, that in the period from 1400 until 1600 the top 10 percent (tenth decile) of the population owned on average 43.09 percent of total wealth in the communities in my dataset. This is considerably less than the wealth share of the same decile in contemporary Germany. For instance, Albers et al. (2020: 37) report for Germany a top 10 percent wealth share of about 51 percent in 1993, but one should be very cautious when comparing these two estimates.[19] Table 2 also indicates that communities were exposed frequently and repeatedly to warfare. For instance, in the period from 1400 until 1600, 79.9 percent of the observations refer to communities that were exposed at least once and 72.7 percent were exposed repeatedly. Yet the high standard deviation indicates that there was a lot of variation in that exposure.

4. Main Empirical Analysis

4.1. Econometric Methodology

The preferred econometric specification used to measure the impact of conflict exposure on economic inequality are variants of the following:

$$I_{i,t} = \alpha_i + \pi_t + \beta CE_{i,t-1} + \gamma\prime X_{i,t} + \epsilon_{i,t} \tag{4}$$

$I_{i,t}$ is wealth inequality of locality i in year t (t = 1400, 1425,... until 1600 and from 1675 until 1800).[20] The Gini coefficient is the main left-hand-side inequality measure, but I also consider as alternatives wealth percentiles. Unless otherwise indicated the results will refer to the period from 1400 to 1600 and from 1675 to 1800. The observations from 1625 and 1650, approximately the period of the Thirty Years' War, are considered separately because it is the only period for which a negative warfare-inequality relationship has been documented (Alfani et al. 2022). $CE_{i,t-1}$ is the measure of conflict exposure that takes the value one if there was a military conflict within 200 kilometres of locality i over the previous period.[21] Accordingly, I obtain reduced form or "intention-to-treat" estimates of how exposure to warfare in the surrounding area of a community affected its economic inequality. The choice of the 200 kilometres threshold is discussed below. α_i are a full set of locality fixed effects and π_t are time fixed effects (years). Hence, the estimated correlations are identified from time variation within communities.

$X_{i,t}$ is a vector of locality-level controls (explained below). These are included in the robustness checks only because the time-variant characteristics could be "bad controls" or collider variables (Angrist and Pischke 2009, Schneider 2020). In most specifications I also include Imperial circle-time interactions. They account for unobserved temporal shocks affecting communities in the same region (Imperial circle) in the same way. Unobserved factors are captured with the random error term $\epsilon_{i,t}$. The standard errors are robust, clustered at the locality level in order to account for the possibility of serial correlation in the error term.

Before proceeding, the data structure and the econometric specification require a clarification. The setup in the main analysis — multiple shocks with periods of non-treatment in between — does not correspond to a conventional difference-in-differences (DD) research design. In my analysis conflict matters in the period after a war, but not permanently, that is, there is no post-period. Because the treatment warfare occurs repeatedly, even before the period of analysis, it is impossible to define a pre- and post-treatment period for this type of multiple shock. Recent critiques of DD research designs employing two-way fixed effects (TWFE) specifications to conventional setups with single shocks (occurring at different points in time) do, therefore, not entirely apply. However, this literature has pointed to a general problem with TWFE specifications that has implications also for my setup: when treatment occurs

[18] The Appendix provides further details about the coding of the variables.

[19] There are several potential economic and methodological reasons for the different magnitudes of the estimates. First, modern industrial Germany is, of course, many times more prosperous in absolute terms than preindustrial Germany, which could imply a more unequal distribution of wealth. Second, the unit of analysis is different. The figures reported in Table 2 refer to averages across single communities, while the estimates of Albers et al. (2020), or of similar studies such as Piketty (2014), typically refer to Germany as a whole country. Quite obviously, socioeconomic differentiation can be much higher in a populous country than in a single community with a few thousand inhabitants. Third, the nature of the sources is very different. For the industrial period information from general property taxes that cover (almost) the whole population — as used in this study — are not available. This has led Albers et al. (2020) to use survey data and to correct the top wealth shares with information from rich lists, obtained from popular media outlets. It should come as no surprise that the resulting wealth share estimates are potentially different from the ones reported in this study.

[20] Inequality measures have been clustered around their closest reference year.

[21] In order to determine as precisely as possible whether a locality was treated or not in a period, I have coded the treatment dummy according to the actual year of inequality measurement (for example, 1522) and not the clustered year (for example, 1525).

at different times and when individual treatment effects vary across time or unit, the estimated average coefficient can be severely biased, potentially leading to a "flip" in the sign of the coefficient. This can happen because previously treated units act as controls in later periods (Goodman-Bacon 2021, Wooldridge 2021).[22] To provide at least a partial remedy to this potential problem, I follow the approach of Wooldridge (2021: 49). He suggests to model the heterogeneity across time and units by making the TWFE specification more flexible with interaction terms. Moreover, in Section 5 I validate the main results with two case studies of single wars using a flexible DD research design. In these cases, treatment occurs about at the same time, which makes a much cleaner comparison of treated and control units possible and circumvents the complications of treatment occurring at different points in time.

4.2. Endogeneity Concerns

I address concerns about endogeneity through several strategies. The modelling approach accounts for unobserved factors that might have had an impact on the dependent and the independent variable of interest. Locality fixed effects account for characteristics that are time-invariant and locality-specific. Several such characteristics might be relevant for explaining differences in inequality *levels* between localities, such as inheritance institutions, taxation laws, city status, land-tenure systems, or the overarching social order of feudal societies (Alfani 2015, Banerjee and Iyer 2005, Piketty 2020). Time fixed effects account for shocks that might have had an impact on inequality in all localities, such as constitutional changes in the Empire. Moreover, macroeconomic trends or a changing frequency of conflicts are captured by the time fixed effects. Imperial circle-time interactions capture temporal economic shocks at the regional level.

To mitigate further the possibility for omitted variable bias, I account for several observable characteristics that have been considered alternative explanations for inequality change. I include two variables that capture economic and demographic development, the log-population size of a locality and the occurrence of epidemics. Population size is a proxy for local economic development, a potential driver of inequality, for example, leading to the rise of a middle class (van Zanden 1995). Moreover, demographic expansion potentially influenced inequality growth (Alfani and Ryckbosch 2016, Pfister 2020).[23] Additionally, conflict exposure might have increased the population in premodern towns because these were "safe harbours" to which people potentially migrated in wartime (Dincecco and Onorato 2016). This might have increased inequality because migrants were often relatively poor. Hence controlling for population size closes a potential alternative causal channel. Finally, military conflicts often spread epidemics, which had a negative impact on the population size and possibly on inequality (Alfani et al. 2022).

I also include several institutional variables. I include a variable that indicates whether a community was Protestant, because of potential differences in how redistributive Protestant and Catholic governments were (Basten and Betz 2013). I also include a variable indicating the log-distance of a town to its nearest university, first, because higher human capital of few individuals might have increased economic inequality (van Zanden 1995). Second, universities provided political elites with the trained officials needed to extract more resources from their subjects in an inequality-promoting way (see Ogilvie 1992: 426). Beyond these standard controls I also test the robustness of my results to the inclusion of several time-invariant controls interacted with time-dummies.

Consistent with the literature, my first econometric strategy exploits the plausibly exogenous occurrence of battle action from the perspective of an individual town (Acemoglu et al. 2011, Karaman and Pamuk 2013, Dincecco and Onorato 2016). This assumption is supported by the focus on military conflicts in which important political actors participated, such as imperial estates (for example, the Duke of Bavaria), the King of France or Italian states. It is unlikely that these authorities started a war because of inequality in some community. It is equally unlikely that communities became involved in war only because they had extracted the necessary economic resources from their population beforehand. Local authorities could justify taxation if the community had to bear a common burden, such as coping with a threat. Authorities that extracted resources from the community without legitimate reason ran the risk of provoking unrest (Isenmann 2014).

To substantiate the claim that wars were by and large exogenous events for the individual locality, I have reviewed the historiography of all 86 wars in the dataset and have recorded the principal causes (one or more) of the outbreak of every war, according to the literature. I have classified the individual causes into the general categories reported in Table 3. (A table including every single war in the dataset and the reasons for its outbreak, including a qualitative evaluation of whether a war was a plausibly exogenous event and the references to the historical literature, is reported in the Appendix.)

As can be seen from the table, many wars broke out because of struggles among rulers over material things (for example, territorial gain, strategic routes, payment of a levy, a trade dispute), especially territory. This is what one would expect given that land was the main source of prestige in preindustrial times. For instance, gaining territory was Louis XIV's motivation when he tried to round off the borders of France at the expense of the Low Countries and the Empire in several wars in the seventeenth century. Many wars also broke out because of the high politics in Germany and Europe (for example, power balance, sphere of influence, political alliance,

[22] This young literature has so far proposed solutions for conventional DD settings with single shocks. These alternative estimators reduce the risk of bias, essentially by comparing outcomes across time and units in a more restrictive way (see among others De Chaisemartin and D'Haultfœuille 2020, Goodman-Bacon 2021, Callaway and Sant'Anna 2021). However, to the best of my knowledge, there is currently no option for the "unconventional" case of having multiple shocks, such as recurring warfare. The lack of satisfactory solutions for all types of setups has also been noted by studies facing similar challenges (see Oliu-Barton et al. 2022: 11).

[23] In agricultural societies with an inelastic supply of land a rise in population implied four things: first, greater demand for agricultural produce leading to higher prices (which disproportionately increased food expenses for the poor); second, a rise in rents for real estate; third a decline in wages, and fourth, "proletarianisation", which concentrated land in the hands of large landowners. All these dynamics led to more poor people and the rich accumulating more income and capital (Alfani and Ryckbosch 2016, Pfister 2020).

Table 3
Principal War Causes

Principal War Causes	% of Wars
Territorial gain	41%
Succession	19%
Confessional dispute	15%
Power balance	14%
Sphere of influence	10%
Independence	9%
Strategic routes	8%
Political alliance	5%
Restitution political order	5%
Payment of levy	3%
Trade dispute	3%
Ecclesiastical control	3%
Privilege restoration	2%
Social discontent	2%
Imperial status	1%
Revenues natural resource extraction	1%

Notes: "Confessional dispute" means for example a dispute over whether a territory became Protestant or remained Catholic. "Ecclesiastical control" means for example a dispute over which church or candidate gets to hold a bishop office. "Succession" means for example a dispute because a ruler dies without male heir. "Imperial status" means for example a dispute over receiving elector dignity. "Independence" means for example a dispute over political independence of a region from a ruler's rule. "Payment of levy" means for example a dispute over the payment of a rent. "Political alliance" means for example a dispute because a ruler leaves a previous political alliance. "Power balance" means for example a dispute to avoid that one polity gets too powerful. "Privilege restoration" means for example a dispute to obtain back an old privilege. "Restitution political order" means for example a dispute to reinstall a discharged ruler. "Revenues natural resource extraction" means for example a dispute over revenues from a mining monopoly. "Social discontent" means for example a dispute because of impoverishment of social classes. "Sphere of influence" means for example a dispute over who is the dominate power in a contested region. "Strategic routes" means for example a dispute over the control of a river. "Territorial gain" means for example a dispute because a ruler attempts to round off his territory. "Trade dispute" means for example a dispute over the right to trade within a region. Categories in italics indicate likely endogeneity. Sources: See Appendix E.

independence). For example, the Cologne War broke out in 1583 to maintain a balance of geopolitical power within the Holy Roman Empire.[24] Religious issues (for example, confessional dispute, ecclesiastical control) sometimes led to warfare too, such as in the "wars of religion" after the beginning of the Protestant Reformation. Yet a number of wars broke out for idiosyncratic reasons. For example, about a fifth of all wars began as a dispute over the succession in ruling a territory, such as in Guelders, Spain, Poland or Austria. Such disputes usually started because a ruler died without heir.

Only in two cases did wars break out because of potentially endogenous social discontent.[25] This is unsurprising given that preindustrial societies were structurally unequal — think of nobility versus commoners — and so economic inequality will tend to be less perceived as a problem (Cohn 2006, Alfani and Frigeni 2016). In 84 out of 86 wars, economic inequality in the population did not play a role according to the historical literature, supporting the notion that these wars were plausibly exogenous events from the perspective of an individual town.[26]

Still, one might be wondering whether wars breaking out because of material interest might have been correlated indirectly with economic inequality. For example, a prince might want to take over a city because of the riches of its merchants. While this cannot be completely excluded, it is unlikely to drive the results for two reasons. First, my conflict variable does not indicate whether a community participated in a war or was actually attacked, which could be endogenous. It only indicates whether a locality was within a fairly large radius of the battle action, that is, indirectly exposed. This contains the possibility of material characteristics of communities determining their treatment status. Second, I control for several proxies of local economic development.

To further address endogeneity concerns, I report a series of robustness checks in the Appendix: the baseline results with spatial autocorrelation-adjusted standard errors and Morans' I test for spatial dependence. Additionally, I consider different treatment vari-

[24] Had Gebhard von Truchsees been successful in his attempt to transform Cologne into a worldly principality with equal rights for both confessions, that would have led to a majority for a different faction in the Kurfürstenkolleg and thus a new political equilibrium in the Holy Roman Empire. This threat induced Bavaria and the Habsburgs to send troops to Cologne, thus starting the war (Schilling 1994).

[25] These were the Swiss Peasant War (1653) and the German Peasants' War (1524-25). In both cases discontent about impoverishment of peasant classes played a role in the outbreak (Schilling 1994: 140-148, Suter 2010).

[26] In the Appendix I repeat the baseline analysis, dropping those potentially endogenous wars that broke out because of social discontent. Coefficient size and significance levels remain the same, indicating that these wars do not drive the results.

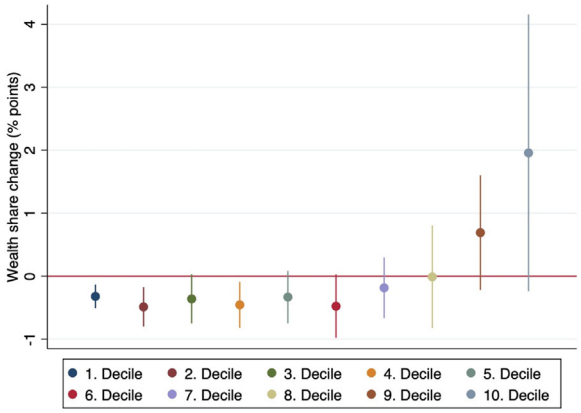

Fig. 5. Changes in the Wealth Distribution during Warfare

Notes: Estimation method is OLS. All regressions include a full set of locality and time fixed effects. The results refer to the period from 1400 to 1600 and from 1675 to 1800. Standard errors clustered at locality level. Confidence intervals indicate significance at the 95-percent level.

Table 4

Conflict Exposure and Economic Inequality: Baseline Results

	1400-1600 & 1675-1800				1400-1800		1625-1650	
	(1) Gini	(2) Gini	(3) Gini	(4) Bot. 50%	(5) Gini	(6) Gini	(7) Gini	(8) Gini
Conflict exposure	0.032***	0.025**	0.062*	-1.659**	0.026**	0.020*	0.014	-0.037**
	(0.010)	(0.011)	(0.029)	(0.687)	(0.011)	(0.011)	(0.024)	(0.018)
Conflict exposure × subperiod 1625-1650						0.040**		
						(0.020)		
Conflict exposure × subperiod 1675-1800						0.035*		
						(0.019)		
Locality FE	YES	YES	YES	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES	YES	YES	NO
Imperial circle × time FE	NO	YES	YES	YES	YES	YES	NO	YES
Only units observed 300+ years	NO	NO	YES	NO	NO	NO	NO	NO
R2	0.267	0.327	0.410	0.307	0.329	0.330	0.365	0.036
Observations	434	434	157	434	504	504	70	70

Notes: Estimation method is OLS. Standard errors clustered at locality level in parentheses. *** p<0.01, ** p<0.05, * p<0.1. In Column 6 the conflict exposure measure has been interacted with a dummy for the subperiod 1400 to 1600.

ables and test whether more unequal places were more likely to be attacked. In a conventional difference-in-differences setting one would perform a balance test, or check for common trends between treatment and control groups. As mentioned, historically warfare occurred repeatedly, even before the period of analysis. So it is impossible to define a pre- and post-treatment period in this setting and perform the relevant tests. For that reason, I move to a flexible difference-in-differences research design in a later section, for two case studies: the Schmalkaldic War (1546-47) and the Palatine Succession War (1688-97). For these single wars I can show common trends, and therefore establish a more robust causal relationship between conflicts and inequality.

4.3. Baseline Results

The main hypothesis is that warfare had two countervailing effects on inequality — equalising destruction and inequality-promoting extraction — and that during ordinary conflicts and throughout most of the period under study the extractive effect was larger. A first piece of evidence comes from changes in the shape of the entire wealth distribution when localities were exposed to a conflict.

In Fig. 5 the plotted coefficients are the point estimates on the conflict exposure indicator when taking the deciles of the wealth distribution as dependent variables. The wealth shares of the lowest six deciles of the population decreased, in most cases significantly. The upper-middle class of the wealth distribution (7. to 8. decile) did not experience significant variation. Only the ninth decile and the top 10 percent (10. decile) did experience a substantial increase. This pattern is exactly what one would expect in a world where the resources to cover the costs of warfare were extracted through regressive taxation.[27]

Table 4 reports the baseline results. Column 1 shows that the occurrence of a conflict was associated with an increase in inequality by 0.032 Gini points, significant at the one-percent level. Adding Imperial circle-time interactions in Column 2 reduces the coefficient and significance, but not much.

In Column 3 I limit the analysis to all units that are part of the dataset for at least 300 years, to address concerns about panel non-response, potentially introduced by selective attrition (Schneider 2020). It seems unlikely that a potential bias leads to an over-estimation of the effect of warfare on inequality. In the Appendix I report results with a balanced sub-panel, which does not change the results either. Column 4 further adds to the picture in Fig. 5, employing the wealth share of the poorest 50 percent as outcome. The reduction of 1.659 percentage points is substantial compared to the about 13 to 15 percentage points owned by this part of the population (see Table 2).

One might wonder how the conflict-inequality relationship looked over the whole period 1400 to 1800, that is, when we pull the Thirty Years' War into the analysis. Column 5 shows that it was still positive and significant. In Column 6 I explore the heterogeneity in treatment timing, which is also important in light of recent critiques of difference-in-differences designs with staggered treatment.[28] Conflict exposure is interacted with dummy variables for the three sub-periods of the analysis, covering approximately the time before, during and after the Thirty Years' War (1400 to 1600, 1625 to 1650, 1675 to 1800). All three coefficients point to a positive warfare-inequality relationship. An interesting facet of this result is that the effect of ordinary conflict on inequality becomes stronger from the seventeenth century. This probably reflects the acceleration of the Military Revolution: when military technology advanced — increased firepower of weapons, stronger fortifications, larger armies — and became more expensive, political authorities had to extract an increasing amount of resources to pay for defence and war (Parker 1996).

On the surface, the results in Columns 5 and 6 might seem puzzling, given that Alfani, Gierok and Schaff (2022) have shown that the Thirty Years' War had a negative effect on inequality in a counterfactual analysis. In Columns 7 and 8 I zoom in on that historically peculiar period. Only when the specification includes no time fixed effects (Column 8) does the substantial inequality decrease during the Thirty Years' War come through. It is significantly different from the relationship in subperiods one and three (Column 2). In other words exposure to single battles does not seem to capture the decrease during the Thirty Years' War, unless one allows the treatment indicator to capture some of the variation in inequality experienced by all localities. This result is consistent with the hypothesis that the Thirty Years' War had a different impact on economic inequality compared to ordinary conflicts, precisely because of its exceptional spill-over effects that turned it into a truly major war. It had a globally destructive impact on inequality, going far beyond the immediate destruction of single battles, to which ordinary conflicts were typically limited.

How economically relevant is the effect of conflict exposure for explaining variation in inequality? During the periods of ordinary preindustrial conflicts (1400 to 1600 and 1675 to 1800) a one standard deviation increase in conflict exposure (0.351-0.402) was associated with an increase in the Gini coefficient by 0.21 to 0.22 standard deviations, which is a sizeable effect (compare estimate in Column 1 of Table 4 with the summary statistics in Table 2). Alternatively consider that the Thirty Years' War reduced inequality by 0.135 Gini points over a century according to Alfani, Gierok and Schaff (2022). Exposure to an ordinary conflict increased inequality by 0.032 Gini points (coefficient in Column 1 of Table 4). Then a back-of-the-envelope calculation indicates that ordinary conflicts, in slightly more than a century, increased inequality by the same amount by which the exceptional Thirty Years' War reduced it.[29]

So far the evidence suggests that ordinary conflicts continuously reinforced inequality during most of the period under study. The theoretical framework suggests that this was so because the extractive effect of wars on inequality likely dominated the destructive one. To shed more light on these two effects, I exploit heterogeneity in conflict distance. I repeat the baseline specification, now testing different distances between localities and conflicts, from 25 to 300 kilometres.[30] Intuitively, if there existed a destructive effect that could outweigh the extractive one to reduce inequality, we should see it most clearly in a geographically limited radius around battle action. It is likely that at least some of the communities falling into the 25-kilometres threshold were directly hit by destructive battle action. Then, at greater distances, we would expect to see an increasingly positive effect of conflict on inequality, because more and more places that were too far away to experience destruction are pulled into the treatment group. Yet these places were treated indirectly. They had to protect themselves against the risk of warfare, inducing them to extract resources in an inequality-promoting way.

The pattern of results in Fig. 6 confirms that intuition. The point estimate for conflicts taking place within 25 kilometres of a locality is negative, suggesting a reduction of inequality. The estimates at greater distances suggest that the association of conflicts and inequality gradually became ever more positive the larger the distance, until a threshold of about 200 kilometres. This pattern reflects that the further away a conflict was from a locality, the more inequality-reducing destruction was outweighed by inequality-promoting extraction. Beyond 200 kilometres the effect seems to peter out, which one would expect, also because the group of untreated places becomes ever smaller at greater distances. The results leave a range between 125 and 250 kilometres around a

[27] Note that the growing confidence intervals are the result of a simple scale effect. Wealthier deciles experience higher variation.

[28] To explore the heterogeneity in treatment timing, Wooldridge (2021: 49) proposes to use the TWFE estimator in a more flexible way, and to interact the treatment indicator with dummies corresponding to "early", "middle" and "late" treatment timing. In this way I account to some extent for heterogeneity in treatment timing.

[29] The average inequality growth explained by warfare during a century of ordinary conflicts, in the period from 1400 to 1600 and from 1675 to 1800, has been obtained by multiplying the regression coefficient with the average conflict exposure of about 0.8 or 0.86 times per quarter century (see Table 2). This result is then multiplied with four, the number of periods per century, leading to a range of 0.102 to 0.0110.

[30] One might wonder whether there was a significant relationship between inequality and the actual distance to the nearest conflict. However, such a measure would be problematic because it could not capture the changing nature of the conflict-inequality relationship across different distances, as documented in Fig. 6.

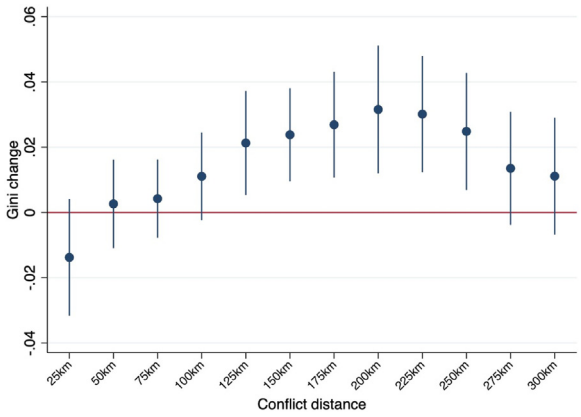

Fig. 6. Conflict Distance Test: Destruction vs. Extraction (1)
Notes: Estimation method is OLS. All regressions include a full set of locality and time fixed effects. The results refer to the period from 1400 to 1600 and from 1675 to 1800. Confidence intervals indicate significance at the 95-percent level.

locality of positive and highly significant estimates. One might wonder whether also conflict risk declined with distance from a conflict. To some extent this was probably the case, because a conflict around Munich was unlikely to continue near Hamburg soon. But it is unlikely that risk declined linearly with distance, because how conflicts spread geographically was hard to foresee precisely. Military historians have documented, first, that opposing armies often chased or tried to escape from each other, depending on their strength. Second, armies split up in legions and merged as needed. In doing so early modern German armies often operated a so-called "five-day-march system" (*Fünf-Tage/Märsche System*). It meant that two parts of the same army should be not more than five day marches away from each other — which corresponded to an operation radius of about 100 kilometres — mainly for reasons of supply (see Gallina 1860: 22-24, 93-99, Fiedler 1986: 205). These movements made it practically unpredictable where armies would meet and fight within a radius of a few days of marching. Any community within that radius was, therefore, potentially at risk.

The results also substantiate the assumption that 200 kilometres is a reasonable threshold distance for conflict exposure. In fact, they show that the exact distance of 200 kilometres did not matter a great deal, because the positive effect holds at a wide range. This is important because it makes it unlikely that the main results are driven by selection into treatment. I have chosen 200-kilometres as threshold for the rest of the analysis for two reasons. First, it seems historically reasonable and in line with other studies' results. 200 kilometres corresponds approximately to the distance that messengers on horseback — and with them important information, for example about a conflict or an investment opportunity — could cover in about two days in early modern Germany (Volckart 2000: 274). For example, 200 kilometres is approximately in line with Chilosi et al. (2018), who document that capital investment activity declined at around 200 kilometres away from a town in preindustrial Germany. It is also roughly in line with the threshold used in recent studies on the impact of conflicts on locality-level outcomes (Dincecco and Onorato 2016).[31] Second, 200 kilometres maximises the goodness of fit of the model (R-squared) among all estimates, but actually does not minimise the p-value (full regression output reported in the Appendix).

Table 5 provides further evidence for the hypothesis that there existed two countervailing effects of warfare. In Column 1 I include conflict exposure variables for the 25- and 200-kilometres threshold in the same specification. The coefficient on the 25-kilometres indicator points towards a direct inequality-reducing effect. Instead, the 200-kilometres indicator points towards an indirect inequality-promoting effect. Note that this approach accounts to some extent for heterogeneity in treatment intensity across units, which is again a major concern of the recent critiques of difference-in-differences designs (see Wooldridge 2021). In Column 2 I drop all the conflicts within 25 kilometres and keep only those within the "donut" from 25 to 200 kilometres, that is, those communities that were indirectly exposed to warfare. The inequality-promoting effect of warfare holds for these indirectly exposed places.

4.4. Controls for Locality Characteristics

The analysis has so far controlled for several time-invariant local and time-variant regional and
"national" characteristics. In Table 6 I first assess the robustness of the main results to time invariant locality-specific characteristics that might have had an impact on inequality. The results should be interpreted keeping in mind that some of these covariates could be "bad controls". Details about the coding of control variables are provided in the Appendix. In Column 1 economic and demographic controls — the log-population size and the occurrence of epidemics — are included. The main result is robust to the inclusion of these controls. This is an important result because by controlling for population size I close a potential alternative mechanism to the one I propose connecting conflict exposure and inequality.

[31] Dincecco and Onorato (2016) geographically sorted conflicts into rectangular grid cells of 150 × 150 kilometres.

Table 5
Conflict Distance Test: Destruction vs. Extraction (2)

	1400-1600 & 1675-1800	
	(1) Gini	(2) Gini
Conflict exposure (25km)	-0.019**	
	(0.009)	
Conflict exposure (200km)	0.034***	
	(0.010)	
Conflict exposure (25-200km)		0.029***
		(0.010)
Locality FE	YES	YES
Time FE	YES	YES
R2	0.272	0.263
Observations	434	434

Notes: Estimation method is OLS. All regressions include a full set of locality and time fixed effects. Standard errors clustered at locality level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 6
Controls for Observables

	1400-1625 & 1675-1800									
	(1) Gini	(2) Gini	(3) Gini	(4) Bot. 50%	(5) Top 10%	(6) Gini	(7) Gini	(8) Gini	(9) Gini	(10) Gini
Conflict exposure	0.026**	0.025**	0.025**	-1.684**	1.404	0.030**	0.025**	0.028**	0.020**	0.020*
	(0.010)	(0.011)	(0.011)	(0.713)	(1.010)	(0.012)	(0.011)	(0.013)	(0.009)	(0.011)
Econ. & demogr. controls	YES	NO	YES	YES	YES	NO	NO	NO	NO	NO
Institutional controls	NO	YES	YES	YES	YES	NO	NO	NO	NO	NO
Agricultural potential	NO	NO	NO	NO	NO	YES	NO	NO	NO	NO
Seaside location	NO	NO	NO	NO	NO	NO	YES	NO	NO	NO
Latitude & longitude	NO	NO	NO	NO	NO	NO	NO	YES	NO	NO
City	NO	NO	NO	NO	NO	NO	NO	NO	YES	NO
Hanse member	NO	NO	NO	NO	NO	NO	NO	NO	NO	YES
R2	0.362	0.330	0.364	0.339	0.307	0.348	0.353	0.388	0.416	0.355
Observations	434	434	434	434	434	434	434	434	434	434

Notes: Estimation method is OLS. All regressions include a full set of locality and time fixed effects, and imperial circle-time interaction effects. Standard errors clustered at locality level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Column 2 includes institutional controls: the introduction of the Protestant Reformation and the log-distance to the closest university. Again the main result is robust to the inclusion of these institutional controls. In Column 3 all time-variant controls have been included. The estimate and significance are the same as in the previous column. In general, the point estimate on the conflict exposure variable remains very stable across specifications. This hints at conflict exposure being uncorrelated with locality characteristics, that is, indeed exogenous from the perspective of an individual town.

In Columns 4 and 5 I estimate the fully saturated specification, taking an indicator of the wealth share of poor and rich strata as outcomes. The results confirm that warfare redistributed wealth shares from the poor part of the population to the rich. In addition to the standard controls, I also interact several time-invariant characteristics with time dummies in Columns 6 to 10, to account for differential trends. I consider agricultural potential, whether a place was located at the seaside, longitude and latitude, whether a place had city status and whether it was member of the Hanse. The main coefficient of interest remains positive and significant.

Further robustness checks are reported in the Appendix. I assess the presence of spatial dependence, robustness to the Conley-correction of standard errors, whether more unequal places were more likely to be exposed to warfare (reverse causality) and employ alternative conflict measures. None of these checks change the main results.

4.5. Mechanisms: Evidence on Defence Infrastructure and Extraction

In this section I provide evidence for the hypothesised mechanism: warfare increasing the demand for costly defence infrastructure. As mentioned, this led to an increase in inequality-promoting resource extraction and defence expenditure. Ideally one would like to have information about the actual resource extraction in communities via taxes and public loans,[32] and one would like to know for

[32] Such data has recently been collected and analysed by Fochesato (2021) for Siena in Renaissance Italy, from 1337 until 1556. He finds that warfare was indeed associated with higher resource extraction by public authorities via taxation and credit.

Table 7
Evidence on Defence Infrastructure and Extraction

	1400-1600 & 1675-1800								
	(1) Gini	(2) Gini	(3) Gini	(4) Gini	(5) Gini	(6) Gini	(7) Gini	(8) Top 10%	(9) Top 10%
Conflict exposure	0.034***	0.024*			0.019	0.018			1.849
	(0.010)	(0.013)			(0.010)	(0.010)			(1.880)
Conflict exposure (1. lag)	0.024**	0.015							
	(0.010)	(0.011)							
Garrison			0.048***	0.048***	0.049***	0.047***			
			(0.013)	(0.013)	(0.012)	(0.013)			
Military construction							0.020	2.484*	2.408*
							(0.013)	(1.422)	(1.344)
All controls	NO	NO	NO	YES	NO	YES	NO	NO	NO
Locality FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Imperial circle × time FE	NO	YES	YES	YES	YES	YES	YES	YES	YES
*R*2	0.284	0.335	0.347	0.358	0.356	0.366	0.262	0.304	0.311
Observations	365	365	249	249	249	249	185	185	185

Notes: Estimation method is OLS. All regressions include a full set of locality and time fixed effects. Controls include the log-population size, the occurrence of epidemics, the introduction of the Protestant Reformation and the log-distance to the closest university. Standard errors clustered at locality level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

what these resources were spent. Unfortunately such information is not systematically available for German communities, leading me to rely on indirect evidence.

In Column 1 and 2 of Table 7 the conflict exposure indicator has been added lagged by one period. As mentioned, one would expect that the effect of warfare on wealth inequality was not just short-lived, but was semipermanent. Since wealth is a stock its distribution adjusts in longer cycles than income. Moreover, increases in regressive taxation were to some extent sticky, for example because resources were extracted over a period longer than the actual war, thus carrying part of the financial need over into the following period. Then previous conflicts should still have a positive impact on inequality. The estimates point into that direction: the coefficients of the lagged conflict exposure indicator are smaller and less significant, but still positive. In line with this evidence, I report additional results in the Appendix, showing that places that were exposed to conflicts repeatedly experienced a higher inequality increase. This most likely reflects that authorities had to extract resources from the population repeatedly via inequality-promoting channels.

In Columns 3 to 6 I include a variable indicating whether a community stationed one or more garrisons. Military forces were a central and costly part of communities' defence systems, especially in larger towns. They mediated the effect of warfare on inequality.[33] The results indicate that garrisons were positively and highly significantly related to inequality. The main conflict exposure indicator loses in size and significance when added to the specification in Columns 5 and 6. This is precisely what one would expect if stationing costly troops was one of the mediators through which wars led to higher inequality.

Another potential mediator was the construction of buildings and sites with a military purpose, such as arsenals, stables for war horses (*Marstall*), or foundries for weapons. The coefficient in Column 7 points towards inequality growth, but is borderline insignificant. However, it is significant when taking the top 10 percent wealth share as outcome (Column 8). This relationship holds when adding the main conflict exposure indicator, which loses significance, suggesting that military construction was a mediator connecting warfare and inequality growth (Column 9).

5. Evidence from Case Studies: the Schmalkaldic War (1546-47) and the Palatine Succession War (1688-97)

In the previous section the repeated occurrence of wars made it impossible to define a never-treated control group for the post-treatment period. In this section I move to a flexible difference-in-differences (DD) or event study research design for two case studies: the Schmalkaldic War (1546-47) and the Palatine Succession War (1688-98), one case in the period before and one after the Thirty Years' War. In these settings it is possible to define a never-treated control group, to show common trends and to establish a more robust causal relationship between conflicts and inequality. The cases also avoid the potential problems of staggered treatment discussed above, and should be interpreted as a validation exercise for the main results. The historical reasons why these wars broke out suggest that they were plausibly exogenous events for individual towns. The Schmalkaldic War broke out because of religious issues, the Palatine Succession War started because the Elector Palatine died without heir. (The Appendix provides more historical information about why these wars broke out.)

In both cases the dataset is limited to all those localities for which a comparison of inequality before and after the respective war is possible. The respective treatment-group consists of all localities that were exposed to battle action within the usual 200 kilometres threshold. The control group consists of all localities that were not exposed to the war. The main identifying assumption is that

[33] For the ability of army expenditure to affect the income and wealth distribution, see Alfani and Di Tullio 2019: 171.

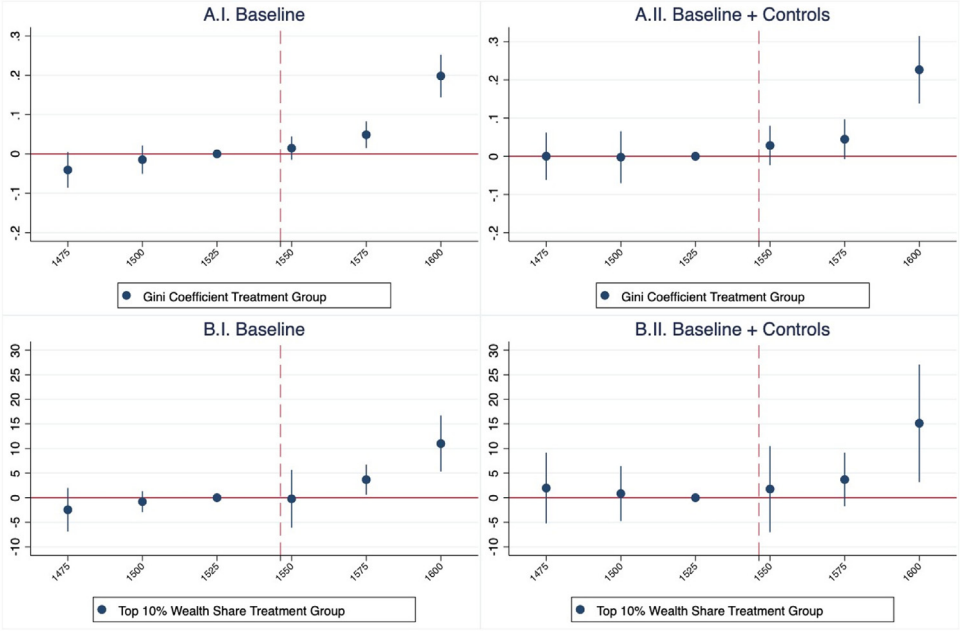

Fig. 7. The Effect of the Schmalkaldic War (1546-48) on Inequality: Flexible Difference-in-Differences Estimates
Notes: Estimation method is OLS. All regressions include a full set of locality and time fixed effects, imperial circle-time interaction effects and a variable controlling for exposure to other wars. Controls include the log-population size, the occurrence of epidemics, the introduction of the Protestant Reformation and the log-distance to the closest university. The reference year is 1525. Standard errors clustered at locality level in parentheses. The dashed vertical line indicates the beginning of the Schmalkaldic War. Confidence intervals indicate significance at the 95-percent level.

inequality in communities with exposure to the respective war would have evolved similar to non-affected communities had the war not occurred.

One might wonder about the implications of setting the conflict-proximity threshold wrongly, which determines the allocation to treatment and control groups. Both the Schmalkaldic and the Palatine Succession War were certainly noted throughout the Empire. It could be that places further away than 200 kilometres reacted to this event. However, in actuality this is only a minor concern. The consequence would be that localities which were effectively treated, are considered part of the control group. This would bias my estimates downwards and lead me to underestimate the true effect, but not overestimate it.

In order to identify the effect of the wars on economic inequality Equation 5 is estimated, separately for the Schmalkaldic War and the Palatine Succession War. The specification is almost identical to Equation 4:

$$I_{i,t} = \alpha_i + \pi_t + \sum_{t=1}^{5} \theta_t \left(W_i \times P_t\right) + \beta CE_{i,t-1} + \gamma' X_{i,t} + \epsilon_{i,t} \tag{5}$$

The main difference is the inclusion of an interaction term between a treatment status indicator (W_i) and a set of five time dummies (P_t) covering the pre- and post-treatment periods. The theta (θ) coefficients are the main coefficients of interest. In addition to the vector of controls ($X_{i,t}$), I add a variable ($CE_{i,t-1}$) indicating the exposure to other conflicts than the respective war of interest.

Fig. 7 reports the results. The plotted coefficients represent an average inequality difference in the respective period. In Panel A I take the Gini coefficient as inequality outcome, and in Panel B the top 10 percent wealth share. The left frame reports results for the baseline specification, the right frame for the fully saturated specification. The dashed line marks the beginning of the war. The results suggest that there was no statistically significant difference in inequality between treated and untreated communities before the war. These common trends indirectly support the identifying assumption (see Angrist and Pischke 2009). Yet after the war began, the divergence in terms of inequality between the two groups began. This result holds for both outcomes and when controls are added.[34]

Fig. 8 shows an inequality pattern for the Palatine Succession War not dissimilar to the Schmalkaldic War. There was again no statistically significant inequality difference between treated and untreated communities before the war, supporting the claim that common trends are a reasonable assumption for individual towns when warfare is the treatment. Yet after the war began, a substantial

[34] In the Appendix I report results suggesting that the small group of losers of this war, the towns of the Schmalkaldic League, might have experienced inequality decline, relative to other treated communities.

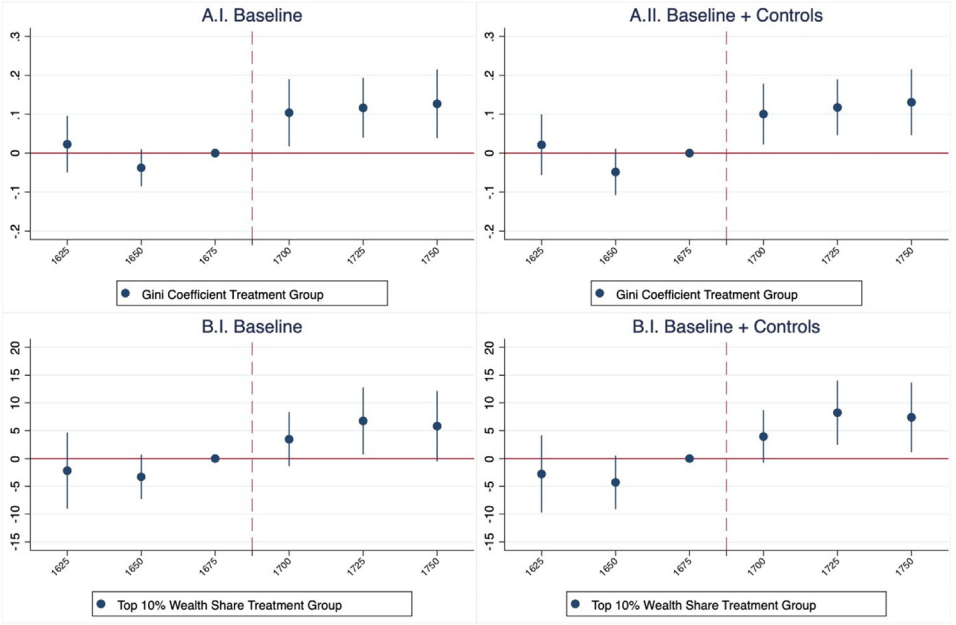

Fig. 8. The Effect of the Palatine Succession War (1688-98) on Inequality: Flexible Difference-in-Differences Estimates
Notes: Estimation method is OLS. All regressions include a full set of locality and time fixed effects, imperial circle-time interaction effects and a variable controlling for exposure to other wars. Controls include the log-population size, the occurrence of epidemics, the introduction of the Protestant Reformation and the log-distance to the closest university. The reference year is 1675. Standard errors clustered at locality level. The dashed vertical line indicates the beginning of the Palatine Succession War. Confidence intervals indicate significance at the 95-percent level.

increase in inequality emerged. The result holds with the Gini coefficient or the top 10 percent wealth share as inequality indicator, and across different specifications.

One might wonder why the inequality differences in Figs. 7 and 8 show signs of persistence or even continued inequality growth, decades after the wars. Two theoretically possible explanations immediately come to mind. Persistent inequality differentials would be plausible if the risk of warfare remained high after these wars, or if risk declined but inequality-promoting resource extraction remained high for some reason. For the Schmalkaldic War, the historiography suggests that both explanations might apply. On the one hand, the conflict was inconclusive. The Emperor formally won the war, but the underlying problem — the religious divide between Protestants and Catholics, and the political frictions in the Holy Roman Empire that derived from that divide — were not resolved for decades (Schilling 1994: 228-232). This could imply that the risk of warfare remained high after the war. On the other hand, the Schmalkaldic War was reportedly a very costly conflict and governments continued for decades to extract the necessary resources. For instance, the city council in Augsburg increased consumption taxes explicitly to cover the costs of the Schmalkaldic War in 1547. This tax increase turned out to be sticky. The increase was partially reduced only after 35 years (Roth 1928: 372). In the meantime this highly regressive tax most likely increased inequality. The case of the Palatine Succession War was in part similar. That war, too, was inconclusive and the underlying political reason of the conflict was not resolved. It was French King Louis XIV's eagerness to extend his territory in the North East, an area that included parts of the Holy Roman Empire. Louis' hunger for territory had previously led to the Franco-Dutch War (1672-79) and the War of the Reunions (1683-84), and it would eventually lead to the Spanish Succession War (1701-14). Ultimately, Louis actually lost substantial territories in the Palatine (Schilling 1998: 199-236). It is therefore plausible that political elites were aware of the continued risk. These historical reasons might explain why war-related extraction continued, making the Schmalkaldic War and the Palatine Succession War particularly well-identified cases with more permanent effects on inequality.

6. Conclusion

This paper has examined whether wars were equalisers of economic inequality, in preindustrial Germany from 1400 to 1800. Previous studies found equalising effects, but have mostly focused on history's largest wars, usually leaving aside ordinary wars (see van Zanden 1995, Piketty 2014, Scheve and Stasavage 2016, Scheidel 2017, Alfani et al. 2022). My findings suggest that the "wars are great equalisers"-hypothesis holds only for truly major wars, such as the Thirty Years' War. However, such major wars were historically the exception and not the rule. The many ordinary conflicts that were paradigmatic of life in preindustrial times led to a redistribution of wealth shares from the poor to the rich. Consequently, they continuously increased inequality. The impact of the many ordinary wars was significantly different from the impact of the one major war this study has looked at, the Thirty Years' War. The main takeaway from the analysis is that rising inequality is an underappreciated negative externality in times of conflict.

The evidence is indicative of these seemingly puzzling findings being due to the existence of two countervailing effects of conflicts on inequality: inequality-reducing destruction, and inequality-promoting extraction. During ordinary wars and throughout most of the period under study the extractive effect outweighed the destructive one. This dynamic of two countervailing effects has been often overlooked. The historical reason for the existence of the extractive effect was that maintaining peace and providing protection for inhabitants were fundamental tasks of towns in preindustrial Germany. The risk of being attacked during a war increased a community's demand for protection, and made the extension of defence infrastructure and other war-related expenses necessary. The resources to cover the costs were extracted through channels that inevitably increased inequality, most importantly regressive taxation and credit.

These findings could potentially lead to far-reaching lessons for the history of economic inequality. They suggest that regular warfare contributes to explaining high levels of inequality across Europe already before industrialisation began (see Alfani 2021). As a final reflection, it should be remembered that military conflicts were an expression of geo-political rivalries and competition. Such competition between polities has been praised for its economically beneficial effects on institutional quality, state capacity and a society's innovativeness in preindustrial times. It has even been suggested that political fragmentation was among the root causes of Europe's rise to riches, especially in comparison with China (North 1981, Hoffman 2015, Mokyr 2016). However, one of the implications of this paper is that geo-political competition between polities had a negative spillover: rising economic inequality, because rivalry induced political authorities to extract economic resources in an inegalitarian way.

Declaration of Competing Interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Supplementary materials

Supplementary material associated with this article can be found, in the online version, at doi:10.1016/j.eeh.2022.101495.

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