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When 'The State Made War', what Happened to Economic Inequality? Evidence from Preindustrial Germany (c.1400-1800)

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Abstract

What was the impact of military conflict on economic inequality? This paper presents new evidence about the relationship between military conflicts and economic inequality in preindustrial Germany, between 1400 and 1800. I argue that ordinary military conflicts increased economic inequality. Warfare raised the financial needs of towns in preindustrial times, leading to more resource extraction from the population. This resource extraction happened via inegalitarian channels, such as regressive taxation. The Thirty Years' War was an exception to that pattern but not the rule. To test this argument a novel panel dataset is constructed combining information about economic inequality in 72 localities and 687 conflicts over four centuries. The analysis suggests that there existed two countervailing effects of conflicts on inequality: destruction and extraction. The Thirty Years' War was indeed a "Great Leveller" (Scheidel 2017), but the many ordinary conflicts – paradigmatic of life in the preindustrial world – were continuous reinforcers of economic inequality.

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1 Introduction

It has become a historic truism that wars are, as Scheidel (2017) argues, a "Great Leveller". Whether this negative relationship between war and economic inequality 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 (see also van Zanden 1995, Piketty 2014: 147, Cosgel and Erene 2012: 321, Milanovic 2016: 56).

This paper examines the accuracy of the "war reduces inequality"-hypothesis for wars in the preindustrial period. Specifically, it argues that most ordinary conflicts between major political authorities in the preindustrial era actually increased economic inequality. The response to the threat of war induced political authorities to extract economic resources via inegalitarian channels, such as regressive taxation. The Holy Roman Empire – Germany, for short[1] – is the ideal testing ground for this question as this was the area where the Thirty Years' War (1618-1648) mostly took place. Moreover, the Holy Roman Empire was a very diverse entity in economic, geographical and institutional terms, and I will account for its diversity.

To test this hypothesis the paper employs novel data on the distribution of wealth at the household-level in 72 cities and villages, collected from new archival tax records and secondary sources. The data on wealth inequality are combined with information about 687 battles and sieges between important political actors to examine conflict and inequality in Germany 1400 to 1800. Consistent with the literature, my first econometric strategy exploits the plausibly exogenous occurrence of battle action from the perspective of an individual town, and I account for potential omitted variable bias and reverse causality. I then move to a difference-in-differences research design to establish a more robust causal relationship between the two variables in question. I combine this quantitative analysis with a historical account of what actually happened in towns that experienced warfare in preindustrial Germany.

I find a strong and positive relationship running from warfare to economic inequality. During ordinary conflicts wealth was transferred 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 15 percent of a standard deviation. This result is robust to several checks. Moreover, the result is supported by the difference-in-differences estimates. A back-of-the-envelope calculation suggests that warfare might explain almost three fourths of the inequality increase in Germany during the century that preceded the outbreak of the Thirty Years' War. Yet 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.

[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: 25-39). 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".

My seemingly puzzling empirical results are due to there being two countervailing effects of conflicts on inequality: destruction and extraction. The risk of being attacked during a war increased the demand for protection. It induced local political authorities to draw revenue from the population to build walls and arsenals, finance armaments, pay for mercenaries and create a bureaucracy to extract resources. Since ordinary conflicts caused a locally very limited destruction, the indirect extractive effect of warfare was likely larger than the direct destructive effect throughout most of the period under study.

This paper makes three contributions to the literature. Empirically, it questions a long-running assumption undergirding the relationship between warfare and inequality. The expansion of markets and the economy (Puga and Trefler 2014: 755, van Bavel 2016, van Zanden 1995), demographic growth (Alfani and Ryckbosch 2016, Pfister 2020), exclusive political institutions (Boix 2003, Scheve and Stasavage 2017, Minns et al. 2020: 603), and cultural norms and ideology have been proposed as major causes of preindustrial inequality growth (Alesina and Giuliano 2010, Basten and Betz 2013, Piketty 2020). Traditionally, warfare has been considered a factor that reduced economic inequality (see Milanovic 2016: 56, Scheidel 2017: 86-112, 130-173). Instead, the findings of this study suggest that warfare was an important factor that contributed positively to the preindustrial inequality rise, a development that saw rising inequality across Europe long before industrialisation began (see Alfani 2020). In other words, warfare might be a significant part of the explanation for why economic inequality was high already when industrialisation was about to start.

Secondly, it also provides a plausible account of why warfare – arguably a defining characteristic of preindustrial life (Pinker 2011; see also Tilly 1992: 72) – might increase inequality. The argument is probably closest to Alfani and Di Tullio (2019, Alfani 2015, 2020), who argue that the way that polities in preindustrial Europe taxed their subjects increased economic inequality, and to Ogilvie (2019: 7, 173, 576) and Acemoglu et al. (2005: 390, Acemoglu and Robinson 2012: 157) who emphasise that extractive institutions in general made the distribution of economic resources more unequal.

Finally, the paper also extends the literature on the nexus between warfare, state formation and fiscal capacity (Tilly 1975, O'Brien 1988, Ertman 1997, Volckart 2000a, Stasavage 2011, Dincecco and Prado 2012, Gennaioli and Voth 2014). 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 communities more unequal.

The next section outlines the conceptual framework of this paper and provides historical background information. Section three describes the data on economic inequality and military conflicts in preindustrial Germany. Section four presents the empirical strategy and the main results of the paper. Section five and six investigate the robustness of these results. Section seven concludes.

2 Conceptual Framework

Economic inequality is an issue of topical importance because it has several socio-economic consequences. It relates to the distribution of political power, has the potential for destabilising the social balance and can have a direct detrimental effect on the economy (Acemoglu and Robinson 2012: 343-344, Puga and Trefler 2014, Persson and Tabellini 1994, Atkinson 2015: 12). Moreover, recent research has shown that economic inequality in Europe had been growing for centuries already before the beginning of the industrial era (Alfani 2020), which has traditionally been considered the starting point for substantially growing inequality (Kuznets 1955, Ray 1998: 209-211. Yet little is known about why inequality reached high levels already before industrialisation began. This section presents a framework that aims to explain how military conflicts are positively related to economic inequality in preindustrial times.

In preindustrial Germany there existed three types of conflicts. First, there were feuds, which were a small form of conflict, usually fought among two opposing individuals, such as knights, merchants or peasants (see Volckart 2004a, Kroener 2013: 60).[2] Secondly, there were bottom-up revolts by peasants or townsmen (see Blickle 2012: 3-44). Thirdly, 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, Swiss Cantons or Italian states.[3] This paper is interested in the third category of conflicts because these were more likely to be exogenous events from the perspective of an individual locality. Focusing the analysis on conflicts between important political actors makes it possible to identify the effect of conflicts on inequality more precisely. The focus is thus on conflicts that were entirely different from those civil wars that have been analysed in studies interested in the contribution of inequality to the outbreak of social conflict (see Esteban and Ray 1999, Blattman and Miguel 2010, Baten and Mumme 2013).

Previous scholarship has predominantly emphasised a negative relationship between conflicts and economic inequality, based on evidence from the world wars and, in the early modern period, from the Thirty Years' War. Often, the destructiveness of wars - widely defined, including factors such as human loss, destruction of capital, confiscation and economic breakdown - is considered a decisive driver behind inequality reduction (see van Zanden 1995, Piketty 2014: 147, Cosgel and Erene 2012: 321, Milanovic 2016: 56, Scheidel 2017: 86-112, 130-173). However, the literature has been silent about the impact of the frequent, less destructive conflicts that were typical of the preindustrial era and that did not reach the dimensions of the Thirty Years' War. Only recently Alfani and Di Tullio (2019, Alfani 2015, 2020) have argued that inequality in preindustrial Italy was driven by "the rise of the fiscal state", and that this rise was related to the "Military Revolution". This paper continues that line of thought. It argues that in addition to the destructive effect much of the literature has focused on, wars had a second, countervailing effect on inequality:

[2]Note that feuds were legally banned by the emperor in 1495. However, some feuds also took place in the early modern period (Volckart 2004a: 287).

[3]Imperial estates were those political authorities that represented their polities (territories and imperial cities) at the *Reichstag* (imperial diet) and that contributed to the costs of the Empire, such as the financing of the *Reichskammergericht* (Imperial Chamber Court). See Oestreich and Holzer (1973) for an overview of the imperial estates. For the time before 1422, when the first catalogue of imperial estates was compiled, the study focuses on those political authorities that would later become imperial estates.

inegalitarian extraction. Extraction happened because maintaining peace and providing protection for inhabitants were fundamental tasks of communities in preindustrial Germany. The channels through which communities extracted the necessary resources – taxation, credit, financially strong organisations – benefited the rich to the detriment of the poor, increasing inequality. The extractive effect of conflict exposure is schematically summarised in Figure 1.

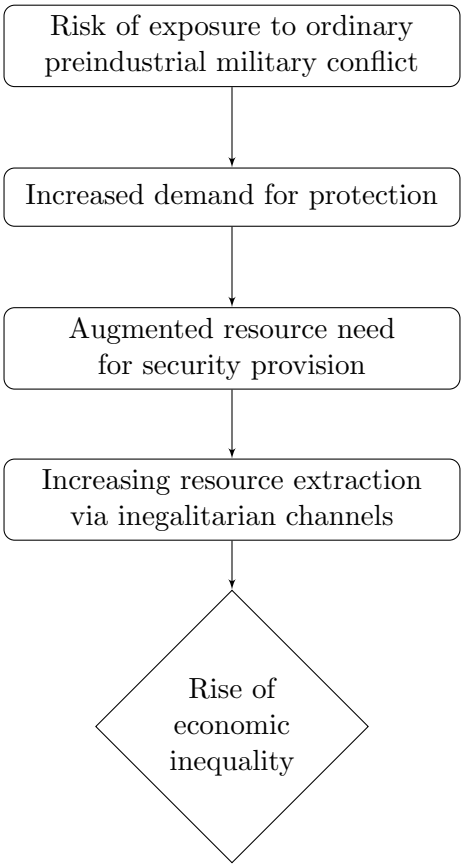


Figure 1: Extractive effect of warfare on economic inequality

During most of the period under study, the extractive effect was presumably larger and so wars constantly increased economic inequality. Importantly, the Thirty Years' War was an exception to that pattern. It was exceptionally destructive, more than any other war in early-modern Europe. Therefore, during the Thirty Years' War the destructive effect prevailed relative to the extractive one, making *this* war an exceptional leveller. Yet declining inequality in the aftermath of conflicts was not the rule.

After elaborating on the hypothesised mechanisms that link military conflicts to economic inequality, some alternative explanations for the increase in economic inequality in premodern societies are discussed.

2.1 Historical Background: Extraction, Destruction and Inequality

It is a well-established fact that warfare increased political elites' need to extract financial resources in preindustrial times (see O'Brien 1988, Karaman and Pamuk 2013). Following the same logic,

the extractive effect of warfare on inequality was a consequence of exposure to military conflicts that increased the financial needs of communities. This resource need derived from the fact that maintaining peace and providing protection for inhabitants were fundamental tasks of towns in preindustrial times. The period was characterised by the absence of monopolies of force in the hands of consolidated 'Weberian' states (Volckart 2000a: 279, 2002b: 25). The wish to protect the own existence, but also trade, manufacturing activity and political independence made defence infrastructure indispensable (Isenmann 2014: 448-452). Exposure to a conflict did not necessarily mean being actually attacked. It was the *risk* of being attacked during a war that was decisive for increasing a town's demand for protection. The capacity to protect inhabitants gave preindustrial towns a "safe harbour" character (Glaeser and Shapiro 2002: 208-210, Dincecco and Onorato 2016: 260). This protective task of towns was only made redundant with the widespread dismantling of walls and fortifications in the nineteenth century (see Heil 2003: 113-116).

Incidents of warfare in the surrounding area increased the risk for towns of being attacked. This risk increased the demand for protection. It made the extended use of costly defence and bureaucratic infrastructure (e.g. more wardens, more weapons and more warriors) and its permanent expansion necessary (e.g. more fortification, bigger arsenals, institutionalization of defence management, more legislation, expansion of the fiscal administration).

This expansion of defence and bureaucratic infrastructure was further promoted by constant innovations in military technology across Europe, making the military sector the most dynamic one of the preindustrial economy (Hoffman 2011: 41, 55). Between the fourteenth and the eighteenth centuries decisive innovations happened, beginning with the replacement of knights with mercenaries, innovations in artillery, subsequent improvements in fortification, the "gun powder revolution", the increasing size of armies and finally the professionalisation of armies. The more military technology advanced, the more costly it became for communities to adapt to it (Kroener 2013: 62, 69, Fouquet 1989: 59).

The evolution of military technology made existing defence infrastructure obsolete in case of attack.[4] As a response, walls and gates became stronger over the centuries, equipped with more towers, wards, several rings of additional walls and earthworks (*Landwehren*).[5] For example, in Nuremberg the fortification was reinforced from 1384, shortly after the War of the Swabian League (1376-1377) and in expectation of an upcoming war between princes and cities, which eventually came about with the War of the Cities (1387-1394). Then, in response to the Hussite Wars (1419-1434), Nuremberg reinforced its town moat. In 1452, just after the First Margrave War (1449-1450), a second city wall was built, including the installation of cannons at every gate. The improvements of

[4] Traditional weapons, such as the crossbow were not very powerful and had a short reach so that effective defence ideally happened from above, requiring high but not particularly strong walls. The appearance of handguns and cannons in the fourteenth century implied two changes for a town's defence. First, in order to keep enemies at a longer distance the defence line of towns was moved outward. Effective defence did not happen anymore from above but rather at a long distance. Second, the switch from stone to iron bullets used in canons increased the effectiveness of cannons so that towns had to upgrade their fortifications and moats (Isenmann 2014: 103, 457). In the seventeenth century the early-modern Military Revolution is generally believed to have accelerated in Germany (Burkhardt 1992: 213-219).

[5] *Landwehren* were planted earth walls, sometimes with additional towers at some distance around a town. These could be very large systems, such as the 120-kilometre-long *Landwehr* of Schwäbisch Hall (Isenmann 2014: 103).

wall and moat were quite elaborate, requiring a workforce of around 800 men. Further improvements of the fortifications were undertaken until well into the seventeenth century, to keep up with the technical and tactical improvements in artillery (Isenmann 2014: 100-102). Similarly, Hamburg enhanced its fortification in the style of the *trace italienne* in the seventeenth century because of continuous conflict with the Danish King, and Freiburg im Breisgau reinforced its fortification in 1744 because of the threat of being attacked by French troops during the Austrian War of Succession (1740-1748) (Weber 2000, Rödel 2003, Hohrath 1996: 323).

Smaller towns reacted in similar ways to exposure to warfare and the risk of being attacked. For example, during the Saxon Fratricidal War (1446-1451) the council of the small town Altenburg ordered to expand the defence infrastructure, although the town was not even directly involved in the conflict. The town built a bulwark, strengthened the town wall and enhanced the moat. Around 70 people participated in the construction works, which was a significant amount in a town of about 600 households at the time (Löbe, 1895, pp. 166–168).[6] Similar developments took place in villages, which, just like towns, had a desire for and took measures to achieve protection and security. Villages had simple defence constructions such as walls and palisades and were clearly able to defend themselves against modest military attacks (Rösener 1996: 98, Blickle 1981: 52-53, Bonney 2002: 75).[7] However, overall the continuous updating of defence-related construction was quite an uneven process across towns in premodern Germany. Some towns still had wooden walls as late as the sixteenth century when others already had sophisticated fortification (Isenmann 2014: 102). The variation in defence infrastructure was to a good extent determined by the intensity of warfare in preindustrial Germany (Dincecco and Onorato 2016: 278-279).

Beyond fortification, warfare induced towns to acquire expensive arms and equipment, expand their storage facilities and professionalise the management of arms and equipment. Since the fifteenth century the increasing number of armed people, the standardisation of arming, its procurement, and new weapons such as cannons made new storage facilities necessary. A town-level administration for procuring, repairing and storing arms and equipment developed. Among other things, expanded armour chambers (arsenals), or stables for horses employed in defence were built (Isenmann 2014: 456). For example Überlingen reacted to the threats of the Swabian War (1499) with the acquisition of new canons, followed by the construction of a new arsenal for storing these and other weapons (Koberg 1975: 62-64). Needless to say that the acquisition of weapons and the building of storage facilities required towns to spend large sums (Isenmann 2014: 454).

In addition to construction activity, exposure to warfare also made it necessary for towns to expand their defence infrastructure in terms of personnel and deploy more people, often in squads of guards (*Schützenkorps*). This was necessary not just to fend off actual attacks, or participate in a battle, but also because of the general insecurity created by roving mercenaries during war periods.[8]

[6] Figure calculated based on population figures provided in the *Deutsches Städtebuch*, assuming an average household size of 4.5 people.

[7] Between the High Middle Ages and the end of the early modern period 35 to 43 percent of very small communities (with less than 1,000 inhabitants) had actual walls in Germany (Tracy 2000: 84).

[8] Standing armies became increasingly important and gradually replaced mercenaries from the second half of the seventeenth century. However, in the eighteenth century only eight out of more than 300 imperial estates actually had standing armies (Schmidt 2009: 37).

These warriors had a high potential for violence, especially when they were laid off after fighting and plundered the surrounding area to make a living (Kroener 2013: 14, 59, Krüger 1980: 3). In risky times of war, therefore, the number of guards had to be augmented, to staff all parts of a town's fortification, for providing safe conduct, for policing and dealing with robbers. For example, Altenburg first doubled and later quadrupled its number of guards during the Saxon Fratricidal War. Moreover, the town sent soldiers – equipped by the town with weapons, horses and provisions – to support the troops of the elector of Saxony in the nearby battle action (Löbe 1895: 166-167). Over time the services of professional mercenaries for protective purposes became more widely used, replacing the military services of laymen. Since mercenaries were quite costly the costs of defence for towns in war times inevitably increased (Kroener 2013: 13, 25).[9]

With the increasing importance of defence infrastructures grew the local bureaucracy (Isenmann 2014: 457). This happened, first, in order to put in place security legislation.[10] Second, the bureaucracy managed defence. Towns hired – often highly paid – specialists for procuring, using, repairing and storing weapons (*Büchsenmeister, Zeugwart*), hired defence construction specialists (*Festungsbaumeister*), established supply managers (*Proviantmeister*) and formed special war councils (*Kriegsrat*) in the town administration to manage the risk of and response to threats (Isenmann 2014: 454-456, Schmidtchen 1985: 287-289). For example, following the building of its new arsenal after the Swabian War, Überlingen prepared its first weapon inventory and attempted to hire a particularly skilled gunpowder producer from Nuremberg as weapon master in its new arsenal (Koberg 1975: 62-64). Thirdly, to extract the necessary resources to pay for protection, towns expanded their fiscal administration. However, that was labour-intensive and expensive in itself, because town officials earned high wages (Schilling 1994: 66, Isenmann 2014: 457). For example, in early eighteenth-century Göttingen a new consumption tax was introduced to cover the costs of warfare. The administrative effort of this new tax increased the costs of the municipal tax administration about fourfold, to ca. 11 percent of the gross tax revenues (Winnige 1996: 80). It comes as no surprise that towns in premodern Germany have been considered the "hothouse of the modern bureaucratic state", and also at this lower level of political organisation it seems true that "War made the state" (see Isenmann 2014: 449, Tilly 1975: 2).

Apart from defence and bureaucratic infrastructure, there were additional expensive efforts that communities had to make in times of warfare. For example, they had to provide board and lodging for soldiers fighting nearby,[11] or ransom imprisoned inhabitants as part of the community's pro-

[9] Originally towns relied on militias consisting of townsmen for their numerous protective tasks, but already in the fourteenth century approximately a third of armed forces consisted of mercenaries (Kroener 2013: 3). Mercenaries were very costly, but they were preferable to untrained laymen, because they could hold out better against growing armies and because military service and invalidity of townsmen were a considerable disruption to a town's economy (Isenmann 2014: 453).

[10] Communities introduced so called "police ordinances" (*Polizey Ordnungen*). These should not be confounded with the modern notion of police. In preindustrial Germany these ordinances regulated social life of communities in order to maintain public and private security in a wide sense. They regulated, among other things, clothing, poor relief, prices, guild rights, but also indicated how to deal with criminal activity of vagrants, robbers, bandits and marauding soldiers (Härter 2010: 44-46, 52).

[11] Again, during the Saxon Fratricidal War the small town Altenburg accommodated 370 man and 180 horses, and deployed 20 cooks for providing them with meals. Given this effort – for construction, personnel and accommodating soldiers – it comes as no surprise that Altenburg had to raise specific war taxes (Löbe 1895: 166-167).

tective task.[12] In sum, warfare was a huge challenge for towns, in terms of effort and economic resources needed. This resource need increased constantly with the technological progress in defence technology during premodern times. The abrupt increase of the resources needed in times of war – to cover defence, bureaucracy and other war-related costs – could become a ruinous burden for a community's budget (Isenmann 2014: 452-453).

In order to obtain the financial means to cover the costs of protection towns had to extract economic resources from their inhabitants and from the surrounding area.[13] Local political authorities had several options to obtain the resources they needed. Presumably the most important channel to extract resources was taxation (Fouquet 1989: 71-74, Becker et al. 2018). There were many different taxes that could be levied and they fall in three main categories: consumption taxes (e.g. *Ungeld, Akzise*), wealth taxes (e.g. *Schoss, Geschoss, Bede, Schatzung, Ordinari Steuer*) or extraordinary war taxes (e.g. *Nürnberger Hussitensteuer, Augsburger Zuschlagssteuer*). The fact that towns in premodern Germany extracted more economic resources from their inhabitants because of warfare is historically well documented. For example, Konstanz tripled its wealth tax to cover the costs of the Old Zurich War (1439-1450) (Kirchgässner 1960: 89), Hamburg levied a special tax to pay for the conflict with the Prince of Brunswick-Wolfenbüttel in 1554 (Potthoff 1911: 19-20, 43-46) and Straubing levied a war tax to pay for the War of the Spanish Succession (1701-1714) (Keim 1957: 67).

Additionally, territorial rulers' requests sometimes increased the local resource burden. From the end of the seventeenth century rulers of the largest territories within Germany started building up defence infrastructure.[14] When rulers were powerful enough they, for example, could impose the construction of a fortress on a community. Inhabitants typically had to bear the brunt of the costs (Kroener 2013: 39-43, Hohrath 1996: 308, Rödel 2003: 98, 103).[15] For example, the reinforcement of the fortification in Freiburg during the War of the Austrian Succession was ordered by the territorial ruler, but townsmen had to pay a daily poll tax to cover the costs (Hohrath 1996: 321-323).

[12] For example, Frankfurt paid the huge sum of 73,000 gulden to ransom 620 people during the War of the Cities (Isenmann 2014: 147).

[13] In Germany towns had a particularly high degree of administrative and fiscal autonomy from their *de jure* overlords, compared to the rest of preindustrial Europe. At the beginning of the early modern period this applied to the circa 85 imperial cities and territorial towns alike (Schilling and Ehrenpreis 2015: 38-40). Yet for territorial towns the growth of territorial states implied an increasing interference in the local administrative autonomy by their overlords. Since most territories could not establish themselves in terms of fiscal- and military-administration until the end of the early modern period, communities maintained considerable administrative autonomy in terms of defence and taxation (Schilling 1994: 21, Volckart 2002c: 163-165, Schilling and Ehrenpreis 2015: 28-29, 37). Maintaining peace and providing protection for inhabitants remained a fundamental interest of German towns in the early modern period (Schilling and Ehrenpreis 2015: 46; see also Wiese-Schorn 1976: 30).

[14] This expansion of territorial military infrastructure did not apply to many territories. Schmidt (1999: 37) only counts eight imperial estates (out of more than 300) that had standing armies of a size worth mentioning at the beginning of the eighteenth-century. But still, in those territories where this process began, extracting resources from the population was needed to cover the costs of defence infrastructure (Kroener 2013: 44).

[15] An additional form of resource extraction that came along with standing armies was billeting. Under that system the civilian population was forced to accommodate soldiers, provide them with basic goods, such as wood, lighting and salt, or make money payments for maintaining the army. This practice was inequality-promoting, because local elites were often exempt from the duty of accommodating or paying for soldiers (Eichberg 1989: 108-109). Moreover, payments in kind were effectively inegalitarian poll-taxes.

Political elites did not only tax their own inhabitants to extract resources for warfare. Typically, towns taxed the villages that surrounded them as well (Isenmann 2014: 454-455), and also villages themselves levied taxes on their inhabitants (Volckart 2004b: 27).[16] For example, after the costly expansion of Altenburg's town moat during the Saxon Fratricidal War, the town levied a special war tax on the peasants of the surrounding villages (Löbe 1895: 167).

Another frequently used mode to finance war-related expenses was the expansion of credit. For example, to cover the costs of the Neuss War (1473-1480), Cologne borrowed more in two years than it had borrowed in the previous half century (Stasavage 2011: 113). However, credit only postponed resource extraction via taxation, because interest had to be paid and eventually the credit had to be paid back (Fouquet 1989: 74). 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).

A final possibility of towns to extract resources in order to pay for warfare was through collaboration with financially strong organisations. Ecclesiastical organisations, such as monasteries, contributed to the resource need caused by warfare, either in kind (e.g. giving horses, wagons) or money (Isenmann 2014: 454). Yet another source of towns' revenues were corporate groups, such as guilds. Since guild masters often were among the richest individuals in their towns, they were important financiers in case of a military conflict (Laufer 1973: 179-189, Ogilvie 2019: 52-54, 576). Moreover, guilds had considerable coercive means at their disposition (Volckart 2002a: 330). Political authorities, therefore, received military services (e.g. guarding, fighting, organizing the militia) and loans from guilds, which were valuable contributions in times of warfare (Isenmann 2014: 451-454).

Why is this relevant for economic inequality? The channels through which towns obtained the necessary economic resources to cover the costs of protection - credit, financially strong organisations, taxation – had potentially inegalitarian effects.

The inegalitarian impact of credit derived from who had to bear the burden of repayment and interest, and who benefited from it. Usually, the creditors were local elites, that is, rich individuals like merchants, guild masters or officials. Warfare increased the demand for credit. Since interest rates were commonly quite high at the time - between ten and three percent (see Chilosi et al. 2018: 647) - rich individuals could accumulate even more wealth due to warfare (Alfani and Di Tullio 2019: 172-173, Schulze 1995: 271, Stasavage 2011: 116). Poor people could not make such profitable investments in public debt because their saving capacity was very low (Dirlmeier 1978: 517).[17] Instead, they could expect to be subject to rising regressive taxes in order to pay for the debt (Stasavage 2011: 116-117). Towns in the Empire had strong incentives to service their debt (Chilosi et al. 2018: 643-664), thus reinforcing the inegalitarian dynamic.

[16] Towns received taxes from villages either in return for offering protection in so-called "peace federations" (*Landfriedensverband*), or for granting economic privileges (e.g. toll exemptions), or because the town was an administrative-protective centre for villages within a larger territory (Isenmann 2014: 672-673, 679-688)

[17] Chilosi et al. (2018: 651-653) find that the average sum creditors invested in public debt even increased from the fourteenth to the eighteenth century, from 31 annual "respectable consumption baskets" to 87 . In a poor preindustrial society (see Broadberry et al. 2015: 423), these figures imply that it must have been rich individuals that invested the bulk of capital and profited from the high interest rates paid for government debt.

Obtaining economic resources from financially strong organisations had inegalitarian effects too. Ecclesiastical organisations extracted economic resources in ways that were quite similar to the taxing practices of secular authorities. The standard ecclesiastical contribution - the tithe (*Zehnt*) - was a flat tax, and also other contributions (for example *Hufengroschen, Häuslergroschen*) were either proportional or poll taxes with regressive effects, just like taxes levied by secular authorities (Borgolte 2004: 52, Blaschke 1989: 63, 69).

The redistributive effect of guilds stemmed from the fact that they provided economic resources in case of warfare to political authorities, in return for receiving privileges that gave guilds market power. These privileges typically comprised the granting of monopolies, control of prices and wages, subsidies, tariffs, entry restrictions, labour market regulations or other legislative favours (Ogilvie 1992: 432-433, Volckart 2002a: 330). Such collaboration between guilds and local political authorities was very common in preindustrial Germany (see Ogilvie 2019: 531, Wahl 2019: 202). Naturally, granting guilds the right to redistribute parts of the economic pie to themselves potentially exacerbated economic inequality (Acemoglu et al. 2011: 3288-3289, Ogilvie 2019: 7, 173, 576).

Possibly the most important redistributive channel of warfare was taxation. Premodern tax systems functioned very differently compared to their modern counterparts. In modern times, progressive taxation in the aftermath of warfare is considered a crucial leveler of inequality (Scheve and Stasavage 2016). Instead, premodern tax systems were structurally regressive, which means that "the poor were taxed proportionally more than the rich" and hence "post-tax inequality was higher than pre-tax inequality" (Alfani and Di Tullio 2019: 147). This characterisation also applied to taxing practices in preindustrial German communities (Scott 2002: 45), and the three major tax types employed for extracting resources for warfare. The first type were wealth taxes. These were usually proportional taxes, levied by applying a flat tax rate, that is, asking the same percentage of wealth or income from rich and poor. For example, in 1420 Frankfurt the tax rate was 7 percent, in 1530 Rostock 10 percent and 3.5 percent in 1676 Nördlingen (Bücher 1917: 417, Staude 1912: 154, Friedrichs 1979: 158). Modern notions of progressive taxation did not exist.[18] The regressive character was exacerbated by partial tax exemptions, for privileged, presumably wealthy groups and organisations. This could be the case, depending on the local context, for the property of nobles, the church and of other elite individuals, such as officials (see Alfani et al. 2020: 67-68).

The second tax type – consumption or excise taxes, levied on basic consumption goods – was potentially even more regressive. They have been labelled "the most unsocial tax imaginable" (Boelcke 1971: 171). The reason for this lies in Engel's law: taxes on basic consumption goods, such as beer or grain, weighed particularly heavily on the budgets of the poorer strata of society because they spent a larger part of their household budgets on such basic goods compared to rich people (Winnige 1996: 81, Alfani and Di Tullio 2019: 149-150). Such taxes were substantial, increasing the price of goods such as bread or beverages by 10 to 30 percent. Given that urban middle class households spent approximately 70 percent of their budget on foodstuffs, taxation could be a heavy burden on them (Isenmann 2014: 525, Dirlmeier 1978: 420).

[18] It has been shown that the tax system in Germany was regressive until at least 1871 (Buggeln 2019: 79-81, 511).

The third tax type that communities used to extract resources in times of war was potentially the most regressive one (Isenmann 1999: 268): extraordinary war taxes, often levied as poll- or semi-poll tax, that is, either setting a fixed amount for every member of a certain profession or setting a high basic amount for everybody. Poll taxes were highly regressive because by asking everybody to pay the same amount, they effectively took an increasing share of income or wealth the poorer the taxpayer was (Isenmann 2014: 521). Yet these taxes were appealing to authorities because by levying the same amount on everybody, poll taxes required low administrative effort. A poll tax was for example levied in Damme in 1602 because of the nearby Dutch War of Independence (1568-1648) (Imsiecke et al. 2010: 91-131), in Straubing during the War of the Spanish Succession (Keim 1957: 67), or in Freiburg during the War of the Austrian Succession (Hohrath 1996: 321-323).

In sum, the more economic resources political elites extracted through the described channels, the more unequal the society became. However, the inegalitarian effect of resource extraction was typically intensified on the spending side. In modern, industrialised societies the provision of welfare makes up approximately two thirds of government budgets. In preindustrial societies things were radically different (Alfani and Di Tullio 2019: 165-167). Social spending was a marginal part of a town-government's budget. The bulk was spent for construction, personnel and equipment for defence, bureaucracy, the service of debt, or was lost in the form of rents from officeholding. A town government in preindustrial Germany typically spent around 6 to 8 percent for bureaucracy, 9 to 19 percent for construction (which was mostly defence-related), 29 to 41 percent for security and external affairs, 31 to 67 percent for paying for debt and interest, and only 0 to 2 percent for welfare, such as health and poor relief (Isenmann 2014: 519-521). This spending pattern favoured members of the socio-economic elite, such as military contractors, producers of military equipment, merchants, officials and those wealthy enough to lend money to the government (Schulze 1995: 278; see also Kroener 2013: 15). Consequently, spending could not counterbalance the inegalitarian effect of resource extraction, but potentially exacerbated it.

For these reasons, when there was warfare and towns extracted resources to pay for defence, bureaucratic infrastructures and the direct costs of war, economic inequality grew. There was a redistribution of economic resources from relatively poor people to socio-economic elites.

Yet many studies suggest that wars actually reduce economic inequality, because of some form of destructiveness, such as the demolition of physical capital, expropriation, or population decline (Piketty 2014: 147, Cosgel and Erene 2012: 321, Milanovic 2016: 56, Scheidel 2017: 86-112, 130-173). However, for late-medieval and early-modern times, the only war for which there is actual evidence of a significant inequality reduction is the Thirty Years' War (van Zanden 1995, Pfister 2020, Alfani et al. 2020). Indeed, there are at least four theoretical reasons that suggest that economic inequality could have declined during the Thirty Years' War: demolition of physical capital, expropriation and plundering, contraction of trade and commerce, and population decline.

First, due to battle action and troop movement large quantities of physical capital were destroyed. This included buildings, cropland, tools or other assets relevant for production. The damage to livestock was particularly pronounced, with losses of more than 90 percent in some regions. This destruction of physical capital potentially hit harder rich individuals who had lots of capital to lose

(van Zanden 1995: 646, Wilson 2009: 783-806, Schmidt 2018: 624).

Secondly, the war brought about large-scale expropriation and plundering (Asch 1997: 158). To cover their costs, armies extorted immense sums from occupied provinces or towns, threatening the population to burn down localities, plunder and apply crude force if they did not pay. The demanded amounts frequently went into the 100,000s of florin or thalers within weeks. Additionally, soldiers often plundered their surrounding areas. In contrast to resource extraction during ordinary wars, also the rich were expropriated in these ad hoc actions, as famously happened in Augsburg (Asch 1997: 152, 158, 177-178, Redlich 1959: 247-250, 254, Scheidel 2017: 339). Moreover, political authorities imposed "haircuts" on their creditors – another form of expropriation – and refused to pay interest during the war. This brought huge losses for the rich (Press 1991: 250, Scheidel 2017: 338, Schmidt 2018: 632).

Thirdly, the war led to a decline of trade and commerce, due to inflation, the loss of commercial ties, a lack of transport animals and widespread insecurity. Many businesses failed (Röck 1989: 941, Asch 1997: 156, Wilson 2009: 795-801). This potentially hit producers with high market quotas and wealthy merchants particularly hard (Röck 1989: 943, Schmidt 2018: 632).

The three named factors most likely had egalitarian consequences. However, these factors also interacted and entailed important spill-over effects, which shaped the overall economic and distributive impact of the Thirty Years' War. Physical destruction of capital goods, extortions, plundering, and the reduction of trade and commerce triggered an economic crisis of production, especially in agriculture.[19] This was made worse by harvest failures in 1635/36 (Outram 2001: 162). A slump in agricultural production brought food scarcity and famine, which in turn was excellent breeding ground for the return of plague between 1632 and 1640. Soldiers roving throughout Germany helped to diffuse the plague (Press 1991: 250, Asch 1997: 178-179). Famine and plague led, fourthly, to an enormous decline of the German population – of about 40 percent – that went far beyond the immediate impact of battle action (Outram 2001: 152-153, Wilson 2009: 790-792). Demographic decline possibly reduced inequality through two channels: first, it raised the land available per person, reduced the price of agricultural produce and real estate, increased marginal productivity of labour and thus the income of labour.[20] Second, death was socially selective. Poor people were more likely to suffer from famine and plague and had a higher probability of dying (Pfister 2020: 303-9, 318, Scheidel 2017: 335, Asch 1997: 180, Outram 2001: 165). This curtailed the share of poor people in the wealth distribution, which mechanically reduced inequality.

All the named factors have potentially egalitarian effects and it is, therefore, no surprise that the Thirty Years' War led to a reduction in inequality. The inequality-decline seems almost overexplained. However, the decisive point for the history of inequality is that the Thirty Years' War was an *exceptionally* destructive event, proportionally even more destructive than the two world

[19] For example, in 1633 Bamberg, the loss of draught animals, which were an essential capital good in agriculture, left the peasants to pull their own ploughs by hand. This had obvious negative consequences for productivity and food production (Wilson 2009: 801).

[20] Note that the egalitarian effect of a reduction of prices for agricultural goods is the result of poorer households spending a relatively larger share of their household budget on foodstuffs and fuels. If prices decline, as after the Thirty Years' War, purchasing power of the poor improves (Pfister 2020: 307-309). This enables the poor to buy, for example, real estate and experience upward social mobility.

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 War of Succession	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). Casualties p.a. indicate the average number of deaths per year of the respective war.

wars and definitely more destructive than any other war in early-modern Europe (Wilson 2009: 787 Ogilvie 1992: 439).[21] It does not represent the historical norm. This exceptionality of the Thirty Years' War can be seen clearly in the extraordinarily high number of people that were killed during the war compared with all other preindustrial wars for which data are currently available (see Table 1).

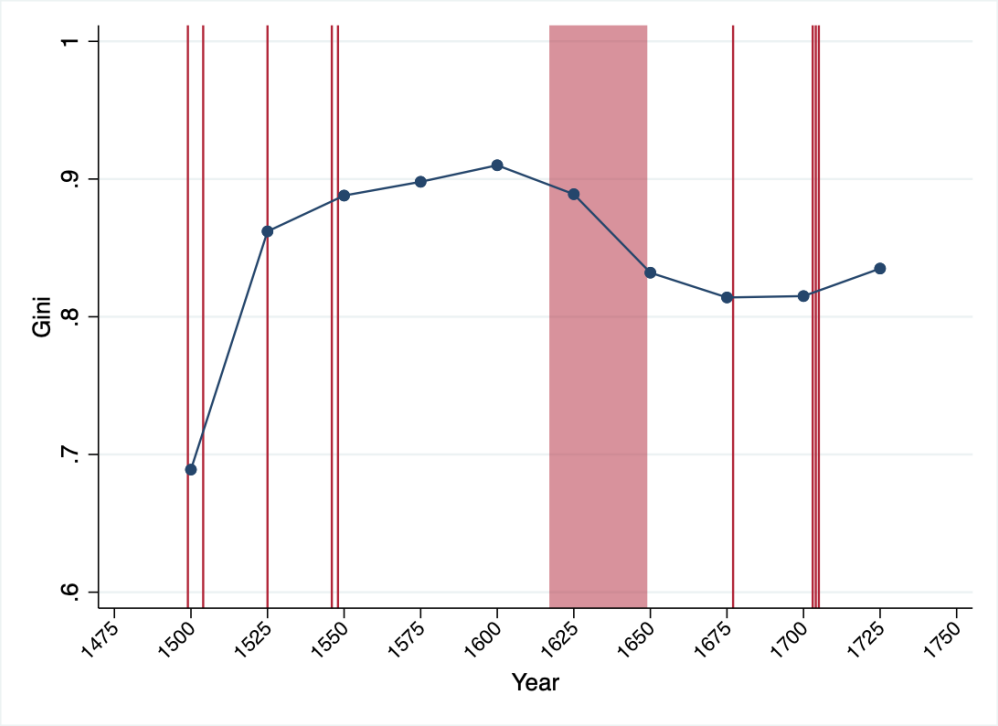
However, egalitarian destruction was only one of two countervailing effects of preindustrial conflicts on inequality, the other one being inegalitarian extraction. This is why the Thirty Years' War contrasts with ordinary premodern conflicts. The destruction caused by ordinary conflicts was relatively small (see Table 1) because geographically more limited, that is, without the spill-over effects that were characteristic of the Thirty Years' War.

Yet ordinary conflicts were quite extractive for the wider population, as described above. Throughout most of the period under study this extractive effect of conflicts was most likely the dominant one. Consequently, ordinary conflicts increased economic inequality, and the Thirty Years' War reduced it.

Case study evidence for the existence of these two countervailing effects comes from early modern Augsburg. Traditionally, scholars have employed the case of Augsburg to exemplify the inequality decline triggered by the Thirty Years' War (van Zanden 1995: 647, Scheidel 2017: 337). However,

[21] There has been a controversial debate among historians about how destructive the Thirty Years' War actually was. The first author to support statistically the hypothesis of a highly destructive Thirty Years' War was Franz (1961). His figures have subsequently been heavily criticized as exaggerated, ideologically biased and methodologically flawed. However, recent studies agree that Franz' main conclusion of a relatively destructive Thirty Years' War that killed approximately 40 percent of the German population, are, notwithstanding some methodological shortcomings, ultimately probably correct (Outram 2001, Wilson 2009: 790-792).

Figure 2: Inequality and conflicts in early-modern Augsburg



Notes: Own calculations based on data from Hartung (1898). Gini coefficients clustered around reference years. Red lines indicate years in which Augsburg was exposed to conflicts. The red box marks the time of the Thirty Years' War.

as can be seen from Figure 2, Augsburg was exposed to other conflicts during the early modern period: to battles that were part of the Swabian War, the War of the Succession of Landshut, the German Peasant's War, the Schmalkaldic War, the Franco-Dutch War and the War of the Spanish Succession.

Indeed, contrary to what happened during the Thirty Years' War, after all conflicts, inequality in Augsburg increased. This suggests that the relationship that ran from conflict exposure to inequality was not generally negative or positive. Most likely, there were two countervailing effects: egalitarian destruction dominated during the Thirty Years' War, and inegalitarian extraction during ordinary conflicts.

2.2 A Simple Model of Wealth Accumulation During Warfare

To fix ideas, the countervailing effects of warfare on economic inequality can be analysed through a simple non-structural model of capital accumulation. We are interested in the development of wealth (or capitalised income) K between two periods t and $t+1$:

$$K_t \rightarrow K_{t+1}.$$

Without warfare the accumulation process of K can be described as the following function:

$$K_{t+1} = [S + (1 + r) \times (1 - \delta) \times K_t]. \quad (1)$$

S are total savings (where $S = s \times Y$, s being the savings rate and Y income), r is the interest rate and δ the rate of depreciation. For simplicity we assume that everybody in the population has access to the same interest rate. We differentiate between the wealth of high-wealth holders K_H and low-wealth holders K_L, and their corresponding savings rates are S_H and S_L. Wealth accumulation in peace times can therefore be characterised as

$$K_{H,t+1} = [S_H + (1 + r) \times (1 - \delta) \times K_t] \quad (2)$$

and

$$K_{L,t+1} = [S_L + (1 + r) \times (1 - \delta) \times K_t]. \quad (3)$$

Warfare introduces two additional parameters in the process. First, the risk of being involved in a war increased the demand for protection of communities. Political authorities extracted economic resources in order to fulfil that protective task. er is the extraction rate by which capital K is reduced. However, a characteristic feature of resource extraction in preindustrial communities, and in particular of taxation, was its regressive character. For example, the reliance of authorities on wealth taxes with flat rates and exemptions for the rich, on consumption taxes that weighed heavier on poorer households, or on poll taxes that effectively asked a higher share of income or wealth the poorer the household, hit the poor strata much harder than the rich (Boelcke 1971: 171, Isenmann 2014: 521, Alfani and Di Tullio 2019). For example, the increase of the tax on beverages in Augsburg to cover the costs of the Schmalkaldic War in 1547, clearly hit the poor harder than the rich. This was the result of the poor typically spending a higher share of their household budget on basic consumption goods (see Roth 1928: 372, Dirlmeier 1978: 420). Moreover, rich individuals probably benefited disproportionately in the form of higher capital accumulation from the war-time expansion of credit at high interest rates. We differentiate therefore again between high-wealth holders K_H and low-wealth holders K_L, and to both groups apply different extraction rates $er_L > er_H$.

Secondly, military conflicts also caused destruction and many scholars have emphasised the importance of war-related destruction for explaining phases of inequality decline (see van Zanden 1995, Cosgel and Erene 2012: 321, Piketty 2014: 147, Milanovic 2016: 56, Scheidel 2017: 86-112, 130-173). Destruction reduced wealth through a multitude of factors, such as the demolition of capital, expropriation and plundering or economic decline: w represents the share of wealth that is reduced because of destruction. Historically, destruction was locally quite limited for ordinary conflicts, but much more pronounced during the Thirty Years' War. Poor strata of the population experienced comparatively less wealth reduction due to destruction. They simply had less capital goods that could be demolished or plundered than rich people, and potentially even benefited from increased mortality that brought about lower prices for foodstuffs and real estate, and higher wages due to

the scarcity of labour (van Zanden 1995: 646, Scheidel 2017: 335 Pfister 2020: 303-309). Moreover, if a war became so immensely expensive that governments defaulted on their debt - as happened for example during the Thirty Years' War - then the rich lost substantial parts of the money they typically lent to governments (Schmidt 2018: 632). In other words, while extraction was regressive, destruction was progressive. We differentiate therefore again between high- and low-wealth holders and assume $w_H > w_L$.

In sum, model parameters that change due to a military conflict are the extraction rate er and the destruction rate w. Through simple re-arrangement we can derive a function of wealth accumulation for high- and low-wealth owners during warfare:

$$K_{H,t+1} = (1 - er_H) \times (1 - w_H) \times [S_H + (1 + r) \times (1 - \delta) \times K_t] \tag{4}$$

and

$$K_{L,t+1} = (1 - er_L) \times (1 - w_H) \times [S_L + (1 + r) \times (1 - \delta) \times K_t]. \tag{5}$$

If we assume that all variables in the square brackets are fixed during warfare, then Equations 4 and 5 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, and therefore wealth inequality.

Equations 4 and 5 also suggest that depending on whether the conflict was small or large, either the effect of extraction or destruction prevailed. Low-destruction conflicts had a relatively higher er but a relatively lower w, and vice versa. This implies that in high-destruction conflicts w could offset the extractive effect of er.

These predictions can be simulated by plugging in reasonable values for the parameters of the model. For simplicity I assume that the population consists only of two groups, rich and poor, and I define the top 20 percent of the population as rich and all others as poor.[22]

The part of the model written in square brackets is the fixed part of the wealth accumulation process, which is assumed to be unaffected by warfare. As values for initial capital $K_{H,t}$ and $K_{H,t}$ I assume that rich households have on average eight times more wealth than poor households, which corresponds approximately to what has been found in recent studies (see Schaff 2020).

The measure of inequality is the ratio between the average wealth share between rich and poor households:

$$K_H / K_L.$$

This is a simple inequality indicator in the spirit of "Kuznets-shares" (see also Piketty 2020: 530).

[22]This definition is based on the observation that only the two top deciles of the population owned a consistently higher wealth share relative to their population share in preindustrial Germany (see Schaff 2020).

Since rich households have on average eight times the wealth of poor households, the initial inequality value is eight.

I assume that the interest rate was five percent, which corresponds approximately to the nominal interest rates on annuities found by Chilosi et al. (2018: 647) for the bulk of cases in late medieval and early modern Germany. The depreciation rate is set to zero because it does not change the dynamics we are interested in.

It is difficult to come up with precise estimates of income and savings rates for rich and poor parts of the population. Yet it is reasonable to assume that poor people had a lower savings rate than rich people, even in preindustrial times. The higher budget surplus of the rich could have been the result of a lower share of available income spent for consumption purposes or because rich had to pay fewer contributions (Alfani and Di Tullio 2019: 154-155). Yet it is at best possible to approximate roughly how much lower the savings rate of the poor in preindustrial Germany was.[23] I have made the conservative assumption that the poor saved on average one fourth less than the rich and for simplicity I assume that savings are one percent of a rich household's capital stock.[24] These parameters determine wealth accumulation in peace time.

The parameters that change wealth accumulation during wars are the extraction rate er and the rate of destruction w. It is again difficult to come up with absolute extraction and destruction rates, but to highlight the countervailing effects of the two factors it suffices to make an assumption about their relative importance. I assume that the extraction rate for rich people is half the extraction experienced by poor people, and that the destruction rate for the poor is half the destruction rate of the rich. Note that although these might seem arbitrary values, the direction of inequality development remains the same regardless of the actual values, as long as the extraction rate remains higher for poor people and the destruction rate remains higher for rich people.

Figure 3 shows how these assumptions affect the development of wealth inequality in a simple simulation. Three scenarios of inequality development have been simulated: peace time, a war where extraction dominates and a war where destruction dominates. Note that the peace-time scenario could also be interpreted as a war in which extraction and destruction exactly offset each other.

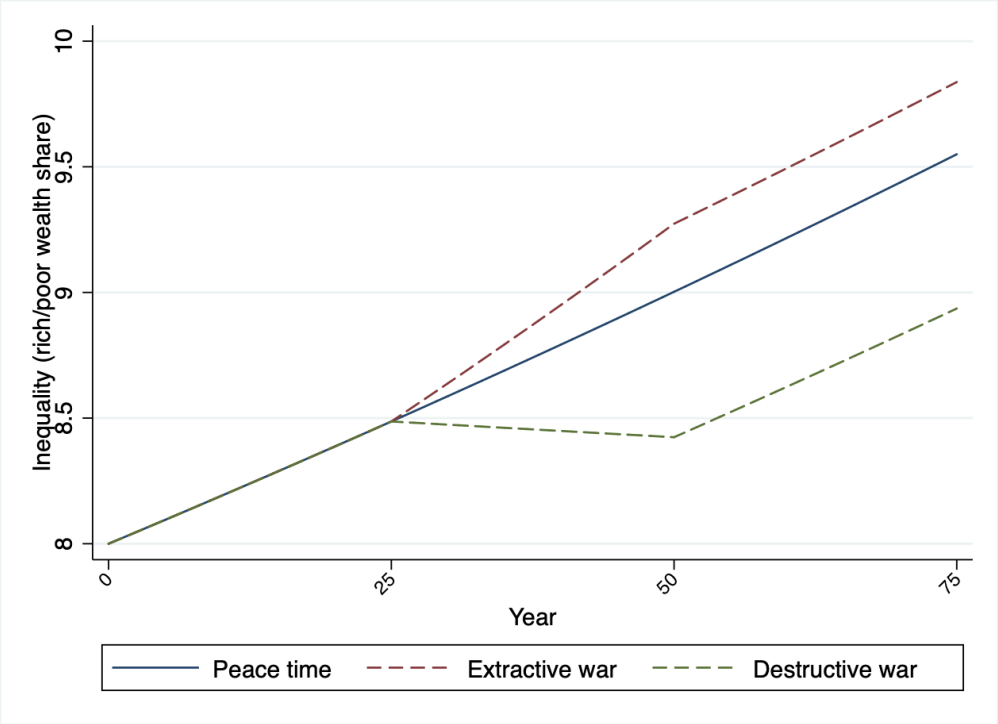
The simulation covers three periods of 25 years each. During each period the parameters take the same values. In the first period, there is no warfare and therefore neither extraction nor destruction result from conflict. Wealth accumulation and inequality move into the same direction in all three scenarios. Note that the increase of inequality is the result of the differential savings rates of rich

[23] Dirlmeier (1978: 517) shows that poor people too saved *some* money. Instead, the figures of Chilosi et al. (2018: 653) suggest that most investments in public annuities were actually made by wealthy individuals,and one might be tempted to assume based on their figures that the savings rate of the poor was practically zero. However, the definition of the poor in the simplified model is very wide: "poor" was who belonged to the bottom 80 percent of the population and the top 20 percent are defined as "the rich". It is therefore reasonable to assume that the poor, as defined in the model, had a savings rate that was on average smaller than those of the rich ($S_H > S_L$), but nevertheless positive.

[24] Note that the amount that rich households saved in relation to their *income* was probably much larger than what they saved in relation to their capital stock. However, for keeping the model simple, I calculate savings in relation to already accumulated capital. See Alfani and Di Tullio (2019: 152-165) for a similar approach.

and poor households, which make the rich move apart.

Figure 3: Simulated Wealth Inequality during Warfare



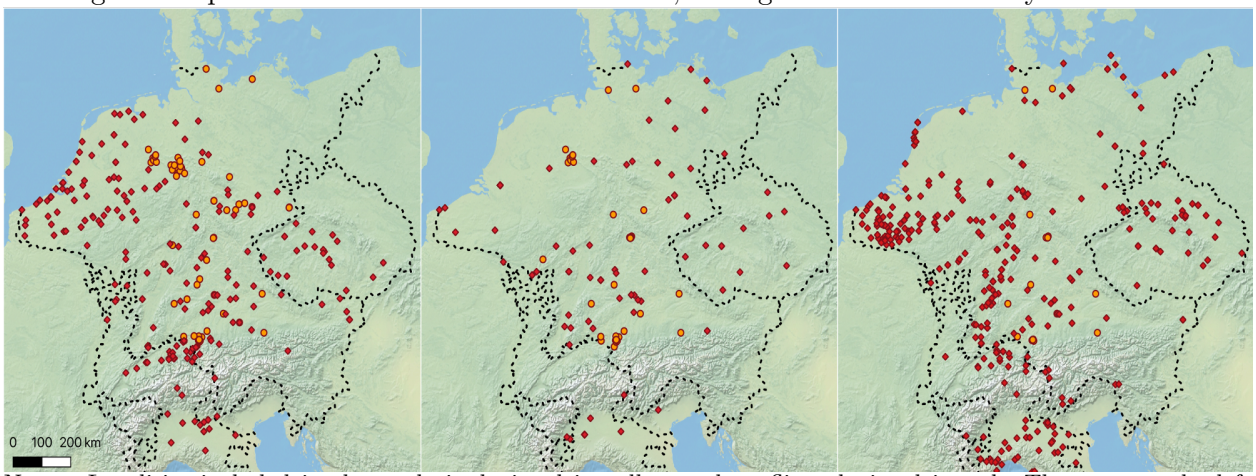
In period two, warfare comes into play in scenarios two and three. The effects of warfare are assumed to continue over the whole period. The peace-time scenario represents the counterfactual inequality-development, a situation without warfare. It can be clearly seen that when a war is comparatively more extractive than destructive, wealth inequality increases with respect to the counterfactual situation. This scenario of the extractive effect being large enough to increase wealth inequality represents the typical development during ordinary wars. Conversely, inequality is reduced with respect to the counterfactual scenario and even declines in absolute terms when the destructive effect is large enough to offset the extractive effect. The destructive-war scenario can be seen as historically analogous to the development of inequality during the Thirty Years' War in Germany. In the third period no warfare happens and inequality develops into the same direction in all three scenarios. However, the inequality levels of the three scenarios differ because the redistributive effect of warfare in the previous period has permanently changed the distribution of wealth in scenarios two and three.

The main insight of this simple microeconomic model is that when the relative importance of extraction and destruction shifts during warfare, so do inequality trends. This model is of course highly stylised, and we are not going to test it as such. However, the econometric reduced-form estimates presented below are in line with the predictions of the model. They suggest that ordinary low-destruction conflicts had a relatively high extraction rate. This could exacerbate inequality, but destruction could become so large that it offset the extractive effect.

3 Data

Figure 4 provides an overview of the localities (yellow dots) for which inequality data is available and the conflicts (red diamonds) that happened before, during and after the Thirty Years' War. They are the main parameters of the following analysis.

Figure 4: Spatial distribution of conflicts before, during and after the Thirty Years' War



Notes: Localities included in the analysis depicted in yellow and conflicts depicted in red. The map on the left shows conflicts that happened before the Thirty Years' War (1375-1617). The map in the middle shows conflicts that happened during the Thirty Years' War (1618-1648). The map on the right shows conflicts that happened after the Thirty Years' War (1649-1800). The maps show those localities that are included in the dataset during the respective time periods. Borders of the inner and wider Holy Roman Empire around 1545 from Volckart (2020).

The main data for economic inequality come from a novel panel dataset (Schaff 2020; see also Alfani et al. 2020). It contains data on the distribution of wealth and wealth inequality (expressed in Gini coefficients, ranging between zero and one, in steps of 25 years) for rural and urban communities within the borders of the inner part of the Holy Roman Empire (see Appendix for spatial and temporal distribution of communities). The data are based on tax registers or from wealth lists prepared to levy taxes. These were locally collected property taxes levied on households of citizens and non-citizens. Subject to the limitation discussed below, such tax registers give a fairly accurate picture of household-level wealth inequality in a locality. Similar sources have been used in other studies on the long-term development of preindustrial economic inequality, in Germany and other regions of Europe (Warde 2006, Alfani and Ryckbosch 2016, Alfani et al. 2020).

It should be noted that the data refer to the distribution of wealth (or capitalised income), while in modern times income inequality is usually the object of interest (see Milanovic 2005). Wealth inequality is interesting in itself, even in the industrial period (Saez and Zucman 2016, Scheve and Stasavage 2017). Yet it is also the best, and usually the only proxy for income inequality in preindustrial economies, where income mainly derived from land, since 80 to 90 percent of GDP derived from the agricultural sector (Lindert 2014: 8, Alfani 2015: 1062).

The wealth distributions that this study uses are biased towards the middle. Parts of the very top (such as nobles, clergy, officials and other privileged groups) of the distributions are sometimes but not always missing because of tax exemptions. It is difficult to quantify the exclusions, but some tentative approximations of the size of the excluded groups are possible. The nobility and clergy

made up approximately 1.5 percent of the total population in the sixteenth century and 0.5 percent in the first half of the nineteenth century (Saalfeld, 1980, p. 480). Most excluded households were probably part of these groups, but not all members of clergy and nobility were excluded from taxation.[25] Note that propertyless households are included in this dataset. However, there were more exemptions. For example, officials (who were occasionally part of the nobility as well) were sometimes exempt.[26] In consequence, I probably underestimate overall inequality. Moreover, the missing parts of the distribution are likely to bias my estimates of the impact of conflict exposure downwards. According to the conceptual framework, it should for example be highly paid town magistrates that benefited from inegalitarian resource extraction during warfare. The fact that these groups were sometimes missing from the wealth tax data probably leads me to underestimate the true impact of conflicts on economic inequality. My estimates should therefore be considered as being on the conservative side.

The dataset has a panel structure and includes 72 places for which at least two data points are available. Over the whole period of interest – from 1400 to 1800 – the dataset contains 436 observations. This might seem a small number and the econometric analysis ahead is limited by the current scarcity of observations. However, it is substantially more than those in recently published studies of the determinants of inequality in the preindustrial period.[27]

The potential cause of inequality in which this paper is interested in are military conflicts, or more precisely military conflicts in which at least one imperial estate or authorities of similar rank participated. As explained above, these conflicts are more likely to be exogenous from the perspective of an individual locality than for example bottom up revolts and make it easier to identify the effect of conflicts on inequality. Still, there are obvious endogeneity concerns, which will be addressed in the next section.

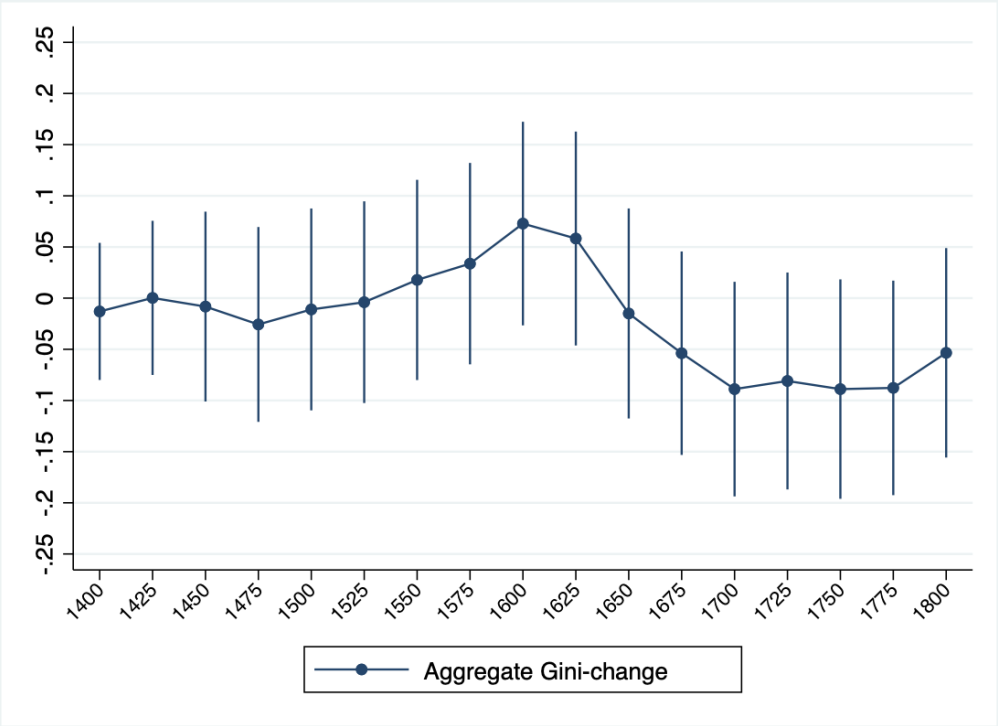
The relevant data for historical military conflicts come from several secondary source. The main sources are Dincecco and Onorato (2018) and Jaques (2007). These have been complemented with sources that cover more specifically the wider Holy Roman Empire (see for example Darby and Fullard 1970, Schubert 2011, Glasauer 2017; see Huning and Wahl (2018) for a similar approach). The dataset contains 687 conflicts (battles and sieges), making it the most comprehensive one for preindustrial Germany. For all conflicts the year and the settlement closest to an individual battle action were recorded, which made it possible to geo-reference them with high precision. Unfortunately, it is not possible to measure more precisely the intensity of a conflict, for example with the number of casualties. At the current state of research, the best systematic data available tell us whether there was a conflict or not. It is likely that the dataset does not record all conflicts – a general problem of studies of historical warfare, especially in the preindustrial period (Pinker 2011: 229) – 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

[25] See Alfani et al. (2020) for a more detailed discussion of the sources.

[26] However, also in the case of officials there were exceptions to the exemptions. For example, in Augsburg the Syndicus, one of the highest officials of the city, was not exempt from paying taxes on real estate (Hartung 1898: 169).

[27] Milanovic (2018) has 41 observations in his cross-country study of pre-modern societies.

Figure 5: 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 all Gini coefficients for the period of study on a full set of locality fixed effects and year dummies (reference year 1375). The coefficients on the year dummies represent aggregate Gini change (for a detailed discussion of the aggregation method see Clark (2005: 1322-1323) and Alfani et al. (2020: 82-84).

individual conflicts.

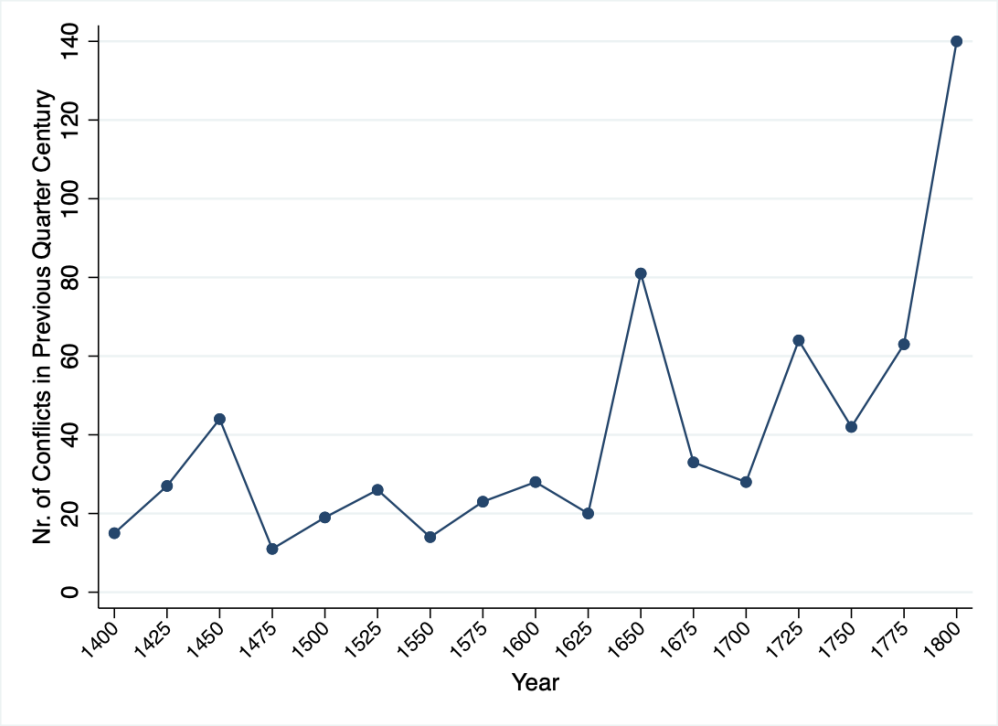
I have selected all conflicts from the dataset that fall within the wider Holy Roman Empire. This means that besides the core regions of the Empire, Germany and Austria, conflicts from Bohemia, the Low Countries and Switzerland, as well as from parts of Italy, France and Poland are considered. These regions were part of, or associated with, the Empire at some point in their history. Given the permeability of borders in the premodern era, it is reasonable to assume that a conflict, for example near Bautzen in Lusatia, had an impact on nearby Saxonian towns.

Figure 6 shows the number of conflicts per quarter century. There are two major spikes in the graph: the Thirty Years' War in the seventeenth century and the French Revolutionary and Napoleonic Wars at the end of the eighteenth century (see Dincecco and Onorato 2016: 266 for a similar pattern). The latter one comprises a seemingly exceptional number of conflicts but many of them actually happened at the margins of the Holy Roman Empire, especially in the Low Countries, Italy and Bohemia (see map above).

What do these figures tell us about bellicosity in preindustrial Germany? From the end of the fifteenth century onwards the Empire took measures to increase security and limit bellicosity (Schmidt 1999: 33).[28] In fact, "promoting peace between its members [was] the Empire's core function"

[28]Major measures of the Empire to increase security were the *Ewige Landfrieden* (Eternal Peace) of 1495 and the ordinance concerning the implementation of imperial law (*Reichsexekutionsordnung*) of 1555. These imperial acts

Figure 6: Number of conflicts per quarter-century, c. 1400-1800



Notes: The number of conflicts is counted over the period before each year t = 1400, 1425, . . . , 1800. For example, conflicts that occurred from 1501 to 1525 count for the number of conflicts in year 1525. The number of conflicts refers to all conflicts that were counted in this period in the wider Holy Roman Empire.

(Chilosi et al. 2018: 665). Nonetheless, the numerous recorded conflicts in the late Middle Ages and the early modern period suggest that the Empire has not been entirely effective in avoiding conflicts and even less in shielding itself from foreign interference (Ogilvie 1996: 122, Rödel 2003: 99, 109, Kroener 2013: 21).

In line with previous research on the topic (see Besley and Reynal-Querol 2014, Dincecco and Onorato 2016, 2018), I construct a dummy variable that equals one if there was at least one conflict between two inequality measurement points within a distance of 150 kilometres from a locality. This measure of conflict exposure of a locality is the main variable of interest in the analysis.

Using a dummy variable as an indicator of conflict exposure has the advantage of reducing the potential for error in the measurement of conflict frequency (Dincecco and Onorato 2016: 267). However, in order to address concerns about the reliability of this simple measure, a robustness check will be performed with alternative categorical variables that indicate whether a locality was exposed to more than one conflict. Moreover, the distance of 150 kilometres has been chosen empirically. 150 kilometres is approximately the threshold with the highest explanatory power. It

outlawed feuds among the nobility and the rest of the population, thus prohibiting the common man from using the legitimate use of force and initiating a separation between state and society in the Empire. Moreover, it legally banned conflicts between imperial estates. In the territories, the princes were responsible for the implementation of the *Landfrieden*. Conflicts between imperial estates were referred to the *Reichskammergericht* (Imperial Chamber Court), or, in case of large conflicts, to the *Reichskreise* (imperial circles) (Schulze 1986: 131-132, Schmidt 1999: 33-37, 104-105, Westphal 2018: 402-405).

is roughly in line with the maximum distance between conflicts and localities employed in recent studies.[29]

A natural question might be why a locality should have reacted to a conflict in a way that increased inequality if it was not directly hit by the conflict. It should have reacted because the actual combat operations were, as mentioned, by no means the only, or decisive, element of a conflict in relation to economic inequality. What mattered for a locality was that it had to protect its inhabitants when there was the *risk* of being attacked during a war (Glaeser and Shapiro 2002: 208-210, Dincecco and Onorato 2016: 260). That risk made the extended use of defence and bureaucratic infrastructure, and its permanent expansion necessary.

One might also ask whether the effect of a conflict on inequality nevertheless differed when a polity was actually one of the belligerents. Unfortunately, at the current state of research, this question is impossible to answer systematically. Often, a conflict party was composed of a union of several polities and every one of these union members could have been in further alliances with other polities, often smaller ones that did not have the capacity to maintain a sufficient number of armed forces. They then paid a larger polity for protection (Schilling 1998: 122, 162), which implies that conflict exposure to polity A might have increased the financial need of polity B, although B was not formally involved in the conflict. Unfortunately, reconstructing all alliances for all belligerents is simply beyond our current historical knowledge at such a geographical, temporal and systematic scale, not to speak of information about victors and losers for every battle.[30] Yet the analysis will provide case-study evidence that suggests that being among the victors or losers of a war did not have a significant impact on economic inequality.

Similarly, in conflicts that were part of a larger war, troop movements might have had effects similar to an actual battle. Because of a lack of systematic data, troop movements, too, cannot be considered. In substance, this analysis is limited to information about where and when a conflict took place. Information about conflicts is only a proxy for the totality of militant events during a war. However, it should be noted that when there was indirect exposure to militant events through troop movements which my conflict data do not adequately capture, a locality would appear as non-exposed while in reality it was exposed. This leads most likely to an underestimation of the true effect of conflicts, but not to an overestimation.

4 Empirical Analysis

4.1 Econometric Methodology

In this section I employ a two-way fixed effects panel estimator to measure the impact of conflict exposure on economic inequality. I estimate variants of the following specification:

$$I_{i,t} = \alpha_i + \pi_t + \beta C_{i,t-1} + \boldsymbol{\gamma'} \boldsymbol{X_{i,t}} + \epsilon_{i,t} \tag{6}$$

[29] Dincecco and Onorato (2016, 2018) geographically sorted conflicts into rectangular grid cells of 150 × 150 kilometers.

[30] See Scheidel (2017: 199) for the argument that premodern wars increased inequality among the victors and reduced it among the losers.

$I_{i,t}$ is wealth inequality of locality i in year t (t = 1400, 1425,... until 1600 and from 1675 until 1800).[31] The Gini coefficient is the main dependent variable, but also alternative inequality measures are employed based on wealth percentiles, such as the wealth share of the top 5% or the bottom 50% of the population. 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, that is, those that fall into the period of the Thirty Years' War, are considered separately because a different relationship between inequality and warfare is hypothesised during that period. $C_{i,t-1}$ is the measure of conflict exposure that takes the value one if there was a military conflict within 150 kilometres of locality i over the previous period.[32] The threshold of 150 kilometres has been chosen empirically (see below). α_i are a full set of locality fixed effects and π_t are time fixed effects (years). $\boldsymbol{X_{i,t}}$ 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: 64-68, Schneider 2020). 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. This is a more conservative approach compared to similar studies where standard errors have instead been clustered at the regional level (see Dincecco and Onorato 2016, Cantoni et al. 2018: 2085). In the Appendix some of the baseline results are reported with spatial autocorrelation-adjusted standard errors, as discussed in Conley (1999).

4.2 Endogeneity Concerns

In order to address concerns about endogeneity, I account for potential omitted variable bias and reverse causality through several strategies. The modelling approach accounts for unobserved factors that might have 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 of such characteristics have been considered relevant for explaining differences in inequality *levels* between localities, such as inheritance institutions (Alfani 2015: 1077-1078, Wegge 2020, Bartels et al. 2020), city status (Alfani and Ammannati 2017: 1097), land-tenure systems (Sokoloff and Engerman 2000, Banerjee and Iyer 2005: 1197, Malinowski and van Zanden 2017: 378-379) or the overarching social order of feudal societies (Piketty 2020).[33] Time fixed effects account for shocks that might have had an impact on inequality in all localities. For example, an important issue might have been events at the imperial level, such as constitutional changes. Moreover, general economic trends or a changing frequency of conflicts are captured by the time fixed effects.

To mitigate further omitted variable bias, I account for several observable characteristics that have been considered alternative explanations for inequality change. I include two demographic vari-

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

[32] In order to determine as precisely as possible whether a locality was treated or not in a period, I took as a cutoff between periods the actual year of inequality measurement, not the clustered reference year.

[33] The basic social order in premodern Germany consisted of four different estates or groups: clergy, nobility, burghers in towns and peasants. Belonging to these groups implied different legal privileges and obligations. For example, one's estate determined whether an individual was ruled or was a ruler himself, which profession one could follow, whom one could marry and what one could consume. To which group an individual belonged was determined by birth and generally fixed for lifetime. This *legal* inequality might have implied an underlying economic inequality in premodern times (see Volckart 1998: 52, 74, Scott 2002: 27-55).

ables, the log-population size of a locality and the occurrence of epidemics. It has been argued that demographic expansion played an important role in determining preindustrial inequality growth, and that vice versa population decline led to lower inequality (Alfani and Ryckbosch 2016: 32, Pfister 2020).[34] Moreover, the population size of a locality is a frequently used proxy for economic development (see Dittmar 2011: 1136), which has often been considered an important driver of inequality (Kuznets 1955, van Zanden 1995: 649, 656-658, Deaton 2015: 1-5). Additionally, conflict exposure might have increased the population in premodern towns because these were "safe harbours" to which people migrated in case of warfare (Dincecco and Onorato 2016: 261-263). This might have increased inequality because migrants were often relatively poor (Alfani 2015: 1089-1090). Importantly, such inequality growth as a result of conflict-induced migration could potentially be an alternative mechanism to the one I propose. Finally, military conflicts often spread epidemics, which had a straight-forward impact on the population size and possibly on inequality (Alfani and Ammannati 2017: 1090, Alfani and Murphy 2017: 333-334, Alfani et al. 2020: 30-32).

I also include several institutional variables. A body of literature argues that more oligarchic, closed or less participative forms of government promoted inequality compared to more participative, open or republican institutions (Boix 2003, Ogilvie 2007: 662, Stasavage 2011: 111-117, Minns et al. 2020: 603). To account for this possibility I include a variable that indicates whether a community had a participative government.[35] Also, several studies have claimed that differences in culture or ideology can explain inequality differences. Specifically, the introduction of the Protestant Reformation might have had an impact on economic inequality, because of differences in how redistributive Protestant and Catholic town governments were (Alesina and Giuliano 2010, Piketty 2020, Basten and Betz 2013; see also Dittmar and Meisenzahl 2020: 6). I therefore include a variable that indicates whether a community introduced the Protestant Reformation. I also include a variable that indicates the log-distance of a town to its nearest university. Universities might have had an impact on inequality for two reasons: first, higher human capital, created by universities but to which only few selected individuals had access, might have increased economic inequality (see van Zanden 1995: 658-661, Dittmar 2019). Secondly, universities provided political elites with the trained officials that were needed to extract more resources from their subjects, which potentially increased inequality (see Ogilvie 1992: 426, Ertman 1997: 224). Additionally, a variable is included that indicates the log of the taxes a community's imperial estate had to pay to the Empire. Since imperial taxation is considered highly correlated with territorial taxation, this variable accounts for the possibility that the demands for taxes of the Empire (e.g. for external defence) or of territorial rulers could have increased inequality (see Schilling 1998: 118, 121, Schulze 1978: 256-261).

Ultimately I include several time-invariant geographic controls, all interacted with time-dummies. Agricultural potential captures possible inequality growth due to productivity increases in agriculture (Allen 1992: 283-302). A locality's distance to a navigable river and seaside location proxy

[34] 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 (Rodepeter 1998, Alfani and Ryckbosch 2016: 151, Pfister 2020).

[35] This variable is coded analogous to Wahl (2019). See the Appendix for details.

for market access. This accounts for the possibility that inequality is attributable to the expansion of markets and long-distance trade (van Zanden 1995: 652, Puga and Trefler 2014: 755, van Bavel 2016: 261-263).

Consistent with the literature (Karaman and Pamuk 2013, Dincecco and Onorato 2016, 2018), my first econometric strategy exploits the plausibly exogenous occurrence of battle action from the perspective of an individual town. 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 or the Elector of Brandenburg), the Holy Roman Emperor, the King of France, Swiss Cantons or Italian states. In this way, those types of conflicts that might have been the outcome of economic inequality (see Esteban and Ray 1999, Blattman and Miguel 2010, Baten and Mumme 2013) are generally excluded from the analysis.

Table 2 gives an overview of all wars that are part of the dataset and the main causes of these wars that the historical literature has identified. It shows that the conflicts covered by the dataset most likely broke out due to reasons that were unrelated to the variable this paper is mainly interested in. Wars have been classified as plausibly exogenous from the perspective of an individual town, as ambiguous, or as likely endogenous, depending on whether inequality was among the factors that the historical literature considers relevant for explaining the outbreak of a war. Note that classifying wars as endogenous or exogenous based on a qualitative assessment of their historical reasons is similar to the "narrative approach" used in time-series econometrics to establish causal effects (see Cloyne 2013, Alesina et al. 2015).

There were many different reasons for warfare to start. A number of wars broke out because of disputes over the hereditary succession of a ruler, such as in Guelders, Spain, Poland or Austria. Several conflicts broke out over the question of which was the "right" religious confession. Others broke out because of a ruler's quest for gaining territory, such as Louis XIV's attempts to round off the borders of France at the expense of the Low Countries and the Empire in the second part of the seventeenth century.

Table 2: Wars and their causes

Date	War	Main Causes	Plausibly Exogenous
1351-1401	Florentine-Milanese Wars	Sphere of influence (Jaques 2007: 918)	YES
1369-1389	Caroline War (100-Years' War)	Territorial gain (Wagner 2006)	YES
1371-1379	1. Gueldarian Succession War	Hereditary succession (Sternberg 2004: 111)	YES
1376-1377	War of the Swabian League	Independence (Isenmann 2014: 322)	YES
1386	Sempach War	Independence, territorial gain (Marchal 1987)	YES
1387	Padua-Verona War	Territorial gain (Rogers 2010)	YES
1387-1389	War of the Cities	Power balance, independence (Schubert 2011)	YES
1388-1389	Great Dortmund Feud	Independence (Garnier 2001: 26)	YES
1397	Battle of Kleverhamm	Payment of levy (von Schaumburg 1861)	YES
1400	Bergtheim War	Independence (Leng 2012)	YES

Date	War	Main Causes	Plausibly Exogenous
1401-1429	Appenzell Wars	Independence, payment of levy (Burmeister 2001)	YES
1410-1413	Austrian War	Strategic routes, territorial gain (Brandstätter 2009)	YES
1415-1453	Lancastrian War (100-Years' War)	Hereditary succession (Wagner 2006)	YES
1419-1434	Hussite Wars	Confessional dispute (Moraw 1989: 372-378)	YES
1420-1422	Great War of the Lords	Territorial gain (Glasauer 2017)	YES
1422	Arbedo War	Territorial gain (Jaques 2007: 63)	YES
1423-1444	2. Gueldarian Succession War	Hereditary succession (Ferber 1863: 14-15)	YES
1423-1454	Venetian-Milanese Wars	Sphere of influence (Jaques 2007: 163)	YES
1427	Mainz-Hesse War	Sphere of influence, territorial gain (Demandt 1981: 196)	YES
1439-1450	Old Zurich War	Hereditary succession (Illi 2015)	YES
1444-1448	Soest Feud	Territorial gain, independence (Heimann 2009)	YES
1446-1451	Saxon Fratricidal War	Hereditary Succession (Löbe 1895: 157-161)	YES
1447-1450	Milanese War of Succession	Hereditary succession (Jaques 2007: 200)	YES
1449-1450	1. Margrave War	Territorial gain, revenues resource extraction (Zeilinger 2015)	YES
1453-1474	Franco-Burgundian War	Territorial gain (Jaques 2007: 385)	YES
1459-1463	Bavarian War	Sphere of influence (Seyboth 2014)	YES
1460-1462	Baden-Palatine War	Ecclesiastical succession (Sprenger 1999)	YES
1465-1468	Straelen War	Territorial gain (Heinrichs 1910: 78-87)	YES
1473-1480	Neuss War	Territorial gain (Schmitz 1895: 6-10)	YES
1474-1477	Burgundian Wars	Territorial gain, sphere of influence (Sieber-Lehmann 2011)	YES
1477-1482	Burgundian War of Succession	Hereditary Succession (Hollegger 2005: 42-60)	YES
1477-1487	Austrian–Hungarian War	Political alliance (Fessler 1867: 120)	YES
1494-1498	Italian War of Charles VIII.	Territorial gain, power balance (Alfani 2013: 13-16)	YES
1499	Swabian War	Sphere of influence, strategic routes (Niederstätter 2011)	YES
1503-1505	Landshut Succession War	Hereditary succession (Schmidt 2004)	YES
1508-1516	War of League of Cambrai	Power balance (Clodfelter 2008: 8)	YES
1519-1523	Hildesheim Diocesan Feud	Territorial gain (Brüdermann 2000)	YES
1522-1523	Knights' Revolt	Privilege restoration (Schilling 1994: 131)	YES
1524-1525	German Peasants' War	Privilege restoration, social discontent (Schilling 1994: 140-147)	NO
1526-1533	1. Habsburg–Ottoman War (Hungary)	Hereditary succession (Schilling 1994: 224)	YES
1531	Second War of Kappel	Confessional dispute (Clodfelter 2008: 12)	YES
1534-1535	Muenster Rebellion	Confessional dispute, ecclesiastical control (Schilling 1994: 175)	YES
1542	Brunswick War	Confessional dispute, political alliance (Schilling 1994: 227)	YES

Date	War	Main Causes	Plausibly Exogenous
1543	3. Gueldarian Succession War	Hereditary succession (Schilling 1994: 228)	YES
1546-1547	Schmalkaldic War	Confessional dispute (Schilling 1994: 213, 229)	YES
1551-1559	Italian War of 1551-59	Territorial gain (Mallett and Shaw 2012: 252-254)	YES
1552-1554	2. Margrave War	Territorial gain (Schilling 1994: 239)	YES
1567	Grumbach Feud	Imperial status (Schilling 1994: 246-247)	YES
1568-1648	Dutch War of Independence	Independence, confessional dispute (Burkhardt 1992: 64-68)	YES
1568-1598	French Wars of Religion	Confessional dispute (Clodfelter 2008: 12-13)	YES
1583-1588	Cologne War	Ecclesiastical control (Schilling 1994: 280)	YES
1618-1623	Bohemian-Palatinate War (30-Years' War)	Confessional dispute, territorial gain (Schilling 1994: 414-415)	YES
1618-1639	Bündner Wirren (30-Years' War)	Strategic routes, confessional dispute (Färber 2011)	YES
1623-1629	Danish-Low.-Saxony War (30-Years' War)	Confessional dispute, territorial gain (Press 1991: 201-202)	YES
1628-1631	Mantuan Succession War (30-Years' War)	Hereditary succession, sphere of influence (Press 1991: 214)	YES
1630-1635	Swedish War (30-Years' War)	Sphere of influence, territorial gain, political alliance (Press 1991: 218-220)	YES
1635-1648	Swedish-French War (30-Years' War)	Power balance, political alliance (Schilling 1994: 447)	YES
1648-1659	Franco-Spanish War	Power balance (Press 1991: 214, 387)	YES
1652-1654	1. Dutch War	Trade dispute (Press 1991: 390)	YES
1653	Swiss Peasant War	Economic crisis (Suter 2010)	?
1656	1. Villmergen War	Confessional dispute (Press 1991: 388)	YES
1667-1668	War of Devolution	Territorial gain, power balance (Schilling 1998: 215)	YES
1672-1679	Franco-Dutch War	Territorial gain, power balance (Schilling 1998: 215)	YES
1683-1684	War of the Reunions	Territorial gain (Schilling 1998: 232)	YES
1683-1699	Great Turkish War	Confessional dispute, territorial gain (Schilling 1998: 245-247)	YES
1684	French Conquest of Luxembourg	Territorial gain (Clodfelter 2008: 47)	YES
1688-1698	Palatine Succession War	Hereditary succession, territorial gain (Schilling 1998: 168, 252-255)	YES
1700-1721	Great Northern War	Strategic routes, power balance (Schilling 1998: 275)	YES
1701-1714	Spanish Succession War	Hereditary succession, power balance (Schilling 1998: 214)	YES
1712	2. Villmergen War	Confessional dispute, sphere of influence (Press 1991: 389)	YES
1733-1735	Polish Succession War	Hereditary succession (Clodfelter 2008: 77)	YES
1740-1748	Austrian Succession War	Hereditary succession, territorial gain, power balance (Schilling 1998: 289-290)	YES

Date	War	Main Causes	Plausibly Exogenous
1756-1763	7-Years War	Territorial gain, power balance, trade dispute (Schilling 1998: 450-452)	YES
1768-1774	Confederation of the Bar Rising	Sphere of influence (Clodfelter 2008: 97)	YES
1789-1790	Belgian Revolt	Restitution political order (Clodfelter 2008: 90)	YES
1792-1798	War of 1. Coalition	Restitution political order, territorial gain (Schmidt 2009: 236-237)	YES
1794	Polish Rebellion	Independence (Clodfelter 2008: 100)	YES
1798-1801	War of 2. Coalition	Restitution political order (Schmidt 2009: 241)	YES

Notes: "Confessional dispute" means for example a dispute over being Protestant or Catholic. "Ecclesiastical control" means for example a dispute over which church holds a bishop office. "Ecclesiastical succession" means for example a dispute over who succeeds a bishop. "Economic crisis" means for example a dispute because of a general economic depression. "Sphere of influence" means for example a dispute over who is the dominate power in a contested region such as Italy. "Hereditary succession" means for example a dispute because a king dies without male heir. "Imperial status" means for example a dispute over having elector dignity. "Independence" means for example a dispute over political indolence of a region from a monarch'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 in Europe. "Restitution political order" means for example a dispute to reinstall a discharged king. "Privilege restoration" means for example a dispute to obtain back the old privilege of legitimately using force. "Revenues resource extraction" means for example a dispute over revenues from a monopoly. "Social discontent" means for example a dispute because of impoverishment. "Strategic routes" means for example a dispute over the control of the Baltic Sea. "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. In the column that indicates plausible exogeneity "YES" means that the war was clearly exogenous, "NO" likely endogeneity and a question mark means that it is ambiguous whether the war was endogenous to inequality.

Only in two cases did wars actually break out because of reasons that were not plausibly exogenous from the perspective of an individual town. The Swiss Peasant War (1653) was an ambiguous case: the main motives behind this war were a post-war depression in agriculture and a high inflation due to coin debasement of a number of towns (Suter 2010). Issues of economic inequality might have played in, even though not clearly identified as relevant by the historical literature. Instead, the German Peasants' War (1524-1525) was likely endogenous. Discontent with the impoverishment of lower peasant classes played a role in the outbreak of this war (Schilling 1994: 140-148). However, in 75 out of 77 cases inequality clearly did not play a role in the outbreak of the wars according to the historical literature, which made them plausibly exogenous events from the perspective of individual towns.

One might be wondering whether wars breaking out because of "territorial gain" or "strategic routes" might be correlated with material resources and thus inequality. For example, a prince might want to take over a city because of the riches of its merchants. While this possibility cannot be entirely excluded, it is unlikely to drive the results. My conflict variable does not indicate whether a community participated in a war, i.e. was actually attacked, which could be endogenous, but only whether it was within a fairly large radius of the battle action. This contains the possibility of material characteristics of communities determining their treatment status reasonably well. Moreover, I control for population size - a proxy for economic development -

which further reduces the possibility that local prosperity confounds the war-inequality relationship.

To further address reverse causality, I run a placebo test, where the outcome variable is regressed on the first and second lead of the conflict variable. This is done to evaluate whether future conflicts predict past inequality. Moreover, I test whether inequality helps predict conflict exposure.

In a later section I move to a difference-in-differences research design for the time of the Schmalkaldic War (1546-1547), to establish a more robust causal relationship between conflicts and economic inequality. Such a research design is not suitable for the whole period of study because treatment occurs at different times, which makes it impossible to define a control group for the post-treatment period. In such a set-up, the two-way fixed effects estimator is the most suitable alternative (Goodman-Bacon 2019: 2, Cameron and Trivedi 2005: 738).

4.3 Baseline Results

The main hypothesis of this section is that there existed two countervailing effects of conflicts on inequality – destruction and extraction – and that throughout most of the period under study the extractive effect was larger. Yet the destructive effect on inequality likely prevailed during the Thirty Years' War.

Table 3: Summary statistics of main variables

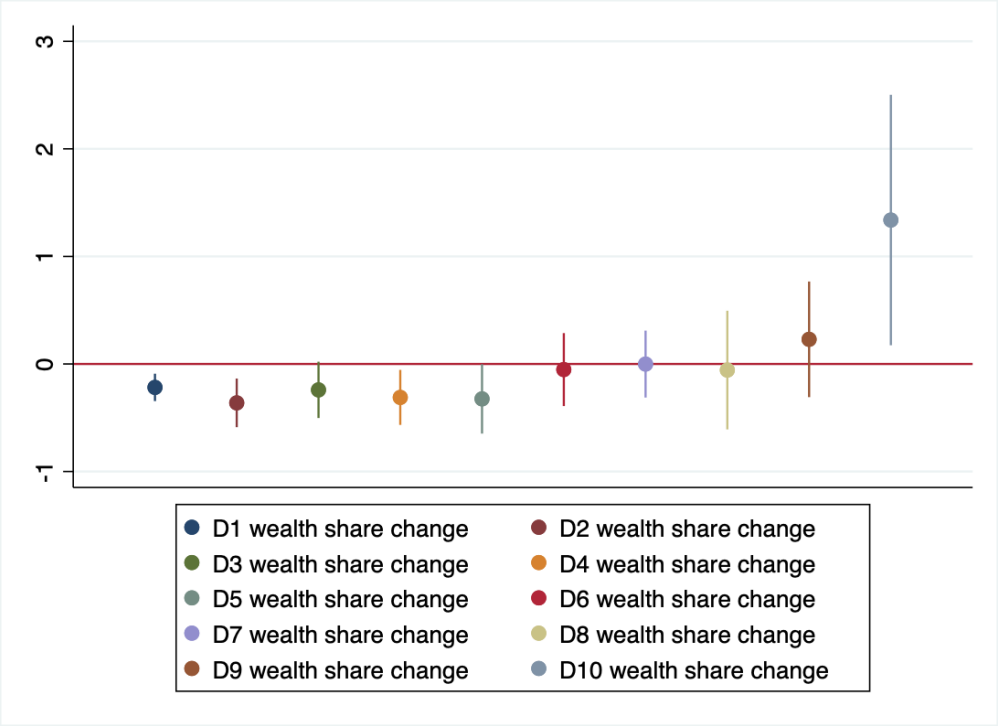
	(1)	(2)	(3)
Variables	N	Mean	Std. deviation
Gini	436	0.561	0.144
D1 wealth share	436	0.721	0.827
D2 wealth share	436	1.640	1.358
D3 wealth share	436	2.596	1.730
D4 wealth share	436	3.636	2.074
D5 wealth share	436	4.924	2.388
D6 wealth share	436	6.480	2.844
D7 wealth share	436	8.530	3.144
D8 wealth share	436	11.76	3.512
D9 wealth share	436	17.70	3.767
D10 wealth share	436	42.02	15.38
Top 5%	436	28.28	14.21
Bottom 50%	436	13.52	7.481
Conflict exposure	436	0.706	0.456

Notes: "D10" is equivalent to the top 10% of the population.

Table 3 reports summary statistics for the inequality and conflict exposure indicators employed in this section. Before analysing how overall economic inequality has changed when a locality was exposed to a conflict, it is worth observing how the shape of the wealth distribution changed when a locality was exposed to a conflict.

In Figure 7 the coefficients of the simplest specification, which includes only locality and time fixed

Figure 7: Conflict exposure and wealth share change during ordinary conflicts



effects, and the main variable of interest, conflict exposure, are plotted. As dependent variables the wealth shares of the deciles of the population have been taken. The plotted coefficients are the point estimates on the conflict exposure indicator. In other words, the graph shows how the entire wealth distribution changed in localities that were exposed to a conflict, in the periods 1400 to 1600 and 1675 to 1800.

In line with the conceptual framework introduced above, the wealth shares of the lowest five deciles of the population did decrease significantly. The upper-middle class of the wealth distribution (deciles 6 to 8) did not experience significant variation. The ninth decile experiences some wealth share increase, but only the top 10% (or D10) of the wealth distribution did experience a significant increase of its wealth share. Note that the growing confidence intervals are the result of a simple scale effect: higher deciles possess larger shares of the total wealth in a locality and thus experience higher variation.

Table 4 reports the baseline results for the impact of conflict exposure within 150 kilometres of a locality and inequality. All estimates include locality and time fixed effects (unless otherwise indicated), which account for time-invariant, locality-specific features and for shocks that influence all localities.

Columns 1 to 4 of Table 4 employ the Gini coefficient as measure of inequality. Column 1 shows that the occurrence of a conflict was associated with an increase in inequality by 0.021 Gini points, significant at the one-percent level.

Table 4: Conflict exposure and economic inequality: baseline results

	(1) Gini	(2) Gini	(3) Gini	(4) Gini	(5) Bot. 50%	(6) Top 5%	(7) Gini	(8) Gini	(9) Gini	(10) Gini	(11) Gini	(12) Gini
	1400-1600 & 1675-1800						1400-1800	1400-1600	1625-1650	1625-1650	1675-1800	1400-1800
Conflict exposure	0.021***	0.013	0.044*	0.028***	-1.456***	1.069**	0.014*	0.013*	-0.012	-0.054***	0.044**	0.015**
	(0.007)	(0.010)	(0.022)	(0.008)	(0.584)	(0.064)	(0.008)	(0.006)	(0.021)	(0.012)	(0.020)	(0.007)
Conflict exposure (1. lag)				0.010								
				(0.007)								
Conflict exposure×subperiod 1625-1650							-0.011					
							(0.021)					
Conflict exposure×subperiod 1675-1800							0.032					
							(0.021)					
Locality FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	NO	YES	YES
Locality time-trends	NO	YES	YES	NO	NO	NO	NO	NO	NO	NO	NO	NO
Observations	380	373	103	314	380	380	436	288	56	56	92	436
Adjusted R^2	0.363	0.498	0.556	0.405	0.242	0.143	0.330	0.227	0.312	0.083	0.084	0.328

Notes: Estimation method is OLS. Standard errors clustered at locality level in parentheses. *** p<0.01, ** p<0.05, * p<0.1. In Column 7 the conflict exposure measure has been interacted with a dummy that indicates the subperiod 1400 to 1600.

One could imagine adding locality-specific time trends to the baseline specification. This is done in Column 2, which yields an estimate on the conflict exposure variable that is just not significant. It should be noted that the panel includes 72 localities, that is, many localities with relatively few observations over time per unit. Including locality-specific time trends amounts to adding 72 variables, which seem clearly too many considering the relatively small number of observations and hence reduces the precision of the estimate of interest. Moreover, many of the units are part of the dataset for few periods only. This makes it more likely that time trends capture Gini-change that is in reality attributable to the conflict exposure measure (see Angrist and Pischke 2009: 239). For that reason, in Column 3, I have only kept those units that are part of the dataset for at least 300 years. Using that restricted dataset, the main variable of interest is significant even when locality-specific time trends are included. This specification addresses also, at least to some extent, concerns about panel non-response, potentially introduced by selective attrition (Lechner et al. 2016, Schneider 2020).[36] Only one locality is part of the dataset over the whole period of study and it is not possible to build a meaningful balanced panel. However, the specification estimated on the limited dataset indicates that it is unlikely that the potential bias introduced by the unbalanced structure of the panel leads to an overestimation of the effect of warfare on inequality.

In Column 4 the first lag of the conflict exposure variable has been added. One could imagine that conflicts which happened further in the past than one period might have had an impact on inequality. The coefficient points into the right direction but is just not significant. The main variable of interest remains highly significant and positive though. The downside of including a lagged variable is that one loses many observations of an already small dataset. Two information criteria tests have been performed to determine the right lag length. The tests weigh the gain of explanatory power through adding lagged variables against the loss of observations (Kennedy 2008: 95). The Akaike Information Criteria and the Bayesian Schwarz Information Criteria tests both suggest that the first lag of the conflict exposure variable should not be included, which is why it is absent in all further specifications.

Figure 7 has already shown that the wealth distribution became more unequal at both ends when a locality was exposed to a conflict. Column 5 of Table 4 shows that the wealth share of the poorest 50% of the population decreased by 1.456 percent when there was a conflict. This is quite a substantial reduction considering the low share of wealth owned by the Bottom 50% (see Table 3). Column 6 shows that the Top 5% of the population gained at the same time 1.069 percent of total wealth. The results suggest that exposure to ordinary conflicts redistributed resources from rich to poor households.

Overall, the results in Figure 7 and Table 4 show evidence for a positive and significant effect of conflict exposure on inequality in preindustrial Germany. This relationship holds when different measures of inequality are employed. Conflicts reduced the wealth share of the poor substantially and increased the wealth share of the rich. This result is in line with the conceptual framework introduced above, which argues that economic resources were extracted regressively in reaction to

[36] Note that the availability of inequality data depends on the survival and availability of relevant archival documents. It is therefore reasonable to assume that attrition is random and uncorrelated with the error term of the model.

conflict exposure.

What was the impact of conflicts on economic inequality during the time of the Thirty Years' War? Column 7 includes also the data for the period 1625 to 1650, approximately the period of the war. Conflict exposure is interacted with two dummy variables: the first one indicates the period from 1400 to 1600 and the third one from 1675 to 1800, the two sub-period where a positive conflict-inequality relationship is hypothesised. The second dummy indicates the period 1625 to 1650. Unsurprisingly, the coefficient on the first interaction term is positive and significant. The second coefficient is negative. This result too reflects what one would expect – that the destructive effect of warfare was larger than the extractive effect during the Thirty Years' War – but the coefficient is not significant. Still, this specification indicates that the warfare-inequality relationship during the Thirty Years' War was significantly different from the relationship during the previous period. The coefficient on the dummy that indicates the period 1675 to 1800 is positive and sizeable but just not significant.

Columns 8 to 11 show a similar picture. The dataset has been split into three subperiods, covering the time before, during and after the Thirty Years' War. The periods covered in Columns 8 and 11 show, as expected, a significant and positive conflict-inequality relationship. The relationship is even stronger towards the end of the early modern period. It probably reflects the acceleration of the Military Revolution: the increased firepower of weapons made it necessary to increase the size of armed forces, to improve equipment and to strengthen fortifications. When military technology advanced political authorities had to extract an increasing amount of resources to pay for defence and bureaucratic infrastructures and the direct costs of war (Kroener 2013: 25, 33, 32–44). This happened via inegalitarian channels and made the extractive effect of warfare outweigh even more than before the destructive effect on inequality.

Instead, the coefficient in Column 9 points towards a negative but insignificant conflict-inequality relationship during the time of the Thirty Years' War. Still, this specification indicates that the warfare-inequality relationship during that war was significantly different from the relationship before and after it in Columns 8 and 11. Moreover, this insignificant result does not indicate that the Thirty Years' War had no negative effect on inequality. It was quite the opposite, as Column 10 shows, where the time fixed effects have been left out. In that specification conflicts were related significantly negatively to inequality. In other words, inequality decreased substantially during the Thirty Years' War, but warfare does not seem to capture this decline, unless one allows the conflict exposure indicator to capture some of the variation in inequality that all localities experienced. This result is consistent with the hypothesis that the Thirty Years' War had a different impact on economic inequality compared to ordinary conflicts because of its exceptional spill-over effects: physical destruction, plundering and the reduction of trade and commerce led to an economic crisis, which brought about food scarcity, famine and created the breeding ground for widespread famine, plague and death. In consequence, the Thirty Years' War had a globally destructive impact that went far beyond the immediate destruction of battle action, to which ordinary conflicts were instead limited. Note that the results in Columns 9 and 10 might also reflect that the distance within which a conflict had an impact on economic inequality increased during the Thirty Years' War. A later

section provides some tentative evidence for this hypothesis.

One might wonder how the conflict-inequality relationship looked like over the whole period 1400 to 1800. The coefficient in Column 12 shows that it was positive and significant but, unsurprisingly, slightly smaller compared to the estimate in Column 1. This suggests that notwithstanding the exceptionally negative effect of the Thirty Years' War, there was a generally positive relationship that ran from conflict exposure to economic inequality in preindustrial Germany.

How economically relevant is the effect of conflict exposure for explaining variation in inequality? One possibility for assessing the magnitude of the estimated coefficient is to compare it to the standard deviation of the dependent variable. A one-standard deviation increase in conflict exposure (0.466) is associated with an increase in the Gini coefficient by 15 percent of a standard deviation, which is a sizeable effect (estimate in Column 1 of Table 4; for summary statistics see Table 3).

An alternative way to assess the magnitude of the estimated coefficient is to consider that aggregate inequality has grown during the century preceding the Thirty Years' War by 0.0839 Gini points (see Figure 5). Localities were exposed to a conflict on average 0.723 times during every of the four periods of that century. Let us assume that every time a locality was exposed inequality increased by 0.021 Gini points, which is the magnitude of the coefficient estimated in Column 1 of Table 4. A back-of-the-envelope calculation indicates that conflicts might explain around 72 percent of the inequality increase during the century prior to the Thirty Years' War.[37] Thus, conflict exposure in any quarter century accounts for about a fourth of the typical increase of inequality over the century. This suggests that the effect of conflict exposure was not only statistically significant, but also economically relevant.

Overall, the evidence presented in this section supports the claim that there were two significantly different conflict-inequality regimes operating in preindustrial Germany from 1400 to 1800: during the Thirty Years' War, the destructive effect of wars on inequality dominated, but during ordinary conflicts the extractive effect was larger. This explains why the coefficients on the conflict exposure measure are significantly different from each other in the two periods. The positive conflict-inequality relationship seems to hold even over the whole period for which data are currently available. In sum, it seems true that the Thirty Years' War was a "Great Leveller" (Scheidel 2017), but the many smaller conflicts – paradigmatic of life in the premodern world – were continuous reinforcers of inequality.

4.4 Threshold Distance Test

In Table 5 I repeat the baseline specification, but now testing different distances (25, 50, 100, 150, 200, 250 and 300 kilometres) between localities and conflicts. This is done to substantiate the assumption that 150 kilometres is indeed a reasonable threshold distance for conflict exposure.

Intuitively, being closer to a conflict could have two opposite effects: it could lead to higher in-

[37] The average inequality growth explained warfare during the sixteenth century has been obtained by multiplying the regression coefficient with the average conflict exposure of 0.723 times per period. This is multiplied with four, the number of periods of the sixteenth century. The result is then expressed as the fraction of the aggregate inequality growth during the sixteenth century.

equality because the costs of warfare are probably greater and hence political authorities need to extract more resources in an inegalitarian way. Instead, being closer to a conflict could also reduce inequality, if the destructive effect outweighed the extractive one in the geographically very limited radius where the battle action took place.

The results in Columns 1 to 7 confirm that intuition. Column 1 shows the impact of conflicts that took place within 25 kilometres of a locality. The point estimate is negative, pointing towards a reduction of inequality, but it is not significant. Columns 2 to 4 show that increasing the threshold distance gradually to 150 kilometres increases the point estimate and its significance. Yet also the goodness of fit of the model improves, as the adjusted R-squared indicates. A 150-kilometres threshold maximise the goodness of fit across all specifications in Columns 1 to 7, which is why it has been chosen as the relevant threshold. At distances greater than 200 kilometres the effect seems to peter out, which is what one would expect.

Table 5: Threshold distance test

	(1) Gini	(2) Gini	(3) Gini	(4) Gini	(5) Gini	(6) Gini	(7) Gini	(8) Gini	(9) Gini	(10) Gini	(11) Gini
	1400-1600 & 1675-1800								1625-1650		
Conflict exposure (25km)	-0.009							-0.019			
	(0.011)							(0.012)			
Conflict exposure (50km)		0.011							-0.018		
		(0.009)							(0.030)		
Conflict exposure (100km)			0.007								
			(0.007)								
Conflict exposure (150km)				0.021***				0.024***		-0.012	
				(0.007)				(0.008)		(0.021)	
Conflict exposure (200km)					0.024**						
					(0.010)						
Conflict exposure (250km)						0.021**					
						(0.010)					
Conflict exposure (300km)							0.013				-0.053**
							(0.011)				(0.020)
Locality FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Observations	380	380	380	380	380	380	380	380	56	56	56
Adjusted R^2	0.349	0.352	0.350	0.363	0.360	0.357	0.350	0.365	0.311	0.312	0.316

Notes: Estimation method is OLS. Standard errors clustered at locality level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

In Column 8 I include the conflict exposure variable for the 25- and 150-kilometres threshold in the same specification. If the hypothesis of this paper is true that there existed two countervailing effects of warfare – destruction in the immediate area around a conflict that reduced inequality and extraction in the wider area that increased inequality – then we would expect a negative coefficient on the 25 kilometres threshold and a positive one on the 150 kilometres threshold. This is exactly what we get. The coefficient on the 25-kilometre indicator, although just not significant (p-value 0.115), points towards a direct effect that reduced inequality. At the same time the coefficient on the 150-kilometre indicator indicates an indirect effect of conflicts that increased inequality. The result also supports the idea that of the two countervailing factors, the positive impact of extraction on inequality was likely larger.

These results give confidence that 150 kilometres is a reasonable threshold distance for conflict exposure. It is roughly in line with the threshold used in recent studies on the impact of conflicts on locality-level outcomes (Dincecco and Onorato 2016, 2018). Interestingly, 150 kilometres is also roughly in line with Chilosi et al.'s (2018 see Appendix C) study, who find empirically that the sum of invested capital declines massively in a radius of 150 to 200 kilometres around a town. Moreover, 150 kilometres seems historically reasonable. It corresponds approximately to the distance that messengers – and with them the information about a conflict – could cover in about one and a half to two days in sixteenth to eighteenth century Germany (Denecke 1990: 217, Volckart 2000b: 274).

In Columns 9 to 11 some conflict distance thresholds have been tested for the time of the Thirty Years' War. Unsurprisingly, the conflict-inequality relationship seems to have been constantly negative during that time. Moreover, one would expect that if the Thirty Years' War was really different from ordinary preindustrial conflicts – as hypothesised in the conceptual framework – then the relevant conflict distance threshold should be wider. The reason for this was most likely that the war entailed important spill-over effects across Germany, which created an impact that went well beyond the immediate battle action. Column 10 suggests that this was exactly the case. The point estimate for a distance of 300 kilometres is negative, sizeable, significant and maximises the goodness of fit of the model for that period. The same threshold distance was not significant at other times (see Column 7). This result reinforces the main hypothesis of this paper: during the Thirty Years' War a significantly different conflict-inequality regime prevailed compared to ordinary wars. It seems that the destructiveness of conflicts during the Thirty Years' War had a much more global impact on inequality, while ordinary conflicts had a destructive effect that was more locally contained.

5 Robustness Checks of the Baseline Results

The evidence presented so far supports the view that there were two countervailing effects of conflicts on inequality – destruction and extraction – and that throughout most of the period under study the extractive effect was larger. Conflicts increased economic inequality in preindustrial Germany, with the period of the Thirty Years' War being an exception to that pattern. This section tests whether these results are robust. The analysis controls for several time-variant and time-invariant locality characteristics that might have had an impact on inequality. Then, a falsification test is performed,

which provides some evidence that reverse causality does not drive the results. Ultimately, I test whether the main results hold when an alternative conflict measure is employed. I focus on the period 1400 to 1618 and 1675 to 1800, first, because the positive relationship running from conflicts to rising inequality during that time is the novel finding of this paper. Secondly, for the period 1625 to 1650 the data are not sufficient to perform meaningful robustness tests.

5.1 Controls for Locality Characteristics

The analysis controls for several time-variant and time-invariant locality characteristics that might have had an impact on economic inequality. Most of the information about locality characteristics comes from the *Städtebuch* for Germany and Austria, a multi-volume encyclopedia of cities and towns in Germany since the high Middle Ages.[38]

In Column 1 (Table 6) demographic controls – the log-population size and the occurrence of an epidemic – are included. Column 1 shows that the result for conflict exposure is robust to the inclusion of demographic controls.

Column 2 includes institutional controls: a participative local government, the introduction of the Protestant Reformation, the log-distance to the closest university and the log of the taxes that the imperial estate had to pay to the Empire. Column 2 shows that the main result is robust to the inclusion of these institutional controls. The point estimate is 0.020, significant at the 1% level.

In Column 3 geographic controls – agricultural potential, log-distance to a major river, seaside locality – are included. Since these are all time-invariant variables, they can only be included in a panel framework with fixed effects when interacted with time dummies. The inclusion of these variables yields a point estimate of 0.017, but significance is reduced. This is probably due to the interaction terms that use many degrees of freedom of a small dataset and reduces the precision of the estimate of interest.

[38]The Appendix provides further details about the coding of variables and additional regression results for all controls. Moreover a test for the presence of spatial dependence following Kelly (2019) has been performed. However, the results indicate that spatial dependence is not a problem for this study.

Table 6: Controls for locality characteristics

	(1) Gini	(2) Gini	(3) Gini	(4) Gini	(5) Gini	(6) Bot. 50%	(7) Top 5%	(8) Gini	(9) Gini	(10) Gini	(11) Gini
	1400-1600 & 1675-1800										
Conflict exposure	0.018***	0.020***	0.017*	0.017**	0.017*	-1.195*	0.980*	0.023*	0.004	0.016*	0.014*
	(0.007)	(0.007)	(0.009)	(0.007)	(0.009)	(0.615)	(0.546)	(0.012)	(0.009)	(0.008)	(0.008)
Conflict exposure× participative government								-0.007			
								(0.013)			
Conflict exposure× imperial city									0.027*		
									(0.014)		
Military construction										0.023**	0.018
										(0.011)	(0.011)
Demographic controls	YES	NO	NO	YES	YES	YES	YES	YES	YES	NO	YES
Institutional controls	NO	YES	NO	YES	YES	YES	YES	YES	YES	NO	YES
Geographic controls×time	NO	NO	YES	NO	YES	YES	YES	NO	NO	NO	NO
Locality FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Adjusted R^2	0.378	0.361	0.429	0.377	0.442	0.412	0.284	0.375	0.381	0.319	0.311
Observations	380	380	380	380	380	380	380	380	380	190	190

Notes: Estimation method is OLS. Standard errors clustered at locality level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

In Column 4 all time-variant controls have been included. The point estimate is the same as in the previous column, significant at the 5% level. In Column 5 all controls are included. This yields a slightly less significant estimate than in Columns 1, 3 and 4, probably because of the "costly" interaction terms of the geographic controls. Yet the point estimate remains very similar. In general, the point estimate on the conflict exposure variable remains very stable across specifications. This could hint at conflict exposure being uncorrelated with locality characteristics, that is, indeed exogenous events from the perspective of an individual town, but it could also be due to some of the controls not varying in every period.

Columns 6 and 7 include all controls but taking the wealth shares of the Bottom 50% and the Top 5% as dependant variables. The results confirm what has been shown in the previous section: warfare redistributed economic resources from the poor part of the population to the rich, that is, it increased inequality.

Columns 8 to 11 provide some tentative evidence for the hypothesised mechanism driving the conflict-inequality relationship. Some locality characteristics have been interacted with conflict exposure, which consume many degrees of freedom and one should not be over-confident in finding significant results. In Column 8 conflict exposure has been interacted with the participation of town inhabitants in their local government. If Minns et al. (2020: 603) are right that in communities with less political participation of citizens the institutional framework reflects interests of elites, then we would expect a negative interaction between participation and conflict exposure. The channels that more inclusive governments chose to cover the costs of warfare might have been less inegalitarian, for example because of levying a general wealth tax instead of a highly regressive poll tax. The coefficient points into this direction but is not significant.

One might be wondering to what extent the wider constitutional context mattered in which a locality was integrated. Specifically, a substantial literature has pointed out that defence infrastructure of pre-modern polities was subject to economies of scale (Glaeser and Shapiro 2002: 209, Volckart 2002b: 326, Alesina and Spolaore 2003: 179). Imperial cities were city-states within the Empire, that is, very small polities. Imperial cities had to maintain a state-like apparatus on their own to provide protection for their population. On a per capita basis, this implied higher costs and a higher need to extract resources via inegalitarian channels,[39] compared to territorial towns that were part of other types of territories and that could potentially benefit from economies of scale. One would expect that imperial cities experienced a significantly higher increase in inequality when exposed to a conflict. The coefficient in Column 9 indicates that this was the case.

The conceptual framework suggests that the construction of buildings that had a military purpose was one of the channels through which conflict exposure had an impact on inequality (see Hohrath 1996, Rödel 2003, Isenmann 2014: 100-102). In Columns 10 and 11 I add a variable that indicates whether there was military construction activity (e.g. barracks, arsenals, *Schützenhaus*) in a locality.[40] The results show some evidence for the existence of a positive relationship between military

[39] In fact, imperial cities are explicitly recognized in the historical literature for their pronounced capacity to tax their inhabitants (North 2012: 146), that is, extract resources via inegalitarian channels.

[40] These data are available for a subset of the localities in the dataset only. I have coded the variable as Cantoni et al. (2018).

construction and wealth inequality. Importantly, the coefficient size of conflict exposure in Column 5 is reduced by about a fourth and significance is reduced compared to the baseline estimate in Table 4. This is exactly what one would expect if construction for military purposes had indeed been one of the channels that linked conflict exposure to rising economic inequality.

5.2 Correlates of Conflict Exposure

One might wonder whether conflict exposure was related to certain locality-specific characteristics. Moreover, one might ask whether increasing the conflict exposure threshold changes the characteristics of localities that are part of the treatment group. To address these concerns Table 7 reports how localities' treatment status at different distances (50km, 100km, 300km) correlates with some locality-specific characteristics over the whole period of study (1400 to 1800).

Table 7: Correlates of conflict exposure

	(1)	(2)	(3)
	Conflict 50km	Conflict 150km	Conflict 300km
	1400-1800		
Gini	-0.130	0.447	-0.191
	(0.352)	(0.310)	(0.150)
Log-population size	-0.094	-0.106	0.057
	(0.100)	(0.077)	(0.056)
Epidemic	-0.232***	-0.128**	0.056
	(0.066)	(0.062)	(0.040)
Participative government	-0.009	0.259**	0.061
	(0.104)	(0.122)	(0.060)
Protestant reformation	0.120	0.233**	-0.074*
	(0.118)	(0.109)	(0.039)
Log-university distance	-0.034	-0.066	-0.006
	(0.095)	(0.099)	(0.048)
Log-imperial tax	0.022	-0.120	0.086
	(0.146)	(0.126)	(0.056)
Geographic controls×time	YES	YES	YES
Locality FE	YES	YES	YES
Time FE	YES	YES	YES
Observations	436	436	436
Adjusted R^2	0.243	0.322	0.489

Notes: Estimation method is OLS. Standard errors clustered at locality level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

The results of the 50- (Column 1) and 150-kilometers threshold (Column 2) show that locality specific characteristics do not change substantially between the two distances. The coefficients point into the same direction for most of the variables. Only for log-imperial taxation we get a different sign between the 50-kilometre threshold compared to the 150-kilometer threshold. The

levels of significance differ between Columns 1 and 2. While the outbreak of an epidemic was related significantly to conflict exposure for the localities within the 50-kilometer threshold, they were not related significantly to conflict exposure within the 150-kilometer threshold.

Instead, there is evidence that conflict exposure within the 150-kilometer threshold was related to the presence of a participative local government and the introduction of the Protestant Reformation. For the localities within the 300-kilometer threshold there is some evidence of a significant relationship between conflict exposure and the Protestant Reformation, but in contrast to the 150-kilometer threshold it is negative.

One might also ask whether conflict exposure was related to inequality. Across all specifications and threshold distances the Gini-coefficient is not significantly correlated with conflict exposure. Overall, none of the factors seem consistently correlated with conflict exposure across different threshold distances. This result provides some additional evidence for the claim that wars were plausibly exogenous from the perspective of an individual locality.

5.3 Falsification Test

To provide further evidence for a robust relationship between conflict exposure and economic inequality, a falsification test is performed for the periods before and after the Thirty Years' War. Future conflicts, that is, those conflicts that happened in the quarter-century after the inequality measurement, are used as placebo. The first and second lead of the conflict dummy are the independent variables of interest. As dependent variables the Gini coefficient and the top 10% wealth share are employed. If the relationship actually ran from conflicts to inequality during the period of interest, then future conflicts should not predict past inequality, evidenced by an insignificant coefficient. This approach is a test for the presence of reverse causality.

Table 8: Falsification test

	(1) Gini	(2) Gini	(3) Gini	(4) Gini	(5) Gini	(6) Gini
	1400-1600 & 1675-1800					
Conflict exposure (1. lead)	0.007	-0.113			0.011	0.041
	(0.011)	(1.052)			(0.014)	(1.525)
Conflict exposure (2. lead)			-0.009	-1.146	-0.006	-1.135
			(0.013)	(1.299)	(0.015)	(1.498)
All controls	YES	YES	YES	YES	YES	YES
Locality FE	YES	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES	YES
Observations	325	325	271	271	271	271
Adjusted R^2	0.456	0.372	0.533	0.456	0.533	0.453

Notes: Estimation method is OLS. Standard errors clustered at locality level in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

Table 8 shows that future conflict exposure (first and second lead) did not predict past inequality, whether measured with the Gini coefficient or the top 10% wealth share. None of the coefficients, neither on the first or second yield, are significant. This strengthens the argument that it was past conflict exposure that led to an increase in inequality.

5.4 Alternative Conflict Measures

As a final robustness check, an alternative conflict measure is employed. So far, the analysis has made use of a simple dummy variable. As discussed above, using a dummy has the advantage of mitigating potential measurement error. I have constructed a categorical variable as an alternative, which takes the value zero if there was no conflict, one if there was exactly one conflict and two if the locality was exposed to more than one conflict.

Table 9: Alternative conflict measures

	(1) Gini	(2) Gini	(3) Gini	(4) Gini
	1400-1600 & 1675-1800			
Conflict exposure (ordered)	0.007*	0.006*	0.006	
	(0.004)	(0.004)	(0.005)	
Conflict exposure (singular exposure)				0.024**
				(0.011)
Conflict exposure (repeated exposure)				-0.010
				(0.010)
Demographic controls	YES	YES	YES	YES
Institutional controls	NO	YES	YES	YES
Geographic controls×time	NO	NO	YES	YES
Locality FE	YES	YES	YES	YES
Time FE	YES	YES	YES	YES
Observations	380	380	380	380
Adjusted R^2	0.373	0.372	0.438	0.443

Notes: Estimation method is OLS. Standard errors clustered at locality level in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

In Columns 1 to 3 of Table 9 the control variables are gradually added to the specification. Unsurprisingly, the coefficients are smaller, but point into the right direction and are significant. Only when the geographical controls interacted with time are added is the coefficient not significant anymore.

Another alternative measure is reported in Column 4, where two dummy variables are employed. The first takes the value one when a community was exposed to exactly one conflict. The second dummy takes the value one if the community was exposed to at least two conflicts. Both coefficients point into the right direction but only the one on the singular-exposure dummy is significant. The coefficient on the repeated-exposure dummy is just not significant.

Overall, the results obtained with alternative conflict exposure measures strengthen the interpretation of the results obtained with the main conflict measure, suggesting a positive effect of conflict exposure on economic inequality.[41]

This section has tested whether the baseline results are robust. The analysis has controlled for several time-variant and -invariant locality characteristics, a falsification test has been performed and it has been shown that the results hold when employing an alternative conflict measure. This strengthens the main hypothesis of this paper that ordinary conflicts (i.e. those that took place between 1400 to 1600 and 1675 to 1800) had a positive and economically relevant impact on inequality in preindustrial Germany. These results have been obtained by applying several strategies for mitigating the possibility of omitted variable bias and reverse causality.

6 Case Study: the Schmalkaldic War (1546-1547)

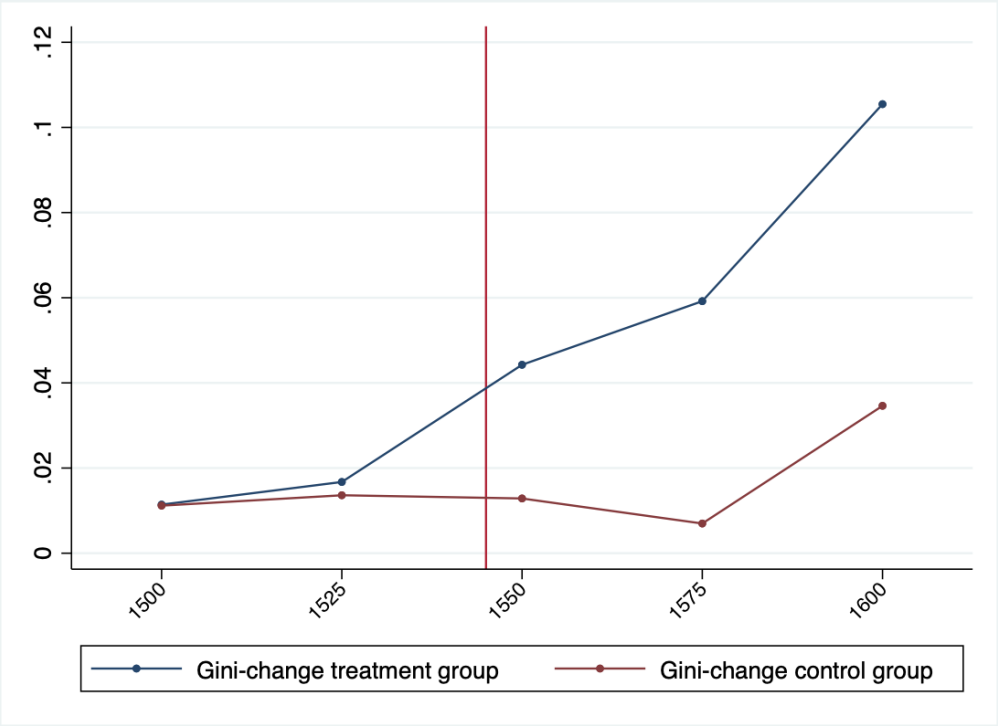
The previous section did not employ an experimental research design. An instrumental variable for conflicts is not available at the current state of research and the continuous occurrence of wars does not allow to employ a difference-in-differences research design because treatment occurs at different times. This makes the typical difference-in-differences estimator unfeasible because it is impossible to define a control group for the post-treatment period (Goodman-Bacon 2019: 2). However, it is possible to use a difference-in-differences research design for single wars. In this section I move to a such a research design for the time of the Schmalkaldic War (1546-1547), to establish a more robust causal relationship between conflicts and economic inequality.

The historical reasons why this war broke out suggest that it was a plausibly exogenous event from the perspective of an individual town. The Schmalkaldic War was one of the "Religious Wars" of the sixteenth century. In 1530 the reformed minority at the Imperial Diet raised a formal protest against the majority-decision to outlaw Luther. Some of these 'Protestant' imperial estates then met in the town of Schmalkalden in Hesse to form the Schmalkaldic League, an alliance for the protection and promotion of Lutheranism. The 'Schmalkaldic League' did this so effectively that Charles V eventually decided that without destroying it, he would never be secure in his position as ruler of the Empire. However, an open confrontation was postponed, due to the emperor's ongoing conflicts with the Ottoman Empire and France. When these were over, Charles V began in secret to prepare his attack in 1546. He obtained 12,500 soldiers and huge sums of money from the Pope and promised the Dukes of Saxony and Bavaria elector dignity for their support in the conflict. Then in 1546, the emperor started his attack against the Schmalkaldic League, in the first battle near Füssen in Southern Germany (Schilling 1994: 213, 229-230, Whaley 2012: 304-320). Other conflicts took place in the Centre-South, the North-West and the East of Germany. Local

[41] One might also ask whether there is 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 shown before. Conflicts that were very close to a locality were probably destructive and reduced inequality. As the distance increased and destruction was not an issue anymore, the conflict-inequality relationship became positive. Yet a too large distance between conflict and locality would capture the conflict-inequality relationship worse. These dynamics are unlikely to be adequately captured by a continuous distance measure.

governments – for example in Augsburg (see Roth 1928: 372) – increased inegalitarian taxation to extract the necessary means to cover the costs of the Schmalkaldic War.

Figure 8: Inequality trends: treatment and control groups



Notes: The trend lines of Gini change have been obtained by regressing an unbalanced panel of Gini coefficients on a full set of locality fixed effects and year dummies (reference year 1475). This has been done separately for the treatment and control groups. The coefficients on the year dummies represent aggregate Gini change for the respective groups (see Clark 2005: 1322-1323 and Alfani et al. 2020: 82-84.

Figure 8 reports inequality trends for the treatment and control groups. The dataset is limited to all those localities for which a comparison of inequality before and after the Schmalkaldic War is possible. The treatment-group consists of all localities that were exposed to battle action of the Schmalkaldic War, taking the usual 150 kilometres threshold. The control group consists of the observations of localities that have not been exposed to the Schmalkaldic War. The main identifying assumption is that inequality in communities with exposure to the Schmalkaldic War would have evolved similar to non-affected communities had the war not occurred. Figure 8 provides visual evidence suggesting that pre-war trends in observables are indeed parallel. These common trends indirectly support the identifying assumption (see Angrist and Pischke 2009: 231-233).

One might wonder about the implications of setting the conflict-proximity threshold wrongly, which determines the allocation to treatment and control groups. The Schmalkaldic War was an important war and a sensational success for Emperor Charles V (Schilling 1994: 230-232). It could be that places further away than 150 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 most likely bias my estimate of the effect of the Schmalkaldic War downwards and lead me to underestimate the true effect of the war but not overestimate it.

In order to identify the effect of the Schmalkaldic War on economic inequality Equation 7 is estimated, which is almost identical to Equation 6 from the previous sections. The main difference is the inclusion of an interaction term between a post-treatment indicator (P_t) and treatment status (W_i), where treated-status means having been exposed to the Schmalkaldic War. The coefficient theta (θ) on the interaction term is the main coefficient of interest. In addition to the vector of controls ($\boldsymbol{X_{i,t}}$), a variable ($C_{i,t-1}$) indicates the exposure to other conflicts in the previous period than the Schmalkaldic War:

$$I_{i,t} = \alpha_i + \pi_t + \theta P_t \times W_i + \beta C_{i,t-1} + \boldsymbol{\gamma'} \boldsymbol{X_{i,t}} + \epsilon_{i,t} \tag{7}$$

Table 10 reports the results. Column 1 reports the baseline specification and in Column 2 all controls have been added. The coefficients indicate a positive, sizeable and highly significant effect of the Schmalkaldic War on economic inequality, measured with the Gini coefficient. In Column 3 the Bottom 50% and in Column 4 the Top 5% wealth share have been employed as alternative inequality measures. They indicate that the Schmalkaldic War led to a sizeable wealth share loss of the poorer half of the population and an even more sizeable gain of the richest part of the population, of 4.8 percent.

Table 10: Difference-in-differences estimates: the Schmalkaldic War

	(1) Gini	(2) Gini	(3) Bot. 50%	(4) Top 5%	(5) Gini	(6) Gini
			1450-1600			1450-1800
Post×War	0.038***	0.045***	-2.196***	4.807**	0.052***	0.034***
	(0.013)	(0.011)	(0.553)	(2.221)	(0.012)	(0.012)
Post×War×League					-0.030	
					(0.021)	
Demographic controls	NO	YES	YES	YES	YES	YES
Institutional controls	NO	YES	YES	YES	YES	YES
Geographic controls×time	NO	YES	YES	YES	YES	YES
Locality FE	YES	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES	YES
Observations	230	230	230	230	230	321
Adjusted R^2	0.261	0.401	0.389	0.185	0.405	0.506

Notes: Estimation method is OLS. Standard errors clustered at locality level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Scheidel (2017: 199) hypothesises that premodern wars decreased economic inequality in a particularly pronounced way among the losers of a war because of the more intense destruction and plundering that the defeated might have experienced. The Schmalkaldic War makes it possible to test this argument. The war had a clear loser: the members of the defence alliance Schmalkaldic League (Schilling 1994: 232). In Column 5 an interaction of three variables has been added: the post-treatment period, whether a locality was exposed to the war and a dummy variable that in-

dicates whether a locality was part of the Schmalkaldic League. A significantly negative coefficient would indicate that among the treated localities, those that were part of the loser group experienced a differential effect of the war on inequality, compared to all other treated localities. The coefficient indicates that being among the losers of the Schmalkaldic War did not significantly reduce a locality's inequality compared to all other treated localities. The main coefficient of interest remains positive, indicating an inequality-increasing effect of the war, and significant at the 1-percent level.

The specification in Column 6 includes also the period after the Thirty Years' War. It shows that notwithstanding the potentially disruptive effect of the Thirty Years' War, localities that were exposed to the Schmalkaldic War (not differentiating between losers and non-losers) were significantly more unequal than localities that were not exposed.

In sum, this section has tested the conflict-inequality hypothesis in a difference-in-differences setting, for the time of the Schmalkaldic War. This establishes a more robust causal relationship between conflicts and economic inequality. I find a strong and positive effect of the Schmalkaldic War on economic inequality, and a negative effect on the wealth of the poor. The results are robust to the inclusion of demographic, institutional and geographic controls. The hypothesis that being among the losers of a war reduced economic inequality could not be confirmed. These case study results strengthen the results obtained in the main analysis.

7 Conclusion

This paper has examined the claim that preindustrial wars were a "Great Leveller" for economic inequality (Scheidel 2017) in preindustrial Germany from 1400 to 1800. The findings suggest that the "war reduces inequality"-hypothesis holds true for the Thirty Years War. However, the Thirty Years' War was the exception and not the rule. Ordinary conflicts led to a redistribution of wealth from the rich to the poor parts of the population and consequently increased inequality. The impact of ordinary wars was significantly different from the impact of the Thirty Years' War.

The explanation for these seemingly puzzling findings is that there were two countervailing effects of conflicts on inequality: egalitarian destruction and inegalitarian extraction. During ordinary wars and throughout most of the period under study the second effect was larger. 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 and bureaucratic infrastructure necessary. The necessary resources were extracted through channels that inevitably increased inequality: regressive taxation, credit and cooperation with financially strong organisations.

These findings could potentially lead to far-reaching lessons for the history of economic inequality. The evidence presented in this paper suggests that regular warfare usually increased inequality. This might explain in part the preindustrial inequality rise, a process that saw inequality rising substantially throughout the early modern period, and which led to relatively high levels of inequality across Europe already before industrialisation began (see Alfani 2020). Warfare seems to explain a good part of this preindustrial inequality rise.

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 (North 1981: 27, Volckart 1999: 32-38, Gennaioli and Voth 2014: 1409, Dincecco 2015: 909-915, Mokyr 2016: 206-207). It has even been suggested that premodern Europe's political fragmentation was among the root causes of Europe's rise to riches, especially in comparison with China (Hoffman 2011: 105, Scheidel 2019). However, one of the implications of this paper is that geo-political competition between polities had considerable economic downsides. One downside was rising economic inequality because rivalry induced political authorities to extract economic resources in an inegalitarian way.

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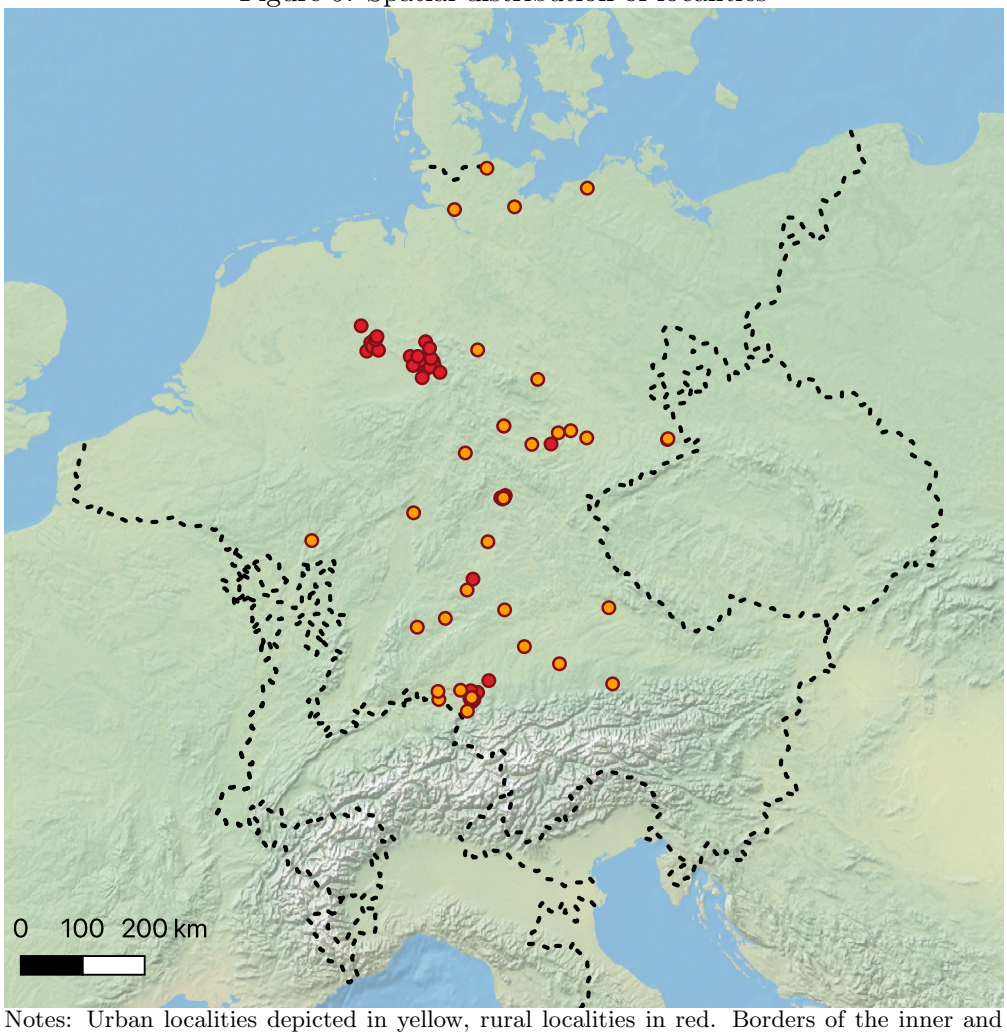
Appendices

A Spatial and temporal distribution of rural and urban communities

Figure 9 shows the spatial distribution of the 72 urban and rural localities from which this study uses inequality data. I have classified a place as rural or urban depending on whether or not it has an entry in the *Deutsches Städtebuch* and the *Österreichisches Städtebuch* (from now on simply *Städtebuch*) (Keyser 1939, 1941, 1952, 1954, 1956, 1957, 1959, 1962, 1964, Keyser and Stoob 1971, 1974, Baltzarek et al. 1973).

Figure 9: Spatial distribution of localities



Notes: Urban localities depicted in yellow, rural localities in red. Borders of the inner and wider Holy Roman Empire around 1545 from Volckart (2020).

Table 11 shows how many communities are included in the dataset for every year of the analysis.

Table 11: Temporal distribution of local wealth distributions, c. 1400 – 1800

Year	Total localities	Urban localities	Rural localities
1400	7	7	0
1425	10	9	1
1450	14	13	1
1475	28	15	13
1500	45	20	25
1525	41	19	23
1550	47	22	26
1575	52	20	33
1600	44	14	31
1625	27	15	13
1650	29	14	16
1675	18	11	7
1700	16	10	6
1725	16	10	6
1750	15	9	6
1775	14	8	6
1800	13	7	6

B Control variables

This appendix explains how the control variables used in the main text have been coded.

Log-population size. The population size of a locality has been obtained by multiplying the number of taxpayers in a given year with the presumed average household size. The household size that is typically assumed for preindustrial German towns is 4.5 Minns et al. 2020: 11.

Epidemic. A dummy that indicates whether there was an outbreak of an epidemic in a locality in the previous period. Information on major outbreaks of epidemics has been taken from the *Städtebuch*. For those villages in the dataset that have no entry in the *Städtebuch* I had to make an assumption. For those villages that were taxed by a nearby town, I have assumed the same occurrence of epidemics as in the taxing town. For those villages that were not taxed by a nearby town I have assumed the same occurrence of epidemics as in the closest town with an entry in the *Städtebuch*. These assumptions are rooted in the regular interaction between village and town inhabitants via urban markets in preindustrial times. Towns were daily markets where peasants from surrounding villages regularly sold agricultural products and bought goods that they could not produce themselves Isenmann 2014: 673. Epidemics indicated by the *Städtebuch* are for example smallpox, syphilis and different forms of plague.

Participative government. A dummy that indicates whether there was burgher participation in the local government. I follow the classification of Wahl (2019), who defines defines participation as one of three arrangements: first, town councils with guild participation (e.g. guilds having seats in the small council (*Kleiner Rat*) or inner council (*Innerer Rat* of a town); secondly, institutionalized

burgher representation (e.g. a regular assembly of burghers often called great council (*Großer Rat*) that usually met when the town council had an important decision to take; thirdly, citizens participate in elections through a participative election procedure (e.g. election of the mayor). Information about participative governments are taken from the *Städtebuch* (Keyser 1939, 1941, 1952, 1954, 1956, 1957, 1959, 1962, 1964, Keyser and Stoob 1971, 1974, Baltzarek et al. 1973). A typical case of non-participation was when a town was governed by a council consisting of patricians that replaced their members with co-optation. For those villages that were too small to be included in the *Städtebuch* I have assumed that the peasant community (Gemeinde) participated in the governance of their village. This is reasonable because German village communities generally had a wide-ranging autonomy in organizing village life. They assigned rights and duties to their members to regulate economic activity through villages by-laws, sanctioned inhabitants and levied taxes (Volckart 2004a: 22-27). This is why German village communities have been emphasised as "microscopic democracies" (Scribner 1996: 300), which makes it reasonable to assume that villages had some sort of participative local government.

Protestant reformation. A dummy that indicates whether the Protestant Reformation has been introduced in a locality. I have taken as introduction date when the council implements the Reformation. This could also be the appointment of a protestant priest by the council. When no introduction is mentioned, or the source indicates that the Reformation had "no substantial impact", I count the locality as remaining Catholic. Information was taken from the *Städtebuch* (Keyser 1939, 1941, 1952, 1954, 1956, 1957, 1959, 1962, 1964, Keyser and Stoob 1971, 1974, Baltzarek et al. 1973). For villages it was decisive whether the Imperial Estate introduced the Protestant Reformation. This information has been taken from the *Städtebuch* and the *Historisches Lexikon der deutschen Länder* (Köbler 2007).

Log-university distance. Distance (km) of a locality to the closest University in every given year. Locations and opening years of German universities are taken from Schilling (1994: 330).

Log-imperial tax. Amount of taxes (Gulden equivalent of so-called "Roman months") that the imperial estate to which a locality belonged paid to the Empire in the previous quarter decade. Information about a locality's imperial estate has been obtained from the *Städtebuch* (Keyser 1939, 1941, 1952, 1954, 1956, 1957, 1959, 1962, 1964, Keyser and Stoob 1971, 1974, Baltzarek et al. 1973) and the *Historisches Lexikon der deutschen Länder* (Köbler 2007). Information about imperial taxation was taken from Wilson (2009: 404, 449-453) Wilson (2009, pp. 404, 449–453). Zeumer (1913) indicates the tax quota of every imperial estate in 1521. I have assumed that this quota remained constant over time (see Huning and Wahl 2018). The tax amount has been obtained by multiplying the share of each imperial estate with the total amount of imperial taxation.

Agricultural potential. Index of agricultural potential of a locality. Data was taken from Ramankutty et al. (2002). The index is a composite indicator that takes into account soil quality and climatic conditions.

Log-river distance. Distance (km) of a locality to the closest major river (Danube, Elbe, Oder and Rhine). The shapefile of major river locations is provided by the European Environment Agency

and can be downloaded here $https://www.eea.europa.eu/data-and-maps/data/wise-large-rivers-and-large-lakes$ (last accessed April, 17th 2020).

Seaside locality. A dummy that indicates localities that lie within 10 kilometers of the seaside. This applies to three places in the dataset: Kiel, Rostock and Lübeck.

Imperial city. A dummy that indicates whether a locality was an imperial city. This information has been obtained from the *Städtebuch* (Keyser 1939, 1941, 1952, 1954, 1956, 1957, 1959, 1962, 1964, Keyser and Stoob 1971, 1974, Baltzarek et al. 1973). Information about the imperial rank of each imperial estate (i.e. elector, bishop, imperial city etc.) have been obtained from Zeumer (1913).

Military construction. Variable that indicates incidents of military construction in the previous period have been counted. Information has been taken from the *Städtebuch*. For classifying military construction (e.g. barracks, arsenals, "gunmen clubhouse" (*Schützenhaus*) I have followed the classification scheme of Cantoni et al. (2018).

Schmalkaldic League. Dummy variable that indicates localities that were part of the Schmalkaldic League in 1546, the year of the beginning of the Schmalkaldic War. The beginning of the war in 1546 has been chosen as cut-off year for the membership of the Schmalkaldic League because the league was officially dissolved in 1547 at the end of the war. For an on-line list of the members of the Schmalkaldic League and the years when they joined see:
$https : de.wikipedia.org/wiki/Schmalkaldischer_Bund$ (last accessed April, 17th 2020).

C Spatial Dependence

Table 12 shows the main baseline specifications when standard errors are adjusted for spatial autocorrelation following Conley (1999; see also Gibbons et al. 2015: 137). I calculate spatial correlation-adjusted standard errors following the routine of Fetzer (2014), who builds on Hsiang (2010). I assume that spatial autocorrelation linearly decreases with distance between localities. Standard errors have been adjusted for different cutoff-distances until which spatial correlation decreases to zero, from 50 to 1000 kilometres.

Columns 1 to 4 show the results of the baseline specification (see Column 1 of Table 4 in the main text) for the period 1400 to 1600 and 1675 to 1800. The estimates of the main variable of interest remain unchanged and significance remains very high, regardless of the cutoff distance. In Columns 5 and 6 I estimate the baseline specification for the whole period 1400 to 1800 (see Table 4 Column 10 above). Again, the estimates of the main variable of interest remain completely unchanged, and are significant at high levels. These results suggests that the baseline results are robust to adjusting standard errors to allow for spatial autocorrelation and that spatial dependence is not a problem for this study.

However, one might still wonder whether there is actually spatial autocorrelation among localities, which could then lead to inflated t-statistics. For addressing this potential concern, Moran's I has been calculated to evaluate whether there is spatial autocorrelation in the regression residuals. This

Table 12: Spatial autocorrelation-adjusted std. errors

	(1) Gini	(2) Gini	(3) Gini	(4) Gini	(5) Gini	(6) Gini
	1400-1600 & 1675-1800				1400-1800	
	50km	150km	500km	1000km	500km	1000km
Conflict exposure	0.021***	0.021***	0.021***	0.021***	0.015**	0.015**
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Locality FE	YES	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES	YES
Observations	380	380	380	380	436	436
Adjusted R^2	0.020	0.020	0.020	0.020	0.008	0.008

Notes: Estimation method is OLS. Standard errors clustered at locality level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

has been done for the respective cross-sections of the dataset (see Kelly 2019: 16-17, 27).

Table 13: Spatial dependence in regression residuals (Global Morans'I-statistic)

	(1)	(2)	(3)
Year	I-statistic	Z-score	P-value
1400	-0.100	0.616	0.269
1425	-0.267	-0.699	0.242
1450	-0.276	-1.156	0.124
1475	-0.219	-1.631	0.051
1500	-0.184	-1.971	0.024
1525	-0.134	-1.366	0.086
1550	0.009	0.445	0.328
1575	-0.047	-0.555	0.289
1600	-0.100	-1.354	0.088
1625	-0.070	-0.339	0.367
1650	-0.194	-1.611	0.054
1675	-0.319	-2.044	0.020
1700	-0.369	-2.556	0.005
1725	-0.385	-2.623	0.004
1750	-0.290	-1.714	0.043
1775	-0.477	-2.987	0.001
1800	-0.333	-1.682	0.046

The specification includes all controls, as in Table 6, but without locality- and time fixed effects due to the cross-sectional data structure. Kelly (2019: 17, 22) considers positive z-scores of two and greater as indicative of relevant spatial autocorrelation. Table 13 shows that the z-score for all years of the analysis is considerably below the critical value of two. This indicates that positive

spatial autocorrelation, which is what one would usually be concerned about, is not an issue for this study. Only in a few years at the end of the study period is there some negative spatial autocorrelation, that is, a tendency that localities with low and high values cluster. However, the degree of negative spatial-autocorrelation is quite low and is present only in four period. Spatial dependence, therefore, seems to be a negligible concern. Moreover, note that more sophisticated methods of dealing with spatial dependence are not feasible here because of the highly unbalanced structure of the panel. For example, because a spatial weighting matrix can not be calculated with the available data, estimating a spatial autoregressive (SAR) model (Gibbons et al. 2015: 128) is impossible.

D Additional Regression Results

Table 14 reports complete regression results of the baseline specification from the main text, including demographic, institutional and geographic controls.

Table 14: Complete regression results

	(1) Gini	(2) Gini	(3) Gini	(4) Gini	(5) Gini
	1400-1600 & 1675-1800				
Conflict exposure	0.018***	0.020***	0.017*	0.017**	0.017*
	(0.007)	(0.007)	(0.009)	(0.007)	(0.009)
Log-population size	0.034*			0.033*	0.037
	(0.019)			(0.020)	(0.025)
Epidemic	-0.012			-0.014	-0.016
	(0.009)			(0.009)	(0.010)
Participative government		-0.002		-0.006	-0.017
		(0.018)		(0.020)	(0.023)
Protestant reformation		0.001		0.002	0.001
		(0.019)		(0.018)	(0.020)
Log-university distance		-0.007		-0.006	0.018
		(0.007)		(0.007)	(0.019)
Log-imperial tax		-0.020**		-0.022**	-0.020
		(0.008)		(0.010)	(0.017)
1400×agricultural potential			0.242		0.259
			(0.214)		(0.249)
1425×agricultural potential			0.209		0.248
			(0.197)		(0.226)
1450×agricultural potential			0.366*		0.420*
			(0.215)		(0.226)
1475×agricultural potential			0.044		0.089
			(0.080)		(0.110)
1500×agricultural potential			-0.025		-0.004
			(0.082)		(0.113)
1525×agricultural potential			0.012		0.043

	(0.101)	(0.115)
1550×agricultural potential	0.106	0.119
	(0.076)	(0.098)
1575×agricultural potential	0.063	0.084
	(0.089)	(0.113)
1600×agricultural potential	0.106	0.122
	(0.090)	(0.118)
1675×agricultural potential	0.062	0.061
	(0.083)	(0.095)
1700×agricultural potential	0.048	0.052
	(0.075)	(0.088)
1725×agricultural potential	0.000	-0.005
	(0.095)	(0.108)
1750×agricultural potential	0.031	0.017
	(0.077)	(0.086)
1775×agricultural potential	-0.050	-0.044
	(0.073)	(0.068)
1400×Log-river distance	0.004	0.008
	(0.034)	(0.037)
1425×log-river distance	0.014	0.024**
	(0.010)	(0.011)
1450×log-river distance	-0.008	0.001
	(0.010)	(0.011)
1475×log-river distance	-0.000	0.008
	(0.009)	(0.011)
1500×log-river distance	0.016	0.023**
	(0.010)	(0.011)
1525×log-river distance	0.016	0.019*
	(0.010)	(0.010)
1550×log-river distance	0.013	0.015
	(0.011)	(0.012)
1575×log-river distance	0.014	0.015
	(0.011)	(0.012)
1600×log-river distance	0.024**	0.026**
	(0.011)	(0.011)
1675×log-river distance	0.005	0.009
	(0.010)	(0.011)
1700×log-river distance	0.015	0.016
	(0.018)	(0.018)
1725×log-river distance	0.027	0.027
	(0.017)	(0.017)
1750×log-river distance	0.023	0.023
	(0.017)	(0.018)
1775×log-river distance	0.012	0.011
	(0.016)	(0.015)
1400×seaside locality	-0.121**	-0.245*
	(0.060)	(0.126)
1425×seaside locality	-0.124***	-0.138**

			(0.044)		(0.059)
1450×seaside locality			-0.009		-0.016
			(0.067)		(0.073)
1475×seaside locality			-0.062**		-0.087*
			(0.031)		(0.048)
1500×seaside locality			-0.072**		-0.096**
			(0.030)		(0.045)
1525×seaside locality			-0.047**		-0.046
			(0.023)		(0.037)
1550×seaside locality			-0.051**		-0.061*
			(0.025)		(0.033)
1575×seaside locality			-0.071***		-0.064**
			(0.026)		(0.031)
1675×seaside locality			-0.112***		-0.108***
			(0.024)		(0.024)
1700×seaside locality			0.113***		0.122***
			(0.018)		(0.016)
1725×seaside locality			0.095***		0.098***
			(0.018)		(0.019)
1750×seaside locality			0.141***		0.140***
			(0.014)		(0.014)
Locality FE	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES
Observations	380	380	380	380	380
Adjusted R^2	0.378	0.361	0.429	0.377	0.442

Notes: Estimation method is OLS. Standard errors clustered at locality level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

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