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Doctoral Thesis

Exploring the Political Economy Causes of Inequality in Preindustrial Germany (c. 1400-1800)

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Declaration of Authorship

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Statement of previous research:

- This thesis builds on my previous research and Masters' theses at Bocconi University and the University of Cambridge.

Statement of conjoint work:

- I confirm that Chapter 2 is part of a collaborative project with Professor Guido Alfani (Bocconi University) and Victoria Gierok (University of Oxford). The building of the dataset was done with Victoria Gierok, the writing of the thesis was entirely mine.

Declaration about working paper versions of the material presented in this thesis:

- A version of Chapter 3 is available as a LSE Economic History Working Paper 2020/311, and is forthcoming in Explorations in Economic History.

- A version of Chapter 5 is available as a European Historical Economics Society Working Paper 2022/225.

Declaration about published articles from the material presented in this thesis:

- Parts of Chapters 2 and 3 became part of a published paper:

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Abstract

This thesis addresses a major puzzle in economic history: why was economic inequality already high when industrialisation and modern economic growth began? In the first part of the project (Chapter 2), I collect new data from archival documents to estimate the extent of wealth inequality in a panel of towns and villages, and at a hypothetical "national" level. This new evidence shows that Germany followed a secular trend of inequality growth between the fourteenth and nineteenth centuries, incidentally interrupted by idiosyncratic shocks, such as the Black Death (1350) epidemic and the Thirty Years' War (1618-48).

In the second part of the project, I explore the causal effects of three central facets of the political economy of early modern Europe on inequality: warfare, the Protestant Reformation and closed governmental institutions in cities. My empirical strategies in these chapters employ difference-in-differences and instrumental variable techniques on the newly assembled town and village-level databases created in the first part of the dissertation.

In Chapter 3 I document that, contrary to the widely held view that wars were "levelers" in history, the frequent military conflicts happening in preindustrial times constantly reinforced inequality. That was the result of warfare increasing the financial needs of communities in preindustrial times, which induced political elites to extract more resources via inequality-promoting channels, such as regressive taxation.

In Chapter 4 I study the effect of the Protestant Reformation on inequality. I argue that the Reformation expanded social welfare, but provided it in a particularistic way. This gave Protestantism an ambiguous character in terms of redistribution and its impact on inequality. I model that trade-off theoretically and test its implications empirically. In line with the theoretical framework, I document that the Reformation exacerbated inequality overall, by making marginal poor people relatively poorer. The result is driven by the introduction of

new particularistic poor relief policies in Protestant communities.

The final chapter investigates the impact of urban political structure on inequality. I document that more closed political institutions were related to higher economic inequality in a panel of early modern German cities. To investigate the mechanisms behind that macro-relationship, I construct an individual-level panel-dataset, containing c.27,000 observations on personal wealth and political office-holding in the city state of Nördlingen from 1585 to 1700. I show that political elites enriched themselves substantially after entering office, but not before. These private gains from public office contributed to economic inequality. Politicians manipulated the crisis of the Thirty Years' War to enrich themselves further. The results are hard to square with a "civic-mindedness" narrative of urban political elites.

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Chapter 1

Introduction

1.1 Motivation

What were the historical causes of economic inequality in preindustrial Germany? This project compiles data from new archival and secondary sources on the distribution of wealth in the Holy Roman Empire (here referred to as Germany),[1] to address this question, with a particular focus on political economy causes.

Following Kuznets (1955), income or wealth inequality growth has long been considered a phenomenon attributable to the Industrial Revolution. More recent research has shown that this view is likely outdated. In several regions of preindustrial Europe, economic inequality rose considerably at least from the end of the Middle Ages and was already relatively high at the onset of industrialisation (see Alfani 2021a). The implication is that one needs to look further into the past than the Industrial Revolution in order to find satisfactory explanations for economic inequality. Many different theories attempt to identify the determinants of economic inequality in the preindustrial period. Economic expansion (van Zanden 1995, Puga and Trefler 2014), political institutions (Alfani and Ryckbosch 2016, Scheidel 2017, Piketty 2020), cultural factors (Basten and Betz 2013), demographic growth (Brenner 1976, Pfister 2020a) and several other potential causes of preindustrial inequality growth have been proposed (see the next Section). Yet there is still little reliable evidence on the importance of these factors. This has led Milanovic (2018: 1031) to emphasise in a recent article in the

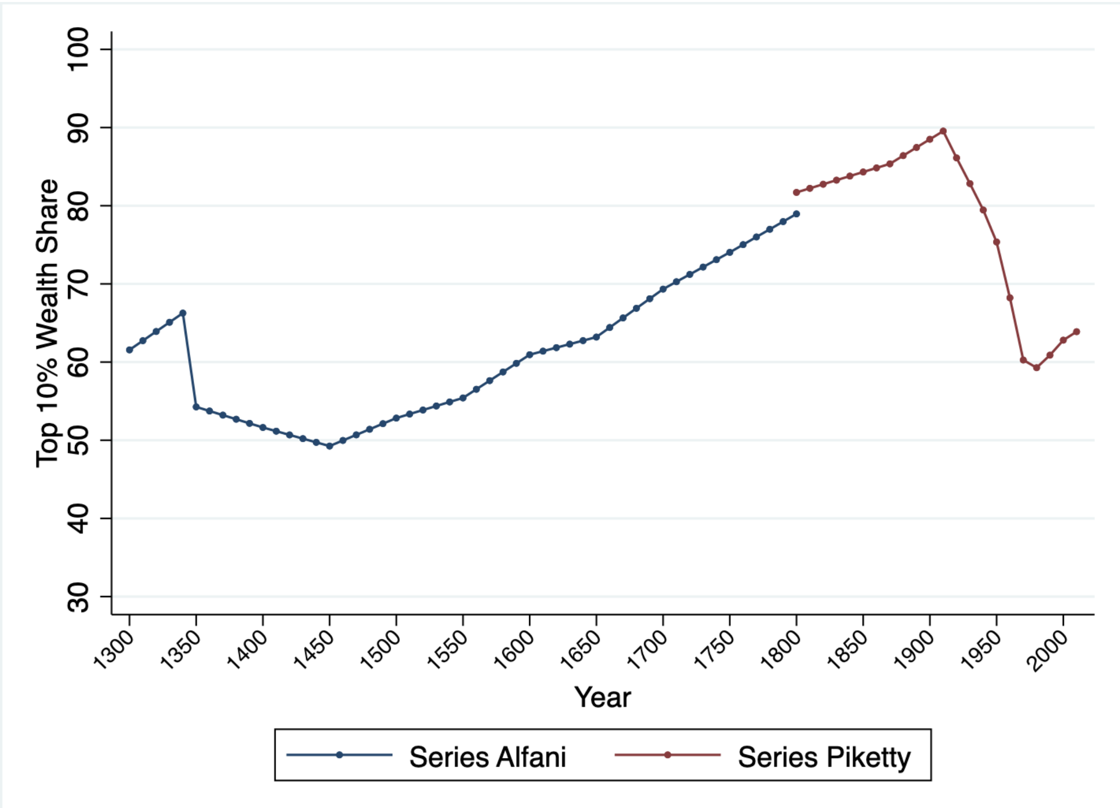
[1] The geographical area this dissertation is interested in, is the Holy Roman Empire of the German Nation, and more precisely its inner part that was subject to the Imperial constitution (see Whaley 2012: 25-39). Geographically this area corresponds roughly to modern Germany and Austria. As a shorthand, I will refer to this area either as "Germany" or "the Empire".

Economic History Review that our understanding of the causes of preindustrial inequality change is at best rudimentary. This dissertation will contribute to bridging this gap in the literature in four chapters.

To begin with, a first question might be why one should care about inequality. Economic inequality is an issue of topical importance because it has several socio-economic consequences. On the positive side, some scholars see inequality as a coordinating mechanism of the market. Inequality allegedly creates the right incentives to make markets function in the first place (Wade 2014: 118). There are, however, also considerable potential downsides to inequality. Among other things it, first, relates to the distribution of political power, as the wealthy usually have easier access to political power, giving them the chance to influence policy-making in a way that is favourable to their own interests (Boix 2003: 129, Acemoglu 2008, Acemoglu and Robinson 2012: 343-344, Puga and Trefler 2014). This is especially true in non-democratic contexts (Milanovic 2019), such as preindustrial Germany. Second, high inequality has the potential for destabilising the social balance. This could potentially lead to social frictions, rebellions and, in the worst case, to civil war (Esteban and Ray 1999, Blattman and Miguel 2010, Baten and Mumme 2013, Piketty 2014: 19-20, Atkinson 2015: 12). Third, high inequality can have a direct detrimental effect on the economy. This could work via various channels: high inequality might affect the capacity to do useful work due to undernutrition of the poor, alter unfavourably the composition of demand, frustrate aspirations and provide bad incentives to save and invest, limit the capacity to invest, restrict the access to credit and finance, and restrict the formation of social and human capital (Persson and Tabellini 1994, Ray 1998: 197-227, Ostrom 2000: 177, Alesina and La Ferrara 2002: 231, Galor et al. 2009).

In sum, there are many reasons why one should be interested in the study of inequality. But, one might ask, why should we care about the study of *historical* inequality? For one thing, all of the political economy forces analysed in this study exist in some form today. Wars still happen, even in Europe. Different forms of Christianity still are the cultural roots of many contemporary societies, and these roots most likely have an impact on policy-making (Enke et al. 2021, 2022). And closed, oligarchic, authoritarian governments, too, have all but disappeared. It would probably not be unreasonable to assume that well-identified impacts of these forces on historical inequality have at least some relevance for contemporary societies, especially in developing economies, which have certain characteristics in common with preindustrial economies. Yet the most important reason to care about historical inequality becomes apparent when we combine the data on inequality trends from scholars working on the industrial period, such as Thomas Piketty, and those working on the preindustrial period. This has recently been done by Alfani (2021a: 37). The combined figures, reproduced in Figure 1.1, show that the historical maximum of inequality in Europe was in the industrial period, around the year 1914 — and some argue that there is a direct causal link between inequality maximum and the disastrous events that happened after 1914 (Piketty 2014).

Figure 1.1: Wealth Inequality in Europe, c. 1300-2010 (Top 10% Wealth Share)



Notes: The Alfani series averages data from the Sabaudian State, the Florentine State, the Kingdom of Naples, and the Republic of Venice. The Piketty series averages data from France, the United Kingdom, and Sweden.
Source: Alfani 2021a: 37, Appendix.

But the most interesting aspect about these figures is the following: about three quarters of the inequality growth that led to the maximum in 1914 happened between the end of the fifteenth and the beginning of the nineteenth century. In other words, if we want to understand where high inequality in the twentieth century came from, we must understand what happened during the preindustrial period, and more precisely, the early modern period, the time this thesis mostly focuses on. Overall, finding out where inequality came from in the long run could speak to several historical and contemporary debates.

Another question could be why Germany has been chosen as the area of analysis for this thesis. The primary reason is that the Holy Roman Empire was a very diverse entity that can potentially teach us several valuable lessons about the causes of inequality (see the historical background section). This area had rich and poor regions, was a cockpit of preindustrial warfare, it had Europe's main cultural dividing line — Protestantism vs. Catholicism — running right through it, and its high degree of political fragmentation resulted in a plethora of different governmental institutions. All these factors potentially mattered for inequality (see the literature review). On top of that, Germany offers so rich archival records that it is not just possible to study inequality at the usual macro-level of towns and villages, but even at the individual micro-level (see Chapter 5), which has hitherto not been possible. The principal aim of this thesis is, therefore, to exploit Germany's variation and data-richness fruitfully to shed light on the causes of inequality, to offer new material to current debates on inequality (see Piketty 2014, 2020, Deaton 2015, Scheidel 2017, Alfani 2021a).

Additionally, there has been no comprehensive study to date that analyses wealth inequality in Germany at the household-level, over longer time periods and a large number of localities, and that attempts to systematically identify causes. There exists an established literature on the legal inequalities that characterized an estate-based society (see Press 1991: 52). These legal inequalities between estates can sometimes feed into economic inequalities (see the literature review and Chapter 5 on political institutions). But unfortunately this literature has not explicitly addressed the issues quantitative economic historians are typically interested in, such as the distribution of wealth and income. For what concerns these economic inequalities, until recently some experts in the field have even thought that data for Germany were non-existent for the preindustrial period (see Piketty 2014: 188). This is most likely exaggerated, especially since there has been a long historiographical interest in the distribution of economic resources in the German literature, going back at least to the German Historical School (see for example Schmoller 1895, Bücher 1917, Kirchgässner 1960, Franz 1976). Yet all of this older literature, mostly published in German, is piecemeal. It hints at the need for greater synthesis, a modern account of inequality in Germany, based on large data to study inequality across time and space, in order to identify general trends and causes. Such a modern account could put Germany on the map of inequality studies in the international literature, which is the second aim of this thesis.

1.2 Literature Review

This literature review gives a broad and synthetic overview of the main hypotheses that have been advanced about the causes of historical inequality change (see also Alfani 2021a), involving economic, demographic, cultural and institutional factors, and how much we know *empirically* about these factors in relation to inequality. The review does not cover the scholarship on the descriptive patterns of inequality development presented in the next chapter. Moreover, it only touches briefly on the specific literature streams on the three political economy forces — warfare, the Protestant Reformation and oligarchic governmental institutions — that are the main focus of the thesis, as also these are covered in detail in the respective chapters.

Economic explanations of inequality are based on the idea that economic expansion translates almost automatically into growing inequality, leading to a pattern that is analogous to the tail of a Kuznets-curve in industrial times (see Kuznets 1955; also Deaton 2015). The connection between economic expansion and inequality can work through several channels, for instance, an increase in the demand and premium for skilled labor, growing urbanization, resource accumulation in free markets or productivity increases in agriculture (van Zanden 1995: 649, 656-658, Puga and Trefler 2014: 755, van Bavel 2016: 261-263, Allen 1992: 283-302).

Notwithstanding the intuitive logic of these economic explanations, it should be noted that most recent studies have failed to detect a robust relationship between economic expansion and preindustrial inequality growth (Alfani 2021a).

Conversely, it has been argued that economically detrimental events — major warfare, state breakdowns, revolutions, extensive fires — were among the principal levellers of inequality (Milanovic 2016: 56, 62-64, Scheidel 2017, Alfani 2021a).

Demographic explanations work to a certain extent analogously to the inequality-increasing effects of economic growth. 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 wealth (Brenner 1976, Rodepeter 1998, Alfani and Ryckbosch 2016: 151, Pfister 2020a). However, and similarly to strictly economic explanations, recent studies suggest that the actual correlation between population and inequality growth was empirically weak at best (Alfani and Di Tullio 2019, Alfani 2021a).

Instead, population decline brought about by epidemics such as the Black Death freed up land, made labour scarcer, increased real wages and reduced land rents and, potentially, inequality (Alfani and Ammannati 2017: 1090, Alfani and Murphy 2017: 333-334, Alfani et al. 2020: 30-32). All this potentially redistributed income and wealth from richer to poorer strata, and reduced inequality. Similarly, population growth through migration is ascribed an important role in explaining inequality growth, especially if migrants were comparatively poor (Alfani 2015: 1089-90).

There is a body of literature that suggests that for several reasons cultural norms feed into inequality. For some the key driver are different family forms, possibly in combination with different inheritance practices. Nuclear families are believed to be less inequality-promoting than patriarchal families. And partible inheritance was potentially more egalitarian than impartible inheritance, which prevented the splitting up of property, increased inheritability of wealth and inequality (Todd 1985: 7-11, 19-32, Piketty 2014, Alfani 2015: 1077-1078, Wegge 2020).

For others, religion determines whether a society is more or less redistributive, which could have obvious consequences for inequality (Guiso et al. 2003, Alesina and Giuliano 2010, Basten and Betz 2013). For example, in preindustrial Germany local governments have been considered more or less prone to provide mitigation from suffering depending on their religious confession: Protestant communities might have been less willing to provide social welfare to certain groups within the poor population (Hartung 1989, Jütte 1994; see for a different view Kahl 2009, Dittmar and Meisenzahl 2020: 6). This could have increased inequality. Moreover, the Reformation brought about huge transfers of economic resources, from religious institutions to secular political elites (Cohn 1987, Cantoni et al. 2018: 2054-2058). This might have reduced the amount of resources that was redistributed especially to poor strata, for example, through religious organisation such as monasteries. However, the Reformation illustrates also that cultural attitudes often get embodied in institutions (Piketty 2020: 12). This brings us to the fourth class of variables that could explain the development of economic inequality: institutions.

More specifically, in many cases key institutional mechanisms that enshrined economic inequality were systems of resource extraction that redistributed economic means from a wider population to a small group of beneficiaries (Acemoglu et al. 2005a: 390, Acemoglu and Robinson 2012: 360). This inequality-promoting resource extraction could take many institutional forms.

One of these forms was the overarching social order in premodern times. *Legal* inequality was built into the system in the estate-based societies of preindustrial Europe and Germany. The basic social order in preindustrial Germany and other parts of Europe 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 (see Scott 2002: 27-55). This implied an underlying inequality of wealth and income in premodern times (Volckart 1998: 52, 74, Piketty 2020). Another but related form of unequal resource extraction were traditional land-tenure systems. In these systems a small group of usually affluent landlords extracted rents and labor services from their tenants — the dominant mode of organizing the preindustrial economy — which has been considered inequality-promoting (Sokoloff and Engerman 2000, Banerjee and Iyer 2005: 1197, Piketty 2020: 51-64). In Eastern-Central Europe, stronger manorialism (or the "second serfdom") may have *ceteris paribus* increased inequality comparatively more (Brenner 1976: 48-58, Malinowski and van Zanden 2017: 378-379, Ogilvie 2014b).

Moreover, medieval and early modern guilds might have been an institution that increased economic inequality because the privileges that governments conferred on them gave guilds market power. This enabled guilds to redistribute economic profits from the wider population to their lucrative businesses, in input- as well as output-markets (Ogilvie 2014a, 2019: 7, 173, 576, 2021, Volckart 2002a: 330, Acemoglu et al. 2011: 3288-3289). However, there exists also the opposing view that preindustrial guilds were particularly egalitarian because they allegedly kept material differences between guild members small, making them cohesive and solidary "sworn fraternities" (Weber 1956: 775-776, Mathis 1992: 97, van Bavel 2022).

Governmental institutions are also considered important for explaining economic inequality (Scheve and Stasavage 2017). More oligarchic, closed or less participative forms of government might have promoted inequality compared to more participative, open or republican institutions. The reasoning behind this argument is that when political power is limited to a small group of people, it is more likely that those in power govern a polity in a way that redistributes economic resources to themselves to the detriment of the wider population (Boix 2003, Alfani and Ryckbosch 2016, Scheidel 2017, Piketty 2020, Alfani 2021a, Minns et al. 2020). Another body of literature points to the risk that even formally republican places can be captured by rent-seeking special interest groups (Volckart 2002c: 121-122, Ogilvie 2011:

419-26, Stasavage 2014: 337), which might promote inequality growth.

An important means for authorities to extract economic resources was — in preindustrial and industrial times — the tax system. It has been argued that the growth of pre-modern fiscal institutions increased inequality, because tax systems were structurally regressive. In consequence, tax systems redistributed shares of income and wealth from poorer strata to the rich. The so-called "Military Revolution" and warfare most likely induced governments to increase the fiscal burden on the population, which might have led to growing inequality (see Dincecco 2015, Alfani 2015, 2021a, Alfani and Di Tullio 2019). Yet the structure of taxation and the levying of contributions has been emphasized more generally as a cause of inequality (see Alfani 2019: 24-27). For example, Piketty argues that what has determined r (return to capital) to be historically larger than g (national income growth rate) — his principal inequality-generating mechanism — is above all how polities have designed their tax systems. He argues that inter-state competition has induced governments to keep taxes on capital low, thus privilege wealth holders and promote inequality growth (Piketty 2014: 354-355, Alvaredo et al. 2013: 7-9).

Additionally, there were other authorities in the preindustrial world that levied some form of taxes, which could have been regressive and inequality-promoting. The church claimed its share from agricultural production, the tithe, but also urged believers to donate and received contributions for its religious services (Blaschke 1989, Alfani 2010: 71, Cantoni 2015: 2049). In preindustrial Germany the Imperial circles and the Empire received contributions too (Schilling 1998: 118, 121).

This brief literature review has pointed out that there exist many theories about what determined economic inequality in preindustrial times. Yet most are hypotheses, backed by little or circumstantial evidence. Only few studies employ econometric techniques to explore systematically the drivers of household-level income or wealth inequality, based on actual historical data (see Alfani and Ammannati 2017, Milanovic 2018). But because data on the distribution of income and wealth are difficult to gather (see the discussion in Chapter 2) even in those published studies the datasets are relatively small.[2]

Moreover, there exists to my best knowledge not a single study that employs a causal research design to identify the drivers of income and wealth inequality in preindustrial Europe in a systematic way.[3] This lack of data and systematic analysis has recently led an expert in the field to conclude that for preindustrial times, "we have at best some *guesses* [italics added] about the forces that might explain changes in inequality" (Milanovic 2018: 1031). For this reason, the following chapters attempt to provide some evidence for the causal effects of three forces grounded in the political economy of early modern Europe: warfare, the Protestant Reformation and oligarchic governmental institutions. My empirical strategies in these chapters employ difference-in-differences and instrumental variable techniques on newly assembled town and village-level databases. The main underlying data are introduced and critically discussed in Chapter 2.

1.3 Historical Context: Conflicts, Religious Cleavages and Urban Political Structure in Preindustrial Germany

This section provides some historical background information about preindustrial Germany that is relevant to the following chapters, especially the three analytical ones on the impact of conflicts, the Protestant Reformation and closed governance institutions on inequality. Before diving into these topics, it is useful to recall the trends in macro-economic development of the area under study.

1.3.1 Economic Development

How, one might ask, did Germany perform during the so-called "Little Divergence", which saw North-Western Europe experiencing quicker economic growth as compared with Southern and Eastern Europe during the early modern period (Allen 2001: 429, Acemoglu et al.

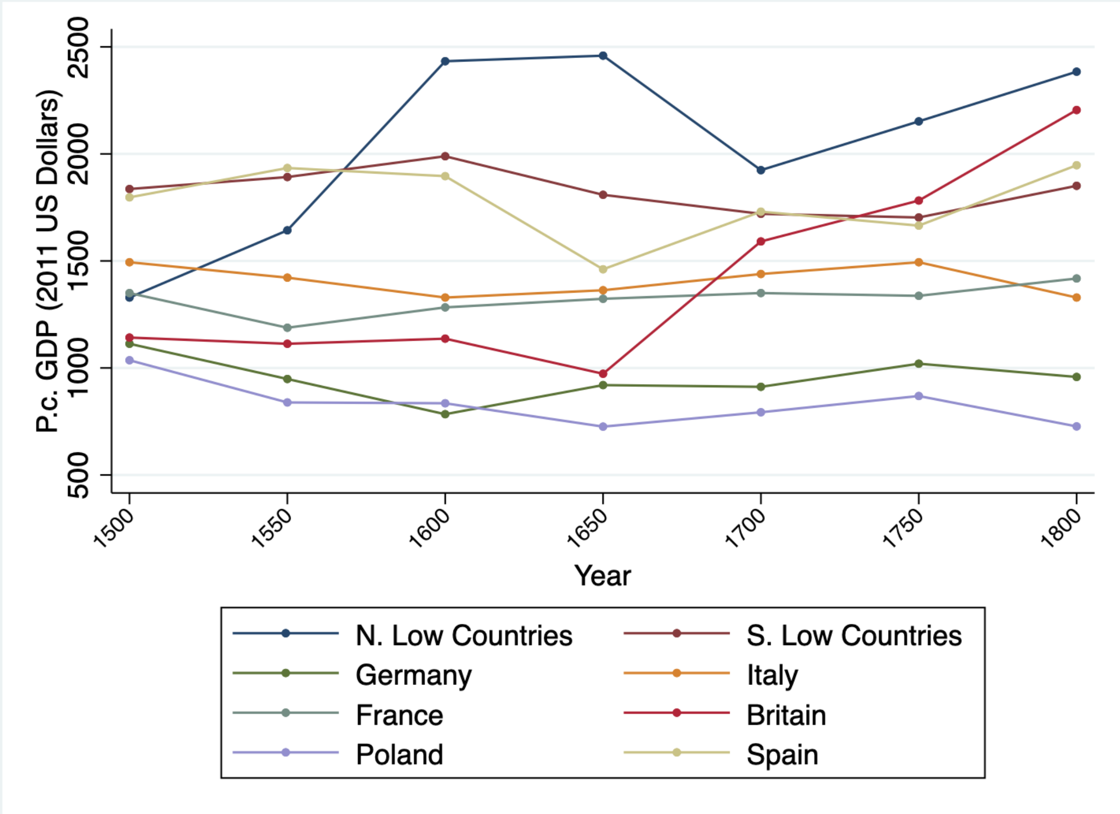
[2]The analysis of Alfani and Ammannati (2017) is based on 130 observations from communities in the Florentine state, and Milanovic (2018) employs 41 observation in his cross-country regressions.

[3]A partial exception is the study of Basten and Betz (2013), which, however, employs modern-day census data to measure income inequality.

2005b: 547)? This question can be answered looking at recent GDP estimates reported in Figure 1.2 (figures taken from Bolt et al. (2018); see also Pfister 2011, 2017). According to these figures Germany as a whole seems to have been on the losing side of the Little Divergence: at the end of the Middle Ages, around 1500, when data for Germany becomes available, the region was at best in a middling position compared to the richest regions, the Northern and Southern Low Countries. It was about as prosperous as Britain, and slightly ahead of Poland. Over the sixteenth century, when the Northern Low Countries experienced substantial growth, making it the most prosperous region in Europe, Germany declined substantially (see also Abel 1961: 489). This was a relative and absolute decline, so that around 1600 Germany may have been even poorer than Poland. During the seventeenth century Germany's GDP grew slightly, but it remained considerably poorer — similar to Poland — than the leading regions in the Little Divergence, that is, the Northern Low Countries and Britain. Over the eighteenth century Germany's economy practically stagnated, similar to Italy, France and Poland, while the leading regions continued their growth path.

However, the fact that Germany was overall on the losing side of the Little Divergence hides the large variation in economic performance within the area (Ogilvie 1996a: 265). Some Northern regions of Germany, which benefited from the trading activity of the German Hanse, might have been the most developed regions at the end of the medieval period. Instead, from the end of the Middle Ages, the most vibrant parts of the economy were located in and around the cities of southern Germany, such as Augsburg, Nuremberg or Ulm. Yet after the beginning of transatlantic trade, the centre of the European economy shifted away from the Mediterranean, to Northwestern Europe. The consequence was that cities in the south of Germany lost their traditional markets. This led to the decline of this area from the end of the sixteenth century. The areas that then became the economically most successful ones were in the west (for example, the Rhineland) and to some extent in the north of Germany (for example, Hamburg). These areas benefited from the shift in European trade (Lütge 1966: 197, 228, 298, 312, North 2005: 143-144, Lindberg 2008: 644-9, 654). At the same time the territories to the east of the river Elbe became the economically

Figure 1.2: Per capita GDP in Europe, c.1500-1800 (2011 US$)



Source: Bolt et al. 2018: Appendix.

most backward regions inside Germany (Ogilvie 2014b: 39-47).[4] Such regional differences are unavoidably obscured by the aggregate measure of economic development used above — GDP — but also by Germany's overall economic backwardness in comparison with the rest of Europe.

1.3.2 Conflicts

Conflicts in preindustrial Germany cannot be explained without understanding the peculiar political structure of the Holy Roman Empire. In the German lands, the historical reasons for state growth go back at least until the Investiture Conflict between the Holy Roman Emperor and the pope, and the end of the Imperial Church System (*Reichskirchensystem*)

[4] Systematic quantitative evidence for comparing the economic development of preindustrial Germany's regions over time is still not available. However, the timing of industrialisation can be used as a proxy of this variation at the end of the preindustrial era. Saxony and the Rhineland most likely industrialised already from about 1780 onward. But the industrial revolution reached Westphalia only around 1850, Baden and Württemberg around 1870, and Bavaria and East Prussia just before 1900 (Ogilvie 1996a: 265).

in the eleventh and twelfth centuries.[5] This led to a decline in central authority in the Empire, put power in the hands of regional and local rulers, and fragmented the political landscape. Additionally, the "Statute in Favour of the Princes" (*Statutum in Favorem Principum*) of 1231 allowed rulers to make their own laws for their lands, which favoured the consolidation of their local authority. As a result, the Holy Roman Empire consisted of more than 300 sovereign territories in the early modern period (Ogilvie 1996b: 121).

Although the Empire provided a uniting political framework, from the fifteenth century rulers were increasingly involved in geo-political rivalry with their neighbours, since "there was hardly a prince within all of the Empire who did not harbor a claim against lands or rights in possession of one of his colleagues, and very few whose territories were not intermingled with those of other rulers" (Spruyt 1994: 48-50, Ertman 1997: 227-31, 238). Typically, the interests of a ruler were to prevent the takeover and integration of his territory, protect or perhaps extend it (Schmidt 2018: 48). This led to several small wars, for example between the dukes of Bavaria and the counts of Palatinate, the counts of Palatinate and the dukes of Württemberg, the duke of Cleve-Mark and the Archbishops of Cologne, the duchies on the Rhine among themselves and between the landgraves of Hesse and the Archbishop of Mainz. These conflicts were small compared to wars between major European powers, but they were enough to be a huge burden on the finances of the conflicting parties. Yet there were also larger conflicts that increased rulers' need for economic resources, often related to the Reformation or Habsburg aspirations for hegemonic rule, such as the Knights' War (1522-23), the Peasants' War (1525-26), the Schmalkaldic War (1546-47), the Princes' Revolt (1552-55) and ultimately, the Thirty Years' War (1618-48) (Ertman 1997: 238-40).

[5] The Investiture Conflict started as a dispute between Pope Gregory VII (1073-85) and Holy Roman Emperor Henry IV (1056-06) over the nomination of the archbishop of Milan. Traditionally, the emperor claimed the right to make such an important nomination, but was contested by the pope. The issue escalated into a general dispute over the question of who had the right to appoint high ecclesiastical officials, and thus who controlled the Church in the Empire. It only ended long after Gregory and Henry were dead, in 1122, with the Concordat of Worms. The imperial right to appoint high officials was replaced by canonical elections. This deprived the Emperor of the church system as the instrument through which he had effectively governed the territories of the Empire. Some clerics remained loyal to the emperor, but others did not, which was a welcome opportunity for aristocrats to further challenge the Emperor's power. This led to a loss of imperial control over the German lands (see Spruyt 1994: 48-51, Ertman 1997: 231-4)

The Empire took measures to increase security and limit bellicosity (Schmidt 1999: 33),[6] and "promoting peace between its members [was] the Empire's core function" (Chilosi et al. 2018: 665). For that reason, some authors even draw parallels between the decentralized Empire and the European Union today (see Scales and Whaley 2018, Volckart 2020). Nonetheless, the recorded conflicts suggest that it has not been entirely effective in doing so (Kroener 2013: 21).

With the Thirty Years' War, if not before, Germany had become a cockpit of European warfare. Several ruling houses of German territories reached out to become also the rulers of larger territories in Europe, for example in Poland, England, Sweden and Spain. To pursue these aims, but also to increase their weight in the Empire, they entered alliances with other European powers, which bore the potential for further conflict. Moreover, Germany had a long-standing rivalry with France, which especially under Louis XIV aggressively attempted to expand its territory towards the river Rhine, at the expense of the Empire. These forces led to several larger wars, such as the Franco-Dutch War (1672-79), the Palatine Succession War (1688-97), the War of the Reunions (1683-84), and the Spanish Succession War (1701-14). All of them took in part place on German territory (Schilling 1998: 54, 149, 199). In the eighteenth century the rivalry between Prussia and Austria for hegemony in the Empire — two German states, both part of the group of major European military powers (together with England, France and Russia) — contributed to further conflict. The culmination was the Austrian Succession War (1740-48) and later the Seven Years' War (1756–63) (Schilling 1998: 281-288, 451).

Apart from these big dynastic clashes, there was another force that favoured conflicts in Germany. With the advancement of the "Military Revolution" in the early modern period,

[6] Major measures of the Empire to increase security were the "Eternal Peace" (*Ewige Landfrieden*) of 1495 and the "ordinance concerning the implementation of imperial law" (*Reichsexekutionsordnung*) of 1555. These Imperial acts outlawed feuds among the nobility and the rest of the population, thus prohibiting the common man the legitimate use of force and initiating a separation between state and society in the Empire. Moreover, it 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 Imperial Chamber Court (*Reichskammergericht*), or, in case of large conflicts, to the Imperial Circles (*Reichskreise*). This legislation was a boost to institutionalization in the territories because it required a functioning bureaucracy (see Schulze 1986: 131-32, Schmidt 1999: 33-37, 104-5, Westphal 2018: 402-5; see Angermeier 1984: 22-30 for an overview of the wide-ranging reforms of the Empire in the fifteenth and sixteenth centuries.

the small and smallest territories within the Empire were less and less able to maintain the costly defence infrastructure necessary to maintain their independence. State size was beneficial, and Germany's political landscape was characterised by many small polities. It sometimes happened that these smaller and often weaker polities were taken over by larger more powerful ones, such as in the cases of the formerly independent cities Erfurt and Straßburg. Overall, the number of city-states within the Holy Roman Empire declined from 87 in 1521 to 37 at the end of the eighteenth century (Schilling 1998: 108, 196-208, 232-236).

1.3.3 Religious Cleavages

There have been several attempts in European history to challenge the religious monopoly of the Catholic Church, for example, the one by the Bohemian theologian Jan Hus in the fifteenth century. Yet the most far-reaching one was arguably the Protestant Reformation. It began with the formulation and circulation of Martin Luther's famous 95 theses in 1517. They were a sharp critique of the Catholic Church and the Pope. The unintended consequence of his critique was not just the splitting of the Church. In Germany, it led to a deep political divide, between Protestant rulers that followed Luther, and a Catholic camp that remained loyal to the Old Church and the Catholic Emperor. When Luther was accused of heresy, Frederick the Wise, the Elector of one of the largest and most powerful principalities in Germany — Saxony — protected him and introduced the new faith in his territory. Many other polities did the same, such as Electoral Saxony or Hesse. However, this happened initially in a legal vacuum, which was ended in 1525 by the Peasants' War. As a consequence of that war, the introduction of Protestantism fell *de facto* into the hands of political authorities, such as princes and the magistrates of Imperial cities. Yet this decision was still highly contested by the Catholic camp, and could provoke military conflicts. Only at the Imperial Diet in Augsburg in 1555, Lutherans and Catholics could agree on a treaty that officially confirmed the existence the Protestant confession in Germany (Schilling 1994: 85-116, 193-254, 445-464, Reinhard 2004, Blickle 2015: 165-191).

That treaty also certified worldly rulers' monopolistic right to determine the confession in the communities of their territories and of their subjects ("*cuius regio, eius religio*"), a principle that even the Thirty Years' War and the Peace of Westphalia did not change. In fact, for the overwhelming majority of subjects the religious denomination of the polities they were living in did not change until at least the nineteenth century. Rulers' right to impose their preferred confession on their subjects had wide-ranging implications — besides the obvious change in religious belief — because princes also became the heads of their respective Protestant territorial or "national" churches (*Landeskirche*). This brought about a much more decentralised system than under the Catholic Church. One consequence was that rulers could confiscate the property of the Catholic Church, such as monasteries. These were valuable assets that came in handy to any ruler who had to sustain, for instance, the costs of warfare or of a new palace. Yet the closure of monasteries also meant that one of the principle providers of social welfare simply ceased to exist in Protestant places, but continued to support the poor and the sick in Catholic ones (Cohn 1987).

A second major consequence was that the new territorial churches regulated social life in a way that was different compared to the regulations of the Catholic Church. It should be remembered that the Church was not just a provider of religious services, such as church service. It also sanctioned corruption, regulated education, public morality, healthcare, poor relief and charity. Protestantism had a different vision of how these areas of public life should be regulated, and with its arrival came new, sometimes radically different provisions. In many places (but not all) these new provisions were put into written law, so-called "church ordinances" (*Kirchenordnungen*) (Hsia 1996: 366, Dittmar and Meisenzahl 2020). Obviously, a change in the regulation of, for example, poor relief, could have profound socio-economic consequences. For example, in line with Luther's new ideology, the Protestant definition of who "deserved" poor relief became narrower, therefore excluding a substantial part of lower strata from public support in comparison to Catholicism (Laube 1981: 134-135, Jütte 1996: 392, Kahl 2009: 271). Moreover, under Protestantism recipients of poor relief had to publicly identify themselves as such, by wearing a poor badge. This practice reportedly stigmatised poor people and worsened their chances in the labour market (Jütte 1994: 161-161, Hartung

1989: 169, 171, 174).

1.3.4 Urban Political Structure

Germany was, as mentioned, characterised by a high degree of political fragmentation. Similarities existed, but there was no common governance model, and so every of the several hundred independent territories came with its own political structure. The same was true for the about 4,000 cities in early modern Germany. Some were territorial cities, that is, under the control of some territorial overlord, others were *de facto* independent city-states, with the Emperor as their formal overlord. Within these cities a plethora of different governance structures existed. Some cities had a strong and legally guaranteed participation of craft guilds in the government, in others patrician families traditionally held most or all of the power. And again in other places regular burgher assemblies representing a broader cross section of the population had to be consulted by the municipal government on important matters (Schilling and Ehrenpreis 2015, Wahl 2019).

An important differentiation was how open or closed (or "oligarchic") these municipal governments were. An indicator of openness is whether becoming a city magistrate was determined by elections or not. In early modern Germany between about 70 and 80 percent of cities might have had no elections in which the population could participate (Wahl 2019). In other words, most but not all cities tended towards a closed form of government, without substantial formal checks. This was a general pattern in preindustrial Europe (Pirenne 1958; see also Alfani 2023: ch. 5, De Vries and Van der Woude 1997: 586-596).

The degree of openness of a municipal government is important because cities with a higher degree of control by their citizens were more likely to be governed in the interest of broader parts of the population. This was in turn important because local authorities were usually more influential than central authorities in matters of commerce, work, and life in general (Schmoller 1896: 7-12, Minns et al. 2020). In other words, magistrates typically had a high degree of discretion over most aspects of urban life, especially economically relevant ones. In Germany this was the result of the imperial constitution leaving large room to urban rulers to regulate trade, commerce, the crafts, to create limits to competition, to set fiscal policy, and to borrow money on behalf of the city. These policy decisions, obviously, involved weighing up different economic interests (Friedrichs 1979: 144-148, 199-206), and they could have a substantial impact on economic outcomes, such as growth, inequality or industrialisation (see Allen 2014). Differences in the policies of urban governments were also crucial for the question of whether or not cities were able to recover after the disastrous Thirty Years' War. There was a clear divergence in economic performance after that event, but the reasons for that divergence still have not been systematically identified (Schilling and Ehrenpreis 2015).

Chapter 2

Wealth Distribution and Economic Inequality: New Estimates for Germany from the Black Death until the Beginning of Industrialization

2.1 Introduction

In contemporary societies we tend to identify inequality as an endemic feature of economic progress (see Deaton 2015: 1–5). But in fact, inequality has ebbed and flowed throughout history.

In his seminal article "Economic Growth and Income Inequality" Simon Kuznets implied that inequality growth dates to the industrial revolution. In essence he believed that the transition from a low-wage agrarian to a high-wage economy rooted in modern industry invariably generated disparities between income groups (Kuznets 1955: 12-18). Yet there is a long-standing tradition in economic history which asserts the existence of economic inequality in preindustrial times (see Soltow 1968, Lindert 1986, 2000). However, that literature has only recently come into fashion (see Roine and Waldenstrom 2015, Lindert and Williamson 2016, Milanovic 2018, Alfani and Di Tullio 2019, Alfani 2021a).

This chapter falls squarely into this second camp. Specifically, it traces the trajectory of household-level wealth inequality in what is now called Germany[1] throughout the period from 1350 until 1850. This 500-year time-period ranges from the Black Death in 1348 to

[1] The geographical area this chapter is interested in, is for the most part 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). During the last half century of the analysis the Empire does not exist anymore but is succeeded by the German Confederation (*Deutscher Bund*). I am using "Germany" as a catch-all term for these different polities.

the beginning of industrialization in Germany. Germany offers an excellent case study for investigating preindustrial economic inequality. First, the area was characterized by a high degree of diversity in terms of economic development, institutional settings, culture and geography. Thus, there is a lot of variation to exploit in the sample. Secondly, there has been no comprehensive study to date that analyses wealth inequality at the household-level over longer time periods and a large number of localities.[2]

This chapter does two things. First, it maps the trajectory of wealth inequality in Germany over the long run. I build on the dataset compiled by Alfani, Gierok and Schaff 2020, in which the authors have calculated Gini coefficients at 50-year intervals across a number of select localities. This dataset expands their work. It provides a more granular understanding of inequality during the period, adds 30 towns and villages to the dataset, and improves the data for several existing communities. Additionally, I calculate inequality based on the full available distributions, that is, including also propertyless individuals where recorded and previously excluded from the sample, and provide estimates over shorter periods of 25-year intervals. The additional data was mostly drawn from new archival sources. In turn, the measure of inequality is both the Gini coefficient and wealth shares of parts of the distribution, including the poorest decile as well as the Top 10 percent. I trace the development of inequality at the level of localities and at a tentative aggregate level.

In addition to providing a more nuanced view of the data, this chapter, secondly, also begins to look at the correlates of inequality across German localities in the period from 1350 to 1850. A host of economic, institutional and geographic factors are examined, employing a correlated random effects approach. These factors include *inter alia* population size, city status, distance to universities, introduction of the Protestant Reformation and membership in the Hanseatic League.

I find that, consistent with the literature, inequality rose and fell across four broad periods:

[2] For case-studies based on single or few localities in Germany, or studies focussing on short time periods, see for example Bátori and Weyrauch (1982), Robisheaux (1989), Sabean (1990), Schlumbohm (1994), Warde (2006), von Hippel (2009). For studies that investigate different aspects of economic inequality than the distribution of wealth or income at the household level, see Pfister (2019, 2020a).

Germany followed a secular trend of inequality growth between the fourteenth and nineteenth centuries, interrupted by two major shocks, the Black Death (1350) epidemic and the Thirty Years' War (1618-48). Specifically, when the Gini coefficient is used as measure, wealth inequality increased during the sixteenth century and again from the late eighteenth century onwards, and inequality declined for a century or so after the Black Death and the Thirty Years' War. Interestingly the data also show that during periods of high inequality as measured by the Gini coefficient, the wealthiest parts of the population enjoyed their highest wealth shares. At the same time the wealth shares of the poor and lower-middle classes were at a historical minimum. In addition, the chapter studies a few key variables that correlate with inequality. The exploratory regression analysis finds that, controlling for other characteristics, more populous and urban and those places that were in the south of Germany were more unequal.

This chapter makes three contributions to the literature: First, based on 432,813 household-level observations it provides the largest single database on preindustrial inequality compared to all other published studies, which puts Germany on the map of inequality studies. Secondly, this is also the first study to examine several measures of economic inequality in preindustrial Germany, not just the Gini coefficient. Finally, the chapter identifies broad correlates of inequality across localities in preindustrial Germany.

The rest of the chapter proceeds as follows: Section 2.2 provides an overview of the literature on economic inequality in preindustrial Germany. In the Section 2.3 I discuss the data and the numerous challenges that one faces when collecting them from archival tax registers. Sections 2.4 and 2.5 present local and aggregate estimates of wealth inequality, from about 1350 until 1850, and explain the methods that have been employed to generate these measures. Section 2.6 explores the characteristics of unequal places and Section 2.7 concludes.

2.2 What we know about Economic Inequality in Preindustrial Germany

This section reviews the literature on what we know about economic inequality in preindustrial Germany. It explores both the highly specific and detailed case studies coming out of Germany, but also briefly examines the literature that maps larger macro trends in other regions of Europe. I argue that we need a modern account of inequality in Germany, one that captures broader trends within German history. This account would be based on large data, and study inequality across time and space, identifying general trends.

2.2.1 Germany: Economic Inequality at a Micro-Level

This section looks at the literature that has studied trends and causes of economic inequality development.

Most historical research on preindustrial inequality in Germany is legal in nature, focussing on the characteristics of an estate-based society. This society consisted of basically four different estates or groups: clergy, nobility, burghers in towns and peasants. Belonging to these groups implied different legal privileges and obligations. One's estate determined whether an individual was ruled or was a ruler himself, which profession one could follow, whom one could marry and even which clothes one could wear. To which group an individual belonged was determined by birth and generally fixed for lifetime. This legal and social inequality was seen as part of God's will (Moraw 1983: 66-68, Press 1991: 52–53, Volckart 1998).

There exists also a literature on wealth inequality in preindustrial Germany.[3] Yet most of this is piecemeal. It begins at least with the work of the German Historical School, and distinguished scholars such as Schmoller (1895), Bücher (1917), Kirchgässner (1960), Franz (1976), Fügedi (1980) and many more have contributed to it. However, most of this literature

[3] There exist, of course, other measures than wealth or income of historical socio-economic inequality. For instance, anthropometric measures, such as height and numeracy have been widely used (see Crayen and Baten 2010, Baten and Llorca-Jana 2022).

has been published in German.[4]

This was an ambitious literature, but it was also problematic: first, because the authors analyzed social structure, or economic inequality usually by producing large tables of the distribution of wealth. These wealth tables, with their numerous categories, make precise comparison difficult (see for example Hartung 1898: 188-189). Secondly, the studies usually have analyzed inequality in single communities or over short time periods only. More recently, there has been new interest in economic inequality in Germany (see for example van Zanden 1995, Wegge 2020). However, most of these studies are again disparate and piecemeal and do not allow one to analyze broad trends.

One can, however, step back and distill four broad phases of the development of economic inequality at the household-level from these case studies: first, from the Black Death (1348-1349) until the end of the fifteenth century; secondly, from the end of the fifteenth century until the beginning of the Thirty Years' War (1618-1648); thirdly, from the war until the beginning of the eighteenth century; fourthly, from the beginning of the eighteenth until the mid of the nineteenth century, when industrialization began on a wider scale (Ogilvie 1996b: 121).

During the first phase, from the Black Death until the end of the fifteenth century, economic inequality is believed to have declined (Rodepeter 1998: 12). During the Black Death population declined by about 50 percent (Jenks 2005: 38). This probably led to a reduction of inequality through three mechanisms: first, agricultural and urban workers received higher wages due to increased marginal productivity; secondly, rural dwellers could increase their plots and reduce feudal burdens; thirdly, large landowners suffered from a decline of value of land, had to pay higher wages and received lower rents. These developments are believed to have reduced inequality until around 1500 (Moraw 1989: 273, Rösener 1996: 65–66, Rodepeter 1998: 7-10, Jenks 2005: 49). However, it should be noted that this contemporary speculation about inequality development is more rooted in economic theory and fragmentary evidence about population, prices and wages than on actual data about the distribution of

[4] See Warde (2006: 120-124) for an exception.

income or wealth at the household level.

The second phase, from the end of the fifteenth century until the beginning of the Thirty Years' War, brought about an increase in economic inequality (Friedrichs 1979: 105-106, Bátori and Weyrauch 1982: 95, Robisheaux 1989: 84-89, Sreenivasan 2004: 146). Related to the increase of inequality, the extent of poverty grew during this period (Wunder 1996: 68, von Hippel 2013: 14). While there is a general agreement about the likely direction of inequality development, the literature disagrees on the causes. Several arguments have been proposed, ranging from economic and demographic expansion, to inegalitarian resource extraction by political elites, landlords or guilds (Scheidel 2017: 334-336, Pfister 2019, 2020a, Haan 1981: 101-103).

During the third phase, from the beginning of the Thirty Years' War until the beginning of the eighteenth century,[5] economic inequality probably declined initially and stagnated later on (Röck 1991: 444, Scheidel 2017: 337). The egalitarian effect of the Thirty Years' War is generally attributed to the multifaceted destructiveness of this truly exceptional conflict: it brought about devastation, widespread plundering and the breakdown of administrative infrastructures, a deep and lasting economic crisis, famine and population decline of about forty percent (van Zanden 1995: 646, North 2005: 159, Wilson 2009: 790-792, 810-811, Scheidel 2017: 339). The interaction of these factors most likely led to a widespread decline of economic inequality across Germany. This pattern of substantially declining wealth inequality differentiates Germany from all other parts of Europe for which comparable data are available for the seventeenth century (see Alfani et al. 2020: 42).

The fourth and last phase of preindustrial inequality development starts from the beginning of the eighteenth and ends around the mid-nineteenth century, when industrialization accelerated. Inequality is believed to have increased substantially during this period, a hypothesis that centres on the stark population growth at the time and presumably growing poverty. However, there is some disagreement about how intense the growth of inequality was at the

[5] There is a debate about whether Germany went through an economic crisis in rural and urban sectors already some decades before the Thirty Years' War (Haan 1981: 100, Gömmel 1998: 1-3, Pfister 2017: 724). This might have interrupted inequality growth already before the outbreak of warfare.

time (Kocka 1990: 117-120, Sabean 1990: 454-455, Schlumbohm 1994: 54, Wunder 1996: 68, von Hippel 2013: 14, Pfister 2020a: 16).

A shortcoming of these earlier historical studies is that they tend as noted to focus almost exclusively on either short time periods or single case studies. Scholars have usually been interested in inequality in Augsburg, Frankfurt, or Hildesheim, sometimes comparing their values with those of another locality (Jütte 1984: 71, Röck 1989, Uthmann 1957: 19-24, Fügedi 1980). Overall, however, a broader perspective is missing so far, one that is based on large data and that discusses general trends and magnitudes.

One exception to this is the work of Ulrich Pfister, who has recently looked at long-term trends of inequality development (Pfister 2019, Bracht and Pfister 2020; see Pfister 2020a for an overview). His work contributes substantially to filling a gap in the literature. However, his data captures economic inequality based on proxies of actual household-level distributions, such as the ratio between wages and rents.

2.2.2 Inequality in Preindustrial Europe: the Promise of a Macro-Approach

A group of recent scholars have begun to compile the sorts of large datasets that make it possible to analyze broader trends of inequality development. We now have a quite precise picture of how economic inequality developed in Italy, the Low Countries, Spain, Portugal, Sweden and England (Alfani 2015, Alfani and Di Tullio 2019, Alfani and Ryckbosch 2016, Álvarez-Nogal and Prados de le Escosura 2013, Reis 2017, Bengtsson et al. 2018, Milanovic 2018). The best studied case so far is probably Italy. In preindustrial Europe there seems to have been a general pattern of decreasing economic inequality in the period after the Black Death, followed by a more or less constant increase during the early modern period (Alfani 2021a). Only Portugal is an exception to that pattern (Reis 2017: 313). This research agenda has also reached beyond Europe, including the pre-revolutionary United States, late Tokugawa Japan and Jamaica (Lindert and Williamson 2016, Saito 2015, Burnard et al. 2019).

While this focus on broader trends is welcome, Germany has long been thought to be an unknown in this debate. Kuznets (1955), for example, relied heavily on data for nineteenth- and twentieth-century Prussia and Saxony. In this period, Germany indeed showed the pattern implied by Kuznets' famous inverted U-curve (Bartels 2019; Albers et al. 2020). Yet for the preindustrial period data have been thought non-existent (Piketty 2014: 188). This chapter seeks to fill that gap. It combines an empirical focus on preindustrial Germany and the periodization revealed by case studies, with a focus on long-term trends from the scholars working on other parts of preindustrial Europe. The next section outlines my approach to compiling a dataset of household-level wealth inequality in preindustrial Germany.

2.3 Data, Sources and Limitations

The main aim of this chapter is to provide additional data on the distribution and inequality of wealth at household-level in preindustrial Germany. These data are based on tax registers that list all tax-paying households in a locality. The registers were prepared to levy taxes and record the wealth estimate or wealth tax paid by each household, for example of the so-called *Beet*, *Beede*, *Schoss*, *Geschoss*, *Schatzung* or *Ordinari Steuer* ("ordinary tax") but also the Imperial tax *Türkensteuer* ("Turk tax"). All these were locally collected wealth taxes and give a fairly accurate picture of the distribution of wealth at the household level. Similar sources have been used in other published studies on the long-run development of preindustrial economic inequality, in Germany and other parts of Europe (see Bátori and Weyrauch 1982, van Zanden 1995, Warde 2006, Alfani and Ryckbosch 2016).

This study builds on the dataset of Alfani et al. (2020) in several ways.[6] First, 30 towns and villages have been added to the dataset, and the data for several existing communities have been improved.[7] Secondly, the dataset has been set up at a higher frequency. Data

[6]The building of the initial dataset (see Alfani et al. 2020) and the extended version employed in this chapter was done with Victoria Gierok.

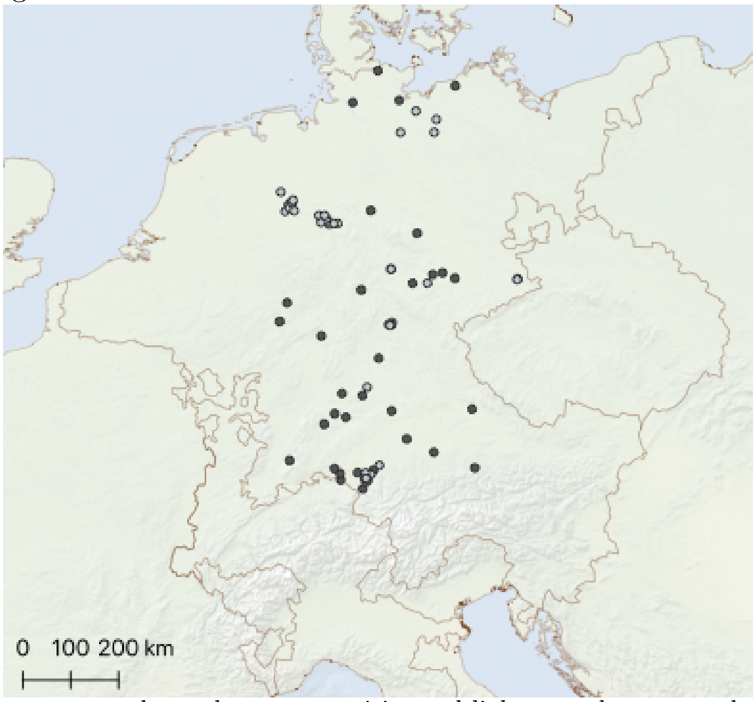
[7]The new data for Bad Koenigshofen, Freiburg, Großeibstadt, Hachenburg, Heilbronn, Herbstadt, Koblenz, Konstanz, Merhatzhofen, Moegloffs, Niederwangen, Nördlingen, Opfenbach, Rudlings, Siggen, Straubing, Thann, Traunstein, Überlingen, Wangen and Wanger Pfarr come from new archival sources. Data for Straubing and for the Mecklenburg communities Boizenburg, Crivitz, Gadebusch and Neustadt come from edited primary sources. Data for Hersfeld, Kitzingen, Leonberg, Leutkirch, Ravensburg, and

on economic inequality is now reported in intervals of 25 years, instead of 50-year intervals. Thirdly, for all localities I have used the full available distributions. Alfani et al. (2020) excluded the propertyless from the distributions, mainly for reasons of better comparability with other European areas. By using the full available distributions, including the propertyless as far as they were recorded in the tax registers, I sacrifice on comparability but gain on comprehensiveness of the wealth distributions for the German case. However, the inclusion or exclusion of the propertyless is a much less decisive issue than it might seem. It slightly shifts the level of the Gini coefficient: upwards if the propertyless are included, downwards if they are excluded. Based on several case studies and modelled aggregate distributions it has been shown that the inclusion or exclusion of the propertyless does not change the trend of the development (Alfani et al. 2020: 85-90). Fourthly, the chapter goes beyond the exclusive focus on the Gini coefficient as the sole indicator of economic inequality and reports the wealth shares of other parts of the wealth distribution too, that is, the shares of poor (Bottom 50 percent) and rich (Top 10 percent, the Top 5 percent and the Top 1 percent) households.

The data collection effort made it possible to compile a dataset for local wealth distributions from 1350 until 1850, in steps of 25 years, clustered around reference years. As a general rule, data points were clustered to their nearest cluster year, but in few cases also to the second nearest, when this was necessary to extend the time series by another period. For example, if an observation actually referred to the year 1555, then it was clustered to 1550. An observation that referred to the year 1562 was clustered to 1575 if there was already another observation for 1550. This handling was necessary because archival tax registers have survived rarely and are not always available for the exact year one might be interested in. For the same reason missing values (11 percent of all observations) were linearly interpolated but never extrapolated. Figure 2.1 shows how the localities are distributed spatially within the borders of the Holy Roman Empire. Not all regions are equally well covered and especially the centre-west is underrepresented. Attrition is therefore most likely to be random. Table

Stockach come from secondary sources. The Appendix provides a full list of the sources.

Figure 2.1: Urban and Rural Communities in the Dataset



Notes: Dark grey dots correspond to urban communities and light grey dots to rural communities. Borders of the Holy Roman Empire around 1545 and Protestant areas around 1559 from Volckart (2020). Not all communities are visible because of visual overlap on the map.

2.1 provides an overview of how many datapoints are available for each year, for rural and urban communities. I classified a place as rural or urban depending on whether or not it has an entry in the *Städtebuch*, although in reality the distinction between town and village was fluent (Blickle 2015: 160).

One might criticise the long-term perspective of the chapter. From the point of view of political and constitutional history, the Imperial Recess (*Reichsdeputationshauptschluss*) of 1803, which saw major territorial reorganizations, or the end of the Holy Roman Empire in 1806 were certainly watershed events (see Whaley 2013: 614–644). Yet in economic terms the beginning of industrialization around 1850 was most likely an even more important development (see Borchardt 1976). This chapter, therefore, exploits the availability of coherent sources for at least a few places until the mid-nineteenth century.[8]

The number of localities in the respective years indicate that the dataset covers the sixteenth and seventeenth centuries better. Information is much sparser at the margins of the period

[8]For a similar approach, see Sabean (1990: 454).

Table 2.1: Temporal Distribution of Local Wealth Distributions

Year	Nr. Rural Dist.	Nr. Urban Dist.	Total Nr. Dist.
1350	0	2	2
1375	0	4	4
1400	0	7	7
1425	1	9	10
1450	3	13	16
1475	12	18	30
1500	23	23	46
1525	24	22	46
1550	26	25	51
1575	31	23	54
1600	28	20	48
1625	13	21	34
1650	16	20	36
1675	7	19	26
1700	6	16	22
1725	6	15	21
1750	6	14	20
1775	6	13	19
1800	6	12	18
1825	3	2	5
1850	3	1	4
Total	220	299	519

Notes: Own calculations.

under study, especially in the fourteenth and nineteenth centuries. Data on the distribution of wealth are very hard to find for these two sub-periods (see Alfani 2021a: 14, Albers et al. 2020: 6-7). I therefore decided to include the few data that I was able to find, in order to provide at least some indication about the likely distribution of wealth during these periods, but the results have to be interpreted with great care.

On average, a locality is included for ten periods, the shortest interval is two periods and the longest eighteen. The total number of local wealth distributions is 519 over the whole period of study. That does not seem like a lot. However, it is considerably more than any other published study on economic inequality in preindustrial times contains. Note that this seemingly small number of observations refers to wealth distributions in a given locality and year. Obtaining one wealth distribution requires information about every household in a locality. Behind every wealth distribution lie on average 832 single household-level wealth observations. In total, the dataset is based on 432,813 household-level wealth observations. As a comparison, the wage series of Clark (2005) for England from 1209 until 2004 is based on 46,000 observations. In other words, calculating inequality indicators requires lots of information. The selection of a place into the dataset simply depended on whether or not the relevant archival documents stood the test of time since their creation and are still available today. Attrition is therefore most likely to be random. Unfortunately, for the pre-census age there are no statistics available for average characteristics of the universe of urban and rural communities in Germany, to which I could compare my sample. For a number of interesting places data sources have survived only for single years, for example for Cologne, Linz, Innsbruck or Stuttgart. Hence these places unfortunately had to be excluded from this panel-dataset.

Most of the data come from new archival sources. In the following discussion I focus on the challenges that arise when analysing archival tax registers.[9] A full list of the sources is provided in the Appendix.

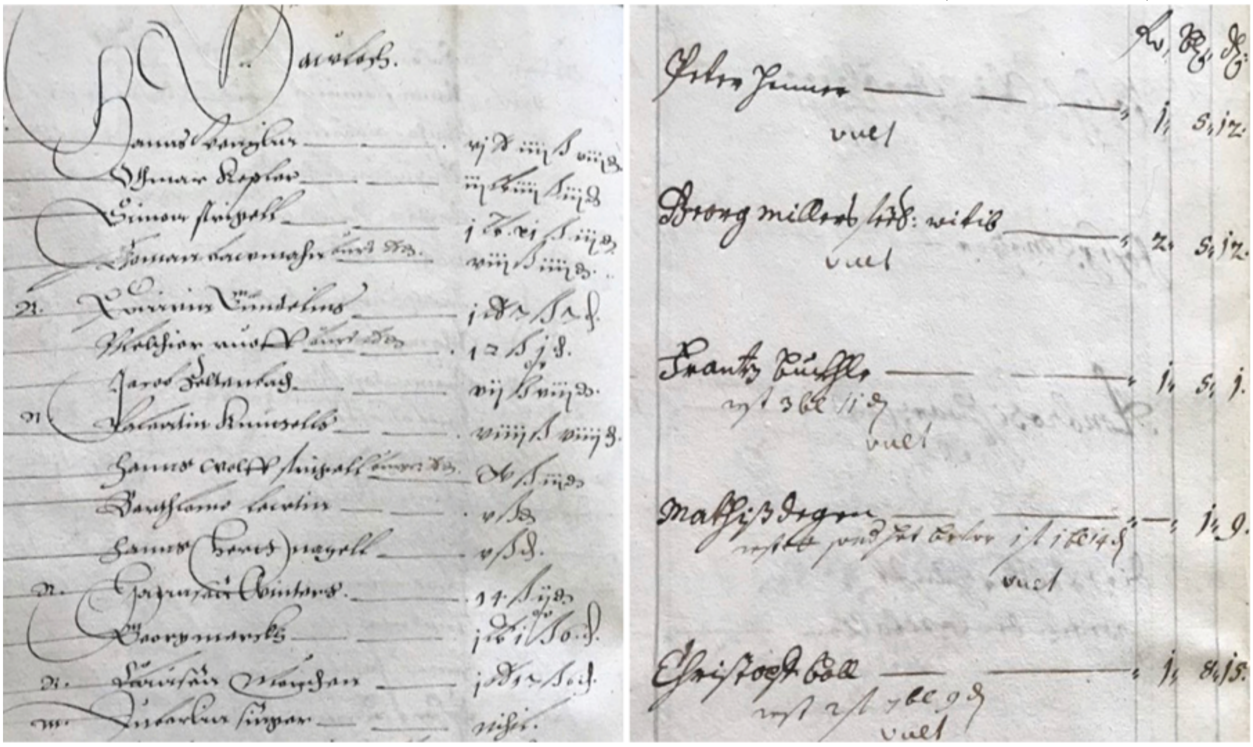
In order to give a better understanding of what kind of information can be obtained from the tax registers, Figure 2.2 shows two representative examples from Überlingen in 1575 and 1750. Überlingen was a medium-size town in Southern Germany in preindustrial times. On the left we see the first page for the district "Hauloch" in 1575.[10] Under the district name begin the names of the household heads. Right next to the name, the tax payment of each household has been recorded. From the first entry in 1575 we learn that Hanns Kreigler paid six pounds, four schilling and eight denarii. All register entries provide this basic information. However, in some cases the tax register states no amount, as can be seen from the last entry on the page. It simply states "*nihil*", meaning "nothing". This person was propertyless, that is, he had no taxable goods. The picture on the right shows the tax register of 1750. The same information has been recorded in 1750. Apart from the handwriting and the currencies in which the payment was recorded, no substantial aspects

[9] For a discussion of the challenges in dealing with the secondary sources that have been used to build up the original dataset see Alfani et al. (2020).

[10] Part of the district name was cut off. This usually happened when the originally loose sheets in which the tax collector recorded payments were bound to a book at the end of the taxation process.

had changed. We learn from the first entry in 1750 that Peter Hamer paid one gulden, five batzen and twelve denarii. The tax register of 1575 contained 1,004 household-level entries and the register of 1750 713 entries.

Figure 2.2: Archival Sources: Tax Registers from Überlingen (1575 and 1750)



Source: Stadtarchiv Überlingen.

The most obvious challenge when analysing tax registers is the heterogeneity of tax systems. Taxing practices differed in preindustrial Germany, a consequence of the Empire's political decentralization. However, some generalisations can be made, notwithstanding the variations in local tax practices. Subject to taxation were usually all citizen- and non-citizen-households of a locality (Isenmann 2014: 244). A "household" usually meant the "complete house" (*Ganzes Haus*), that is, adults, children but also servants and maids that lived in the same place (Schmidt 2009: 43). It was possible that servants and maids formed their own households, in which case they were taxed independently (see Scheurle 1960).

It is important to emphasize that the tax registers record household- and not institutional wealth, such as of organizations like monasteries or universities. Such organizations usually made separate agreements with their localities about contributions. In line with a trifunctional logic of society, clerics were generally exempt from taxation, as were noblemen, although less frequently. The same applied to certain public servants and some groups of professionals that political elites wanted to attract, such as doctors or gunsmiths (Schmidt 2009: 30-32, Isenmann 2014: 536, Alfani et al. 2020).[11] Some parts of the poor could also be excluded (see below). Jews usually had to pay taxes, but since they had a special social and legal status, they were taxed separately from other inhabitants (Uthmann 1957: 8). Additionally, places that were within a town but for some reason not part of a town's jurisdiction were also exempt from taxation. The most important example of this principle were princely courts in capital cities, where all court inhabitants were exempt from town-jurisdiction and taxation (see Richter 1881: 282, Press 1991: 289).

The tax base was the totality of different mobile and immobile asset classes, such as real estate, crops, animals, cash money, household goods and other property, at the moment of taxation. Revenue from interest payments was usually capitalized and added to the other assets. Yet real estate was the most important asset class everywhere. Premodern tax systems did not distinguish between personal and business wealth, and subsumed all assets at the household-level. A uniform tax rate was applied to all households regardless of the size of their property, so that tax payments increased linearly with the amount of wealth owned (Isenmann 2014: 522, 527). Which items were taxed was an ambiguous issue in practice. For example, in Nördlingen, which is among the places in the dataset with the best preserved archival documentation, the sources indicate that no asset of a household should be spared from taxation, but no objects were defined more precisely (Dorner 1905: 12). Additionally, levying a wealth tax could include taxing also labor income, if an individual owned no real estate or other taxable goods but only had labor income. Preindustrial tax collectors were not interested in the modern differentiation between wealth and income, and so they did not make that partial change in the tax base explicit in the registers. Their aim was simply to capture the economic strength of a household in general (Dirlmeier 1978: 494, 513, 516). Because of the large variety of items taxed, one may interpret the tax registers

[11] Note however that officials were not always excluded. In Mühlhausen (Thr.) all members of the city council, the mayor and other officials paid taxes (Vetter 1910: 14).

as indicating also income *above subsistence*. Tax authorities in preindustrial times had to adapt to the economic circumstances of their environment (Blaschke 1989: 55). It is therefore understandable that the tax base was adapted to reflect the context of the local economy: for example Augsburg, a center of manufacturing, taxed among other things tools; Überlingen, located directly on the lakeside, taxed among other things fishing boats; Wangen, a center of cloth production, taxed among other things raw cloth (Hartung 1989: 176, Harzendorf 1954: 36-37, Scheurle 1960: 4-8).

Moreover, the principle of valuing property was ambiguous. As a general rule one can think of the values recorded in the registers as representing approximately the perceived utility of the taxed goods (Dirlmeier 1978: 515). Recorded values do not necessarily represent market values.

On the one hand, certain elements of the tax system such as the tax base, the evaluation method and tax allowances differed from place to place. On the other hand, local tax systems had *central elements* in common in terms of who, how and what was taxed. Based on a systematic comparison of the tax base of a sample of localities it has been shown that real estate (houses and land) was the main element of the tax base everywhere (Alfani et al. 2020: 69-71). Similarities between localities were the result of common legal origins of taxation. The basic procedure for levying wealth taxes goes back to King Rudolf I who decreed in 1287 that local taxes should be levied by applying a uniform tax rate to the wealth owned by every household (Isenmann 2014: 522).

Common elements of taxation were probably also the result of towns modelling law codes on each other (see Chilosi et al. 2018: 663). There was an intense exchange of information about governmental practices, especially related to taxation. For example, in 1476 the city council of Freiburg im Breisgau sent its town clerk Gottschalk to 16 cities in order to study and adopt their tax systems, in terms of tax rate, taxable goods, tax oath and so on (Isenmann 2014: 524). This is not to say that tax systems were identical, but at least it shows that they had important elements in common and that comparing the data is not a purely arbitrary exercise.

The rule-of-thumb that nobles and clergy were excluded from taxation requires further comment. Since these parts of the population were potentially richer than the rest, their exclusion likely biases my inequality estimates downwards. Tentative approximations suggest that 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: 480). However, there were exceptions to the exemptions. For example, in Mühlhausen (Thr.) and Hildesheim nobles actually had to pay taxes on their personal property within the city. In Wangen, Hildesheim and Esslingen the clergy also had to pay taxes (Vetter 1910: 12, Uthmann 1957: 8, Scheurle 1960, Kirchgässner 1964: 87). Unfortunately, in the case of nobles we cannot even be sure whether they were exempt or not, because they were sometimes recorded in the tax registers with bourgeois names only. The problem of missing nobles leading to an underestimation of total wealth inequality was most likely less problematic in cities than in villages, because nobles in the Empire usually resided in the countryside and not in cities (Press 1991: 58).

Another challenge is the incompleteness of tax registers at the lower end of the distribution. Some individuals were excluded because they were too poor to be even considered for taxation. These were for example vagrants who literally had nothing. Problematic is not only that these individuals were to some extent missing. Whether they were included or not, was subject to the often arbitrary decision of tax officials. In other words, who was part of the missing group was not entirely fixed, it depended on the discretion of the official. In general, however, localities sought to extend the obligation to pay taxes to all inhabitants, including the poor (Isenmann 2014: 522), and it is important to recall that the excluded individuals did not represent "the poor" altogether. As we have seen above, tax registers covered the poor. Tiny amounts were recorded — with payments as small as one or two denarii – and even those individuals that had no taxable goods were often covered, labelled "*habnit*", "*nihil*" or "*pauper*" in the registers (all synonyms referring to individuals who had no taxable goods).[12] Those individuals that were completely excluded from the registers were

[12] In some cases the propertyless that had no taxable goods but some income had to pay a small poll tax (Dirlmeier 1978: 494).

so extremely poor that they were not even deemed worthy of recording, notwithstanding a government's interest in taxing all inhabitants (Dirlmeier 1978: 494). One might wonder how large this group was. The unsatisfying answer is that one can only speculate in the absence of alternative sources that record this particular group. However, because even very poor people were indeed recorded, a distinguished expert of the issue has concluded that the size of this particular group must have been "incredibly small" and negligible (Kirchgässner 1967: 81). Moreover, it should be noted that the inconsistency of tax records at the lower end of the wealth or income distribution is a common problem even in studies of contemporary economic inequality, especially in developing economies (see Githinji 2000).

The potential distortions caused by the very poor requires to make a trade-off: on the one hand, including propertyless households in the wealth distributions ensures that inequality indicators represent the wealth distribution in any given year as close to reality as the available sources allow it. On the other hand, excluding the propertyless altogether from the analysis ensures that inequality estimates are less likely to be punctually distorted by the potentially fluctuating prevalence of the propertyless due to arbitrarily changing recording practices (see Alfani 2015, Alfani et al. 2020). Both approaches are reasonable. I have decided to include the information for all available households, including the propertyless, for this study in order to represent the wealth distribution as complete as possible, accepting the potential distortions this may cause. Note that the potential omissions at the extremes of the wealth distribution — extremely poor, such as vagrants, and to some extent nobles — most likely lead me to underestimate total wealth inequality, but not overestimate it.[13] My estimates should, therefore, be seen as being on the conservative side. This implies that one should focus on interpreting the change of the estimates over time rather than exclusively focusing on their absolute values. Moreover, the potential omission of small groups should not conceal that practically the entirety of civilian households, and therefore the economically

[13]Note that for calculating representative inequality estimates it is not necessary to have information about every household of a community. One only needs a representative sample of households. This implies that omissions of certain individuals from some part of the wealth distribution do not necessarily bias inequality estimates. It is concentrated omissions at the margins of the wealth distribution that could lead to substantial bias in the estimates.

most interesting categories like craftsmen, peasants, and merchants, is covered by the wealth distributions.

Differences in the recording of extremely poor individuals lead to another possible challenge: one might wonder whether taxation principles changed substantially over time, which could lead to heterogenous information about the wealth distribution. Fortunately, local tax systems in preindustrial times are viewed as quite stable and as providing coherent information over time. Those that paid at least a minimum amount are recorded reliably. As discussed above, doubts about the reliability of the information explicitly concern marginal groups (Vetter 1910: 7, Kirchgässner 1967: 80, Dirlmeier 1978: 492, 498). The comparability of the tax registers over time has been exploited by several long-term case studies on the distribution of wealth in preindustrial times (Hartung 1898, Friedrichs 1979, Fügedi 1980, Bátori and Weyrauch 1982, Warde 2006).

However, one reason why there will most likely remain residual doubts about the reliability of the sources is that documents explaining local taxation practices are rarely available (Kirchgässner 1960: 127-128). This has several reasons. First, wealth taxes were levied according to stable principles that were bound to tradition. There was no need for contemporaries to write down regularly how taxes were levied. In fact, some tax registers start with a note stating only something similar to "as in 1540", when the tax register actually refers to some later year. The meaning of this remark was probably that the tax officials applied the same taxation principles as in 1540. Historians have often inferred some information about the tax systems from other types of documents, for example from remarks of individuals about their daily lives, including their duty to pay taxes, that were recorded in chronicles (see for example Vetter 1910: 4-31).

A second reason why very few actual records exist about taxation practices is probably related to the fragility of the documents. Usually, when taxation principles were recorded, this happened on the first one or two pages of a register. These are exactly those pages of archival documents that typically suffered the most from decomposition over time and that are in many cases missing. Often one can only infer from the tax register itself and the figures that it provides whether the recorded information is reliable or not (Dirlmeier 1978: 501). A careful check of how sources look like over time, of suspicious clustering of tax values or of omissions in the distribution often are the only available tools.[14] This approach has led to the exclusion of several localities from the dataset. For example, in Münster and Damme, taxes were levied as poll taxes, differentiated by profession of the household head. Calculating distributional indices based on such data would clearly be pointless.

People in preindustrial times were not keen on paying taxes. One might wonder whether tax evasion might have significantly distorted the wealth estimates in the tax registers. It is impossible to estimate tax evasion. We know only of individual cases (Sellin 1978: 116). Yet local authorities put in place a number of measures to ensure honesty in taxation matters. First, taxpayers had to swear an oath on the correctness of their tax payment. One can hardly underestimate the power of spiritual obligation in preindustrial communities to achieve compliant behavior. Secondly, even when tax assessments were completed, authorities did additional verifications, to check the correctness of households' tax estimates, declarations and payments. Thirdly, penalties for tax evasion were quite severe. They included simple penalty payments up to many times the amount of the tax duty, the dishonorable public announcement of the tax evader's name or the confiscation of the undeclared part or even of the total property of an evader. Moreover, authorities often had the right to acquire a household's property at the value that was declared for taxation (Friedrichs 1979: 98-101, Isenmann 2014: 539-541). All these measures increased the incentive not to evade taxes, but they were certainly no perfect guarantee to achieve complete honesty.

A final challenge of the analysis of tax registers concerns the plethora of local currencies in the Holy Roman Empire. Local currencies were deliberately designed to be broadly similar, but there was no common currency (Volckart 2017: 759). Matters become even more complicated

[14]Even such checks are no perfect guarantee for reliably detecting problematic tax registers. If, for example, the number of poor taxpayers in one year is suspiciously low, this does not have to indicate changes in the taxation principles. It could also be that a locality experienced high mortality or migration for some unknown reason. For example, Dirlmeier (1978: 498) claims to show the changing nature of taxing principles in 1440 Konstanz, based on the unusually low number of taxpayers compared to a previous register. Most likely he had simply overlooked the plague wave that had hit Konstanz in 1439. Unsurprisingly, the plague led to a decline of the number of taxpayers (Kirchgässner 1960: 115).

because in administrative documents not coined-, but account-money was used. Households paid their taxes with all the different coins that circulated. When tax officials recorded the tax payment, they converted the real coins directly into uniform account-money. Since these were usually no real currencies, one cannot easily determine the silver content and thus the real value. Fortunately, for calculating distributional indices one does not need the real value of all the currencies used. The only necessary information is the relationship between the coins. This relationship could be worked out from the sums of tax payments that were noted in every register, although this task was sometimes made difficult by the lack of arithmetic skills of tax officials.

It should be noted that the data from the tax registers refer mostly to the distribution of wealth, while in modern times income inequality is often the object of interest (see Milanovic 2005). Many social scientists think that wealth inequality is interesting in itself (Persson and Tabellini 1992, Alesina and Rodrik 1994, Scheve and Stasavage 2017). Some scholars even deduce wealth inequality from capitalizing information about income (Saez and Zucman 2016). Yet although wealth — or capitalized income — and income are not the exact same thing, wealth is the best and usually the only proxy for the development of income inequality in preindustrial economies. The reason is that income mainly derived from land, since 80 to 90 percent of GDP was derived from the agricultural sector. It is therefore highly unlikely that income and wealth inequality could have developed into different directions over longer time periods (Cipolla 1974: 108-109, Lindert 2014: 8, Alfani 2015: 1062). It is important to keep in mind that the data mostly refer to wealth, because it implies that interventions affecting specifically income, such as taxation or redistribution through poor relief, likely took time until they affected the wealth distribution significantly.

One objection that could be raised about the use of tax records to study inequality might be that wealth inequality based on the distribution of land, underrepresents actual inequality, since economic development in the preindustrial period involved a sectoral shift of the economy away from agriculture, towards a greater dependence on manufacturing and services (mostly commerce). This might imply that the rich no longer invested their wealth in land, but instead in houses or other goods. If this were the case, wealth registers, based on the distribution of land alone, would underrepresent the actual inequality. However, in actuality this objection is not a matter of concern for Germany. First, the tax base was quite broad in most localities, including not just land but also houses and in many cases even clothing, tools, weapons, horses, jewellery, cash, fishing rights and other forms of mobile and immobile wealth (Alfani et al. 2020: 69-71). Secondly, merchants often bought land once they became rich, due to the status that derived from it (De Vries 1976: 216-218, Press 1991: 53; see also Mandrou 1998: 46-65, 73-78). Consequently, such rich individuals would appear in the wealth tax registers, even if these were based on land only. Additionally, note that all inequality studies — in preindustrial but also in industrial and contemporary economies — are limited to those types of wealth that authorities decide to levy taxes on. The unequal distribution of those assets that are not taxed inevitably escape our attention.

Note also that preindustrial tax sources do not entail certain challenges that scholars of wealth inequality in the industrial era face. Modern wealth data usually constrain scholars to infer the wealth distribution based on a small observed part of the population because general wealth taxes rarely exist (see Albers et al. 2020: 7). Then one has to make an assumption about the likely functional shape of the distribution, which usually is the log-normal distribution (Cummins 2019: 3). In preindustrial times we can model the "real" distribution based on information from the tax registers and do not have to decide *a priori* that the distribution must follow a certain functional shape. This is possible because taxes were general wealth taxes levied on (almost) every household.

All the limits and challenges regarding the sources and their capacity to inform us about economic inequality have to be taken into consideration when interpreting the results presented below. Notwithstanding the numerous challenges, social historians unanimously agree that in the context of preindustrial Germany, wealth tax registers are the best source available to study social structure and economic inequality. In many cases they are the only sources available (Maschke 1967: 4, Eitel 1970: 6, Blendinger 1972: 35, Dirlmeier 1978: 503-506, von Hippel 2013: 18). A potential alternative source could be so-called benefice registers (*Pfründeregister*) or probate inventories, but these are considered even more biased towards the middle class (see Dirlmeier 1978: 526-529, Rebell 1983). In order to provide some estimate of the uncertainty of the inequality data, for example due to possible mistakes or omissions in the archival sources, I will employ bootstrapping techniques. These make it possible to calculate standard errors and confidence intervals of the local and aggregate inequality estimates.

2.4 Local Inequality

2.4.1 Methodology

The wealth distributions obtained from local tax sources make it possible to calculate several indicators of inequality. My main indicator is the Gini coefficient. Equation 2.1 shows how the Gini has been calculated for obtaining the main results:

$$Gini = \left(\frac{2}{(n-1)}\right) \times \sum_{i=n-1}^{1} (F_i - Q_i) \tag{2.1}$$

The number of tax-paying households is n. The position of each individual household in the distribution sorted by increasing wealth is i. F_i is the position i divided by n. Q_i is the sum of wealth of the households from position 1 to i in the distribution, divided by the total wealth of all households. The Gini is bounded between 0 and 1, which gives it a straightforward and intuitive interpretation. In the hypothetical case of all individuals owning the same amount of wealth, the Gini would take the value 0. If one individual owned everything, the Gini would be 1 (Alfani 2015: 1067).

The Gini indicator has been criticised on several grounds (Piketty 2020: 657–659; see also Atkinson 1970). It is certainly true that it does not convey a complete picture of all the details of an income or wealth distribution, but so does no other single inequality statistic. In contrast to other popular measures such as top wealth shares or Kuznets ratios, the Gini fulfils the important Lorenz consistency criterion for "good" inequality measures (Foster and

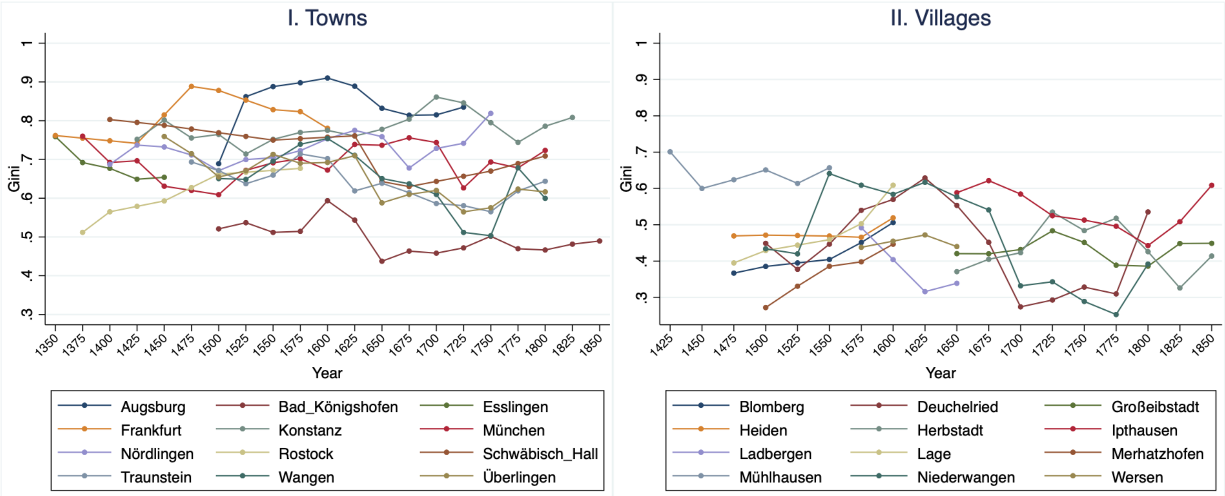
Lustig 2019: 136-137). Moreover, because the Gini is bounded between one and zero, unlike other measures as the Theil index, it has an intuitive interpretation. These properties make the Gini still the most widely used indicator in inequality studies and is the reason why also this chapter relies on it (Foster and Lustig 2019: 138). To provide a more nuanced picture of the distributions and a better intuition for certain inequality levels, wealth shares will be reported as well.

2.4.2 Results in Localities

Figure 2.3 reports the Gini coefficients for selected towns (Panel I.) and villages (Panel II.) (for reasons of clarity not all towns that are part of the dataset are included in the graph). A full list of all Gini coefficients and wealth shares is provided in the Appendix. Data for the period immediately after the Black Death are rare. Fortunately, for a few German towns such data are available. The figures for Esslingen, Frankfurt (Main) and München clearly indicate a decline in inequality during the second half of the fourteenth century. For example, in Frankfurt inequality declined by 0.031 Gini points, and in Esslingen by 0.082 points from 1350 to 1400. There is some variation until when this decline continued. Some towns show a relatively quick return to increasing inequality, at least temporarily, from as early as 1400 or 1425. This is the case for example in Frankfurt, Nördlingen or Konstanz. In other places the decline continued much longer. In München it lasted at least until 1500. An interesting exception to the pattern of generally declining inequality is Rostock, as noted already by Alfani et al. (2020: 15). Inequality increased monotonously for two centuries from 1375. It could be that inequality might have simply rebound very quickly, possibly because of reportedly booming Hanse trade at the time (Alfani et al. 2020: 15-16). Overall, during the post-Black Death period German towns seem to have followed the first phase of the general pattern of inequality development outlined above (see Rodepeter 1998: 12): inequality declined in most localities, although there were local exceptions and variations.

Panel II. shows Gini coefficients for selected villages. Unfortunately, information for the distribution of wealth in villages is scarce in the late Middle Ages. The earliest available continuous data refer to the early fifteenth century and come, for example, from a few villages around Mühlhausen (Thuringia).[15] The development of inequality in rural areas cannot be analysed with certainty right after the Black Death. The information from the Mühlhausen-villages points towards inequality decline, followed by a phase of inequality increase, similar to what happened in towns.

Figure 2.3: Wealth Inequality in Preindustrial Towns and Villages (Gini Coefficients)



Source: See the text.

From around the end of the fifteenth century inequality started to grow again in towns. In some places this happened earlier, such as Rostock, in others it started only around 1525, such as in Traunstein. The most extreme case is Augsburg, where inequality went up by 0.199 Gini points during the first half of the sixteenth century, and by 0.221 Gini points over the whole sixteenth century. Augsburg reached a peak-Gini of 0.91 in 1600. Such a stark inequality growth does not represent the experience of all towns, as for example the figures for Bad Königshofen and Überlingen show. However, the general tendency was certainly for urban inequality to grow, until about 1600, the eve of the Thirty Years' War. However, substantial inequality growth was not only an urban phenomenon. Inequality growth during the sixteenth century is clearly visible in almost all villages for which data are available. For example, in Deuchelried inequality grew by 0.252 Gini points between 1525 and 1625.

[15] These villages — St. Nikolaus, St. Peter, St. Margarete, St. Georg and St. Martin — are suburbs of the city of Mühlhausen. Due to the nature of the source the information about the distribution of wealth allows only for the calculation of a Gini for all villages taken together.

Inequality in towns and villages followed the general pattern of increasing inequality during the century or so before the outbreak of the war, as suggested by the literature (see for example Friedrichs 1979: 105-106, Robisheaux 1989: 84-89). Comparing the village-level Gini values in the peak years 1600 or 1625 with the levels in towns shows that inequality was generally lower in rural places. Villages could become quite unequal places, with several of them having Gini coefficients above 0.6, but the majority of cities was still more unequal (see also Alfani et al. 2020: 18, Wegge 2020).

A pattern of inequality development that distinguishes preindustrial Germany from all other parts of Europe emerges during the first part of the seventeenth century, during the time of the Thirty Years' War (see also Scheidel 2017: 337, Pfister 2020a: 5). This war was the most destructive conflict in early modern Europe, proportionally even more destructive than the two world wars and was mostly fought on the territory of the Empire (Ogilvie 1992: 439, Wilson 2009: 787). It is, therefore, not surprising that the Thirty Years' War left a clear mark on economic inequality in towns and villages. In some places inequality declined already from 1600, in others only from 1625 onwards. The second period from 1625 until 1650 was generally the one with the larger decline in inequality. Bad Königshofen experienced the largest decline over the first half of the seventeenth century, with a drop in the Gini coefficient of 0.156 points. Überlingen experienced the largest decline in a single period, of 0.122 Gini points between 1625 and 1650. In several towns, for example in Augsburg or Schwäbisch Hall, inequality continued to decline after the Thirty Years' War. In Wangen and Traunstein this decline even continued until the mid-eighteenth century. However, it is unclear to what extent this long-lasting decline was still the result of the Thirty Years' War or other factors. Villages show a similar picture. Inequality declined during the war, as for example in Deuchelried, Ladbergen or Wersen. But in several villages the decline of inequality lasted quite long, from the second half of the seventeenth century until well into the eighteenth century. The starkest drop experienced Deuchelried, where inequality was reduced by 0.355 Gini points between 1625 and 1700.

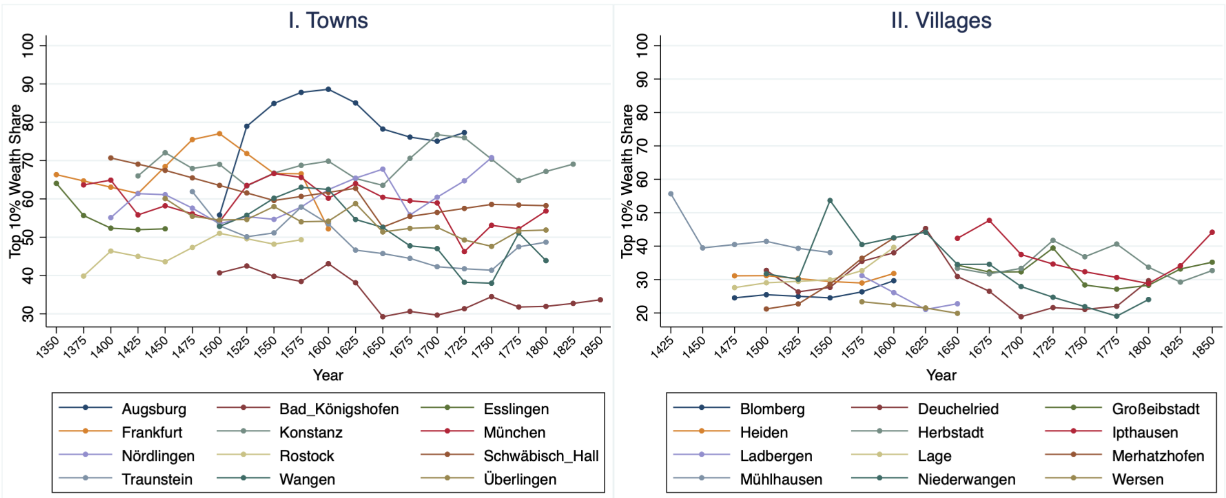
Around the beginning of the eighteenth century the fourth and last phase of preindustrial inequality development began, which lasted until the mid-nineteenth century. The general view is that inequality increased substantially during this period (Kocka 1990: 117-220, Wunder 1996: 68, von Hippel 2013: 14, Pfister 2020a: 16). The data for towns support that view to some extent. From 1700 until 1800 inequality increased in Schwäbisch Hall by 0.066 Gini points, and in Bad Königshofen by 0.031 points from 1700 until 1850. Compared to the previous century of declining inequality, the inequality-growth during the eighteenth and early nineteenth century was a clear break in the trend. Yet this inequality growth was not enough to compensate for the decline of inequality during the century of the Thirty Years' War. In villages inequality growth seems to have been more substantial, once it started. For example, in Deuchelried inequality increased by 0.261 Gini points from 1700 until 1800. However, in most places, inequality started to grow only towards the end of the eighteenth century or even only in the nineteenth, such as in Ipthausen or Großeibstadt. Just like in towns, substantial inequality growth during the eighteenth and nineteenth centuries hardly compensated for the substantial inequality reductions of the seventeenth and early eighteenth centuries.

Overall, the development of economic inequality in towns and villages seems to have followed the four phases outlined above and identified already by Alfani et al. (2020): Germany experienced a secular trend of inequality growth between the fourteenth and nineteenth centuries, interrupted by two major shocks, the Black Death (1350) epidemic and the Thirty Years' War (1618-48). However, the expanded dataset has revealed an aspect that previous studies have not fully acknowledged. The inequality-rise during the eighteenth and nineteenth centuries might have been not as intense as some previous studies have suggested (see Sabean 1990: 454-455, Kocka 1990: 117-120, Beck 1993: 233-234). Inequality growth was not pronounced enough to compensate for the huge decline after the Thirty Years' War. The aggregate figures presented in a later section will show this more clearly.

One might be concerned about attrition biasing the observed trends. It could potentially be that localities with certain characteristics drop out of the dataset. As mentioned, the inclusion of a place in the dataset depended on the availability of archival tax registers, which in turn depends mostly on whether these documents survived over time. There is only one place that I observe for almost the whole period of study: the town Schwäbisch Hall, from around 1400 until 1800. The development of the Gini coefficient in Schwäbisch Hall follows quite closely the four broad phases of inequality, although the decline of inequality after the Black Death continues somewhat longer than in most other places. Overall, the development in Schwäbisch Hall suggests that the broader trends are not significantly biased by attrition.

The Gini coefficient summarizes in an intuitive way the distribution of wealth. However, the share of wealth that was owned by different parts of the population can also be informative, because orders of magnitude become clearer (Alvaredo et al. 2013, Piketty 2020: 26). Wealth shares have not been analysed by Alfani et al. (2020).

Figure 2.4: Wealth Inequality in Preindustrial Towns and Villages (Top 10% Wealth Share)



Source: See the text.

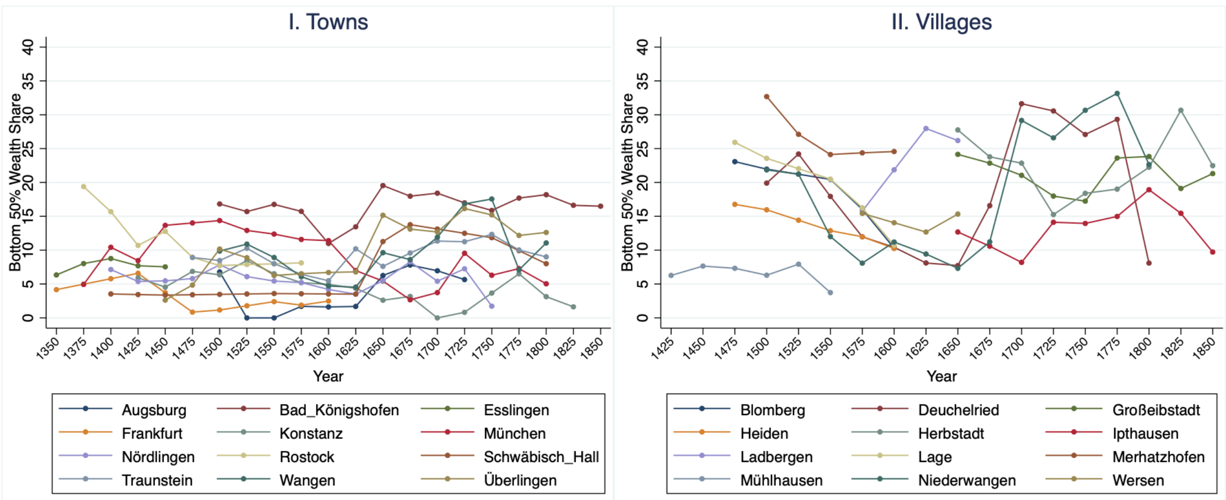
Figure 2.4 shows the share of total wealth that was captured by the Top 10 percent of the population in towns and villages. One could think of the Top 10 percent of the population as elite in economic terms, or "the rich" (Piketty 2014). The figures reveal a pattern that is almost identical to the development of the Gini coefficient: the Top 10 percent lost wealth after the Black Death, became richer during the sixteenth century, suffered heavy losses during and after the Thirty Years' War and then made some gains during the eighteenth and early nineteenth centuries, in towns and villages. There are some exceptions though.

For example, the rich in Frankfurt suffered heavy losses during the sixteenth century, of 24.85 percent of total wealth. Overall, the Top 10 percent had the greatest wealth share in the mid fourteenth and late sixteenth centuries and were relatively worse off around the end of the fifteenth and seventeenth centuries.

It is worth concentrating on the actual magnitudes of economic inequality that the Top 10 percent shares reveal. For example, Augsburg in 1600 not just had a Gini of 0.91, as mentioned above, but this also corresponded to a wealth share of the top decile of 88.61 percent. Moreover, while the Top 10 percent in towns generally had more than half of the wealth before the Thirty Years' War, this threshold was in many cases not reached anymore after the war. In contrast, the share of wealth captured by the Top 10 percent in villages was constantly below 50 percent, but almost never went below 20 percent.

The Top 10 percent are not the only interesting part of the taxpaying population. Figure 2.5 shows the wealth share owned by the Bottom 50 percent of the population. One could interpret this part of the population as representing the lower classes or the poor in economic terms (Piketty 2014).

Figure 2.5: Wealth Inequality in Preindustrial Towns and Villages (Bottom 50% Wealth Share)



Source: See the text.

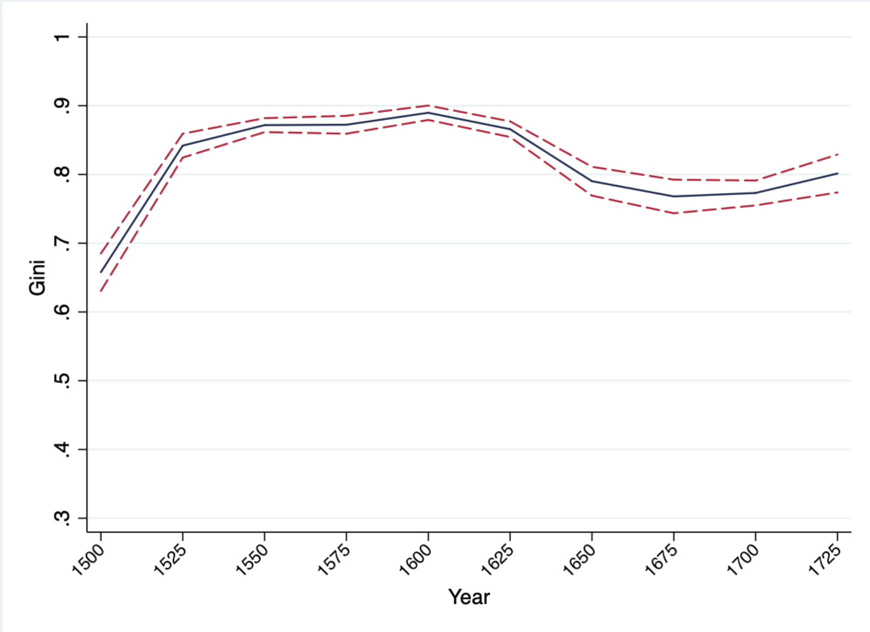
Although there are exceptions to a general pattern, it is striking that the development of the wealth share owned by this group was practically a mirror image of the Top 10 percent and of overall inequality. Poor peoples' wealth share tended to increase somewhat in several localities after the Black Death, but then fell again during the sixteenth century. For example, in Augsburg the wealth share of the Bottom 50 percent dropped to zero in 1525. This implies that at least half of the population in sixteenth century Augsburg was propertyless. The reduction of the wealth owned by the Bottom 50 percent was quite pronounced in sixteenth-century villages, too.

During and after the Thirty Years' War the Bottom 50 percent experienced substantial gains, and again tended to lose wealth in the eighteenth or nineteenth centuries, in towns and villages. The time when the poor were enjoying their highest wealth share was around the end of the seventeenth century and its worst time was on the eve of the Thirty Years' War. Overall, the wealth share of the Bottom 50 percent was quite low in towns, rising rarely above fifteen percent. In villages values were much more dispersed.

One might be wondering how uncertain the local inequality estimates are, especially in light of the discussion of the sources and the many challenges involved in their analysis. In order to provide an estimate of the uncertainty in the inequality data, I employ bootstrapping techniques to produce confidence intervals for the Gini coefficient. This does not account for all the potential distortions caused by possible omissions in the tax registers. Yet the confidence intervals provide an estimate of the effect of random variation in the sample, for example because of mistakes in the archival sources. It can be shown how dependent the Gini is on single values in the underlying distribution.

Bootstrap techniques have been employed in a number of studies of historical economic inequality (see Mills and Zandvakili 1997, Steckel and Moehling 2001, Santiago-Caballero 2011, Alfani 2021a). The procedure works the following way: for a distribution of size n in year t, one builds a resampled distribution of n elements. This is done by randomly drawing with replacement from the original distribution. I have used 200 iterations (see Alfani 2021a). These bootstrapped distributions are used to calculate Gini coefficients (Steckel and Moehling 2001: 168-169).

Figure 2.6: Observed Gini Coefficients and Confidence Intervals in Augsburg, c. 1500-1725



Source: Own calculations based on data from Hartung (1898). Gini coefficients clustered around reference years. The blue line indicates the observed Gini coefficient. The red lines indicate upper and lower bound of the 95% confidence interval.

I have applied this procedure to the case of Augsburg. Figure 2.6 shows the results of the bootstrap exercise: the blue line indicates the observed Gini coefficient (the coefficient one obtains when calculating the Gini on the actual distribution), and the red line indicates the corresponding 95 percent confidence interval. The interval is quite tight. This has two implications: first, the uncertainty of the Gini estimate, caused for example by potential omissions in the tax registers, is small. Secondly, the values of the confidence intervals do not overlap. This means that inequality changes observed in Augsburg — inequality growth during the sixteenth century and decline during the seventeenth century — are statistically significant. The re-distributions that the Gini coefficient suggests are real and not the result of random variation in the sample. Overall, the inequality estimates seem robust to this kind of statistical test.

2.5 Aggregate Inequality

In the previous section inequality development has been discussed in a number of localities. It has already been suggested that inequality and wealth shares of different parts of the population followed broader trends, notwithstanding local exceptions. This section provides a clearer picture of these broader trends. This is accomplished by aggregating inequality across all places in preindustrial Germany for which data are available. Aggregating the local indices does not only help to identify broader patterns of development. It also makes it possible to account for unobserved time-invariant heterogeneity of localities, that might for example be due to differences in the tax system.

It would certainly be preferable to aggregate inequality at the level of the main territories within the Holy Roman Empire. I propose this aggregation for such a vast area because the dataset – although being larger than the database of any other published study on preindustrial inequality – is small in relation to the number of political entities within the Empire. The Holy Roman Empire consisted of more than 300 Imperial estates. For none of these territories are enough inequality data available to aggregate them in a meaningful way. The aggregate results likely hide regional variation.

2.5.1 Methodology

The published literature offers two substantially different methodologies for aggregating inequality data. I briefly outline the main pros and cons of these methods to motivate my approach to aggregation. One approach is the "EINITE-method", employed by members of the EINITE-group[16] that have published inequality series for several parts of preindustrial Europe (see Alfani 2015, Alfani and Ammannati 2017, Alfani and Di Tullio 2019). It is analogous to the method discussed by Milanovic (2005) to calculate "weighted international inequality" for different contemporary countries and similar to the method employed by Nicolini and Ramos-Palencia (2016) in their work on Old Castile in the eighteenth century. A second method that could be adapted to aggregate the heterogeneous local Gini indexes is the one used by Clark (2005: 1322–1323, see also Pfister 2017) to aggregate wages in preindustrial England.

Both methods have advantages and disadvantages. The EINITE-method consists of sampling

[16] See http://www.dondena.unibocconi.it/wps/wcm/connect/cdr/centrodondena/home/research/einite.

local wealth distribution, based on which one overall distribution of wealth is modelled, which is representative of the area under study in a given year. This distribution can then be used to compute almost any conceivable inequality indicator beyond the Gini index, such as Theil indices, wealth shares or polarization measures. As shown above, alternative measures can potentially be very informative in addition to the Gini index. In contrast, the "Clark-method" would be a shortcut that leads only to an aggregate time series of a single inequality indicator, such as the Gini coefficient. However, there is a downside to the EINITE-method too. The result of that method is potentially sensitive to sample variation. Since localities have different inequality levels, adding or losing a locality might distort the aggregate distribution and the indices that were calculated based in that distribution. This is only a minor issue as long as the number of localities from which one obtains sample distributions is large, but it could potentially become a serious problem when sample sizes are small.

Adapting the Clark method to the data structure of this study leads to aggregating the heterogenous local inequality data by using unbalanced-panel regressions with locality fixed effects and estimate the parameters on year indicators. The implicit assumption behind that approach is that averaging local inequality measures produces a decent representation of the overall distribution of wealth in a given area. The Clark-approach aggregates local inequality values to a dimensionless index of inequality-change. There are two downsides to this approach. First, the outcome is only an average of local values of one inequality indicator and not a wealth distribution based on which one could calculate a variety of inequality indicators. Secondly, the Clark-method needs a reliable calibration value of the indicator that one wishes to aggregate, for example from the time when representative national statistics began. Obtaining such a calibration value is usually not a problem for studies of wages or GDP (see Pfister 2020a). Yet it is a serious problem when studying inequality in preindustrial Germany because the earliest national wealth inequality estimate becomes available at the turn of the twentieth century, long after my data end, and then refers to the top of the wealth distribution only (see Albers et al. 2020). However, the major advantage of the Clark method is that it is much less sensitive to changes in sample variation when inequality indicators have different levels.

My approach to aggregation is, therefore, to employ a mix of the EINITE- and Clark-methods in order to combine the merits of both approaches. I apply the Clark-method to obtain dimensionless estimates of wealth-distribution and inequality change. This avoids distortions in the indices due to sample variation. But before doing that, I apply the EINITE-method to model the wealth distribution for the year 1575, the year where my dataset has the highest density (see Table 2.1 above). This provides me with a complete wealth distribution for the year with the densest and most reliable data. Based on that reliable distribution I calculate the Gini index and wealth shares, which will then be used to calibrate the set of regression-based time series of inequality indicators obtained with the Clark-method.

I first explain how I have modelled the distribution for the calibration year 1575. I follow the procedure outlined by Alfani et al. (2020) for the case of preindustrial Germany.[17]

The first step to aggregation is to model for every sample community a fictitious distribution of 100 elements (or fictitious households). Each fictitious household receives a wealth value. These wealth values come from the wealth shares that have been calculated for every community. For example, the first ten fictitious households (1-10) in Augsburg in 1575 get as value the wealth share that has been calculated for the first decile of the population in Augsburg in 1575, the next ten fictitious households (11-20) get as value the wealth share of the second decile of the population and so on. The top decile (households 91-100) is modelled in greater detail, based on information about the richest Top 10 percent, Top 5 percent and Top 1 percent of the tax-paying population. It is important to model the top decile in greater detail because the upper strata of the distribution are empirically decisive for the overall distribution.

One might ask why we take the wealth shares of the population deciles and not the actual wealth of households. Taking the actual wealth is not possible because one cannot directly

[17] For further step-by-step explanations of the EINITE-method for aggregating local inequality data see Alfani (2015) and Alfani and Ryckbosch (2016: Appendix D).

compare the taxable wealth of different communities because it is expressed in different currencies that cannot be converted into a common one. Even the most comprehensive dataset on currencies in preindustrial Germany (see Boerner and Volckart 2011) does not allow to convert for the whole period of study and all communities the plethora of local currencies that characterised preindustrial Germany. Allocating each fictitious household a value based on the wealth share of the respective decile circumvents the problem of incomparable currencies. However, the downside of this circumvention is that we assume implicitly that every community had on average the same per-capita wealth. This is unlikely to be true and we will weight the aggregate distribution (see below) to make more realistic assumptions about wealth differences between communities.

These fictitious households of sample communities are then collapsed into a rural distribution, containing the villages, and an urban one, containing larger towns and cities. I distinguish rural from urban communities based on whether or not a locality has an entry in the *Städtebuch* for Austria and Germany (Keyser 1939, 1941, 1952, 1954, 1956, 1957, 1959, 1962, 1964, Keyser and Stoob 1971, 1974, Baltzarek et al. 1973), the ultimate encyclopaedia for towns and cities in the Holy Roman Empire.

In order to arrive at a combined distribution of rural and urban places, two weighting issues need to be addressed. First, it has to be considered that rural communities had a lower per capita wealth than cities. For Germany, Alfani et al. (2020: 38) have calculated that the rural-urban wealth ratio was on average 21 percent in the fifteenth and sixteenth centuries, which is similar to what has been found in Tuscany (Alfani and Ryckbosch 2016: Appendix D). This is to say that a rural household, on average, had approximately one fifth of the wealth of an urban household. All rural households are weighted with this factor.

Secondly, one has to take into account that only a small share of the German population lived in cities in preindustrial times. The majority lived in the countryside. To address this issue, I have weighted urban and rural places with the urbanization rate (threshold 5,000 inhabitants) so that they reflect the actual share of people living in these different environments. According to the most recent estimates Germany had an urbanization rate of

10 percent in 1500 and also in 1800 (Pfister 2020b: 16). An urbanization rate of 10 percent has therefore been assumed, which implies that for every city, nine rural towns enter the combined distribution.

Note that it would be desirable to account also for regional wealth differences in the aggregate wealth distribution. It is likely that, for example, the Rhineland was more prosperous than Pomerania in the early modern period. In order to account for such differences in economic development across the regions of preindustrial Germany, one would need regional GDP estimates, wages or urbanization rates for the year 1575. Unfortunately, at the current state of research, no systematic measure of regional economic development is available for the sixteenth century. The earliest data available refer to the beginning of the nineteenth century (see Pfister 2020b: 18). Germany experienced substantial changes in its economic geography during the early modern period (Schilling 1994: 72-81). It would therefore be unrealistic to assume nineteenth-century prosperity levels to reflect accurately the situation two and a half centuries earlier. Differences in regional prosperity can, therefore, not be considered for modelling the wealth distribution of 1575.

After having weighted wealth values of the fictitious households with the rural-urban wealth difference and the urbanization rate, we merge the rural and urban distribution into one aggregate distribution for 1575.

Based on the combined aggregate distribution of wealth for 1575, all kinds of inequality indicators can be calculated, such as the Gini coefficient. One might be wondering how uncertain this aggregate inequality estimate is. As for the local Gini estimates, there could be random variation in the sample, for example because of mistakes in the archival sources, mistakes in the transcription of the sources or of imprecisions in independently estimated variables that affect the composition of the aggregate distribution, such as the urbanization rate. The answer to that question is particularly challenging for aggregate inequality estimates, because aggregating the local distributions in a given year yields only one total distribution. However, it is possible to employ the same bootstrap techniques as in the previous section to produce resampled distributions, calculate a standard error and confidence intervals. Again, for a distribution of size n in year t (1575 in this case), I build a resampled distribution of n elements by randomly drawing with replacement from the original distribution and calculate Gini coefficients on these resampled distributions (Steckel and Moehling 2001: 168-169).

Table 2.2: Observed Gini Coefficient and Bootstrap Statistics for Aggregate Distribution of 1575.

Observed Gini	Replications	Bootstrap SE	Lower B. 95% CI	Upper B. 95% CI
0.721	200	(0.004)	0.712	0.729

Notes: For Gini calculation and bootstrapping procedure see text.

Table 2.2 shows the results: The observed Gini coefficient (the coefficient one obtains when calculating the Gini on the actual aggregate distribution), the bootstrap standard error and the corresponding 95 percent confidence interval. The window in which the Gini fluctuates is quite tight, of 0.017 Gini points only, or of 2.36 percent of the observed Gini. The implication is that the fluctuation of the Gini in 1575 that can be attributed to random variation in the sample is quite small.

Based on the aggregate distributions I have calculated not only the Gini coefficient, but also the wealth shares of the first to the ninth decile, the shares of the Top 10 percent, the Top 5 percent, and the Top 1 percent, for the year 1575. These figures, from the year where my data is the densest, are my anchor-values. The next step is to project these values backward and forward in time, with evidence from years where the data are less dense. This projection of aggregate Gini- and wealth-share change estimates is obtained with the adapted Clark-method (see Clark 2005: 1322–1323).

In order to obtain time series of aggregate inequality- or wealth-share-change I estimate unbalanced-panel regressions of the following form:

$$Inequality_{i,t} = \alpha_i + \sum_{t=1350}^{1850} + \beta_t Year + \epsilon_{i,t} \tag{2.2}$$

The dependent variable is the respective *Inequality*-indicator (Gini as well as top or bottom wealth shares) in locality i in year t (1350 to 1850, in steps of 25 years). These are the same unbalanced panel of local inequality indicators that I have presented in the previous section. α_i is a full set of locality fixed effects, which capture time-invariant locality-specific characteristics. The aim is to estimate the parameters on the $Year$-dummies (1350 to 1850). These parameters give me the aggregate change of the respective inequality indicator, net of time-invariant locality-specific characteristics. The omitted reference category is the calibration year 1575.

As a final step, the set of aggregate Gini or wealth-share-change time series are calibrated to the respective values in 1575. The next section reports the results. As with all attempts to reconstruct hypothetical "national accounts" for the preindustrial era, the results have to be considered tentative. They are only approximations of the development of inequality in preindustrial Germany. Future scholarship will hopefully refine the methods and expand the data base in order to increase the accuracy of the estimates.

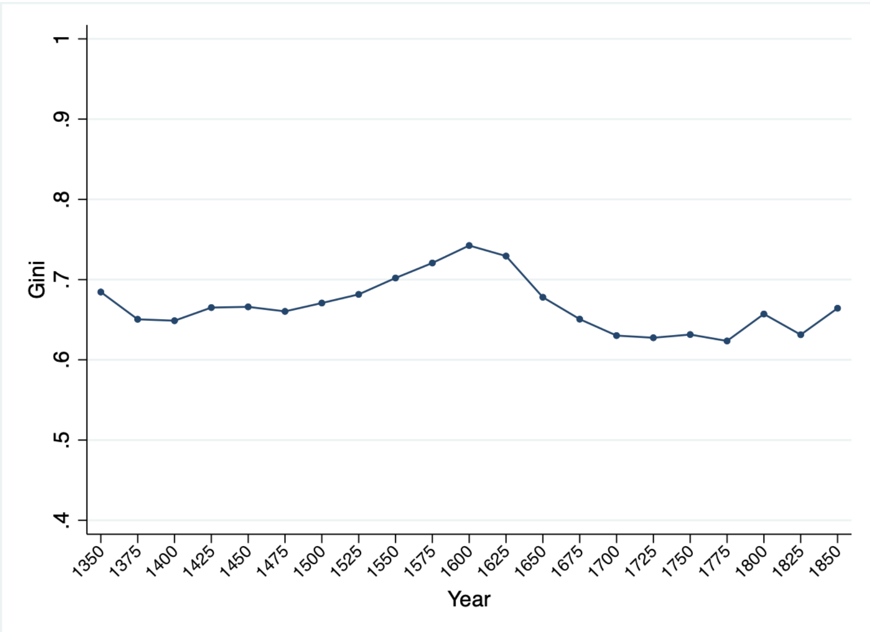
2.5.2 Aggregate Results

Figure 2.7 reports aggregate wealth inequality for Germany, from the Black Death until the eve of the Industrial Revolution, expressed in Gini coefficients. The aggregate series is in line with the results from the localities reported above, but the general trends — the four broad phases of preindustrial inequality development: a tendency for inequality to grow between the fourteenth and nineteenth centuries, interrupted by the Black Death and the Thirty Years' War — are visible more clearly. The reconstruction shows that Germany started with a relatively high Gini value of 0.685 in 1350. This was followed by a period of substantial inequality decline, of 0.036 Gini points until 1400, followed by a period of stagnation. From about 1475 onwards a long phase of almost monotonous inequality growth began which ended only in 1600 and during which Germany experienced an increase of inequality of 0.082 Gini points. In 1600 we observe the highest inequality level of the whole period under study, of 0.742 Gini points.

During the Thirty Years' War and the half-century that followed, Germany experienced a massive decline of inequality, by 0.112 Gini points. This decline was more than three times as large as the decline after the Black Death. This decline and the following years of stagnation brought Germany to the lowest Gini level of the whole period under study. Until the mid-eighteenth century, inequality stagnated, but then started to grow again. In 1800 the Gini coefficient was at 0.657 and, after a brief decline, at 0.664 in 1850.

Scarcity of data from the mid-eighteenth century onwards led Alfani et al. (2020) to avoid reconstructing an aggregate series for that final period. Thanks to new archival sources, this study has some data available for that period. Still, the estimates for the first half of the nineteenth century are based on few observations and the results are therefore very tentative. The drop of inequality between 1800 and 1825 is most likely not representative of a general trend because only one community actually experienced a substantial drop of inequality in that year. Due to the sparse sample for these years, the development in that community dominates the aggregate trend.

Figure 2.7: Aggregate Wealth Inequality (Gini Coefficients)

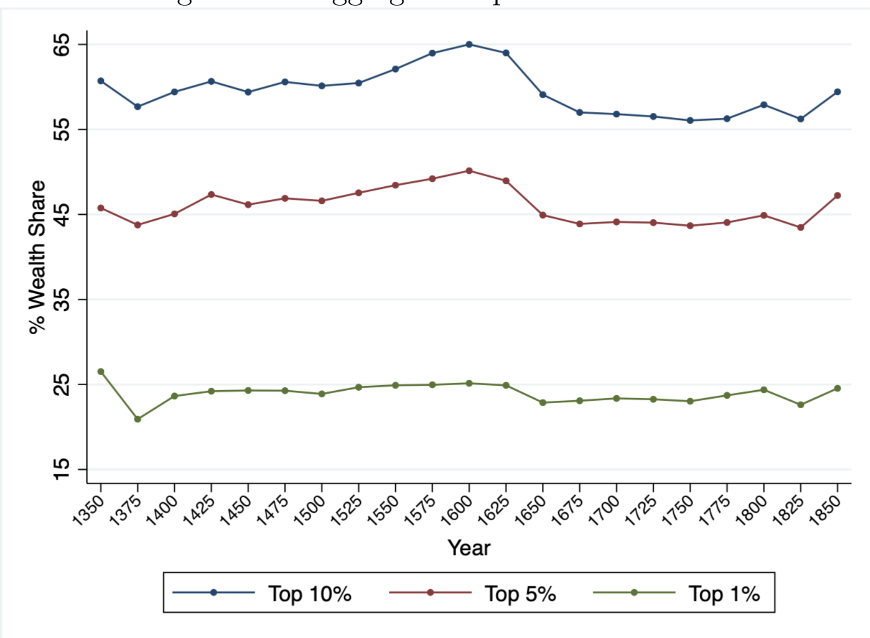


Source: See the text.

Figure 2.8 reports the wealth shares of, the Top 10 percent, the Top 5 percent and the Top 1 percent of the population, the rich strata or elites in economic terms (Piketty 2014). Note that the line of the Top 1 percent seems flatter because of a scale effect. They have lower levels of wealth compared to Top 5 percent and Top 10 percent, and the variation they experience, therefore, seems smaller.

Unsurprisingly, the figures roughly follow the development of inequality as measured with the Gini coefficient. Upper classes enjoyed the highest wealth shares in the mid-fourteenth century and on the eve of the Thirty Years' War. In 1600 the Top 10 percent of the population owned 65 percent of the total wealth. The time when they owned the smallest share of wealth was in the first part of the eighteenth century. Notwithstanding some growth of the wealth share in the second half of the eighteenth and early nineteenth centuries, the economic elites could not recover the losses they suffered during the seventeenth century. Note that the Top 1 percent of the population owned between 27 and 21 percent of the total wealth, which is substantial considering how small their population share was.

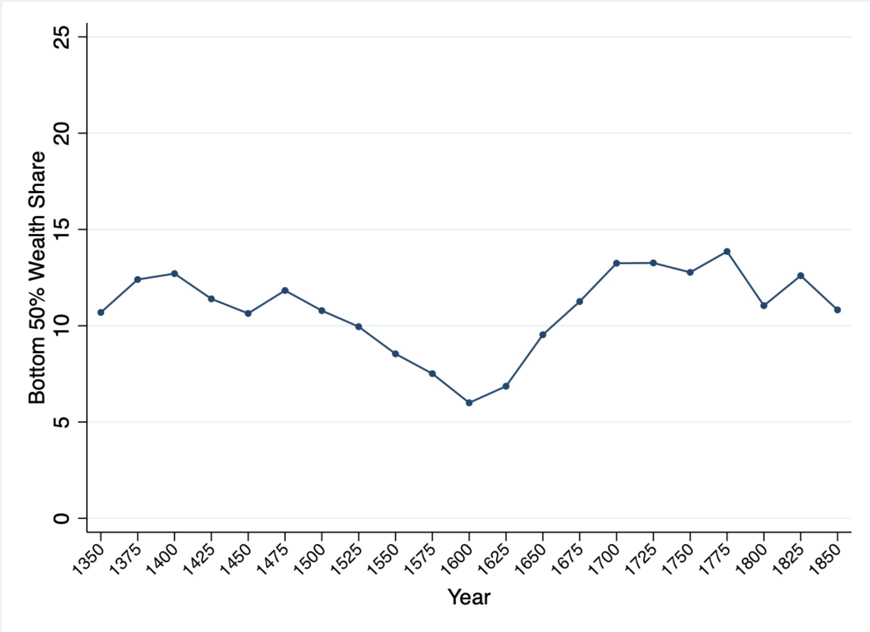
Figure 2.8: Aggregate Top Wealth Shares



Source: See the text.

Figure 2.9 reports the wealth shares of the Bottom 50 percent of the population, which one could interpret as the lower class in economic terms, or the poor (Piketty 2014). These parts of the population experienced a wealth-share development that was quite different from that of the upper class. The early fourteenth century and the beginning of the seventeenth century were low points and the early to mid eighteenth century the high point during the period under study.

Figure 2.9: Aggregate Bottom Wealth Shares



Source: See the text.

Note how dramatic the losses were that the poor suffered from the end of the 15th century until the beginning of the seventeenth century: between 1475 and 1600 they lost 49 percent of their relative wealth.

2.6 Exploratory Analysis

The previous sections have presented the core of this chapter, local and aggregate estimates of wealth inequality. This leads to further questions: what were the characteristics of more or less unequal places? In what kind of communities were the poor poorer and the rich richer? Germany's high degree of political decentralization might open up interesting avenues to explore. Within the Empire, one finds Protestant and Catholic communities, in the west weaker feudal institutions and in the east the second serfdom, everywhere free Imperial cities alongside ecclesiastical territories, to name a few of the possible characteristics that could be relevant for understanding inequality. However, it is not claimed that such an exploratory analysis shows causal relationships. This section only starts mapping the contours of the issue, hinting at possibly fruitful directions for further, more profound studies. There exist many theories about what determined economic inequality, but most are hypotheses, backed by little or circumstantial evidence. This has recently led an expert in the field to conclude that for preindustrial times, "we have at best some *guesses* [italics added] about the forces that might explain changes in inequality" (Milanovic 2018: 1031). This suggests that it is worth beginning to examine the subject.

2.6.1 Data and Methodology

This section employs panel regressions to explore the relationship between wealth inequality and a number of fundamental economic, geographic, and institutional characteristics. In a micro-econometric setting of this kind one would typically employ unit-fixed-effects regressions. However, since I am interested not only in differences *within* localities but also in those *between* localities, I follow the correlated random effects approach, sometimes also referred to as "hybrid" approach (Wooldridge 2002: 290, Greene 2012: 421, Bell and Jones 2015: 144). This approach is appealing because it makes it possible to take advantage of random and fixed effects and include time-variant and time-invariant variables. I estimate variants of the following specification:

$$Inequality_{i,t} = \alpha_i + \pi_t + \boldsymbol{\beta' X_{i,t}} + \boldsymbol{\gamma' \overline{X}_{i,t}} + \boldsymbol{\delta' Z_i} + \tau_i + \upsilon_{i,t} \tag{2.3}$$

The dependent variable is some inequality indicator in locality i and year t (= 1350, 1375,... 1850). The inequality measures are the Gini coefficient, the wealth shares of the Bottom 50 percent and the Top 10 percent. α is a constant and π_t are a full set of time fixed effects. $\boldsymbol{X_{i,t}}$ is a vector of locality-level time-variant controls, such as the log-population size. $\boldsymbol{\overline{X}_{i,t}}$ i is a vector of the locality specific time averages of these variables. By including the time averages the specification controls for the average level of the time-variant variables, while estimating the partial effect of these variables. β is the coefficient of interest. It is a fixed-effects estimate of the time-variant variables that are part of the vector $\boldsymbol{X}$. $\boldsymbol{Z_i}$ is a vector of time-invariant variables, such as longitude and latitude, estimated by random effects. The specification has a composite error term, consisting of $\tau_i \upsilon_{i,t}$, a time-constant unobservable, and of $\upsilon_{i,t}$, the idiosyncratic shocks. The standard errors are robust, clustered at the locality level in order to account for the possibility of serial correlation in the error term. The panel is unbalanced as information about inequality is not available for all years of the analysis for every community.[18]

The Appendix provides details about the coding of variables. The first right-hand-side variable is the log of a locality's population size. Many studies of preindustrial inequality consider population growth a fundamental determinant of rising inequality (Brenner 1976: 32, Alfani and Ryckbosch 2016, Pfister 2019: 19). Population size, important in itself, can also proxy the economic success of a community in the absence of other estimates of economic prosperity, such as real wages or per capita GDP, which could have also been an inequality driver (van Zanden 1995).

A group of variables controls for geographic characteristics that could be relevant for explaining inequality, usually through demographic, economic or institutional channels. Longitude and latitude capture geographical patterns. For example, it has been hypothesized that differences in the strength of manorialism had an impact on inequality and that agricultural relations under the second-serfdom were particularly exploitative in the east of Germany (Malinowski and van Zanden 2017: 378-379, Milanovic 2018: 1041-1042, Ogilvie 2014b).

A variable that indicates agricultural potential controls for the possibility that differences in agricultural productivity could have an impact on inequality (Allen 1992: 283-302). The log-distance to a major river and a variable that indicates whether or not a locality was located at the seaside, should reveal if there was a relationship between inequality and market access (van Bavel 2016: 261-263).

Several institutional aspects might have been important for explaining inequality. A first variable captures the novel institutions, for example in terms of social welfare provision, introduced by the Protestant Reformation (Basten and Betz 2013). Another aspect could have been a locality's proximity (measured as log-distance) to a university. Universities

[18]Note that the availability of inequality data depends principally on whether or not the relevant archival documents have survived over time. It is therefore reasonable to assume that the missing observations are random and uncorrelated with the error term of the model.

formed human capital, but usually only for very few elite individuals. For example, they provided political elites with trained officials. These could be potentially relevant aspects for explaining economic inequality (van Zanden 1995: 658-661, Ogilvie 1992: 426, Dittmar 2019).

Yet another institutional variable indicates whether a locality was a member of the German Hanse. The Hanse was a commercial and political association that included between 180 and 200 cities. These cities could be seen as being particularly oriented towards commerce and long-distance trade (Ogilvie 2011: 20). Commerce and trade have been considered major drivers of economic inequality in the preindustrial period (Puga and Trefler 2014: 755).

An additional variable indicates whether a locality was a city (or town), in contrast to being a village. This controls for structural differences between rural and urban communities, such as the presence of certain institutions, a bureaucracy and certain high-skill, high-salary jobs. It could be that these differences are not captured by the population size but it is likely that they were related to economic inequality (van Zanden 1995: 645-646, Alfani and Ammannati 2017: 1096-1097).

Ultimately, the Holy Roman Empire was probably the most complex political system of premodern Europe, characterized by a high degree of constitutional decentralisation and variation (Chilosi et al. 2018: 662-666). One might be wondering whether aspects of this constitutional diversity were related to higher or lower economic inequality. It is difficult to systematize the territories within the Empire according to criteria that are historically sound and for which data are available. I have systematized localities based on the status of their Imperial estate in the Imperial constitution.[19] I differentiate between electorates, ecclesiastical polities (that is, territories governed by bishops other than the electors, by prelates or abbesses[20]), mundane principalities, counties and Imperial cities.

[19]Imperial estates were those political authorities that represented their polities (territories and Imperial cities) at the *Reichstag* (Imperial diet). See Oestreich and Holzer (1973) for an overview of the Imperial estates. For the time before 1422, when Imperial estates were for the first time defined, I have classified localities based on the rank that their Imperial estate would later obtain. Not all ranks of Imperial estates are part of the dataset or are sufficiently numerous to test them as an independent category.

[20]The small number of observations within each of the categories made it necessary to collapse the three different ranks into one group.

Time fixed effects control for shocks that have an impact on all localities. These could be, for example, climatic fluctuations, constitutional changes in the Empire but also the Thirty Years' War.

One might be wondering why I do not use the household-level data to construct a large panel for exploring the correlates of inequality. The main problem is, again, that we cannot convert wealth estimates across time into a common currency for all the communities. Moreover, wealth registers give information about wealth, but rarely about other household-level characteristics. One would lack individually varying covariates. Additionally, matching individual households across tax registers is difficult. Names were not written uniformly in different tax registers. These obstacles make it not generally possible to link individuals over time in a meaningful way.

Table 2.3 provides summary statistics of all variables that have been employed in the analysis. They reveal, for instance, that about half of the observations come from communities with city-status, but only a small number of places was located at the seaside, had Hanse membership, or was part of an Electorate.

2.6.2 Empirical Analysis

Table 2.4 reports the first set of results. All specifications include time fixed effects and locality specific averages of the time-variant variables. Each specification in the first three columns has a different inequality indicator as dependent variable: the Gini coefficient (Column 1) indicates overall inequality, the wealth share of the Bottom 50 percent (Column 2) represents the poorer half of the population, and the wealth share of the Top 10 percent (Column 3) represents the economic elite or the rich. Higher inequality is indicated by positive coefficients when the outcome is the Gini or the Top 10 percent wealth share, and by negative coefficients when the outcome is the Bottom 50 percent wealth share. In Columns 4 to 8 I test whether the type of Imperial Estate a community belonged was related to inequality.

Table 2.3: Summary Statistics

Variable	(1) N	(2) Mean	(3) SD	(4) Min.	(5) Max.
Gini	519	0.563	0.152	0.201	0.890
Bottom 50% Wealth Share	519	13.64	7.915	0	36.93
Top 10% Wealth Share	519	43.04	16.07	15.37	86.66
Log-population size	519	7.242	1.536	3.584	10.72
Longitude	519	9.885	1.251	7.594	13.75
Latitude	519	49.89	2.024	47.50	54.32
Seaside locality	519	0.0385	0.193	0	1
Log-distance to river	519	3.599	1.677	-1.185	5.289
Agricultural potential	519	0.676	0.210	0.196	0.962
Protestant Reformation	519	0.360	0.481	0	1
Hanse member	519	0.0886	0.284	0	1
City	519	0.576	0.495	0	1
Log-university distance	519	4.130	1.323	-2.303	6.156
Electorate	510	0.0627	0.243	0	1
Ecclesiastical polity	510	0.110	0.313	0	1
Mundane principality	510	0.233	0.423	0	1
County	510	0.180	0.385	0	1
Imperial city	510	0.414	0.493	0	1

Notes: Own calculations.

I interpret the results variable-by-variable, across the different specifications, as the results generally point into the same direction. The log-population size was generally related positively to inequality, but never more significant than at the 10 percent-level. Moreover, there is no significant relationship with the wealth shares of the poor and rich. This result is somewhat surprising, because several studies that adopt a Malthusian interpretative frame of the preindustrial German economy have claimed that growing population size was the most important factor for explaining inequality trends in preindustrial Germany (Pfister 2019, 2020a). Similarly, if we interpret the population size as a proxy for economic development, we would probably expect stronger results, as economic progress is seen as a major driver of rising inequality, especially through wealth concentration at the top (see Kuznets 1955, van Zanden 1995, Ray 1998, Deaton 2015). However, the results are in line with the recent empirical findings of Alfani and Di Tullio (2019).

The coefficients on longitude and latitude indicate that there were some significant geographic patterns. Overall inequality and top wealth shares were lower in the north, or, relatively higher in Southern communities.

There is no significant evidence that agricultural potential was related to higher inequality or a specific part of the population being relatively better or worse off (see Allen 1992: 283–302), when other factors such as the population size are controlled for.

The indicators for market access — log-river distance and seaside locality – show somewhat contradictory results. Being closer to a river was related to lower inequality in some specifications (Columns 2 and 4). Instead, inequality in seaside localities was not significantly different from zero, but the coefficients point towards a potentially positive relationship. One could interpret this result as evidence that the *type* of market access available to communities mattered for inequality, when controlling for other factors.

A number of institutional variables are statistically insignificant, but their coefficients consistently point into certain directions, regardless of the inequality indicator used, which suggests that further research might unveil interesting dynamics. The introduction of the Protestant Reformation and proximity to a university might have been related to higher inequality, while communities that were part of the Hanse might have been less unequal.

The clearest results of the analysis come from the indicator of whether or not a community had city-status: urban localities were significantly more unequal than rural places as measured with the Gini, the poor had lower wealth shares, and the Top 10 percent had higher wealth shares. This result is consistent with the idea that structural differences between villages and towns, such as the presence of a bureaucracy with highly paid officials, were related to higher inequality, even when conditioning on the population size and other factors (see Alfani and Ammannati 2017: 1096–1097).

The final variables in Table 2.4 all refer to the status of communities' Imperial estate in the Imperial constitution. For this part of the analysis (Columns 4 to 8) I drop all observations for the time after the end of the Empire in 1806 and take the Gini coefficient as dependent variable in all specifications. Moreover, I treat Imperial status as time-invariant variables.

Table 2.4: Characteristics of Unequal Communities (1)

	(1) Gini	(2) Bot. 50%	(3) Top 10%	(4) Gini	(5) Gini	(6) Gini	(7) Gini	(8) Gini
Log-population size	0.035*	-2.096	2.777	0.036*	0.038*	0.035*	0.038*	0.037*
	(0.020)	(1.356)	(1.842)	(0.020)	(0.020)	(0.020)	(0.020)	(0.020)
Longitude	-0.001	0.196	0.119	-0.003	-0.001	0.000	0.004	-0.000
	(0.009)	(0.498)	(0.846)	(0.009)	(0.009)	(0.009)	(0.011)	(0.009)
Latitude	-0.017**	0.801	-1.938**	-0.018**	-0.018**	-0.016*	-0.018**	-0.015
	(0.009)	(0.504)	(0.819)	(0.009)	(0.009)	(0.009)	(0.009)	(0.011)
Agricultural potential	0.058	-3.612	5.871	0.061	0.054	0.061	0.070	0.052
	(0.046)	(2.894)	(4.034)	(0.048)	(0.046)	(0.045)	(0.044)	(0.051)
Log-distance to river	0.013	-0.941*	0.747	0.014*	0.013	0.012	0.012	0.014
	(0.009)	(0.489)	(0.760)	(0.008)	(0.009)	(0.008)	(0.009)	(0.009)
Seaside locality	0.030	-1.399	1.926	0.033	0.039	0.037	0.032	0.023
	(0.058)	(3.076)	(6.105)	(0.058)	(0.063)	(0.057)	(0.055)	(0.063)
Protestant Reformation	0.013	-0.551	1.563	0.016	0.014	0.014	0.013	0.013
	(0.018)	(1.048)	(1.960)	(0.018)	(0.018)	(0.019)	(0.018)	(0.017)
Log-university distance	-0.009	0.643	-0.430	-0.009	-0.009	-0.009	-0.009	-0.009
	(0.006)	(0.446)	(0.607)	(0.007)	(0.006)	(0.007)	(0.006)	(0.006)
Hanse member	-0.026	0.118	-4.823	-0.023	-0.028	-0.033	-0.025	-0.030
	(0.062)	(3.277)	(6.305)	(0.062)	(0.063)	(0.060)	(0.062)	(0.061)
City	0.103**	-5.701**	9.862***	0.100**	0.098**	0.110**	0.110**	0.109**
	(0.042)	(2.511)	(3.645)	(0.042)	(0.043)	(0.044)	(0.047)	(0.048)
Electorate				0.020				
				(0.030)				
Ecclesiastical polity					0.016			
					(0.032)			
Mundane principality						-0.022		
						(0.026)		
County							0.035	
							(0.049)	
Imperial city								0.012
								(0.041)
Locality-specific averages	YES	YES	YES	YES	YES	YES	YES	YES
Random Effects	YES	YES	YES	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES	YES	YES	YES
Observations	519	519	519	510	510	510	510	510
Communities	75	75	75	75	75	75	75	75
Betw. R^2	0.595	0.469	0.700	0.596	0.591	0.609	0.598	0.601

Notes: Estimation method is GLS. Standard errors clustered at locality level in parentheses. Locality specific averages have been included for the time-variant variables "log-population size", "Protestant reformation" and "Log-university distance". *** p<0.01, ** p<0.05, * p<0.1

Apart from very few exceptions — such as the capture of the Imperial city Konstanz by Austria in 1548, or the switch of elector dignity within the Wettin dynasty in 1547 — Imperial status was generally time-invariant for the places in my dataset. The coefficients are all insignificant, although the point estimates are not precisely zero. The implication is that whether a community was part of an electorate, ecclesiastical polity, mundane principality, county or Imperial city did probably not matter a great deal for inequality, in comparison to all other types of Imperial status and when controlling for other factors. Moreover, controlling for Imperial status does not lead to major changes in other coefficients of the analysis.

One might be wondering to what extent the correlations revealed in Table 2.4 hold over shorter time periods. The previous results covered a very long period and it could be that the results are different for sub-periods. I have divided the dataset into four cross-sections to study whether the correlations hold in these sub-periods. Dividing the dataset reduces the number of observations. One should therefore not be overconfident of finding significant results. Since this is a cross-sectional analysis, I have not included the time-averages of the time-variant variables.

Table 2.5 shows some interesting heterogeneity over time. Some coefficients change sign over time, and show almost surprising significance levels, such as the highly significantly positive estimates of the distance to a university and Hanse membership in the seventeenth century. It is unclear how robust these results are and should probably be cautious when interpreting them.

However, two patterns seem to appear clearly from the results and require further comment. First, the population size and agricultural potential, two indicators of economic development, are insignificant until about 1600, but turn significantly positive afterwards. This could suggest that economic progress became a better predictor of inequality from the seventeenth century on. Second, while cities were clearly more unequal than villages until about 1600, this difference, at least temporarily disappears after 1600. This was probably a consequence of the Thirty Years' War, which led to a larger inequality decline in urban than in rural places (Alfani et al. 2020).

Table 2.5: Characteristics of Unequal Communities (2)

	1350-1475	1500-1575	1600-1675	1700-1850
	(1)	(2)	(3)	(4)
	Gini	Gini	Gini	Gini
Log-population size	0.011	0.022	0.043**	0.045**
	(0.020)	(0.013)	(0.017)	(0.019)
Agricultural potential	0.035	-0.061	0.142**	0.303**
	(0.137)	(0.051)	(0.066)	(0.131)
Longitude	0.013	-0.008	0.010	0.076**
	(0.014)	(0.010)	(0.011)	(0.036)
Latitude	-0.012	-0.003	-0.032***	-0.019
	(0.020)	(0.010)	(0.011)	(0.019)
Log-distance to river	0.009	0.016	0.004	-0.030*
	(0.008)	(0.010)	(0.009)	(0.017)
Seaside locality	0.023	0.023	-0.383***	0.082
	(0.077)	(0.080)	(0.040)	(0.096)
Protestant Reformation		0.014	0.055	0.117
		(0.022)	(0.041)	(0.101)
Log-university distance	0.000	0.007	0.032***	-0.039
	(0.010)	(0.013)	(0.007)	(0.054)
Hanse member	-0.098	-0.025	0.299***	
	(0.071)	(0.053)	(0.055)	
City	0.206*	0.186***	0.000	0.024
	(0.108)	(0.039)	(0.048)	(0.081)
Time FE	YES	YES	YES	YES
Observations	69	197	144	109
R^2	0.798	0.655	0.704	0.677

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

2.7 Conclusion

This chapter has added more data to the work of Alfani et al. (2020) in order to build an extended dataset of wealth inequality, of 432,813 household-level observations and covering 75 localities in preindustrial Germany. Based on higher-frequency data, four broad phases of inequality development have been identified between the Black Death and the beginning of industrialization. Inequality declined substantially after the Black Death and the Thirty

Years War, but apart from these two shocks and their aftermaths inequality tended to grow. Inequality was at a historical peak in 1600. At the same time economic elites enjoyed their highest, poor classes their lowest wealth shares. The poorer half of the population had its comparatively most wealthy period from early to mid-eighteenth century. These patterns have been identified based on local and aggregate inequality estimates. The figures should be interpreted as approximations because of the challenges involved in reconstructing the distribution of wealth from archival tax registers.

An exploratory regression analysis has found that, controlling for several factors, economic inequality was related positively to higher population levels, urbanity and geographical location in the south. A number of institutional variables are statistically insignificant, but their coefficients consistently point into certain directions, which suggests that further research might unveil interesting dynamics. The introduction of the Protestant Reformation and proximity to a university might have been related to higher inequality, while communities that were part of the Hanse might have been less unequal. Similarly, Imperial status of a locality's Imperial estate did not matter significantly for inequality.

This chapter puts Germany on the map of preindustrial inequality studies. The data collection effort has provided a more nuanced view of the German case. With this data in place, we are in a good position to explore a variety of topics of theoretical and historical interest. It is possible to identify causes and understand why certain places were more unequal than others in the preindustrial period.

First, the question of the impact of warfare on inequality arises. On the one hand wars have been considered a major cause of inequality decline (see Scheidel 2017). On the other hand, a large stream of literature in historical political economy has emphasized that wars induced political elites to extract more economic resources from the population (see Gennaioli and Voth 2014, Dincecco 2015). Since wars occurred very frequently in preindustrial Europe (Tilly 1992: 72), it is worth investigating which of these two potentially countervailing effects on inequality actually dominated.

Secondly, the distributional effects of the Protestant Reformation are poorly understood. This event caused a major cultural divide within Germany. It has already been shown that it brought about a considerable redistribution of economic resources from the church to secular authorities (Cantoni et al. 2018). However, to date there is no systematic study of the distributional consequences of the Reformation at the household level in historical perspective.

Thirdly, we know very little about the role governments played for the distribution of wealth. A number of studies have speculated that access to political power facilitated personal enrichment and mattered for explaining inequality growth in preindustrial times (Alfani and Ryckbosch 2016, Scheidel 2017, Piketty 2020, Alfani 2021a). The consistently positive relationship between city-status and inequality in the regression analysis — even when controlling for a host of other factors such as the population size — point towards potentially fruitful future research. The case of Germany could, therefore, offer new and relevant material to current debates on the causes of inequality change in the long run of history.

2.8 Appendices

2.8.1 Primary and Secondary Data Sources

This Appendix lists the primary and secondary sources that have been used to build up the dataset of the distribution of wealth and inequality.

Primary Manuscript Sources

Stadtarchiv Bad Königshofen: I/65 Beet Register 1515/16 - I/65 Beet Register 1526/27 - I/65 Beet Register 1550 - I/65 Beet Register 1573 - I/65 Beet Register 1601 - I/65 Beet Register 1630 - I/65 Beet Register 1664 - I/65 Beet Register 1675 - I/65 Beet Register 1700 - I/65 Beet Register 1726 - I/65 Beet Register 1750 - I/65 Beet Register 1773 - I/65 Beet Register 1800 - III/21 Beet Brau Rechnungen Beilagen 1820 - III/21 Beet Brau Rechnungen Beilagen 1850

Stadtarchiv Freiburg: Gewerft 1481 - Gewerft 1500 - Gewerft 1523 - Gewerft 1550 - Gewerft 1575 - Gewerft 1600 - Gewerft 1626 - Gewerft 1651 - Gewerft 1675

Stadtarchiv Heilbronn: B1/29 Beethbuch 1525-27 - B1/37 Beethbuch 1550-52 - B1/45 Beethbuch 1574-76 - B1/53 Beethbuch 1598-1600 - B1/62 Beethbuch 1625-27 - B1/70 Beethbuch 1649-51 - B1/79 Beethbuch 1676-78 - B1/87 Beethbuch 1700-02 - B1/96 Beethbuch 1724-26 - B1/105 Beethbuch 1751-53 - B1/120 Beethbuch 1775-77 - B1/141 Beethbuch 1799-1801 - B1/142 Beethbuch 1799-1801 - B1/143 Beethbuch 1799-1801 - B1/144 Beethbuch 1799-1801 - B1/145 Beethbuch 1799-1801 - B1/146 Beethbuch 1799-1801

Landesarchiv Koblenz: 1E/1354 Koblenz Güterverzeichnis 1624 - 1E/1357 Koblenz Verzeichnis der Steuerpflichtigen 1675 1697

Stadtarchiv Koblenz: 623-4023 Landes- und Reichssteuern 1599

Stadtarchiv Konstanz: L 4 Steuerbuch 1425 - L 28 Steuerbuch 1450 - L 58 Steuerbuch 1480 - L 78 Steuerbuch 1500 I - L 79 Steuerbuch 1500 II - L 128 Steuerbuch 1550 - L 158 Steuerbuch 1580 - L 182 Steuerbuch 1600 - L 205 Steuerbuch 1620 - L 235 Steuerbuch 1650 - L 260 Steuerbuch 1675 - L 285 Steuerbuch 1700 - L 310 Steuerbuch 1725 - L 334 Steuerbuch 1750 - L 356 Steuerbuch 1775 - L 381 Steuerbuch 1800 - L 391 Steuerbuch 1813

Archiv der Hansestadt Lübeck: 03.04-05 01.01 Marien-Quartier: 001 Schoßbuch Marien-Quartier 1622 - 03.04-05 01.01 Marien-Quartier: 003 Schoßbuch Marien-Quartier 1664 - 03.04-05 01.01 Marien-Quartier: 012 Schoßbuch Marien-Quartier 1701-1709 - 03.04-05 01.01 Marien-Quartier: 015 Schoßbuch Marien-Quartier 1737-1750 - 03.04-05 01.01 Marien-Quartier: 022 Schoßbuch Marien-Quartier 1774-1784 - 03.04-05 01.02 Johannis-Quartier: 023 Schoßbuch Johannis-Quartier 1633 - 03.04-05 01.02 Johannis-Quartier: 026 Schoßbuch Johannis-Quartier 1664 - 03.04-05 01.02 Johannis-Quartier: 027 Schoßbuch Johannis-Quartier 1701-1709 - 03.04-05 01.02 Johannis-Quartier: 032 Schoßbuch Johannis-Quartier 1740-1751 - 03.04-05 01.02 Johannis-Quartier: 035 Schoßbuch Johannis-Quartier 1774-1784

Stadtarchiv Nördlingen: R7 F12 Nr 3 Steuerbuch 1404 - R7 F12 Nr 4 Steuerbuch 1423 - R7 F12 Nr 6 Steuerbuch 1447 - R7 F13 Nr 2 Steuerbuch 1471 - R7 F13 Nr 4 Steuerbuch

1495 - R7 F14 Nr 6 Steuerbuch 1525-27 - R7 F14 Nr 9 Steuerbuch 1543-45 - R7 F15 Nr 6 Steuerbuch 1567-69

Hauptstaatsarchiv Stuttgart: A 261 Steuereinschätzung 1480, 1522-1807 - A 573 Stadt und Amt Wildberg 1488-1882

Stadtarchiv Traunstein: R5 Steuerregister 1492 - R5 Steuerregister 1506 - R5 Steuerregister 1525 - R5 Steuerregister 1548 - R5 Steuerregister 1584 - R5 Steuerregister 1600 - R5 Steuerregister 1631 - R5 Steuerregister 1649 - R5 Steuerregister 1672 - R5 Steuerregister 1703 - R5 Steuerregister 1725 - R5 Steuerregister 1750 - R5 Steuerregister 1774 - R5 Steuerregister 1800

Stadtarchiv Überlingen: Steuerbuch 1444 - Steuerbuch 1478 - Spektavit 1503 - Spektavit 1530 - Spektavit 1563 - Spektavit 1575/77 - Spektavit 1596 - Spektavit 1625 - Spektavit 1656 - Spektavit 1675 - Spektavit 1700 - Spektavit 1725 - Spektavit 1750 - Spektavit 1773 - Steuerbuch 1800

Stadtarchiv Wangen im Allgäu: Steuerbuch 1505 - Steuerbuch 1525 - Steuerbuch 1546 - Steuerbuch 1575 - Steuerbuch 1600 - Steuerbuch 1625 - Steuerbuch 1650 - Steuerbuch 1674 - Landsteuerbuch 1696-1702 - Steuerbuch 1703-1706 - Landsteuerbuch 1721-26 - Steuerbuch 1721-1727 - Ordinari Steuerbuch der Reichsstadt Wangen 1750 - Ordinaristeuerbuch der des heil. röm. Reiches Stadt Wangischen Landschaft 1750 - Ordinari Steuerbuch der Reichsstadt Wangen 1775 - Ordinaristeuerbuch der des heil. röm. Reiches Stadt Wangischen Landschaft 1775 - Ordinari Steuerbuch der Reichsstadt Wangen 1800 - Ordinaristeuerbuch der des heil. röm. Reiches Stadt Wangischen Landschaft 1800

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2.8.2 Inequality Panel

This Appendix reports all the inequality indices that have been calculated from primary and secondary sources, for the 75 rural and urban communities in the dataset. The Column "ID" identifies the community, "Year" the year to which the estimates refer, "Gini" refers to the Gini coefficient, and the Columns "D1, D2…D9" refer to the wealth shares in percentage points of the first to the ninth decile of the population. The Columns "Top 10 %, Top 5 %, Top 1 %" refer to the wealth shares of the upper ten, five and one percent of the population. The Column "Ur." reports whether a locality was considered an urban community, that is, a city or larger town.

Table 2.6: Wealth Inequality in Communities, 1350-1850

ID	Year	Gini	D1	D2	D3	D4	D5	D6	D7	D8	D9	D10	Top5%	Top1%	Ur.
1	1500	0.658	0.000	0.000	0.000	0.000	6.776	10.511	10.511	10.511	10.511	51.179	45.924	26.809	1
1	1525	0.842	0.000	0.000	0.000	0.000	0.000	4.103	6.812	6.812	6.812	75.461	72.055	45.662	1
1	1550	0.872	0.000	0.000	0.000	0.000	0.000	3.319	4.857	4.857	4.857	82.111	77.090	43.043	1
1	1575	0.872	0.000	0.000	0.000	0.000	1.700	3.070	3.070	3.070	5.344	83.745	74.401	43.520	1
1	1600	0.890	0.000	0.000	0.000	0.000	1.617	2.196	2.196	2.196	6.239	85.555	76.981	46.605	1
1	1625	0.866	0.000	0.000	0.000	0.000	1.681	2.717	2.717	2.717	8.642	81.527	70.012	37.573	1
1	1650	0.790	0.000	0.000	0.000	2.678	3.574	3.574	3.574	3.574	10.077	72.947	60.345	32.616	1
1	1675	0.768	0.000	0.000	1.176	3.314	3.314	3.314	3.314	3.314	11.427	70.827	58.265	30.621	1
1	1700	0.773	0.000	0.000	0.598	3.179	3.179	3.179	3.179	3.179	13.333	70.173	56.525	29.048	1
1	1725	0.801	0.000	0.000	1.079	2.289	2.289	2.289	2.289	6.242	10.012	73.512	62.068	36.875	1
2	1625	0.674	0.008	0.514	1.222	2.348	2.984	4.832	6.791	10.716	20.043	50.541	35.615	11.973	1
2	1650	0.696	0.007	0.522	1.047	1.995	2.696	4.341	6.213	9.877	18.745	54.558	38.110	13.715	1
3	1475	0.763	0.000	0.000	0.000	1.522	1.722	2.352	5.073	10.960	17.026	61.345	44.511	19.689	1
3	1500	0.795	0.000	0.000	0.000	0.541	2.077	2.077	2.077	9.210	19.590	64.428	48.416	20.390	1
4	1475	0.402	2.876	5.405	5.405	5.405	5.405	5.405	8.471	13.231	15.042	33.355	21.400	7.077	0
4	1500	0.417	1.351	6.376	6.376	6.376	6.376	6.376	6.376	6.376	13.989	40.030	28.654	8.868	0
5	1525	0.549	0.683	2.049	2.561	4.098	4.098	5.464	8.879	14.115	18.896	39.158	25.043	7.399	1

ID	Year	Gini	D1	D2	D3	D4	D5	D6	D7	D8	D9	D10	Top5%	Top1%	Ur.
5	1550	0.587	1.175	1.513	1.991	3.026	3.026	5.301	8.903	12.911	18.839	43.314	27.378	7.234	1
5	1575	0.638	0.528	0.954	1.585	2.861	3.171	3.971	7.361	12.739	18.815	48.014	32.309	9.609	1
6	1500	0.824	0.000	0.289	0.621	0.621	0.621	1.491	3.216	6.448	13.538	73.155	56.827	24.575	1
6	1525	0.832	0.000	0.193	0.431	0.558	0.678	1.476	3.098	6.080	13.537	73.949	58.281	26.988	1
6	1550	0.841	0.000	0.096	0.240	0.494	0.734	1.462	2.980	5.712	13.537	74.743	59.736	29.401	1
6	1575	0.849	0.000	0.000	0.050	0.431	0.790	1.448	2.862	5.345	13.537	75.537	61.190	31.814	1
6	1600	0.851	0.000	0.000	0.025	0.391	0.744	1.395	2.763	5.401	13.634	75.648	61.186	30.365	1
6	1625	0.852	0.000	0.000	0.000	0.351	0.697	1.342	2.665	5.457	13.731	75.759	61.183	28.916	1
6	1650	0.797	0.000	0.000	0.208	0.703	1.240	2.222	4.038	8.332	16.545	66.712	49.853	22.546	1
7	1350	0.752	0.795	0.795	0.795	0.951	3.006	3.006	3.006	5.981	18.179	63.487	50.251	22.352	1
7	1375	0.690	0.732	1.041	2.083	2.083	2.083	2.117	5.944	10.732	17.645	55.542	45.050	13.906	1
7	1400	0.672	0.685	1.257	2.275	2.275	2.275	2.275	6.128	11.442	19.340	52.048	42.378	16.641	1
7	1425	0.646	0.649	1.571	1.571	1.571	2.331	4.063	11.116	12.667	12.667	51.794	39.253	14.203	1
7	1450	0.651	0.454	1.148	1.148	1.952	2.834	7.003	9.191	9.191	14.970	52.108	38.734	16.206	1
8	1350	0.762	0.568	0.568	0.676	0.908	1.451	2.500	4.643	7.800	15.869	65.018	47.547	17.725	1
8	1375	0.755	0.505	0.587	1.066	1.228	1.590	2.290	3.718	9.230	15.150	64.636	48.149	18.296	1
8	1400	0.748	0.441	0.605	1.456	1.549	1.730	2.080	2.794	10.660	14.430	64.255	48.750	18.866	1
8	1425	0.741	0.377	0.623	1.846	1.870	1.870	1.870	1.870	12.091	13.711	63.873	49.351	19.436	1
8	1450	0.815	0.220	0.376	0.987	1.040	1.095	1.137	1.276	6.873	11.727	75.269	57.868	18.473	1
8	1475	0.888	0.063	0.128	0.128	0.210	0.321	0.405	0.683	1.655	9.744	86.664	66.385	17.510	1
8	1500	0.878	0.194	0.194	0.194	0.194	0.387	0.687	1.497	2.884	10.455	83.315	68.273	23.291	1
8	1525	0.853	0.105	0.276	0.276	0.392	0.732	1.365	2.038	4.169	11.300	79.346	63.974	22.197	1
8	1550	0.829	0.016	0.358	0.358	0.591	1.078	2.044	2.579	5.454	12.145	75.378	59.675	21.103	1
8	1575	0.823	0.000	0.053	0.302	0.557	0.959	2.244	3.118	5.617	13.504	73.645	56.685	18.236	1
8	1600	0.780	0.295	0.317	0.317	0.436	1.113	2.383	3.985	8.253	15.901	66.998	46.669	9.515	1
9	1600	0.657	0.000	0.000	0.880	2.050	3.650	4.970	7.560	11.500	19.270	50.120	25.060	5.012	1
9	1625	0.630	0.000	0.000	1.950	2.120	3.740	5.040	8.220	12.460	20.430	46.040	23.020	4.604	1
9	1650	0.478	0.580	1.980	3.800	5.000	6.330	7.720	9.410	12.450	17.810	34.920	17.460	3.492	1
9	1675	0.455	0.775	2.180	3.835	5.355	6.610	8.080	10.040	12.855	17.830	32.440	16.220	3.244	1
9	1700	0.433	0.970	2.380	3.870	5.710	6.890	8.440	10.670	13.260	17.850	29.960	14.980	2.996	1
9	1725	0.470	0.835	2.025	3.370	5.030	6.200	7.825	10.205	13.200	18.075	33.235	16.618	3.324	1
9	1750	0.508	0.700	1.670	2.870	4.350	5.510	7.210	9.740	13.140	18.300	36.510	18.255	3.651	1
10	1400	0.559	1.665	1.665	1.665	6.013	6.125	6.125	6.125	6.125	17.687	46.807	29.957	13.050	1
10	1425	0.573	1.868	1.868	1.868	5.218	5.839	5.839	5.839	5.839	16.043	49.782	33.816	12.995	1
10	1450	0.514	1.746	2.393	5.042	5.042	5.042	5.042	5.042	5.897	26.211	38.542	25.437	11.865	1
10	1475	0.554	1.345	1.345	3.750	5.084	5.084	5.084	5.084	6.795	26.151	40.278	27.202	12.960	1
10	1500	0.746	0.523	0.523	0.523	0.523	0.523	4.874	6.160	6.160	22.906	57.283	42.694	22.296	1
10	1525	0.738	0.303	0.303	0.303	0.303	2.563	3.658	3.658	14.341	18.100	56.469	43.917	16.294	1
10	1550	0.679	0.213	0.213	1.453	1.845	1.845	5.105	9.865	9.865	15.778	53.818	36.703	9.354	1
10	1575	0.719	0.089	0.307	1.014	1.014	3.094	5.757	5.757	6.939	14.892	61.137	41.197	24.191	1
11	1450	0.681	0.000	0.000	0.000	0.000	4.832	4.895	7.969	13.576	23.429	45.299	30.960	11.189	1
11	1475	0.510	0.000	3.624	4.494	4.494	4.494	5.892	8.434	11.349	18.474	38.744	24.644	6.263	1
11	1500	0.564	0.000	2.265	4.089	4.089	4.089	4.229	8.632	12.533	17.083	42.991	30.759	10.948	1
12	1425	0.752	0.000	0.572	1.297	1.727	2.316	3.302	4.549	7.501	12.736	66.000	52.017	23.987	1
12	1450	0.802	0.000	0.000	0.628	1.700	2.211	2.698	3.321	5.622	11.752	72.068	59.951	30.107	1
12	1475	0.756	0.000	0.656	1.717	2.226	2.259	3.029	3.978	6.005	12.185	67.945	55.888	24.686	1
12	1500	0.765	0.000	0.000	1.675	2.349	2.349	2.710	3.653	5.903	12.357	69.005	54.332	20.920	1
12	1525	0.714	0.408	1.685	1.739	2.096	2.529	3.211	4.805	7.314	12.854	63.361	50.508	24.427	1
12	1550	0.752	0.000	0.969	1.558	1.768	2.224	2.802	4.203	7.189	12.534	66.752	54.241	22.891	1
12	1575	0.770	0.395	0.769	1.001	1.299	1.744	2.812	4.430	6.739	12.049	68.762	56.397	26.652	1
12	1600	0.775	0.278	0.582	0.895	1.242	1.947	2.856	4.094	6.277	11.979	69.849	54.409	24.121	1
12	1625	0.761	0.002	0.541	0.775	1.240	1.815	2.959	4.679	7.831	14.829	65.330	47.984	16.897	1
12	1650	0.778	0.000	0.000	0.351	0.968	1.287	2.372	4.339	8.499	18.647	63.536	45.064	15.454	1
12	1675	0.804	0.000	0.000	0.246	1.064	1.855	2.568	3.467	6.277	13.946	70.576	53.740	23.904	1
12	1700	0.861	0.000	0.000	0.000	0.000	0.000	0.721	3.017	5.332	14.156	76.773	58.985	23.676	1

ID	Year	Gini	D1	D2	D3	D4	D5	D6	D7	D8	D9	D10	Top5%	Top1%	Ur.
12	1725	0.846	0.000	0.000	0.000	0.000	0.821	2.474	3.622	4.772	12.344	75.967	60.579	30.000	1
12	1750	0.795	0.054	0.191	0.657	0.936	1.828	3.000	3.921	5.598	13.442	70.374	54.887	25.342	1
12	1775	0.744	0.109	0.381	1.314	1.871	2.834	3.526	4.220	6.425	14.539	64.782	49.196	20.685	1
12	1800	0.786	0.000	0.000	0.411	0.905	1.820	2.940	4.400	7.425	14.935	67.164	51.173	21.171	1
12	1825	0.808	0.000	0.000	0.000	0.411	1.219	2.476	4.424	7.166	15.226	69.077	53.104	20.685	1
13	1625	0.532	3.312	3.312	3.312	3.312	3.312	3.312	5.452	13.358	16.492	44.828	25.872	5.174	1
13	1650	0.509	3.454	3.454	3.454	3.454	3.454	3.672	7.218	13.099	16.760	41.979	26.027	5.397	1
13	1675	0.487	3.597	3.597	3.597	3.597	3.597	4.033	8.985	12.841	17.027	39.131	26.182	5.620	1
13	1700	0.465	3.739	3.739	3.739	3.739	3.739	4.393	10.751	12.582	17.295	36.282	26.337	5.843	1
13	1725	0.466	3.415	3.415	3.415	3.415	4.560	5.579	10.420	12.851	17.637	35.293	24.760	5.336	1
13	1750	0.467	3.090	3.090	3.090	3.090	5.381	6.765	10.089	13.121	17.979	34.304	23.183	4.828	1
13	1775	0.468	2.766	2.766	2.766	2.766	6.202	7.951	9.758	13.390	18.322	33.315	21.606	4.321	1
13	1800	0.518	2.875	2.875	2.875	2.875	2.875	6.431	8.450	13.296	15.373	42.075	22.460	4.492	1
14	1525	0.382	1.389	2.862	4.504	5.949	8.433	9.822	10.930	12.572	17.020	26.519	13.259	2.652	0
14	1550	0.462	0.876	1.847	3.046	5.094	5.801	8.338	11.368	14.813	18.184	30.633	15.317	3.063	0
14	1575	0.589	0.520	0.950	1.459	2.350	4.007	6.578	9.343	12.663	19.378	42.751	21.375	4.275	0
15	1475	0.359	2.759	3.236	5.517	5.517	6.048	10.239	11.034	14.854	16.552	24.244	14.218	3.310	0
15	1500	0.384	1.405	3.049	4.225	6.099	7.195	10.105	12.197	13.473	17.778	24.474	13.861	3.659	0
15	1525	0.394	1.185	2.457	3.760	6.245	7.565	10.057	12.301	14.322	17.762	24.344	13.569	3.322	0
15	1550	0.405	0.966	1.866	3.295	6.392	7.936	10.009	12.405	15.170	17.746	24.214	13.277	2.985	0
15	1575	0.456	0.970	1.182	2.037	4.015	7.839	9.769	12.738	16.004	19.203	26.244	14.332	3.258	0
15	1600	0.516	1.162	1.209	1.590	2.418	3.923	9.200	12.756	16.783	21.044	29.915	16.576	3.868	0
16	1475	0.328	4.286	4.286	4.286	7.143	8.571	8.571	8.571	10.476	18.095	25.714	12.857	2.571	0
16	1500	0.446	2.083	2.083	4.167	4.167	4.167	8.333	8.333	16.667	25.000	25.000	12.500	2.500	0
16	1525	0.455	1.805	1.805	3.529	3.609	4.212	8.665	10.753	17.490	23.424	24.709	12.354	2.471	0
16	1550	0.465	1.526	1.526	2.892	3.052	4.257	8.996	13.173	18.313	21.847	24.418	12.209	2.442	0
16	1575	0.469	1.532	1.532	2.723	3.064	4.340	8.851	12.596	18.383	22.468	24.511	12.255	2.451	0
16	1600	0.564	1.674	2.366	2.457	2.457	2.749	4.498	5.581	15.577	26.489	36.152	19.658	3.932	0
17	1475	0.374	2.500	3.409	5.000	5.455	7.500	9.318	10.000	13.864	16.591	26.364	16.250	3.955	0
17	1500	0.374	1.319	3.127	4.521	6.330	7.084	10.098	12.660	14.167	17.182	23.512	13.263	3.135	0
17	1525	0.360	1.302	2.915	5.259	6.394	7.512	10.519	12.788	14.019	16.663	22.629	13.419	3.182	0
17	1550	0.346	1.285	2.702	5.997	6.458	7.941	10.939	12.916	13.871	16.145	21.746	13.575	3.229	0
17	1575	0.347	1.245	2.743	5.834	6.405	7.990	11.066	12.810	14.174	16.013	21.720	13.413	3.196	0
17	1600	0.449	0.451	1.171	2.026	5.381	7.723	9.528	13.927	15.542	19.245	25.006	15.352	3.722	0
18	1475	0.471	2.545	2.545	2.545	4.042	5.090	7.186	10.180	13.772	20.659	31.437	18.263	4.581	0
18	1500	0.481	1.445	3.160	3.227	3.227	4.908	6.454	9.950	14.521	23.261	29.849	16.134	3.227	0
18	1525	0.505	1.442	2.466	2.817	3.386	4.310	6.354	9.089	14.953	24.072	31.112	16.930	3.386	0
18	1550	0.528	1.438	1.773	2.408	3.545	3.712	6.254	8.227	15.385	24.883	32.375	17.726	3.545	0
18	1575	0.532	1.027	1.712	2.397	3.425	3.425	6.164	9.247	16.438	24.658	31.507	17.123	3.425	0
18	1600	0.574	0.221	1.289	1.900	3.053	3.800	5.394	8.075	15.878	25.920	34.470	18.999	3.800	0
19	1475	0.390	3.000	3.000	5.000	5.000	6.000	10.000	11.000	15.000	18.000	25.000	14.000	3.000	0
19	1500	0.410	1.000	3.000	4.000	6.000	8.000	8.000	9.000	15.000	20.000	26.000	14.000	3.000	0
19	1525	0.435	1.000	2.000	3.500	5.500	8.000	8.000	10.000	15.500	19.000	27.500	15.500	3.500	0
19	1550	0.460	1.000	1.000	3.000	5.000	8.000	8.000	11.000	16.000	18.000	29.000	17.000	4.000	0
19	1575	0.490	1.000	1.000	2.000	4.000	7.000	9.000	10.000	16.000	19.000	31.000	18.000	4.000	0
19	1600	0.560	1.000	1.000	1.000	2.000	4.000	8.000	11.000	14.000	22.000	35.000	21.000	5.000	0
20	1475	0.319	2.778	4.630	5.556	5.556	7.407	11.111	11.111	12.963	16.667	22.222	11.111	2.222	0
20	1500	0.364	3.185	3.185	4.459	6.369	6.369	9.554	10.828	13.376	17.834	24.841	14.331	3.185	0
20	1525	0.384	2.400	3.021	3.782	5.856	6.974	9.560	11.190	14.079	18.234	24.905	14.122	3.021	0
20	1550	0.405	1.615	2.857	3.106	5.342	7.578	9.565	11.553	14.783	18.634	24.969	13.913	2.857	0
20	1575	0.458	0.902	1.707	3.285	3.800	6.570	9.275	12.238	15.201	19.452	27.568	15.813	3.414	0
20	1600	0.519	0.490	0.846	1.183	2.091	6.098	8.857	13.793	16.552	21.488	28.603	15.898	3.920	0
21	1475	0.398	2.000	4.000	5.000	5.000	7.000	8.000	10.000	11.000	17.000	31.000	17.000	4.000	0
21	1500	0.528	1.000	2.000	2.000	4.000	6.000	7.000	9.000	13.000	22.000	35.000	19.000	4.000	0
21	1525	0.540	1.000	2.000	2.000	4.000	5.000	7.000	8.500	14.000	21.000	36.500	21.000	4.500	0

ID	Year	Gini	D1	D2	D3	D4	D5	D6	D7	D8	D9	D10	Top5%	Top1%	Ur.
21	1550	0.552	1.000	2.000	2.000	4.000	4.000	7.000	8.000	15.000	20.000	38.000	23.000	5.000	0
21	1575	0.554	1.000	2.000	2.000	3.000	4.000	7.000	9.000	15.000	21.000	37.000	22.000	5.000	0
21	1600	0.643	0.000	1.000	1.000	2.000	3.000	5.000	9.000	14.000	24.000	43.000	27.000	7.000	0
22	1475	0.438	2.000	3.000	5.000	5.000	5.000	9.000	9.000	12.000	17.000	33.000	19.000	4.000	0
22	1500	0.459	2.000	2.000	3.000	5.000	6.000	9.000	9.000	14.000	19.000	31.000	17.000	5.000	0
22	1525	0.515	1.500	1.500	2.000	3.500	5.000	9.000	10.000	15.000	20.000	33.000	18.500	5.500	0
22	1550	0.571	1.000	1.000	1.000	2.000	4.000	9.000	11.000	16.000	21.000	35.000	20.000	6.000	0
22	1575	0.570	1.000	1.000	1.000	2.000	4.000	9.000	12.000	15.000	21.000	35.000	21.000	6.000	0
22	1600	0.656	0.000	1.000	1.000	1.000	1.000	3.000	10.000	16.000	24.000	43.000	26.000	8.000	0
23	1450	0.476	0.744	2.144	2.581	4.287	5.538	7.717	11.140	15.715	22.658	27.478	13.759	2.783	1
23	1475	0.546	0.469	0.880	1.951	3.407	4.737	6.802	10.044	15.684	22.721	33.305	18.014	3.687	1
23	1500	0.568	0.582	1.054	1.417	3.461	3.924	6.517	9.432	14.307	22.562	36.744	20.348	4.288	1
23	1675	0.435	1.509	2.951	4.224	4.929	6.776	7.792	10.527	13.450	17.567	30.274	18.766	7.334	1
23	1700	0.613	0.000	0.000	3.124	4.616	5.844	6.923	6.923	8.133	12.564	51.872	39.524	18.781	1
23	1725	0.652	0.000	0.000	1.562	2.308	5.317	6.959	7.475	9.020	15.665	51.694	37.698	16.740	1
23	1750	0.691	0.000	0.000	0.000	0.000	4.790	6.994	8.027	9.908	18.765	51.516	35.871	14.699	1
23	1775	0.536	0.110	2.901	4.379	4.509	5.572	6.764	6.764	8.734	15.703	44.563	30.671	11.475	1
24	1425	0.693	0.000	0.511	0.778	0.778	4.198	5.549	5.549	9.098	18.338	55.199	32.079	13.583	1
24	1450	0.597	0.106	0.738	0.738	2.134	3.930	8.204	12.082	12.082	20.807	39.178	22.675	9.473	1
24	1475	0.616	0.033	0.589	0.791	1.397	4.518	4.518	13.084	14.155	20.808	40.106	23.298	9.851	1
24	1500	0.649	0.039	0.416	0.713	1.095	4.038	4.038	10.500	12.654	25.102	41.404	26.046	13.759	1
24	1525	0.608	0.161	0.603	0.603	2.857	3.704	7.009	11.329	11.329	23.254	39.150	25.380	12.686	1
24	1550	0.652	0.045	0.337	0.678	0.792	1.880	2.889	13.133	13.133	29.285	37.829	22.015	9.364	1
25	1425	0.683	0.000	0.000	0.000	3.581	3.994	3.994	4.111	16.413	16.413	51.494	42.392	15.065	0
25	1450	0.593	0.147	0.414	2.768	2.768	2.768	4.682	14.657	14.657	14.657	42.479	30.745	8.928	0
25	1475	0.680	0.137	0.185	0.185	0.790	3.456	3.456	8.432	17.093	17.093	49.174	29.668	9.522	0
25	1500	0.633	0.165	0.219	1.912	3.291	3.291	3.291	8.503	17.480	17.480	44.367	35.628	12.438	0
25	1525	0.732	0.218	0.253	0.253	0.253	3.128	4.081	4.081	11.861	21.347	54.526	43.853	15.591	0
25	1550	0.771	0.319	0.351	0.351	0.351	0.351	3.246	4.910	4.910	26.358	58.855	44.694	13.159	0
26	1375	0.760	0.341	0.764	0.764	0.797	2.267	2.640	3.939	5.465	19.364	63.659	45.320	13.449	1
26	1400	0.692	0.001	0.951	2.740	2.924	3.805	3.805	3.974	5.707	11.191	64.903	42.074	15.797	1
26	1425	0.697	0.426	0.697	1.758	2.790	2.790	3.792	4.185	5.845	21.889	55.827	39.089	12.275	1
26	1450	0.631	0.362	1.103	3.383	4.411	4.411	4.411	5.991	6.616	11.113	58.200	39.006	15.425	1
26	1475	0.620	0.636	2.137	3.407	3.921	3.921	3.975	5.568	6.517	13.798	56.119	36.230	13.994	1
26	1500	0.609	0.911	3.172	3.431	3.431	3.431	3.540	5.146	6.417	16.484	54.039	33.455	12.563	1
26	1525	0.672	0.436	2.769	3.237	3.237	3.237	3.237	3.616	5.678	11.051	63.502	51.124	24.951	1
26	1550	0.691	0.583	2.831	2.986	2.986	2.986	2.986	3.059	4.817	10.140	66.624	54.894	27.532	1
26	1575	0.702	0.000	1.413	3.391	3.391	3.391	3.391	3.415	5.387	10.607	65.614	54.187	29.669	1
26	1600	0.673	0.579	1.636	2.653	3.272	3.272	3.384	5.328	6.822	12.873	60.181	47.038	23.237	1
26	1625	0.739	0.029	0.527	1.663	2.365	2.409	3.453	4.556	7.500	13.492	64.006	51.632	27.915	1
26	1650	0.737	0.000	0.000	0.233	2.439	2.758	3.942	5.470	9.075	15.656	60.427	46.170	22.047	1
26	1675	0.756	0.000	0.003	0.047	0.360	2.257	4.286	6.016	10.309	17.207	59.515	45.435	23.551	1
26	1700	0.744	0.000	0.052	0.083	0.853	2.745	4.363	6.168	10.233	16.551	58.953	45.035	23.866	1
26	1725	0.627	0.019	1.171	2.087	2.317	3.946	5.072	7.787	12.035	19.325	46.242	30.979	11.763	1
26	1750	0.693	0.000	0.000	0.810	2.289	3.194	4.610	6.546	10.938	18.507	53.105	37.637	15.039	1
26	1775	0.678	0.000	0.014	1.818	2.397	3.051	4.788	6.468	10.911	18.346	52.206	36.764	15.573	1
26	1800	0.723	0.000	0.000	0.742	1.564	2.727	3.808	6.274	9.997	18.036	56.852	40.839	18.116	1
27	1550	0.425	1.129	1.969	4.541	4.541	4.541	11.624	14.723	14.723	14.723	27.486	20.124	4.530	1
27	1575	0.555	1.034	1.034	1.034	3.852	5.213	5.213	11.597	16.592	16.592	37.837	28.146	6.630	1
28	1400	0.687	0.223	1.327	1.352	1.629	2.613	4.012	6.379	10.407	16.943	55.115	39.711	16.636	1
28	1425	0.737	0.287	0.877	0.894	1.366	1.954	3.071	5.357	9.028	15.786	61.380	46.950	21.330	1
28	1450	0.732	0.000	0.827	1.277	1.327	2.038	3.597	6.052	8.454	15.323	61.105	46.915	21.617	1
28	1475	0.711	0.083	1.102	1.103	1.189	2.325	4.084	6.268	9.277	16.975	57.594	42.383	18.892	1
28	1500	0.671	0.401	1.309	1.309	1.717	3.092	4.758	7.164	9.817	16.935	53.498	39.153	17.283	1
28	1525	0.700	0.000	0.696	1.305	1.429	2.661	4.236	7.064	9.846	17.401	55.361	39.961	17.386	1

ID	Year	Gini	D1	D2	D3	D4	D5	D6	D7	D8	D9	D10	Top5%	Top1%	Ur.
28	1550	0.705	0.000	0.326	1.236	1.535	2.347	4.248	6.704	10.332	18.606	54.666	38.358	15.602	1
28	1575	0.723	0.357	0.407	0.940	1.372	2.140	3.662	5.749	9.527	17.964	57.883	42.120	18.336	1
28	1600	0.753	0.240	0.259	0.725	1.165	1.809	3.033	5.043	8.700	16.603	62.423	46.060	18.779	1
28	1625	0.775	0.202	0.403	0.559	0.849	1.488	2.549	4.474	8.017	16.041	65.418	49.338	20.975	1
28	1650	0.759	0.620	0.699	0.952	1.207	2.013	2.817	4.207	6.645	13.073	67.768	50.951	22.513	1
28	1675	0.678	0.631	0.943	1.398	2.184	3.123	4.400	6.158	9.189	16.179	55.795	40.714	14.684	1
28	1700	0.728	0.260	0.664	1.010	1.373	2.105	3.437	5.588	8.813	16.312	60.437	43.803	17.068	1
28	1725	0.742	0.000	0.111	1.759	2.690	2.690	2.710	4.230	7.437	13.644	64.729	50.090	24.407	1
28	1750	0.819	0.000	0.000	0.000	0.429	1.301	2.338	3.474	6.701	14.981	70.777	54.785	21.714	1
29	1525	0.400	4.000	4.000	4.000	4.000	7.000	9.000	14.000	14.000	14.000	30.000	19.000	4.000	1
29	1550	0.530	3.000	3.000	3.000	3.000	3.000	6.000	7.000	13.000	15.000	44.000	30.000	6.000	1
29	1575	0.570	0.000	0.000	0.000	5.000	7.000	7.000	7.000	15.000	17.000	41.000	27.000	5.000	1
30	1375	0.512	0.000	0.739	4.774	6.937	6.937	6.937	6.937	9.642	17.224	39.875	23.764	7.518	1
30	1400	0.564	0.000	0.637	2.209	5.234	7.607	7.607	7.607	8.242	14.496	46.360	28.691	11.377	1
30	1425	0.598	0.000	0.602	2.583	2.583	4.938	8.898	8.898	9.015	15.787	46.696	32.092	11.729	1
30	1450	0.593	0.237	1.803	1.803	2.726	6.211	6.211	6.211	10.692	20.559	43.548	29.124	10.038	1
30	1475	0.631	0.202	1.962	1.962	1.962	2.851	6.757	6.757	10.054	19.560	47.935	32.241	12.078	1
30	1500	0.662	0.000	1.146	2.193	2.193	2.193	3.593	7.552	12.459	17.648	51.025	33.484	11.907	1
30	1525	0.684	0.000	1.597	2.094	2.094	2.094	2.094	5.369	10.374	23.104	51.181	34.432	12.255	1
30	1550	0.671	0.000	1.655	2.109	2.109	2.109	2.109	6.706	8.868	26.199	48.134	31.259	11.695	1
30	1575	0.676	0.000	1.156	2.324	2.324	2.324	2.324	5.948	8.508	25.756	49.337	30.745	10.645	1
31	1400	0.802	0.001	0.003	0.904	1.124	1.499	2.368	3.523	6.016	13.833	70.729	57.762	22.254	1
31	1425	0.795	0.001	0.002	0.771	1.009	1.661	2.683	4.005	6.640	14.134	69.094	56.560	21.067	1
31	1450	0.787	0.000	0.002	0.637	0.894	1.824	2.998	4.487	7.265	14.435	67.458	55.358	19.880	1
31	1475	0.778	0.000	0.002	0.595	0.938	1.879	3.035	4.727	7.793	15.511	65.519	52.265	18.893	1
31	1500	0.768	0.000	0.002	0.554	0.982	1.934	3.072	4.967	8.321	16.587	63.580	49.171	17.906	1
31	1525	0.759	0.001	0.002	0.512	1.027	1.989	3.109	5.207	8.849	17.663	61.641	46.078	16.919	1
31	1550	0.749	0.001	0.002	0.470	1.071	2.044	3.146	5.447	9.377	18.739	59.702	42.984	15.931	1
31	1575	0.753	0.001	0.042	0.580	0.981	1.956	3.064	5.253	9.136	18.275	60.713	43.866	16.079	1
31	1600	0.757	0.001	0.082	0.690	0.891	1.868	2.981	5.058	8.895	17.811	61.723	44.747	16.226	1
31	1625	0.760	0.001	0.121	0.800	0.800	1.780	2.899	4.864	8.654	17.347	62.733	45.629	16.373	1
31	1650	0.643	0.000	0.743	1.930	3.387	5.213	5.244	5.597	8.611	16.668	52.608	36.367	11.789	1
31	1675	0.629	0.342	1.934	3.831	3.831	3.831	3.831	5.070	7.696	14.217	55.420	42.339	18.421	1
31	1700	0.643	0.228	1.603	3.704	3.796	3.796	3.796	4.796	7.422	14.374	56.485	43.671	17.776	1
31	1725	0.656	0.114	1.272	3.577	3.762	3.762	3.762	4.522	7.148	14.532	57.550	45.002	17.132	1
31	1750	0.669	0.000	0.942	3.451	3.727	3.727	3.727	4.248	6.873	14.690	58.614	46.334	16.488	1
31	1775	0.689	0.000	0.471	2.022	3.440	3.995	3.995	4.255	7.152	16.289	58.382	43.674	17.214	1
31	1800	0.709	0.000	0.000	0.592	3.152	4.262	4.262	4.262	7.430	17.888	58.150	41.013	17.940	1
32	1450	0.699	0.100	1.219	2.250	2.441	3.414	4.134	4.335	6.880	13.850	61.377	47.328	23.817	1
32	1475	0.643	0.308	1.938	2.934	3.147	4.082	4.447	5.166	7.387	14.839	55.752	41.397	18.135	1
32	1500	0.586	0.517	2.658	3.618	3.853	4.749	4.759	5.997	7.895	15.828	50.128	35.467	12.453	1
32	1525	0.581	0.571	2.706	3.701	4.006	4.603	4.901	5.951	7.982	15.578	50.001	34.753	11.959	1
32	1550	0.576	0.625	2.753	3.785	4.159	4.458	5.043	5.906	8.068	15.327	49.875	34.039	11.465	1
32	1575	0.571	0.679	2.801	3.868	4.313	4.313	5.185	5.860	8.155	15.077	49.748	33.326	10.971	1
32	1600	0.635	0.172	1.861	2.875	3.336	3.701	4.766	5.432	7.959	16.434	53.463	37.257	13.592	1
32	1625	0.611	0.086	0.974	2.324	3.896	4.345	5.607	7.705	9.890	17.807	47.365	32.070	11.151	1
32	1650	0.587	0.000	0.088	1.774	4.456	4.989	6.448	9.978	11.820	19.180	41.267	26.883	8.710	1
32	1675	0.575	0.000	0.261	2.624	4.177	5.057	7.708	8.890	12.576	17.229	41.478	29.439	12.655	1
33	1575	0.435	0.831	1.855	3.308	4.937	7.514	9.717	11.940	14.647	18.485	26.768	14.938	3.856	0
33	1600	0.411	0.755	2.132	3.710	5.504	7.708	10.042	12.310	15.115	18.182	24.542	13.517	3.333	0
33	1625	0.388	0.679	2.409	4.113	6.071	7.902	10.367	12.680	15.584	17.879	22.317	12.095	2.809	0
33	1650	0.377	0.966	2.851	4.212	6.132	7.881	10.047	12.614	15.637	17.638	22.022	12.120	3.005	0
34	1575	0.486	0.838	2.059	3.414	4.131	5.326	6.989	10.196	16.040	20.428	30.581	17.225	4.230	0
34	1600	0.399	1.556	3.536	4.712	5.360	6.707	8.250	10.468	14.587	18.612	26.213	14.634	3.454	0
34	1625	0.313	2.274	5.012	6.011	6.590	8.088	9.510	10.741	13.134	16.797	21.845	12.042	2.678	0

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34	1650	0.335	1.605	4.223	5.898	6.718	7.748	9.731	11.117	13.341	17.081	22.537	12.454	2.693	0
35	1575	0.418	1.406	2.126	3.354	4.967	7.028	9.401	12.900	15.450	18.656	24.713	14.041	3.178	0
35	1600	0.432	0.703	1.824	2.819	4.683	7.807	10.121	13.179	15.472	18.178	25.212	15.010	4.015	0
35	1625	0.447	0.000	1.523	2.284	4.399	8.587	10.841	13.458	15.495	17.700	25.712	15.978	4.851	0
35	1650	0.397	0.499	1.785	3.051	5.395	9.440	10.862	13.645	15.856	18.049	21.417	11.501	2.626	0
36	1575	0.368	1.510	2.804	4.949	6.522	8.080	9.495	11.355	14.034	18.018	23.231	12.811	2.596	0
36	1600	0.362	1.148	2.972	4.930	6.880	8.304	9.913	11.649	13.942	17.407	22.855	12.935	2.852	0
36	1625	0.357	0.787	3.140	4.910	7.238	8.527	10.330	11.943	13.850	16.797	22.479	13.059	3.107	0
36	1650	0.347	1.060	3.205	5.123	7.202	8.422	10.584	12.222	13.491	16.581	22.110	12.848	3.077	0
37	1575	0.524	0.507	1.244	2.167	3.641	5.935	7.768	10.167	13.641	21.502	33.427	18.998	5.074	0
37	1600	0.524	0.338	1.223	2.060	3.418	5.894	8.213	10.682	14.306	21.182	32.684	18.844	5.030	0
37	1625	0.523	0.169	1.201	1.952	3.195	5.854	8.658	11.198	14.971	20.862	31.941	18.689	4.986	0
37	1650	0.523	0.000	1.179	1.845	2.972	5.813	9.103	11.713	15.636	20.542	31.197	18.535	4.943	0
38	1575	0.487	0.796	1.801	2.763	3.848	6.119	7.935	10.542	14.882	21.538	29.777	15.733	3.329	0
38	1600	0.456	0.841	1.624	3.081	4.951	6.858	8.799	11.292	14.827	20.036	27.692	15.207	3.411	0
38	1625	0.425	0.886	1.447	3.399	6.053	7.597	9.662	12.041	14.773	18.535	25.607	14.680	3.493	0
38	1650	0.430	0.908	1.435	3.150	5.801	7.573	9.735	12.118	14.873	18.891	25.518	14.498	3.561	0
39	1575	0.450	1.149	1.622	2.994	4.593	6.817	9.630	11.832	14.310	19.630	27.423	15.525	3.360	0
39	1600	0.460	0.575	1.502	2.571	4.500	6.882	9.792	11.825	15.340	20.484	26.529	14.412	3.100	0
39	1625	0.471	0.000	1.382	2.148	4.408	6.947	9.955	11.819	16.369	21.337	25.635	13.300	2.840	0
39	1650	0.447	0.552	1.564	2.737	4.954	6.895	9.462	11.915	16.199	21.600	24.122	12.415	2.688	0
40	1575	0.441	1.238	2.434	3.615	4.259	5.919	8.897	12.022	15.753	18.565	27.298	15.669	3.260	0
40	1600	0.426	1.015	2.215	3.900	4.885	6.610	9.331	12.053	15.435	18.750	25.807	14.329	2.975	0
40	1625	0.412	0.791	1.996	4.185	5.512	7.301	9.765	12.084	15.116	18.934	24.316	12.990	2.690	0
40	1650	0.398	0.568	1.778	4.469	6.138	7.992	10.199	12.114	14.798	19.118	22.826	11.650	2.406	0
41	1575	0.429	0.458	0.778	1.442	3.384	9.367	13.640	15.035	16.224	18.040	21.632	11.325	2.336	0
41	1600	0.446	0.445	0.887	1.527	3.139	8.054	12.126	15.426	17.389	18.866	22.140	11.797	2.474	0
41	1625	0.463	0.432	0.996	1.613	2.895	6.741	10.612	15.818	18.553	19.691	22.647	12.270	2.612	0
41	1650	0.432	0.793	1.298	2.181	4.262	6.799	10.561	15.413	18.072	19.265	21.356	11.281	2.551	0
42	1475	0.693	1.102	1.699	1.721	2.085	2.313	3.094	4.510	7.023	14.566	61.886	44.471	14.322	1
42	1500	0.672	0.668	1.475	1.682	1.968	2.675	3.398	5.047	10.909	19.180	52.996	38.157	13.002	1
42	1525	0.637	0.413	1.469	2.355	2.584	3.457	4.509	7.211	9.121	18.762	50.119	35.805	13.568	1
42	1550	0.660	0.393	0.822	1.432	2.166	3.186	4.659	7.087	11.370	17.746	51.139	35.338	10.588	1
42	1575	0.714	0.481	0.949	1.054	1.495	2.459	3.167	4.913	9.280	18.314	57.889	41.730	15.271	1
42	1600	0.702	0.245	0.720	1.143	1.202	2.170	3.364	5.970	11.890	19.790	53.505	38.322	11.377	1
42	1625	0.619	0.546	1.366	1.701	2.325	4.263	5.176	7.130	11.291	19.567	46.636	29.179	8.198	1
42	1650	0.639	0.349	0.788	1.008	2.033	3.428	5.479	7.809	12.206	21.155	45.745	28.278	6.957	1
42	1675	0.614	0.677	1.011	1.247	2.636	4.004	5.306	8.198	12.339	20.099	44.483	27.841	8.761	1
42	1700	0.586	0.903	1.132	1.800	2.977	4.533	5.661	8.007	11.829	20.862	42.298	25.034	5.852	1
42	1725	0.581	0.960	1.119	2.264	2.664	4.245	6.337	8.311	11.742	20.587	41.771	26.158	6.126	1
42	1750	0.565	1.021	1.554	2.607	2.934	4.218	6.280	8.929	11.843	19.221	41.392	26.358	7.151	1
42	1775	0.619	0.603	1.253	2.332	2.919	2.919	5.089	8.071	11.307	18.019	47.489	32.429	12.679	1
42	1800	0.644	0.062	0.955	1.896	3.049	3.049	4.448	7.768	11.370	18.687	48.716	33.418	14.303	1
43	1500	0.469	1.464	3.422	4.279	4.279	5.502	7.093	8.440	11.560	18.292	35.670	19.548	4.228	0
43	1525	0.425	1.667	1.667	4.350	5.198	7.222	7.881	11.554	15.226	18.004	27.232	16.182	4.194	0
43	1550	0.361	1.232	3.008	5.114	5.970	7.944	11.398	11.892	13.603	16.630	23.210	12.658	3.558	0
43	1575	0.409	0.606	2.668	4.414	5.843	7.970	9.334	11.413	13.816	17.453	26.482	15.872	4.129	0
44	1500	0.651	0.405	0.931	2.164	2.438	3.934	4.935	7.171	8.860	16.339	52.823	39.073	14.344	1
44	1525	0.649	0.244	1.212	2.065	3.377	3.998	5.527	7.037	7.587	13.204	55.748	42.474	17.943	1
44	1550	0.696	0.179	0.852	1.696	2.590	3.597	4.238	5.331	7.448	13.877	60.191	44.907	19.611	1
44	1575	0.739	0.082	0.600	1.142	1.967	2.292	3.018	4.354	7.751	15.769	63.026	46.826	16.877	1
44	1600	0.754	0.087	0.385	0.820	1.532	1.875	2.570	4.669	8.776	16.822	62.464	46.794	19.911	1
44	1625	0.711	0.101	0.422	0.946	1.142	1.929	3.817	6.906	11.057	19.040	54.642	37.253	14.905	1
44	1650	0.651	0.000	0.096	1.210	3.357	4.967	6.043	7.299	9.538	14.930	52.560	38.438	11.172	1
44	1675	0.637	0.000	0.501	1.277	2.163	4.667	6.352	8.226	11.343	17.718	47.752	32.220	11.182	1

ID	Year	Gini	D1	D2	D3	D4	D5	D6	D7	D8	D9	D10	Top5%	Top1%	Ur.
44	1700	0.610	0.111	0.960	2.607	3.567	4.665	5.696	7.264	10.506	17.599	47.025	33.180	14.554	1
44	1725	0.512	1.361	2.824	3.479	4.098	5.073	5.938	8.249	12.468	18.256	38.253	25.578	8.635	1
44	1750	0.503	0.485	2.920	3.792	4.601	5.759	6.871	8.723	11.949	16.907	37.993	24.766	10.073	1
44	1775	0.678	0.000	0.000	0.354	2.696	4.037	5.330	7.254	10.857	18.381	51.092	36.981	17.293	1
44	1800	0.600	0.000	0.000	2.374	3.710	4.983	6.683	9.081	11.762	17.522	43.885	30.824	12.437	1
45	1500	0.449	1.454	2.451	4.144	5.600	6.254	8.276	9.659	11.769	17.659	32.735	20.531	4.440	0
45	1525	0.377	0.955	3.073	4.832	6.356	8.985	10.401	10.487	12.112	16.484	26.315	14.287	3.247	0
45	1550	0.447	0.759	2.476	3.269	4.648	6.787	9.089	11.408	14.526	19.374	27.662	16.273	4.903	0
45	1575	0.540	0.000	0.789	2.020	3.257	5.909	9.272	10.905	13.760	18.636	35.453	22.184	6.350	0
45	1600	0.570	0.134	1.013	1.924	2.772	4.582	7.421	10.215	13.764	20.154	38.022	23.498	6.440	0
45	1625	0.629	0.109	0.834	1.877	1.998	3.290	5.949	8.575	12.594	19.489	45.285	30.913	9.278	0
45	1650	0.553	0.000	0.000	0.000	1.415	6.311	10.802	13.137	16.413	21.013	30.910	17.494	4.026	0
45	1675	0.452	0.000	0.901	2.581	4.563	8.530	11.186	13.334	15.049	17.363	26.493	15.928	3.571	0
45	1700	0.274	0.834	3.694	7.514	9.341	10.253	11.020	11.508	12.450	14.498	18.888	10.540	2.563	0
45	1725	0.293	1.783	3.196	6.693	8.829	10.064	10.600	11.469	12.104	13.661	21.601	12.887	3.709	0
45	1750	0.328	0.811	2.664	4.817	8.426	10.372	11.267	12.532	13.184	14.866	21.061	12.382	3.581	0
45	1775	0.310	1.560	2.926	6.466	8.692	9.676	10.461	11.369	12.271	14.575	22.004	13.682	3.474	0
45	1800	0.535	0.000	0.000	0.542	2.894	4.655	11.055	14.782	16.719	19.694	29.659	17.286	5.164	0
46	1500	0.272	1.524	4.986	8.172	9.003	9.003	9.557	10.388	11.496	14.681	21.191	12.881	3.740	0
46	1525	0.331	1.645	3.425	5.540	7.750	8.752	9.916	11.114	13.481	15.680	22.697	13.069	2.773	0
46	1550	0.386	1.310	4.447	5.378	5.378	7.619	8.917	10.756	11.928	15.651	28.618	19.193	6.184	0
46	1575	0.398	3.514	5.215	5.215	5.215	5.215	7.114	9.636	10.430	12.074	36.372	25.120	7.084	0
46	1600	0.447	1.810	5.690	5.690	5.690	5.690	5.690	8.147	8.556	10.625	42.414	36.207	14.224	0
47	1550	0.351	1.909	5.477	5.975	5.975	6.909	8.506	10.882	12.324	13.942	28.102	18.050	3.983	0
47	1575	0.411	2.907	3.591	3.591	5.130	6.499	7.525	9.149	13.596	19.282	28.730	15.434	3.382	0
47	1600	0.493	0.000	2.286	2.286	4.571	6.857	8.952	10.476	13.143	17.714	33.714	22.286	4.571	0
48	1500	0.434	0.942	3.312	4.058	5.952	7.597	8.117	9.053	12.178	17.067	31.723	20.706	6.810	0
48	1525	0.420	0.646	2.639	4.649	5.823	7.462	9.417	11.688	13.309	14.280	30.087	21.415	6.072	0
48	1550	0.641	0.041	1.743	2.549	3.355	4.322	5.209	6.581	8.621	13.917	53.660	44.076	27.466	0
48	1575	0.609	0.000	0.000	1.375	2.771	3.932	6.638	9.610	13.636	21.555	40.483	23.955	5.415	0
48	1600	0.584	0.452	1.685	2.460	2.794	3.817	6.017	8.737	13.502	18.027	42.508	29.980	10.736	0
48	1625	0.617	0.434	1.608	1.973	2.109	3.308	4.912	8.497	12.830	20.176	44.153	30.493	14.522	0
48	1650	0.577	0.000	0.000	0.000	1.273	6.048	9.866	12.828	15.617	19.847	34.521	21.583	6.782	0
48	1675	0.541	0.000	0.000	0.853	2.973	7.399	10.289	12.679	14.465	16.764	34.578	23.809	6.886	0
48	1700	0.332	1.907	4.622	6.547	7.574	8.513	9.053	9.940	10.970	12.972	27.903	19.010	7.194	0
48	1725	0.343	1.624	3.094	5.053	7.816	9.005	10.189	11.230	12.700	14.585	24.703	14.136	3.709	0
48	1750	0.289	1.629	3.758	7.257	8.414	9.604	10.344	11.469	12.199	13.438	21.888	12.581	3.243	0
48	1775	0.253	1.669	4.471	8.208	8.927	9.882	10.600	11.383	12.190	13.624	19.047	11.180	2.858	0
48	1800	0.392	0.000	0.713	3.588	8.059	10.290	11.276	12.575	13.947	15.524	24.028	14.208	3.430	0
49	1500	0.558	0.000	0.000	0.000	2.302	6.331	8.921	13.813	14.964	21.823	31.847	16.978	3.453	0
49	1525	0.635	0.000	0.000	0.000	1.151	3.165	4.460	7.885	21.504	27.868	33.967	17.674	3.564	0
49	1550	0.712	0.000	0.000	0.000	0.000	0.000	0.000	1.957	28.043	33.913	36.087	18.370	3.674	0
49	1575	0.638	0.000	0.324	3.568	4.541	4.541	4.541	5.694	7.063	11.351	58.378	36.973	7.568	0
49	1600	0.707	0.000	0.000	0.000	0.000	0.000	0.000	2.817	28.169	31.690	37.324	18.662	3.732	0
50	1500	0.461	0.226	1.583	3.166	4.975	6.860	7.765	13.607	15.831	17.867	28.119	16.736	3.483	0
50	1525	0.434	0.154	2.394	4.054	5.097	7.156	8.327	12.278	16.525	17.104	26.911	14.865	2.973	0
50	1550	0.458	0.000	1.493	4.800	6.720	6.987	8.462	9.849	11.467	17.244	32.978	18.133	3.920	0
51	1500	0.434	0.000	0.740	4.671	7.060	9.380	10.420	10.420	11.838	12.979	32.493	20.439	5.531	0
51	1525	0.369	1.353	3.707	4.397	6.427	8.710	9.401	10.909	13.192	16.533	25.370	13.784	2.858	0
51	1550	0.381	0.000	1.621	4.476	7.577	10.291	10.855	12.229	12.863	15.471	24.617	14.692	3.344	0
51	1575	0.258	2.171	6.612	6.612	7.253	9.918	9.918	10.609	13.224	13.931	19.753	11.719	3.306	0
51	1600	0.334	1.859	6.052	6.577	6.577	6.815	9.865	9.865	9.865	14.091	28.435	19.690	4.932	0
52	1500	0.368	0.000	1.319	5.936	9.343	10.003	10.003	10.442	12.421	14.922	25.611	14.564	4.270	0
52	1525	0.468	0.000	0.000	2.809	6.624	8.040	9.782	11.706	14.344	17.736	28.959	17.787	5.591	0
52	1550	0.394	1.415	4.760	4.776	6.272	6.899	7.907	10.147	11.820	15.205	30.799	19.865	5.572	0

ID	Year	Gini	D1	D2	D3	D4	D5	D6	D7	D8	D9	D10	Top5%	Top1%	Ur.
52	1575	0.525	0.000	0.562	2.773	4.105	5.471	8.518	10.337	15.253	20.434	32.547	19.904	5.383	0
52	1600	0.550	0.210	1.313	2.250	3.423	4.604	7.672	9.966	13.594	19.919	37.049	21.923	5.513	0
52	1625	0.590	0.118	1.112	2.054	2.177	3.809	6.211	9.480	14.439	22.228	38.371	24.343	6.852	0
52	1650	0.549	0.000	0.000	0.000	0.614	7.494	10.768	13.751	16.591	20.139	30.642	17.523	4.000	0
52	1675	0.478	0.000	0.000	1.374	3.840	8.843	11.501	13.703	16.025	18.141	26.573	15.197	3.710	0
52	1700	0.221	1.863	6.202	7.746	9.071	9.838	11.051	11.544	12.389	13.583	16.712	9.424	2.495	0
52	1725	0.277	1.414	3.901	7.109	8.634	9.684	10.844	12.126	12.803	14.446	19.040	10.540	2.508	0
52	1750	0.242	1.011	6.014	8.117	8.952	9.778	10.711	11.294	12.176	13.625	18.321	10.477	3.077	0
52	1775	0.253	1.468	6.319	8.022	8.835	9.438	9.908	10.602	11.223	12.606	21.579	13.228	3.851	0
52	1800	0.421	0.000	0.000	2.771	7.968	10.404	11.445	12.417	13.226	14.865	26.904	17.110	4.593	0
53	1500	0.602	0.000	0.330	1.760	2.640	3.703	9.441	9.899	10.119	15.252	46.856	24.748	4.950	0
53	1525	0.466	0.510	1.317	3.527	5.269	7.224	7.790	11.792	14.086	18.527	29.958	17.953	4.334	0
53	1550	0.538	0.195	1.669	2.493	3.187	5.138	6.656	10.323	15.548	18.341	36.448	20.546	4.958	0
53	1575	0.641	0.000	0.743	2.025	2.797	3.761	4.436	6.365	12.797	19.315	47.763	33.134	8.819	0
53	1600	0.757	0.000	0.256	0.959	1.591	2.183	3.262	4.873	8.044	14.305	64.528	53.844	16.361	0
53	1625	0.696	0.091	0.493	0.694	1.116	2.308	3.493	7.111	11.315	21.707	51.672	29.570	6.517	0
53	1650	0.607	0.000	0.000	0.000	1.386	6.616	8.611	9.316	13.735	18.694	41.641	23.528	4.776	0
53	1675	0.520	0.000	1.246	1.836	3.725	7.506	9.742	10.439	11.845	17.777	35.883	22.097	4.900	0
54	1500	0.305	0.834	3.429	5.375	8.525	11.645	11.645	11.645	11.645	12.741	22.517	12.602	2.687	0
54	1525	0.525	0.220	1.981	2.421	3.962	6.603	8.070	9.391	11.886	18.048	37.417	27.513	5.943	0
54	1550	0.608	0.000	0.366	2.927	2.927	3.841	8.171	8.780	11.098	15.427	46.463	35.732	13.171	0
54	1575	0.372	4.190	4.190	4.190	4.952	8.190	8.381	8.381	11.429	15.619	30.476	20.000	4.190	0
54	1600	0.354	4.035	4.035	4.035	7.895	8.070	8.070	8.070	8.772	18.070	28.947	18.860	4.035	0
55	1550	0.680	0.832	0.832	0.832	0.832	2.676	3.246	6.551	16.398	16.398	51.401	36.239	14.996	1
55	1575	0.671	0.416	0.416	0.416	0.867	2.629	4.226	7.871	15.789	20.576	46.794	28.666	9.607	1
55	1600	0.663	0.000	0.000	0.000	0.902	2.583	5.206	9.191	15.179	24.753	42.187	21.093	4.219	1
55	1625	0.657	0.359	1.195	2.134	2.613	3.648	4.516	6.021	9.533	16.330	53.652	39.727	17.017	1
55	1650	0.627	0.397	1.207	2.190	2.873	4.082	5.274	6.758	10.229	18.026	48.965	34.440	12.386	1
55	1675	0.525	0.935	2.483	3.633	4.465	5.327	6.800	8.412	10.726	15.234	41.985	29.980	13.743	1
55	1700	0.530	0.806	2.067	3.376	4.517	5.350	6.839	8.544	11.155	16.135	41.211	28.417	10.861	1
55	1725	0.535	0.678	1.650	3.120	4.568	5.373	6.877	8.675	11.583	17.037	40.438	26.854	7.979	1
55	1750	0.554	0.621	1.752	3.133	4.360	4.999	6.578	8.380	10.680	15.783	43.713	31.151	10.322	1
55	1775	0.573	0.565	1.854	3.146	4.153	4.624	6.280	8.084	9.777	14.529	46.989	35.448	12.665	1
55	1800	0.593	0.508	1.957	3.159	3.946	4.249	5.981	7.789	8.874	13.274	50.264	39.746	15.009	1
56	1550	0.566	0.390	1.793	1.964	1.964	6.249	6.249	6.249	17.389	20.259	37.495	27.365	6.234	1
56	1575	0.584	1.046	1.046	1.046	4.053	5.271	5.271	5.271	16.530	16.775	43.690	27.192	5.438	1
57	1450	0.759	0.000	0.000	0.068	0.728	1.828	3.360	5.505	10.008	18.414	60.089	43.390	16.627	1
57	1475	0.715	0.000	0.026	0.621	1.455	2.743	4.518	6.548	10.214	18.407	55.468	39.936	14.525	1
57	1500	0.655	0.000	0.899	2.312	2.990	3.971	5.227	7.042	9.071	13.996	54.492	41.965	18.042	1
57	1525	0.671	0.000	1.050	1.843	2.485	3.515	4.775	6.468	9.527	15.706	54.631	41.210	17.968	1
57	1550	0.713	0.000	0.601	1.168	1.863	2.635	3.827	5.796	9.467	16.637	58.007	42.759	16.576	1
57	1575	0.690	0.090	0.723	1.139	1.843	2.729	4.146	6.490	10.674	18.129	54.037	37.763	13.379	1
57	1600	0.692	0.073	0.790	1.303	1.924	2.622	3.951	6.319	10.331	18.506	54.181	38.779	15.115	1
57	1625	0.710	0.193	0.899	1.358	1.867	2.473	3.595	5.461	8.426	16.939	58.789	41.558	16.901	1
57	1650	0.588	0.762	2.098	3.740	3.965	4.588	5.638	6.732	8.408	12.693	51.378	39.640	20.434	1
57	1675	0.610	0.872	2.420	2.769	3.230	3.821	4.801	6.642	8.905	14.221	52.318	38.056	16.648	1
57	1700	0.620	0.542	1.901	2.814	3.385	4.051	4.924	6.231	8.537	15.032	52.583	38.197	15.645	1
57	1725	0.565	0.986	2.894	3.929	3.960	4.383	5.343	6.590	8.510	14.133	49.272	35.437	13.433	1
57	1750	0.575	0.537	1.684	3.826	4.302	4.863	5.603	6.834	9.357	15.372	47.621	34.655	13.832	1
57	1775	0.624	0.302	1.094	2.193	3.982	4.600	5.649	7.264	9.467	13.797	51.653	40.483	18.082	1
57	1800	0.616	0.324	1.273	2.671	3.677	4.675	5.748	7.004	8.914	13.833	51.882	38.549	16.239	1
58	1500	0.521	1.217	2.867	3.400	4.259	5.083	6.528	7.761	10.882	17.310	40.693	27.661	10.141	1
58	1525	0.537	1.603	2.861	3.189	3.624	4.426	5.853	7.974	10.924	17.054	42.493	29.674	10.598	1
58	1550	0.512	2.015	2.638	3.086	3.874	5.150	6.237	8.105	11.942	17.172	39.782	25.904	7.510	1
58	1575	0.514	0.971	2.648	3.089	3.711	5.309	7.145	9.212	11.870	17.583	38.463	23.726	6.037	1

ID	Year	Gini	D1	D2	D3	D4	D5	D6	D7	D8	D9	D10	Top5%	Top1%	Ur.
58	1600	0.594	0.466	1.818	2.242	2.906	3.563	5.336	7.736	12.487	20.347	43.102	27.173	8.240	1
58	1625	0.544	1.252	1.708	2.478	3.455	4.559	5.820	9.292	14.010	19.300	38.127	24.991	6.984	1
58	1650	0.437	1.183	2.352	3.775	5.329	6.910	8.579	10.966	13.841	17.801	29.264	17.524	4.853	1
58	1675	0.464	0.592	2.125	3.439	5.075	6.745	8.248	10.946	13.985	18.206	30.638	18.637	5.823	1
58	1700	0.458	0.212	2.282	3.576	5.173	7.178	8.873	10.553	13.287	19.191	29.676	18.134	5.430	1
58	1725	0.472	1.016	2.304	3.052	4.409	6.203	8.365	10.789	13.176	19.338	31.348	18.492	4.990	1
58	1750	0.502	0.558	1.981	3.308	4.399	5.631	7.508	9.963	13.331	18.826	34.497	21.248	6.912	1
58	1775	0.470	0.806	2.347	3.418	4.726	6.404	8.244	10.454	13.480	18.331	31.789	19.920	6.021	1
58	1800	0.467	1.123	2.279	3.550	4.794	6.447	8.045	10.052	13.316	18.433	31.962	20.084	6.373	1
58	1825	0.482	0.984	1.787	3.271	4.649	5.936	8.272	10.430	13.560	18.382	32.729	19.606	5.515	1
58	1850	0.490	0.705	2.165	3.339	4.398	5.892	7.769	10.287	13.150	18.618	33.677	20.681	5.848	1
59	1650	0.421	2.542	3.644	5.424	5.763	6.780	6.780	6.780	10.169	17.797	34.322	21.356	4.271	0
59	1675	0.420	2.857	3.673	3.673	5.306	7.347	7.347	8.082	11.510	17.959	32.245	20.204	4.041	0
59	1700	0.432	2.485	2.895	4.269	4.971	6.433	7.485	9.474	12.047	17.632	32.310	18.765	3.753	0
59	1725	0.483	1.520	2.449	3.885	4.730	5.405	7.770	9.797	10.980	14.020	39.443	25.253	5.051	0
59	1750	0.451	1.084	2.167	2.562	4.039	7.389	9.261	12.512	13.793	18.818	28.374	16.946	3.389	0
59	1775	0.389	1.970	3.485	4.545	6.389	7.222	8.157	10.101	12.273	18.737	27.121	14.855	3.020	0
59	1800	0.386	1.484	3.561	4.748	6.726	7.319	8.704	10.979	12.760	15.430	28.289	17.606	3.917	0
59	1825	0.449	2.484	2.700	3.888	4.536	5.508	7.235	10.583	12.959	16.955	33.153	19.492	4.104	0
59	1850	0.449	1.967	2.923	4.071	5.984	6.366	7.213	7.923	12.186	16.175	35.191	24.044	5.279	0
60	1650	0.371	3.317	4.054	5.209	7.076	8.108	8.108	8.108	8.108	14.545	33.366	21.069	5.108	0
60	1675	0.405	1.853	4.253	4.421	5.347	7.916	8.842	8.842	10.105	16.716	31.705	19.579	4.863	0
60	1700	0.423	2.344	3.320	4.141	6.406	6.641	6.875	9.531	11.719	15.664	33.359	21.582	5.014	0
60	1725	0.535	2.296	2.566	2.566	2.984	4.848	6.036	7.643	12.505	16.826	41.729	30.398	7.672	0
60	1750	0.484	2.608	3.294	3.322	3.940	5.230	6.589	7.179	11.283	19.725	36.829	22.512	5.666	0
60	1775	0.518	0.000	0.415	5.814	6.395	6.395	6.395	6.545	9.983	17.425	40.631	26.267	6.651	0
60	1800	0.426	3.037	4.802	4.802	4.802	4.802	5.734	8.460	12.006	17.853	33.701	21.921	6.339	0
60	1825	0.326	2.790	5.313	7.303	7.631	7.631	7.631	7.631	10.359	14.482	29.231	18.985	4.833	0
60	1850	0.414	2.699	4.193	5.189	5.205	5.205	7.518	9.365	10.763	17.157	32.707	21.992	6.983	0
61	1650	0.588	1.106	1.106	2.802	3.245	4.425	4.425	5.752	9.587	25.221	42.330	25.111	5.022	0
61	1675	0.622	1.137	1.784	2.408	2.586	2.676	3.434	5.886	9.699	22.698	47.692	25.886	5.177	0
61	1700	0.585	0.000	0.718	0.943	2.290	4.242	7.744	11.291	13.850	21.437	37.486	20.404	4.081	0
61	1725	0.525	0.569	1.465	2.676	4.259	5.158	7.333	9.666	12.226	22.008	34.641	20.372	4.520	0
61	1750	0.513	0.000	0.670	2.102	3.866	7.312	8.480	11.767	14.861	18.617	32.326	18.885	4.517	0
61	1775	0.496	0.375	1.718	2.901	4.555	5.433	7.455	10.744	15.630	20.566	30.623	18.065	3.964	0
61	1800	0.443	1.456	2.811	3.818	4.839	6.005	7.053	11.166	14.114	19.939	28.799	15.267	3.204	0
61	1825	0.508	0.137	1.592	3.068	4.386	6.263	7.710	9.871	13.489	19.365	34.119	20.439	4.427	0
61	1850	0.609	0.338	1.398	1.877	2.525	3.585	5.145	8.420	13.349	19.190	44.173	28.947	7.260	0
62	1500	0.365	1.223	2.446	2.446	7.339	9.174	9.786	14.373	14.679	18.349	20.183	10.092	2.018	0
62	1525	0.411	1.495	2.617	3.178	4.486	7.850	11.215	11.776	13.084	17.383	26.916	13.458	2.692	0
62	1550	0.423	2.260	3.243	3.243	4.128	6.781	8.108	11.204	14.742	17.396	28.894	15.000	3.000	0
63	1500	0.582	0.740	1.300	2.130	3.060	4.220	5.709	7.959	11.309	18.398	45.175	22.588	4.518	1
63	1525	0.575	0.570	1.440	2.410	3.360	4.380	5.930	7.930	10.760	18.140	45.080	22.540	4.508	1
63	1550	0.604	0.510	1.150	1.880	2.850	3.830	5.379	7.759	11.109	17.718	47.815	23.908	4.782	1
63	1575	0.606	0.400	1.040	1.860	2.720	3.800	5.271	7.671	11.851	18.942	46.445	23.222	4.644	1
63	1600	0.629	0.270	0.920	1.720	2.530	3.390	4.890	7.331	10.631	18.472	49.845	24.922	4.984	1
64	1475	0.863	0.000	0.000	0.000	0.187	0.903	0.903	1.605	7.939	10.622	77.840	67.105	32.650	1
64	1500	0.856	0.000	0.000	0.000	0.429	0.699	0.824	1.911	8.218	10.114	77.806	64.407	24.554	1
64	1525	0.849	0.000	0.000	0.038	0.618	0.618	0.986	3.310	7.267	9.163	78.000	65.390	21.801	1
64	1550	0.798	0.000	0.000	0.567	0.599	1.019	2.069	7.045	7.045	11.142	70.515	55.850	19.787	1
65	1450	0.251	2.674	3.491	6.701	6.833	9.990	13.666	13.666	13.666	13.666	15.647	8.814	2.545	0
65	1475	0.201	2.696	4.807	6.800	10.903	11.723	11.723	11.723	11.723	11.723	16.178	9.730	2.345	0
65	1500	0.219	2.708	4.176	6.709	9.411	11.299	12.481	12.481	12.481	12.481	15.772	9.239	2.165	0
65	1525	0.236	2.719	3.546	6.619	7.920	10.875	13.239	13.239	13.239	13.239	15.366	8.747	1.986	0
65	1550	0.274	2.686	3.405	5.228	6.811	10.008	13.621	13.621	13.621	13.621	17.378	10.352	2.624	0

ID	Year	Gini	D1	D2	D3	D4	D5	D6	D7	D8	D9	D10	Top5%	Top1%	Ur.
65	1575	0.274	2.880	3.456	4.916	6.912	10.023	13.134	13.825	13.825	13.825	17.204	9.716	2.304	0
65	1600	0.350	1.219	3.657	3.810	7.315	7.315	10.607	14.630	14.630	14.630	22.188	13.654	4.389	0
66	1525	0.374	2.002	2.334	2.588	6.527	9.336	9.336	12.588	15.730	18.673	20.885	11.549	2.801	0
66	1550	0.375	1.800	2.311	2.739	6.751	9.243	9.243	12.437	15.970	18.486	21.022	11.779	2.773	0
66	1575	0.377	1.597	2.287	2.889	6.976	9.150	9.150	12.285	16.209	18.299	21.158	12.009	2.745	0
66	1600	0.428	0.103	2.632	2.705	3.999	9.144	10.820	11.085	15.642	20.112	23.758	12.937	3.246	0
67	1450	0.328	1.475	2.273	5.346	8.356	8.356	11.152	12.535	14.316	16.713	19.478	10.937	2.716	0
67	1475	0.345	2.230	2.230	3.416	7.628	8.920	9.699	13.345	15.434	17.841	19.257	10.336	2.676	0
67	1500	0.364	2.271	2.271	2.884	7.100	9.044	9.474	12.170	15.506	18.170	21.109	11.933	2.725	0
67	1525	0.383	2.312	2.312	2.353	6.572	9.168	9.249	10.994	15.578	18.499	22.961	13.529	2.775	0
67	1550	0.388	0.239	2.231	3.665	7.649	8.924	9.163	12.829	14.821	17.849	22.629	13.386	2.677	0
68	1550	0.270	1.126	5.158	7.292	8.537	9.782	9.812	12.183	12.361	14.614	19.134	11.464	3.928	0
68	1575	0.272	1.971	4.025	6.876	6.876	8.344	13.194	13.753	13.753	13.753	17.456	10.580	2.718	0
69	1625	0.689	0.000	0.000	0.300	1.099	2.398	4.196	7.692	12.787	19.181	52.348	26.174	5.235	1
69	1650	0.647	0.433	0.733	1.000	1.666	2.865	4.764	7.495	12.191	19.687	49.165	24.583	4.917	1
69	1675	0.604	0.867	1.467	1.700	2.233	3.333	5.332	7.297	11.596	20.194	45.983	22.991	4.598	1
69	1700	0.561	1.300	2.200	2.400	2.800	3.800	5.900	7.100	11.000	20.700	42.800	21.400	4.280	1
69	1725	0.559	0.882	1.863	2.451	3.333	3.824	5.392	8.725	12.745	18.235	42.549	21.275	4.255	1
69	1750	0.569	0.602	1.807	2.309	3.313	4.418	5.522	6.727	12.249	20.783	42.269	21.135	4.227	1
69	1775	0.569	0.451	1.554	2.605	3.857	4.709	5.661	6.663	11.524	18.292	44.685	22.342	4.468	1
69	1800	0.570	0.300	1.300	2.900	4.400	5.000	5.800	6.600	10.800	15.800	47.100	23.550	4.710	1
70	1475	0.536	2.021	3.337	3.968	3.968	4.454	5.165	6.336	8.880	15.388	46.484	33.086	14.088	1
70	1500	0.500	2.049	3.634	4.023	4.542	4.695	5.589	7.193	10.373	16.506	41.395	28.587	9.530	1
70	1525	0.487	2.286	3.457	3.676	4.321	5.082	6.422	8.069	10.801	16.229	39.657	26.399	8.164	1
70	1550	0.463	3.080	3.737	3.737	4.738	4.983	5.891	7.580	11.243	17.778	37.233	23.090	6.503	1
70	1575	0.470	1.645	4.286	4.286	4.859	5.715	5.901	7.440	10.866	16.619	38.381	24.915	7.962	1
70	1600	0.455	2.496	4.922	4.922	4.922	5.428	6.563	6.563	8.433	14.679	41.072	27.866	12.211	1
70	1625	0.465	1.998	3.785	3.785	4.943	5.104	6.939	8.484	11.550	16.916	36.496	23.495	7.276	1
70	1650	0.334	2.783	4.597	5.761	6.712	8.135	8.700	10.238	11.943	14.716	26.415	17.180	6.249	1
70	1675	0.411	0.367	3.770	5.533	6.088	7.053	8.387	9.925	12.191	16.986	29.702	18.283	5.376	1
71	1475	0.663	0.784	1.382	2.956	2.956	2.956	2.956	10.316	14.323	18.845	48.897	41.825	22.899	1
71	1500	0.683	0.140	0.739	2.312	2.312	2.377	4.005	6.220	10.228	16.631	55.036	41.812	19.098	1
71	1525	0.708	0.000	0.599	2.172	2.172	2.248	3.113	5.329	9.336	17.405	57.627	42.222	20.089	1
71	1550	0.662	0.667	1.044	1.477	1.807	2.999	4.442	7.008	10.917	18.191	51.448	36.112	14.815	1
71	1575	0.702	0.672	1.002	1.059	1.646	2.441	3.502	5.674	9.307	17.082	57.615	41.006	15.209	1
71	1600	0.747	0.479	0.608	1.024	1.191	1.753	3.026	5.065	7.847	15.612	63.396	48.562	19.472	1
71	1625	0.737	0.056	0.428	0.829	1.484	2.354	3.497	5.371	8.404	16.512	61.065	44.706	16.982	1
71	1650	0.735	0.002	0.592	0.931	1.540	2.191	3.371	5.198	8.726	16.761	60.687	44.189	16.598	1
71	1675	0.726	0.877	1.463	1.463	1.697	2.310	3.133	4.268	6.345	13.454	64.990	50.923	23.341	1
71	1700	0.694	0.915	1.660	1.660	1.821	2.617	3.613	5.170	7.856	14.501	60.188	46.773	20.993	1
71	1725	0.653	1.678	2.272	2.272	2.290	2.849	3.826	5.296	7.725	13.657	58.136	44.693	21.963	1
71	1750	0.695	0.578	1.530	1.530	1.991	2.744	3.760	5.500	8.336	14.340	59.692	45.983	21.443	1
71	1775	0.734	0.454	1.111	1.431	1.890	2.311	3.224	4.794	6.923	12.748	65.113	51.626	26.254	1
71	1800	0.808	0.293	0.764	0.764	0.962	1.490	2.194	3.158	5.144	10.893	74.337	61.053	33.621	1
72	1600	0.501	0.327	2.694	3.685	4.514	5.538	6.686	9.319	12.338	19.611	35.287	21.189	5.201	1
72	1625	0.717	0.000	0.674	1.243	1.735	2.674	3.780	5.546	8.998	17.259	58.090	42.584	21.084	1
72	1650	0.393	1.192	4.214	5.659	5.779	7.260	8.801	9.487	11.871	15.856	29.882	18.908	6.463	1
72	1675	0.475	0.596	3.073	4.331	5.895	6.036	7.259	8.657	11.097	16.139	36.917	25.031	8.356	1
73	1475	0.686	0.500	1.000	1.200	1.600	2.200	3.600	5.400	9.000	17.000	58.500	29.250	5.850	1
73	1500	0.626	0.900	1.600	2.000	2.300	3.200	4.200	6.600	9.700	16.300	53.200	26.600	5.320	1
73	1525	0.621	0.850	1.300	1.800	2.350	3.450	4.650	7.200	10.050	17.200	51.150	25.575	5.115	1
73	1550	0.616	0.800	1.000	1.600	2.400	3.700	5.100	7.800	10.400	18.100	49.100	24.550	4.910	1
73	1575	0.578	1.300	2.000	2.500	3.100	4.000	5.500	7.200	9.900	13.800	50.700	25.350	5.070	1
73	1600	0.582	1.150	1.850	2.450	3.100	3.900	5.250	7.050	10.200	15.950	49.100	24.550	4.910	1
73	1625	0.586	1.000	1.700	2.400	3.100	3.800	5.000	6.900	10.500	18.100	47.500	23.750	4.750	1

ID	Year	Gini	D1	D2	D3	D4	D5	D6	D7	D8	D9	D10	Top5%	Top1%	Ur.
74	1775	0.589	1.596	1.739	1.739	1.739	2.269	5.257	13.037	14.272	14.272	44.083	33.865	13.522	1
74	1800	0.616	0.000	1.773	2.077	2.077	2.077	5.347	10.326	16.419	16.419	43.485	35.276	14.820	1
75	1600	0.379	1.704	3.895	4.828	6.268	7.748	9.655	9.655	11.866	16.349	28.032	17.323	4.268	1
75	1625	0.321	1.045	4.492	6.322	7.440	9.455	10.341	10.441	11.923	14.601	23.940	14.032	3.594	1
75	1650	0.282	2.775	4.963	6.413	7.169	8.432	9.459	11.712	13.765	15.691	19.622	10.513	2.431	1
75	1675	0.283	2.672	4.751	6.136	7.456	8.835	9.679	11.801	13.522	15.393	19.754	11.074	2.653	1
75	1700	0.325	2.358	4.402	5.626	6.841	7.936	9.497	10.922	13.111	16.394	22.913	12.732	3.264	1

Notes: Own calculations.

2.8.3 Coding of Independent Variables

This Appendix describes how the independent variables employed in the exploratory regression analysis 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 typically assumed for preindustrial German towns is 4.5 (Minns et al. 2020: 611).

Seaside locality. A dummy that indicates localities that lie within 10 kilometres of the seaside.

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 itself, but also climatic conditions.

Protestant Reformation. A dummy that indicates whether the Protestant Reformation was been introduced in a locality after 1517. I have taken as introduction date when a town council or local ruler officially introduced the Reformation. However, when no precise year is indicated I took as alternative date the appointment of a Protestant priest by the town council. When no introduction is mentioned, or the source indicates that the Reformation had “no substantial impact”, I code the locality as 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), a multi-volume encyclopedia of German cities.[21] For communities without entry in the *Städtebuch* usually the relevant Imperial Estate introduced the Protestant Reformation. This information has been taken from the *Städtebuch* and the *Historisches Lexikon der deutschen Länder* (Köbler 2007).

Hanse member. A dummy that indicates whether a city was member of the Hanseatic league, as indicated by Dollinger (1981: 68).

City status. A dummy that indicates whether a locality was a city. I consider all those localities as cities that have an entry in the *Städtebuch.*

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

Imperial status: Electorate, ecclesiastical polity, mundane principality, county, Imperial city. A set of dummies that indicates whether a locality's Imperial estate was an electorate, ecclesiastical polity, mundane principality, county, Imperial city. This information has been obtained from the *Städtebuch* and the *Historisches Lexikon der deutschen Länder* (Köbler 2007). Information about the Imperial rank of each estate (that is, elector, bishop, Imperial city, and so on) have been obtained from Zeumer (1913).

[21] The encyclopedia was a collaborative project of hundreds of local historians, and provides information about several city characterics in a very systematic way, for example about political institutions, the population or important events such as epidemics and wars.

Chapter 3

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

3.1 Introduction

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

This chapter examines the accuracy of the "wars are great equalisers"-hypothesis for wars in the preindustrial period. It argues that the conventional view has focused mostly on major wars, and therefore overlooks that warfare had two countervailing effects: multifaceted destruction could reduce inequality, but inegalitarian resource extraction could also increase inequality. Because this second factor often outweighed the limited destruction of *ordinary* conflicts, most wars between major political authorities in the preindustrial era actually led to higher economic inequality. This was the result of political authorities being induced by the threat of war to extract economic resources. Extraction happened via inequality-promoting channels, most importantly regressive taxation, and credit. Only in truly major wars could destruction outweigh extraction and reduce inequality. The Holy Roman Empire — Germany, for short — is the ideal testing ground for this hypothesis. Germany was a cockpit of preindustrial warfare. It saw many ordinary wars, but was also the area where a truly major war took place, the Thirty Years' War (1618-48). Moreover, Germany was historically a very diverse entity: economically, geographically and institutionally. It was politically and in terms of security provision a highly decentralised area. I will account for this diversity and make use of it to document the countervailing effects of warfare on inequality.

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

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

Palatine Succession War (1688-97), using a flexible difference-in-differences set up.

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

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

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

The chapter makes three principal contributions. First, it contributes to the literature on economic inequality. Recent research has revealed a striking empirical pattern: contrary to a conventional "Kuznetsian view" (see Kuznets 1955), inequality across Europe did not start to grow with the beginning of industrialisation, but increased almost constantly since about the sixteenth century. In other words, much of the high levels of inequality observed in the early twentieth century might actually have preindustrial roots (Alfani 2021a). Yet little is known about why inequality reached high levels before industrialisation began. The expansion of markets and the economy (van Zanden 1995, van Bavel 2016), demographic growth (Milanovic 2016, Pfister 2020a), exclusive political institutions (Alfani and Ryckbosch 2016, Minns et al. 2020), and ideological factors have been proposed as major causes of preindustrial inequality growth (Piketty 2020). Major warfare has commonly been considered a factor that reduced economic inequality (for example, Scheidel 2017).[1] This chapter empirically shows that the focus on big wars might be misleading when we want to understand the general relationship between warfare and inequality. My findings suggest that we need to consider the indirect effects of conflicts (see Glick and Taylor 2010), especially the many ordinary ones, because they generated a negative externality in the form of increasing economic inequality. Conflicts might thus be a significant part of the explanation for why

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

economic inequality was high already when industrialisation was about to start. This is in line with Alfani and Di Tullio (2019), who suggest that the rise of the fiscal state might explain preindustrial inequality growth. The results are also in line with research showing that even a conflict as disruptive as the U.S. Civil War could not diminish the concentration of wealth in the hands of elites in the medium run (Ager et al. 2021). More broadly the chapter is in line with Ogilvie (2021) and Acemoglu et al. (2005a) who point to extractive institutions as general causes of economic inequality in history.

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

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

The next section outlines the conceptual framework and provides historical background information. Section 3.3 describes the data. Section 3.4 presents the empirical strategy, the main results of the chapter, investigates the robustness of these results and provides direct evidence on defence infrastructure and extraction, to shed light on the hypothesised mechanisms. Section 3.5 provides additional case study evidence, and Section 3.6 concludes.

3.2 Conflicts and Economic Inequality: Historical Evidence and Theoretical Framework

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

3.2.1 Historical Evidence: Extraction, Destruction and Inequality

In preindustrial Germany, three types of conflicts existed. First, there were feuds, a small form of conflict, usually fought among two opposing individuals. Second, there were bottom-up revolts by peasants or townsmen. Third, there were conflicts between important political actors — analogous to "interstate wars" in modern times — such as Imperial estates and authorities of comparable or higher rank like the King of France or Italian states (see Kroener 2013).[2] I focus on the third category of conflicts because these were more likely to be exogenous events from the perspective of an individual locality and hence less likely to be correlated with local inequality. The focus is thus on conflicts that were entirely different from those civil wars analysed in studies interested in the contribution of inequality to the outbreak of social conflict today (see Blattman and Miguel 2010, Baten and Mumme 2013).

It is well known that exposure to warfare increased political elites' need to extract financial resources in preindustrial times. This was true at the level of centralised polities or "states", but also at the local level (see Kamen 1984, Parker 1996, Karaman and Pamuk 2013). For

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

individual towns this resource need derived from their fundamental task to maintain peace and provide protection for their inhabitants.[3] In Germany, the preindustrial period was still characterised by the absence of monopolies of force in the hands of consolidated states, and so towns had a central role in providing protection locally (Volckart 2002b, Isenmann 2014).[4] The capacity to protect inhabitants gave preindustrial towns a "safe harbour" character (Dincecco and Onorato 2016).

Incidents of warfare in the surrounding area increased the *risk* for communities of being attacked — for instance, in a subsequent battle of the same war or potentially a future war (see the Appendix for historical examples) — even if war did not actually come to town. This risk increased the demand for protection. It made the extended use of costly defence infrastructure necessary, in terms of fortification, equipment and buildings, and personnel (for example, stronger walls, more weapons, extended arsenals and more military forces). For instance, the political elites of Nuremberg decided to reinforce the town's fortification just after the War of the Swabian League (1376-77) and reportedly in expectation of an upcoming war between princes and cities, which eventually came about with the War of the Cities (1387-94) (Isenmann 2014: 101).[5] The more military technology advanced during the "Military Revolution", the more costly it became for communities to adapt to it (Parker 1996, Kroener 2013).

In order to obtain the necessary funds local political authorities had to extract economic resources from their inhabitants and from the surrounding area. There were two major channels to obtain resources: taxation and credit (see again the Appendix for historical

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

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

[5] Another example of a community experiencing warfare in its surrounding repeatedly in a relatively short period and improving its defence infrastructure is Speyer: it was exposed to battle action during the Palatine Succession War in 1688, and again to two battles of the War of the Spanish Succession in 1703 and 1713. In between, the town improved its fortification (Keyser 1964: 386, Dincecco and Onorato 2016). More systematic empirical evidence suggesting that it was rational for political elites to consider past conflicts to assess risk, which then influenced the decision to expand local defence infrastructure, comes from Table 3.2. It shows that almost three quarters of the community-period observations in my dataset experienced warfare not just once, but outright twice in the same period between 1400 and 1600.

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

Wealth taxes were at best inequality-neutral, because they were levied proportionally, asking the same share from rich and poor households.[6] Still, parts of the riches of the Church and the nobility were exempted, making theses taxes weigh most likely more on lower segments. However, the regressive character of preindustrial tax systems principally came from consumption taxes, and tax systems at the time relied much more on these indirect taxes than on direct taxation of wealth or income. Consumption taxes — "the most unsocial tax imaginable" (Boelcke 1971: 171; my translation) — were regressive because they weighed disproportionally on the poor strata. They spent a larger share of their budgets on basic goods compared to rich people, and therefore paid proportionally higher taxes (Winnige 1996, Alfani and Di Tullio 2019: 149).[7] Extraordinary war taxes, the third type of taxes, were potentially the most regressive ones. They were often levied as poll tax, setting a fixed amount for everybody, regardless of personal economic strength. By construction, these taxes took an increasing share of income or wealth the poorer the taxpayer was. Yet poll taxes were appealing to authorities, especially in wartime when funds were needed urgently, because they required low administrative effort (Isenmann 2014).[8] Sometimes, high taxes

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

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

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

even pressured poor people into selling their real estate to large landowners (Hoffman 1996).

Credit was the second principle mode to finance war-related expenses. Warfare increased the demand for credit substantially (Stasavage 2011). However, credit only postponed regressive taxation, because eventually the borrowed money had to be paid back with interest.[9] Moreover, creditors were usually local elites, for instance rich merchants, guild masters or officials. Since interest rates were relatively high in preindustrial times — between three and ten percent (see Chilosi et al. 2018: 647) — rich lenders could accumulate even more wealth when warfare increased political authorities' demand for credit. Poor people could not make such profitable investments in public debt (Alfani and Di Tullio 2019).

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

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

[9] 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).

deriving for example from the need to maintain fortification or to store military equipment (Hohrath 1996: 308, Isenmann 2014: 454-456).[10]

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

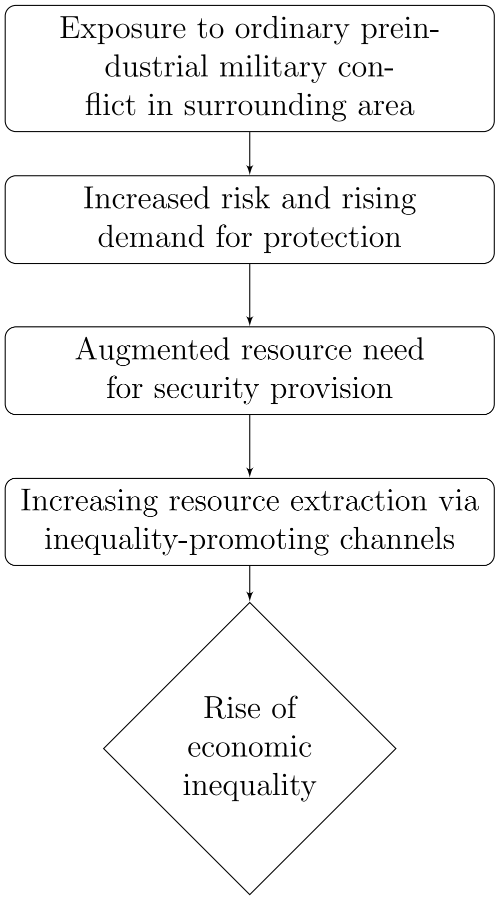


Figure 3.1: Extractive Effect of Warfare on Economic Inequality

Figure 3.1 puts the historical argument schematically together, and summarises why one would expect that conflict-induced resource extraction led *ceteris paribus* to a redistribution of economic resources from poorer people to socio-economic elites.

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

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

The exceptional destructiveness of the Thirty Years' War is mirrored in its extraordinarily high number of people killed during that period compared with other preindustrial wars (see Table 3.1). A few other conflicts saw high casualties too, such as the War of Spanish Succession, but only in case of the Thirty Years' War deaths were almost completely concentrated on one country: Germany (Wilson 2009: 787). Other conflicts apparently caused relatively small destruction, most likely because they were geographically more limited, without the spill-over effects characteristic of the Thirty Years' War.

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

3.2.2 Theoretical Framework: Personal Wealth Accumulation During Warfare

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

Table 3.1: Wars in Preindustrial Germany and Their Casualties

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

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

the destructive and extractive effect on inequality, to highlight the logic of the following empirical analysis.

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

$$W_{t+1} = [S + (1 + r) \times W_t]. \tag{3.1}$$

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

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

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

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

$$W_{t+1}^{H} = (1 - er^{H}) \times (1 - w^{H}) \times [S^{H} + (1 + r) \times W_t] \tag{3.2}$$

and

$$W_{t+1}^{L} = (1 - er^{L}) \times (1 - w^{H}) \times [S^{L} + (1 + r) \times W_t]. \tag{3.3}$$

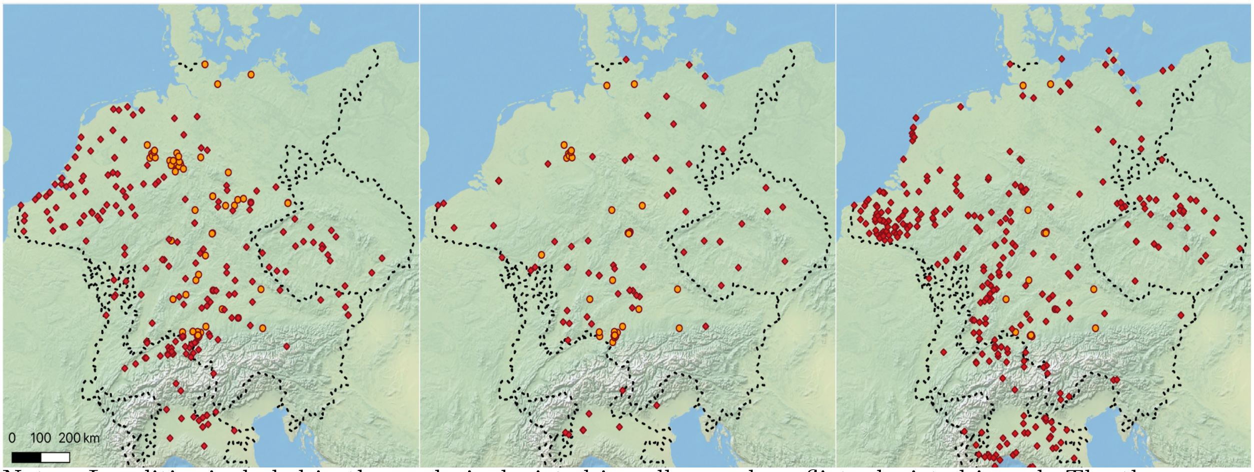
If we assume that all variables in the square brackets are fixed during warfare, then Equations 3.2 and 3.3 suggest that after a military conflict in $t + 1$ the change in wealth possessed by high- and low-wealth holders depended on the magnitudes of the extraction rates (er^{H} and er^{L}) and the rates of destruction (w^{H} and w^{L}). These parameters determined the wealth differentials between rich and poor caused by warfare. If one takes a basic inequality measure, such as the ratio between rich and poor households' wealth share (see Piketty 2020) — W^{H}/W^{L} — then it is straightforward to see how the extraction and destruction rate determined wealth inequality. Equations 3.2 and 3.3 also suggest that depending on whether the conflict was small or large, the importance of extraction and destruction shifts, and with them inequality. Low-destruction conflicts had a relatively higher er but a relatively lower w, which could exacerbate inequality. But in a major war destruction could become so large that it offsets the extractive effect and reduce inequality. (A simple simulation of these predictions is reported in the Appendix.) This framework is of course highly stylised, and we are not going to test it as such. However, the empirical results below are consistent with the framework.

3.3 Data

Figure 3.2 provides an overview of the localities (yellow dots) for which inequality data are available, and the conflicts (red diamonds) that happened during the period of analysis. They are the main variables of the following analysis.

I use the data on community-level inequality introduced in Chapter 2. As mentioned, some

Figure 3.2: Spatial Distribution of Localities and Conflicts (3 Periods)



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

small parts of the population might not be fully represented in the wealth distributions. These missing parts are likely to bias my estimates of the impact of ordinary conflicts downwards. It is likely that, for instance, highly paid town magistrates benefited from inegalitarian resource extraction during warfare. Since these groups were sometimes missing from the wealth tax data, I might underestimate the true impact of conflicts on economic inequality.[11] The results are therefore to be considered lower-bound estimates. The panel-regression setup makes it possible to limit the analysis to within-community variation, and account as good as possible for the possibility that differences in taxation practices between localities could systematically influence the measured wealth distribution.

One might be concerned that survival of tax registers is negatively correlated with exposure to warfare. While this possibility cannot be entirely excluded, it is historically not very likely, because communities were usually quick to levy taxes again, even after the most destructive wars. For example, Nördlingen was exposed to outright two major battles during the Thirty Years War, the "First-" and "Second Battle of Nördlingen" (1634, 1645). Yet there exist tax registers for the years 1636 and 1646 in the city archive.

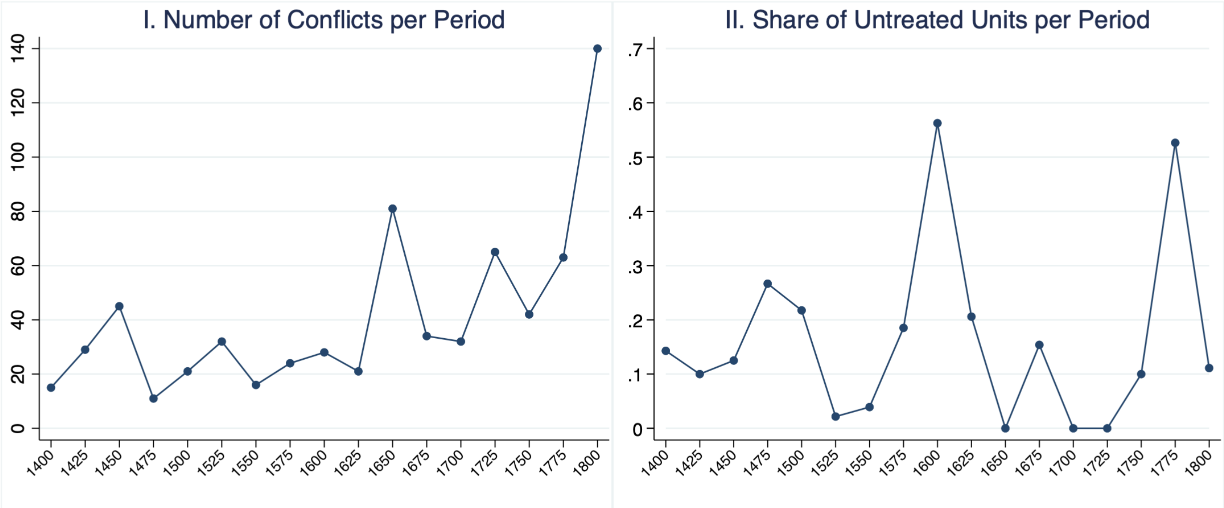
[11] See the Appendix for more discussion of the coverage of poor strata in the tax registers, and for alternative estimates where propertyless households have been dropped from the wealth distributions, which does not substantially change the results of the analysis.

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

Figure 3.3 provides an overview of the amount of conflicts and to what extent communities were affected by them. The left frame shows the number of conflicts per period. The two major spikes are the Thirty Years' War in the seventeenth century and the French Revolutionary and Napoleonic Wars at the end of the eighteenth century (for a similar pattern see Dincecco and Onorato 2016). The latter one comprises a seemingly exceptional number of conflicts but many of them actually happened at the margins of the Empire, especially in the Low Countries, Italy and Bohemia, as can be seen in Figure 3.2. The right frame of Figure 3.3 shows the share of communities that were not exposed to warfare in a given period, that is, that were not treated (for information on treated units, see Table 3.2 below). This share fluctuated considerably. In some periods, especially those of intense warfare, such as during the Thirty Years' War or during the four major succession wars in the seventeenth and eighteenth centuries (see Table 3.1), all communities in the panel experienced at least one conflict. This is little surprising given the relatively wide threshold radius of 200 kilometres.

As main variable of interest I construct a dummy that equals one if there was at least one conflict between two inequality measurement points within a distance of 200 kilometres from

Figure 3.3: Conflicts and Treatment Status (200km), c. 1400-1800



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

a locality. This is in line with previous research on the topic (see Besley and Reynal-Querol 2014, Dincecco and Onorato 2016). Using a dummy indicator has the advantage of reducing the potential for error in the measurement of conflict frequency (Dincecco and Onorato 2016). In order to address concerns about the reliability of this simple measure, I perform a robustness check in the Appendix with alternative categorical variables. The 200 kilometres threshold has been chosen by means of iteration and is in line with historical research (see below).

In substance, this analysis is limited to information about where and when a conflict (battle or siege) took place. The treatment indicator is thus imperfect, a proxy for the totality of military events during a war. It is not possible to measure more precisely and systematically the intensity of single conflicts, for example with the number of casualties. One might also ask whether the effect of a conflict on inequality differed when a polity was one of the belligerents. Unfortunately, systematic information about victors and losers for every battle is simply beyond our current historical knowledge, not to speak of alliances. In the Appendix I provide case-study evidence suggesting that being among the losers might have implied a differential impact of warfare on economic inequality. Similarly, in conflicts that were part of a larger war, troop movements and raids might have had effects similar to an actual battle. Because of a lack of systematic data, troop movements and raids, too, cannot be considered.[12]

Ultimately, given the hypothesis that wars increased resource extraction and inequality, it would be insightful to have information about actual resource extraction in communities, such as data on taxes and public loans. Unfortunately, such information, too, is to my best knowledge not systematically available. This motivates the reduced-form approach, which investigates the direct relationship between warfare and inequality. However, to shed some light on the possible mediators connecting warfare and inequality, I gather evidence on communities' defence infrastructure from the *Deutsches Städtebuch* for a subset of the database. First, I record the presence of military garrisons in a community. Second, I assemble information on construction of buildings or sites with a military purpose, such as barracks, arsenals, gunmen houses (*Schützenhaus*), stables for war horses (*Marstall*), firing ranges (*Schiesshaus*) or foundries for weapons.[13]

Table 3.2 reports summary statistics for the inequality and conflict exposure indicators employed in the analysis. There are slightly more observations for the first two centuries of the analysis than for the latter two. The figures indicate, for example, that in the period from 1400 until 1600 the top 10 percent (10. decile) of the population owned on average 43.09 percent of total wealth in the communities in my dataset. This is considerably less than the wealth share of the same decile in contemporary Germany. For instance, Albers et al. (2020: 37) report for Germany a top 10 percent wealth share of about 51 percent in 1993, but one should be very cautious when comparing these two estimates.[14] Table 3.2 also

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

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

[14]There are several potential economic and methodological reasons for the different magnitudes of the estimates. First, modern industrial Germany is, of course, many times more prosperous in absolute terms than preindustrial Germany, which could imply a more unequal distribution of wealth. Second, the unit of analysis is different. The figures reported in Table 3.2 refer to averages across single communities, while the estimates of Albers et al. (2020), or of similar studies such as Piketty (2014), typically refer to Germany as a whole country. Quite obviously, socio-economic differentiation can be much higher in a populous country

Table 3.2: Summary Statistics of Main Variables

	1400-1600			1625-1650			1675-1800		
Variables	N	Mean	Std. dev.	N	Mean	Std. dev.	N	Mean	Std. dev.
Gini	308	0.567	0.155	70	0.568	0.151	126	0.549	0.145
1. Decile share	308	0.816	0.899	70	0.649	0.896	126	0.839	0.920
2. Decile share	308	1.692	1.378	70	1.478	1.415	126	1.845	1.469
3. Decile share	308	2.533	1.730	70	2.355	1.819	126	2.980	1.875
4. Decile share	308	3.523	2.183	70	3.419	2.086	126	4.047	2.166
5. Decile share	308	4.747	2.602	70	4.970	2.435	126	5.203	2.377
6. Decile share	308	6.291	3.018	70	6.568	2.911	126	6.506	2.509
7. Decile share	308	8.327	3.285	70	8.616	3.255	126	8.320	2.763
8. Decile share	308	11.68	3.762	70	11.81	3.323	126	10.95	2.725
9. Decile share	308	17.35	3.948	70	17.73	2.744	126	16.47	2.474
10. Decile share	308	43.09	16.68	70	42.40	16.18	126	42.84	14.66
Bottom 50% share	308	13.31	7.958	70	12.87	7.818	126	14.91	7.753
Conflict exposure	308	0.799	0.402	70	0.900	0.302	126	0.857	0.351
Repeated conf. exp.	308	0.727	0.446	70	0.857	0.352	126	0.802	0.400

Notes: The 10. Decile share is equivalent to the Top 10% wealth share of the population. The Bottom 50% share is the sum of deciles 1 to 5. Conflict exposure means that a community was exposed to at least one conflict, and repeated conflict exposure means that a community was exposed to at least two conflicts.

indicates that communities were exposed frequently and repeatedly to warfare. For instance, in the period from 1400 until 1600, 79.9 percent of the observations refer to communities that were exposed at least once and 72.7 percent were exposed repeatedly. Yet the high standard deviation indicates that there was a lot of variation in that exposure.

than in a single community with a few thousand inhabitants. Third, the nature of the sources is very different. For the industrial period information from general property taxes that cover (almost) the whole population — as used in this study — are not available. This has led Albers et al. (2020) to use survey data and to correct the top wealth shares with information from rich lists, obtained from popular media outlets. It should come as no surprise that the resulting wealth share estimates are potentially different from the ones reported in this study.

3.4 Main Empirical Analysis

3.4.1 Econometric Methodology

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

$$I_{i,t} = \alpha_i + \pi_t + \beta CE_{i,t-1} + \boldsymbol{\gamma}' \boldsymbol{X_{i,t}} + \epsilon_{i,t} \tag{3.4}$$

$I_{i,t}$ is wealth inequality of locality i in year t (t = 1400, 1425,... until 1600 and from 1675 until 1800).[15] The Gini coefficient is the main left-hand-side inequality measure, but I also consider as alternatives wealth percentiles. Unless otherwise indicated the results will refer to the period from 1400 to 1600 and from 1675 to 1800. The observations from 1625 and 1650, approximately the period of the Thirty Years' War, are considered separately because it is the only period for which a negative warfare-inequality relationship has been documented (Alfani et al. 2022). $CE_{i,t-1}$ is the measure of conflict exposure that takes the value one if there was a military conflict within 200 kilometres of locality i over the previous period.[16]. Accordingly, I obtain reduced form or "intention-to-treat" estimates of how exposure to warfare in the surrounding area of a community affected its economic inequality. The choice of the 200 kilometres threshold is discussed below. α_i are a full set of locality fixed effects and π_t are time fixed effects (years). Hence, the estimated correlations are identified from time variation within communities.

$\boldsymbol{X_{i,t}}$ 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, Schneider 2020). In most specifications I also include Imperial circle-time interactions. They account for unobserved temporal shocks affecting communities in the same region (Imperial circle) in the same way. Unobserved factors are

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

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

captured with the random error term $\epsilon_{i,t}$. The standard errors are robust, clustered at the locality level in order to account for the possibility of serial correlation in the error term.

Before proceeding, the data structure and the econometric specification require a clarification. The setup in the main analysis — multiple shocks with periods of non-treatment in between — does not correspond to a conventional difference-in-differences (DD) research design. In my analysis conflict matters in the period after a war, but not permanently, that is, there is no post-period. Because the treatment warfare occurs repeatedly, even before the period of analysis, it is impossible to define a pre- and post-treatment period for this type of multiple shock. Recent critiques of DD research designs employing two-way fixed effects (TWFE) specifications to conventional setups with single shocks (occurring at different points in time) do, therefore, not entirely apply. However, this literature has pointed to a general problem with TWFE specifications that has implications also for my setup: when treatment occurs at different times and when individual treatment effects vary across time or unit, the estimated average coefficient can be severely biased, potentially leading to a "flip" in the sign of the coefficient. This can happen because previously treated units act as controls in later periods (Goodman-Bacon 2021, Wooldridge 2021).[17] To provide at least a partial remedy to this potential problem, I follow the approach of Wooldridge (2021: 49). He suggests to model the heterogeneity across time and units by making the TWFE specification more flexible with interaction terms. Moreover, in Section 3.5 I validate the main results with two case studies of single wars using a flexible DD research design. In these cases, treatment occurs about at the same time, which makes a much cleaner comparison of treated and control units possible and circumvents the complications of treatment occurring at different points in time.

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

3.4.2 Endogeneity Concerns

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

To mitigate further the possibility for omitted variable bias, I account for several observable characteristics that have been considered alternative explanations for inequality change. I include two variables that capture economic and demographic development, the log-population size of a locality and the occurrence of epidemics. Population size is a proxy for local economic development, a potential driver of inequality, for example, leading to the rise of a middle class (van Zanden 1995). Moreover, demographic expansion potentially influenced inequality growth (Alfani and Ryckbosch 2016, Pfister 2020a).[18] Additionally, conflict exposure might have increased the population in premodern towns because these were "safe harbours" to which people potentially migrated in wartime (Dincecco and Onorato 2016). This might have increased inequality because migrants were often relatively poor. Hence controlling for population size closes a potential alternative causal channel. Finally, military conflicts often spread epidemics, which had a negative impact on the population size and possibly on inequality (Alfani et al. 2022).

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

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

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

To substantiate the claim that wars were by and large exogenous events for the individual locality, I have reviewed the historiography of all 86 wars in the dataset and have recorded the principal causes (one or more) of the outbreak of every war, according to the literature. I have classified the individual causes into the general categories reported in Table 3.3. (A table including every single war in the dataset and the reasons for its outbreak, including a qualitative evaluation of whether a war was a plausibly exogenous event and the references to the historical literature, is reported in the Appendix.)

As can be seen from the table, many wars broke out because of struggles among rulers over

Table 3.3: Principal War Causes

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

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

material things (for example, territorial gain, strategic routes, payment of a levy, a trade dispute), especially territory. This is what one would expect given that land was the main source of prestige in preindustrial times. For instance, gaining territory was Louis XIV's motivation when he tried to round off the borders of France at the expense of the Low Countries and the Empire in several wars in the seventeenth century. Many wars also broke out because of the high politics in Germany and Europe (for example, power balance, sphere of influence, political alliance, independence). For example, the Cologne War broke out in 1583 to maintain a balance of geopolitical power within the Holy Roman Empire.[19] Religious

[19]Had Gebhard von Truchsees been successful in his attempt to transform Cologne into a worldly principality with equal rights for both confessions, that would have led to a majority for a different faction in

issues (for example, confessional dispute, ecclesiastical control) sometimes led to warfare too, such as in the “wars of religion” after the beginning of the Protestant Reformation. Yet a number of wars broke out for idiosyncratic reasons. For example, about a fifth of all wars began as a dispute over the succession in ruling a territory, such as in Guelders, Spain, Poland or Austria. Such disputes usually started because a ruler died without heir.

Only in two cases did wars break out because of potentially endogenous social discontent.[20] This is unsurprising given that preindustrial societies were structurally unequal — think of nobility versus commoners — and so economic inequality will tend to be less perceived as a problem (Cohn 2006, Alfani and Frigeni 2016). In 84 out of 86 wars, economic inequality in the population did not play a role according to the historical literature, supporting the notion that these wars were plausibly exogenous events from the perspective of an individual town.[21]

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

To further address endogeneity concerns, I report a series of robustness checks in the Appendix: the baseline results with spatial autocorrelation-adjusted standard errors and Morans’

the Kurfürstenkolleg and thus a new political equilibrium in the Holy Roman Empire. This threat induced Bavaria and the Habsburgs to send troops to Cologne, thus starting the war (Schilling 1994).

[20] These were the Swiss Peasant War (1653) and the German Peasants’ War (1524-1525). In both cases discontent about impoverishment of peasant classes played a role in the outbreak (Schilling 1994: 140-148, Suter 2010).

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

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

3.4.3 Baseline Results

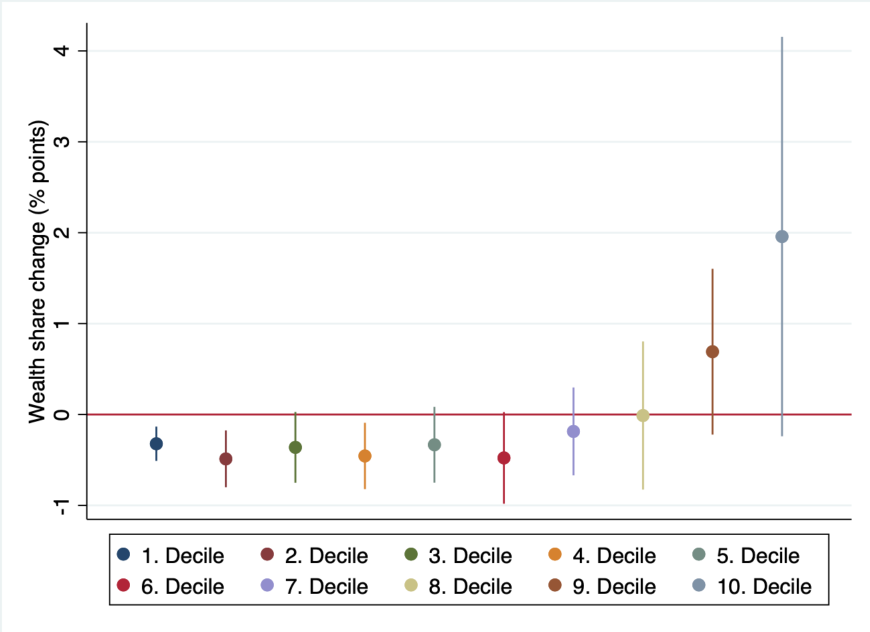
The main hypothesis is that warfare had two countervailing effects on inequality — equalising destruction and inequality-promoting extraction — and that during ordinary conflicts and throughout most of the period under study the extractive effect was larger. A first piece of evidence comes from changes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

In Figure 3.4 the plotted coefficients are the point estimates on the conflict exposure indicator when taking the deciles of the wealth distribution as dependent variables. The wealth shares of the lowest six deciles of the population decreased, in most cases significantly. The upper-middle class of the wealth distribution (7. to 8. decile) did not experience significant variation. Only the ninth decile and the top 10 percent (10. decile) did experience a substantial increase. This pattern is exactly what one would expect in a world where the resources to cover the costs of warfare were extracted through regressive taxation.[22]

Table 3.4 reports the baseline results. Column 1 shows that the occurrence of a conflict was associated with an increase in inequality by 0.032 Gini points, significant at the one-

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

Figure 3.4: Changes in the Wealth Distribution during Warfare



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

percent level. Adding Imperial circle-time interactions in Column 2 reduces the coefficient and significance, but not much.

Table 3.4: Conflict Exposure and Economic Inequality: Baseline Results

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

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

In Column 3 I limit the analysis to all units that are part of the dataset for at least 300 years, to address concerns about panel non-response, potentially introduced by selective attrition

(Schneider 2020). It seems unlikely that a potential bias leads to an overestimation of the effect of warfare on inequality. In the Appendix I report results with a balanced sub-panel, which does not change the results either. Column 4 further adds to the picture in Figure 3.4, employing the wealth share of the poorest 50 percent as outcome. The reduction of 1.659 percentage points is substantial compared to the about 13 to 15 percentage points owned by this part of the population (see Table 3.2).

One might wonder how the conflict-inequality relationship looked over the whole period 1400 to 1800, that is, when we pull the Thirty Years' War into the analysis. Column 5 shows that it was still positive and significant. In Column 6 I explore the heterogeneity in treatment timing, which is also important in light of recent critiques of difference-in-differences designs with staggered treatment.[23] Conflict exposure is interacted with dummy variables for the three sub-periods of the analysis, covering approximately the time before, during and after the Thirty Years' War (1400 to 1600, 1625 to 1650, 1675 to 1800). All three coefficients point to a positive warfare-inequality relationship. An interesting facet of this result is that the effect of ordinary conflict on inequality becomes stronger from the seventeenth century. This probably reflects the acceleration of the Military Revolution: when military technology advanced — increased firepower of weapons, stronger fortifications, larger armies — and became more expensive, political authorities had to extract an increasing amount of resources to pay for defence and war (Parker 1996).

On the surface, the results in Columns 5 and 6 might seem puzzling, given that Alfani, Gierok and Schaff (2022) have shown that the Thirty Years' War had a negative effect on inequality in a counterfactual analysis. In Columns 7 and 8 I zoom in on that historically peculiar period. Only when the specification includes no time fixed effects (Column 8) does the substantial inequality decrease during the Thirty Years' War come through. It is significantly different from the relationship in subperiods one and three (Column 2). In other words exposure to single battles does not seem to capture the decrease during the Thirty

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

Years' War, unless one allows the treatment indicator to capture some of the variation in inequality experienced by all localities. This result is consistent with the hypothesis that the Thirty Years' War had a different impact on economic inequality compared to ordinary conflicts, precisely because of its exceptional spill-over effects that turned it into a truly major war. It had a globally destructive impact on inequality, going far beyond the immediate destruction of single battles, to which ordinary conflicts were typically limited.

How economically relevant is the effect of conflict exposure for explaining variation in inequality? During the periods of ordinary preindustrial conflicts (1400 to 1600 and 1675 to 1800) a one-standard deviation increase in conflict exposure (0.351-0.402) was associated with an increase in the Gini coefficient by 0.21 to 0.22 standard deviations, which is a sizeable effect (compare estimate in Column 1 of Table 3.4 with the summary statistics in Table 3.2). Alternatively consider that the Thirty Years' War reduced inequality by 0.135 Gini points over a century according to Alfani, Gierok and Schaff (2022). Exposure to an ordinary conflict increased inequality by 0.032 Gini points (coefficient in Column 1 of Table 3.4). Then a back-of-the-envelope calculation indicates that ordinary conflicts, in slightly more than a century, increased inequality by the same amount by which the exceptional Thirty Years' War reduced it.[24]

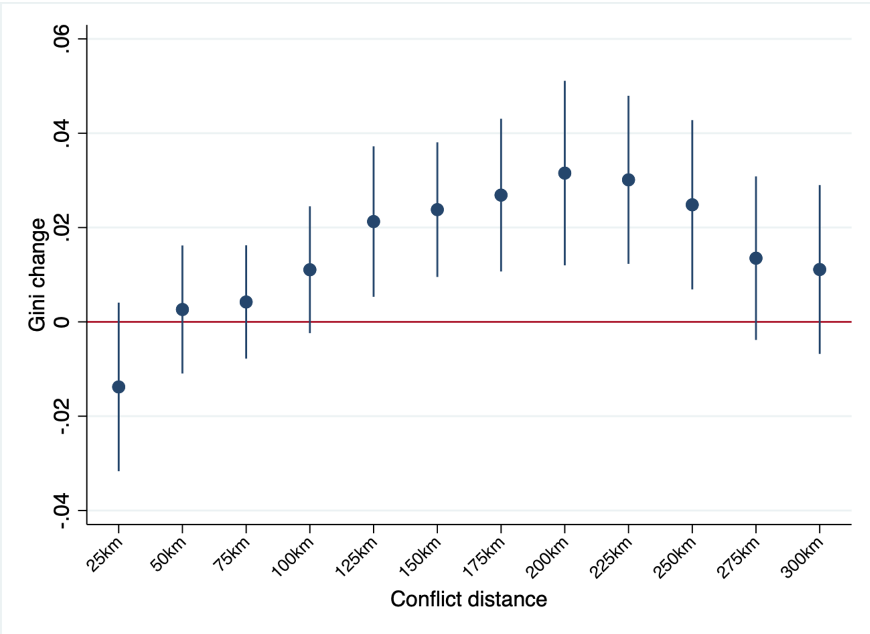
So far the evidence suggests that ordinary conflicts continuously reinforced inequality during most of the period under study. The theoretical framework suggests that this was so because the extractive effect of wars on inequality likely dominated the destructive one. To shed more light on these two effects, I exploit heterogeneity in conflict distance. I repeat the baseline specification, now testing different distances between localities and conflicts, from 25 to 300 kilometres.[25] Intuitively, if there existed a destructive effect that could outweigh the extractive one to reduce inequality, we should see it most clearly in a geographically limited

[24] The average inequality growth explained by warfare during a century of ordinary conflicts, in the period from 1400 to 1600 and from 1675 to 1800, has been obtained by multiplying the regression coefficient with the average conflict exposure of about 0.8 or 0.86 times per quarter century (see Table 3.2). This result is then multiplied with four, the number of periods per century, leading to a range of 0.102 to 0.0110.

[25] One might wonder whether there was a significant relationship between inequality and the actual distance to the nearest conflict. However, such a measure would be problematic because it could not capture the changing nature of the conflict-inequality relationship across different distances, as documented in Figure 3.5.

radius around battle action. It is likely that at least some of the communities falling into the 25-kilometres threshold were directly hit by destructive battle action. Then, at greater distances, we would expect to see an increasingly positive effect of conflict on inequality, because more and more places that were too far away to experience destruction are pulled into the treatment group. Yet these places were treated indirectly. They had to protect themselves against the risk of warfare, inducing them to extract resources in an inequality-promoting way.

Figure 3.5: Conflict Distance Test: Destruction vs. Extraction (1)



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

The pattern of results in Figure 3.5 confirms that intuition. The point estimate for conflicts taking place within 25 kilometres of a locality is negative, suggesting a reduction of inequality. The estimates at greater distances suggest that the association of conflicts and inequality gradually became ever more positive the larger the distance, until a threshold of about 200 kilometres. This pattern reflects that the further away a conflict was from a locality, the more inequality-reducing destruction was outweighed by inequality-promoting extraction. Beyond 200 kilometres the effect seems to peter out, which one would expect, also because the group of untreated places becomes ever smaller at greater distances. The results leave a range between 125 and 250 kilometres around a locality of positive and highly significant estimates. One might wonder whether also conflict risk declined with distance from a conflict. To some extent this was probably the case, because a conflict around Munich was unlikely to continue near Hamburg soon. But it is unlikely that risk declined linearly with distance, because how conflicts spread geographically was hard to foresee precisely. Military historians have documented, first, that opposing armies often chased or tried to escape from each other, depending on their strength. Second, armies split up in legions and merged as needed. In doing so early modern German armies often operated a so-called "five-day-march system" (*Fünf-Tage/Märsche System*). It meant that two parts of the same army should be not more than five day marches away from each other — which corresponded to an operation radius of about 100 kilometres — mainly for reasons of supply (see Gallina 1860: 22-24, 93-99, Fiedler 1986: 205). These movements made it practically unpredictable where armies would meet and fight within a radius of a few days of marching. Any community within that radius was, therefore, potentially at risk.

The results also substantiate the assumption that 200 kilometres is a reasonable threshold distance for conflict exposure. In fact, they show that the exact distance of 200 kilometres did not matter a great deal, because the positive effect holds at a wide range. This is important because it makes it unlikely that the main results are driven by selection into treatment. I have chosen 200-kilometres as threshold for the rest of the analysis for two reasons. First, it seems historically reasonable and in line with other studies' results. 200 kilometres corresponds approximately to the distance that messengers on horseback — and with them important information, for example about a conflict or an investment opportunity — could cover in about two days in early modern Germany (Volckart 2000: 274). For example, 200 kilometres is approximately in line with Chilosi et al. (2018), who document that capital investment activity declined at around 200 kilometres away from a town in preindustrial Germany. It is also roughly in line with the threshold used in recent studies on the impact of conflicts on locality-level outcomes (Dincecco and Onorato 2016).[26] Second,

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

200 kilometres maximises the goodness of fit of the model (R-squared) among all estimates, but actually does not minimise the p-value (full regression output reported in the Appendix).

Table 3.5: Conflict Distance Test: Destruction vs. Extraction (2)

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

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

Table 3.5 provides further evidence for the hypothesis that there existed two countervailing effects of warfare. In Column 1 I include conflict exposure variables for the 25- and 200-kilometres threshold in the same specification. The coefficient on the 25-kilometres indicator points towards a direct inequality-reducing effect. Instead, the 200-kilometres indicator points towards an indirect inequality-promoting effect. Note that this approach accounts to some extent for heterogeneity in treatment intensity across units, which is again a major concern of the recent critiques of difference-in-differences designs (see Wooldridge 2021). In Column 2 I drop all the conflicts within 25 kilometres and keep only those within the "donut" from 25 to 200 kilometres, that is, those communities that were indirectly exposed to warfare. The inequality-promoting effect of warfare holds for these indirectly exposed places.

3.4.4 Controls for Locality Characteristics

The analysis has so far controlled for several time-invariant local and time-variant regional and "national" characteristics. In Table 3.6 I first assess the robustness of the main results to time-invariant locality-specific characteristics that might have had an impact on inequality. The results should be interpreted keeping in mind that some of these covariates could be 'bad controls". Details about the coding of control variables are provided in the Appendix. In Column 1 economic and demographic controls — the log-population size and the occurrence of epidemics — are included. The main result is robust to the inclusion of these controls. This is an important result because by controlling for population size I close a potential alternative mechanism to the one I propose connecting conflict exposure and inequality.

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

Table 3.6: Controls for Observables

	1400-1600 & 1675-1800									
	(1) Gini	(2) Gini	(3) Gini	(4) Bot. 50%	(5) Top 10%	(6) Gini	(7) Gini	(8) Gini	(9) Gini	(10) Gini
Conflict exposure	0.026**	0.025**	0.025**	-1.684**	1.404	0.030**	0.025**	0.028**	0.020**	0.020*
	(0.010)	(0.011)	(0.011)	(0.713)	(1.010)	(0.012)	(0.011)	(0.013)	(0.009)	(0.011)
Econ. & demogr. controls	YES	NO	YES	YES	YES	NO	NO	NO	NO	NO
Institutional controls	NO	YES	YES	YES	YES	NO	NO	NO	NO	NO
Agricultural potential	NO	NO	NO	NO	NO	YES	NO	NO	NO	NO
Seaside location	NO	NO	NO	NO	NO	NO	YES	NO	NO	NO
Latitude & longitude	NO	NO	NO	NO	NO	NO	NO	YES	NO	NO
City	NO	NO	NO	NO	NO	NO	NO	NO	YES	NO
Hanse member	NO	NO	NO	NO	NO	NO	NO	NO	NO	YES
R^2	0.362	0.330	0.364	0.339	0.307	0.348	0.353	0.388	0.416	0.355
Observations	434	434	434	434	434	434	434	434	434	434

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

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

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

3.4.5 Mechanisms: Evidence on Defence Infrastructure and Extraction

In this section I provide evidence for the hypothesised mechanism: warfare increasing the demand for costly defence infrastructure. As mentioned, this led to an increase in inequality-promoting resource extraction and defence expenditure. Ideally one would like to have information about the actual resource extraction in communities via taxes and public loans,[27] and one would like to know for what these resources were spent. Unfortunately such information is not systematically available for German communities, leading me to rely on indirect evidence. In Column 1 and 2 of Table 3.7 the conflict exposure indicator has been added lagged by one period. As mentioned, one would expect that the effect of warfare on wealth inequality was not just short-lived, but was semi-permanent. Since wealth is a stock its distribution adjusts in longer cycles than income. Moreover, increases in regressive taxation were to some extent sticky, for example because resources were extracted over a period longer than the actual war, thus carrying part of the financial need over into the following period.

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

Then previous conflicts should still have a positive impact on inequality. The estimates point into that direction: the coefficients of the lagged conflict exposure indicator are smaller and less significant, but still positive. In line with this evidence, I report additional results in the Appendix, showing that places that were exposed to conflicts repeatedly experienced a higher inequality increase. This most likely reflects that authorities had to extract resources from the population repeatedly via inequality-promoting channels.

Table 3.7: Evidence on Defence Infrastructure and Extraction

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

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

In Columns 3 to 6 I include a variable indicating whether a community stationed one or more garrisons. Military forces were a central and costly part of communities' defence systems, especially in larger towns. They mediated the effect of warfare on inequality. The results indicate that garrisons were positively and highly significantly related to inequality. The main conflict exposure indicator loses in size and significance when added to the specification in Column 2 and 6. This is precisely what one would expect if stationing costly troops was one of the mediators through which wars led to higher inequality.

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

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

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

In both cases the dataset is limited to all those localities for which a comparison of inequality before and after the respective war is possible. The respective treatment-group consists of all localities that were exposed to battle action within the usual 200 kilometres threshold. The control group consists of all localities that were not exposed to the war. The main identifying assumption is that inequality in communities with exposure to the respective war would have evolved similar to non-affected communities had the war not occurred.

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

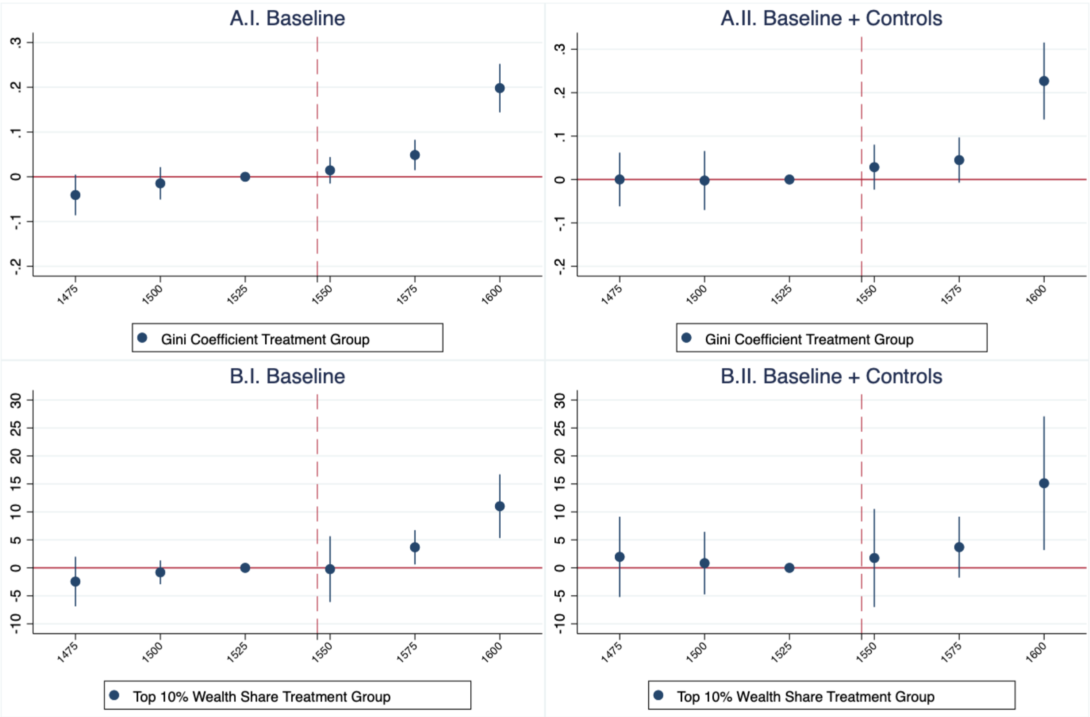
In order to identify the effect of the wars on economic inequality Equation 3.5 is estimated, separately for the Schmalkaldic War and the Palatine Succession War. The specification is almost identical to Equation 3.4:

$$I_{i,t} = \alpha_i + \pi_t + \sum_{t=1}^{5} \theta_t (W_i \times P_t) + \beta CE_{i,t-1} + \boldsymbol{\gamma' X_{i,t}} + \epsilon_{i,t} \tag{3.5}$$

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

Figure 3.6 reports the results. The plotted coefficients represent an average inequality difference in the respective period. In Panel A I take the Gini coefficient as inequality outcome, and in Panel B the top 10 percent wealth share. The left frame reports results for the baseline specification, the right frame for the fully saturated specification. The dashed line marks the beginning of the war. The results suggest that there was no statistically significant difference in inequality between treated and untreated communities before the war. These common trends indirectly support the identifying assumption (see Angrist and Pischke 2009). Yet after the war began, the divergence in terms of inequality between the two groups began. This result holds for both outcomes and when controls are added. In the Appendix I report results suggesting that the small group of losers of this war, the towns of the Schmalkaldic

Figure 3.6: The Effect of the Schmalkaldic War (1546-48) on Inequality (Flexible Difference-in-Differences Estimates)



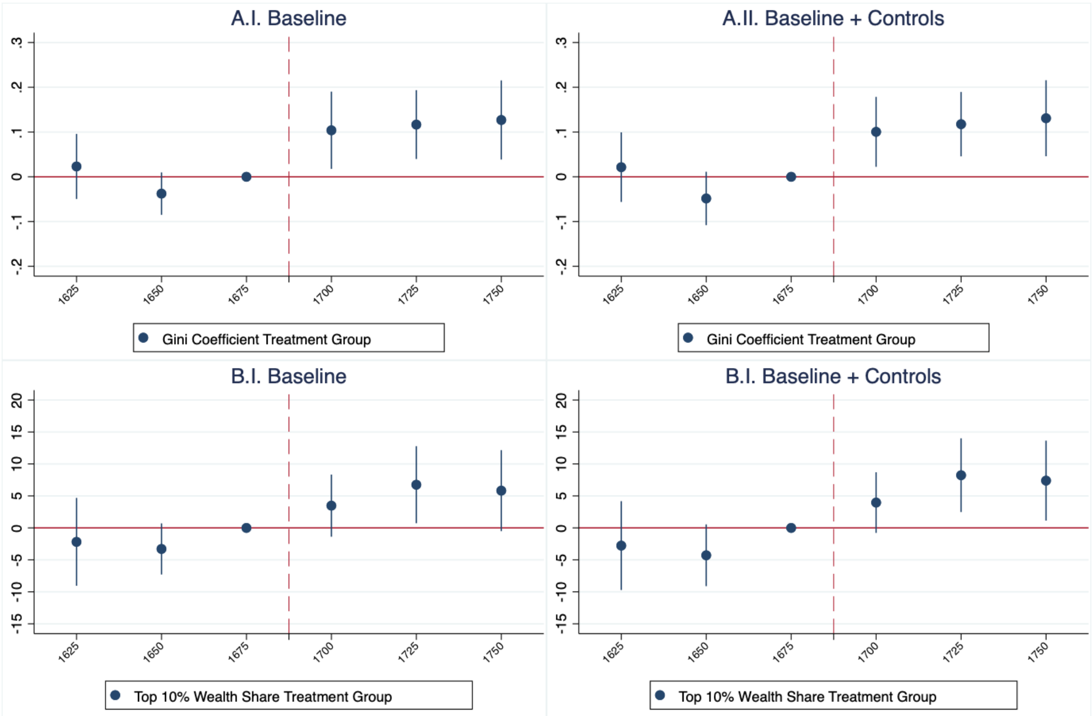
Notes: Estimation method is OLS. All regressions include a full set of locality and time fixed effects, Imperial circle-time interaction effects and a variable controlling for exposure to other wars. Controls include the log-population size, the occurrence of epidemics, the introduction of the Protestant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

League, might have experienced inequality decline, relative to other treated communities.

Figure 3.7 shows an inequality pattern for the Palatine Succession War not dissimilar to the Schmalkaldic War. There was again no statistically significant inequality difference between treated and untreated communities before the war, supporting the claim that common trends are a reasonable assumption for individual towns when warfare is the treatment. Yet after the war began, a substantial increase in inequality emerged. The result holds with the Gini coefficient or the top 10 percent wealth share as inequality indicator, and across different specifications.

One might wonder why the inequality differences in Figures 3.6 and 3.7 show signs of persistence or even continued inequality growth, decades after the wars. Two theoretically possible explanations immediately come to mind. Persistent inequality differentials would

Figure 3.7: The Effect of the Palatine Succession War (1688-98) on Inequality (Flexible Difference-in-Differences Estimates)



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

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

3.6 Conclusion

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

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

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

3.7 Appendices

3.7.1 Historical Background

The aim of this Appendix is to provide more historical background information for the conceptual framework in the main text, and the area of study: the Holy Roman Empire.

Bellicosity in the Holy Roman Empire

It is probably true that "promoting peace between its members [was] the Empire's core function" (Chilosi et al. 2018: 665). Many historians have pointed out that the Empire took measures to increase security and limit bellicosity from the end of the fifteenth century onwards. These measures were the *Ewige Landfrieden* (Eternal Peace) of 1495 and the ordinance concerning the implementation of Imperial law (*Reichsexekutionsordnung*) of 1555. These Imperial acts outlawed feuds, thus prohibiting the common man to legitimately use force. Moreover, conflicts between Imperial estates were legally banned. The princes were responsible for the implementation of the *Landfrieden* in the territories. Conflicts between Imperial estates were referred to the *Reichskammergericht* (Imperial Chamber Court), or, in case of large conflicts, to the *Reichskreise* (Imperial circles) (Whaley 2012). Yet the numerous recorded conflicts (see the main text) suggest that the Empire has not been entirely effective in avoiding conflicts and even less in shielding itself from foreign interference.

Local Protection

The wish to protect the own existence, but also economic activity and political independence made local defence infrastructure indispensable (Volckart 2002b, Isenmann 2014). The expansion of defence infrastructure was accelerated by constant innovations in military technology in preindustrial Europe during the "Military Revolution", which had its early beginnings in the fourteenth century, but is typically considered to have accelerated in the sixteenth until the eighteenth century. 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 and the professionalisation of armies. The more military technology advanced, the more costly it became for communities to adapt to it (Parker 1996, Kroener 2013).

This adaptation happened in three main areas. First, the evolution of military technology made existing fortification obsolete in case of attack. As a response, walls and gates became stronger over the centuries, equipped with more towers, wards, several rings of additional walls and earthworks (*Landwehren*). For example, in Nuremberg the fortification was reinforced after the War of the Swabian League (1376-77) and foreseeing the War of the Cities (1387-94). Then, in response to the Hussite Wars (1419-34), Nuremberg reinforced its town moat. In 1452, just after the First Margrave War (1449-50), a second city wall was built, including the installation of cannons at every gate. Further improvements of the fortifications were undertaken later during the early modern period, to keep up with the technical and tactical improvements in artillery (Isenmann 2014). Hamburg, for instance, 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). Needless to say that the updating of defence construction was an uneven process across Germany and that not all communities built sophisticated fortifications. Some towns still had wooden walls as late as the sixteenth century when others already had sophisticated fortification (Isenmann 2014: 102). This variation in defence buildings across communities was strongly associated with the intensity of warfare in preindustrial Germany (Dincecco and Onorato 2016). But even small towns and villages reacted in ways that were analogous to those of large towns to the risk of warfare, by expanding their simple fortification (Blickle 1981). For example, during the Saxon Fratricidal War (1446-51) the small rural town Altenburg built a bulwark and strengthened town wall and moat, although the town was not even directly attacked (Löbe 1895: 166-168).

Second, warfare triggered the acquisition of weapons and of military construction other than fortification in towns. A local administration for procuring, repairing, using and storing military equipment developed, including the building of expanded armour chambers and arsenals, firing ranges (*Schützenhaus*) or stables for war horses (*Marstall*). For example, Überlingen reacted to the threats of the Swabian War (1499) with the acquisition of new canons and built an arsenal for storing these and other weapons (Koberg 1975, Isenmann 2014).

Third, exposure to warfare required communities to deploy more personnel for a wide range of defence-related tasks. For example, squads of guards (*Schützenkorps*) and sometimes entire garrisons were employed for defence, usually led by highly-paid captains (*Kriegshauptmann*). This was necessary not just to fend off actual attacks, or participate in battles, but also because of the general risk created in wartime, for example by marauding mercenaries. Therefore, the number of military forces had to be augmented, to staff all parts of a town's fortification, for providing safe conduct, for policing and dealing with robbers (Isenmann 2014). Over time, professional, more effective and costly mercenaries became widely used for protective tasks, replacing laymen (Kroener 2013: 58-59). Moreover, communities hired highly paid specialists, for procuring, using, repairing and storing weapons (*Büchsenmeister, Zeugwart*), for defence construction (*Festungsbaumeister*) and for managing supplies (*Proviantmeister*) (Isenmann 2014). 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 as weapon master (Koberg 1975: 62-64). Additionally, communities employed officials for defence-related tasks in their administration. Officials devised security legislation: communities introduced so-called "police ordinances" (*Polizey Ordnungen*), not to be confounded with the modern notion of police. 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 how to deal with criminal activity of vagrants, robbers, bandits and marauding soldiers (Härter 2010: 44-46, 52). Local governments also formed special war councils (*Kriegsrat*) to manage the risk from threats, and administered taxation. Such labour-intensive defence-related tasks were costly, because officials earned high wages (Isenmann 2014).

Yet communities had to make additional expensive efforts in wartime. For example, they had to provide board and lodging for soldiers fighting nearby, ransom the community from the grip of mercenaries or redeem imprisoned inhabitants as part of the community's protective task (Friedrichs 1979). In sum, warfare was a huge challenge for communities, in terms of effort and economic resources needed. This resource need increased constantly with the progress in defence technology during premodern times.

Resource Extraction

The abrupt increase of financial need to pay for protection could become a ruinous burden for a community's budget in wartime. Communities taxing their inhabitants because of warfare is historically well documented. For instance, Konstanz tripled its wealth tax to cover the costs of the Old Zurich War (1439-50) (Kirchgässner 1960: 89). Moreover, larger urban communities protected and taxed small surrounding rural communities, and also rural communities themselves levied taxes on their inhabitants (Isenmann 2014, Volckart 2004: 27). Additionally, territorial rulers' requests sometimes increased the local tax burden. From the late seventeenth century rulers of the largest territories within Germany built up defence systems, consisting mainly of fortifications and troops.[28] Usually the costs were burdened on the communities (Parker 1996, Kroener 2013). For example, during the War of the Austrian Succession townsmen of Freiburg had to pay a daily poll tax to finance the reinforcing of the local fortification and soldiers, as ordered by the territorial ruler (Hohrath 1996: 321-323).

Credit was the second principle mode to finance war-related expenses. Warfare increased the demand for credit substantially. Cologne, for instance, borrowed more in two years to cover the costs of the Neuss War (1473-80) than it had borrowed in the previous half century (Stasavage 2011: 113). In addition to immediate taxation, credit had a further inequality-promoting characteristic, deriving from who had to bear the burden of repayment and interest, and who benefited from it. Individuals of low socio-economic class rarely lent money to authorities in preindustrial times (see Chilosi et al. 2018: 651-653). Often there was a minimum amount required from those who wished to buy titles of the public debt. So the poorest were actively excluded from this investment opportunity. Instead, poor people could expect to be subject to rising regressive taxes in order to pay for the debt (Alfani and

[28] This expansion of territorial military infrastructure did not apply to many territories. Schmidt (2009: 37) only counts eight imperial estates (out of more than 300) that had standing armies of a size worth mentioning in the early eighteenth-century.

Di Tullio 2019, Stasavage 2011, Dirlmeier 1978).

In the main text taxation and credit are the main extraction channels, but also war profiteering — or in other words, the way extracted resources were spent — is discussed. The inequality-promoting effect of defence expenditure is difficult to demonstrate systematically (for some indicative evidence see Table 3.7), even with modern data from the industrial period (see Scheve and Stasavage 2016). Yet there is anecdotal evidence suggesting that war-related expenditure enriched producers and traders of military equipment and services, such as soldiers, their leaders, and producers of military goods, especially if they held monopoly rights (Schulze 1995: 279). This enrichment from extracted resources potentially favoured inequality growth (Alfani and Di Tullio 2019). A case in point is the Holy Roman Emperor's military commander-in-chief Wallenstein (1583-1634). He reportedly became immensely rich thanks to his activities as creator, leader and equipment supplier of the Imperial army in the seventeenth century (Schilling 1994: 423, Asch 1997, Alfani 2023).

Additionally, another extractive force that might have increased inequality during warfare were corporate groups, especially guilds. They provided prompt financial support and military services to political authorities in war times. But guilds did not provide their services for free. In exchange, political authorities granted guilds' businesses monopoly rights, such as control of prices and wages, entry restrictions or labour market regulations. Granting guilds such market power, that is, the right to overcharge consumers, redistributed parts of the economic pie to themselves and exacerbated economic inequality (Ogilvie 2019, 2021).[29]

Destruction and Inequality during the Thirty Years' War

The Thirty Years' War was exceptional in several respects. There are at least four theoretical reasons which suggest that this war reduced economic inequality: demolition of physical capital, expropriation and plundering, contraction of commerce and production, and population

[29] Research on modern development economies has documented that increased market power, in the hands of special interest groups such as associations of businessmen, increases wealth inequality substantially, raising the top 10 percent wealth share by 10 to 24 percent. This is unsurprising because market power allows to charge supracompetitive prices, thus redistributing wealth from consumers to holders of market power (Ogilvie 2021: 194). It is reasonable to assume that granting market power to associations of businessmen might have had qualitatively similar effects in a historical developing economy like preindustrial Germany.

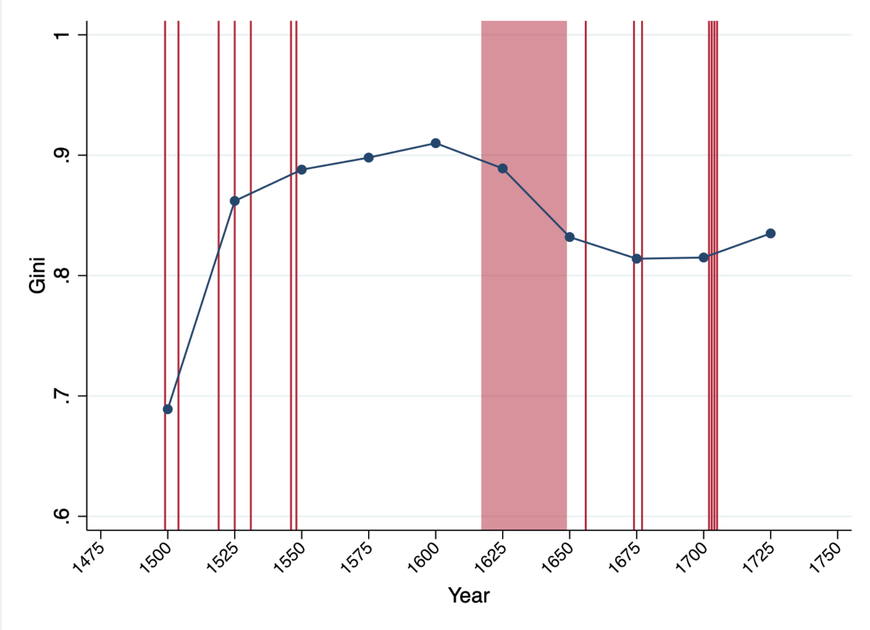
decline in combination with plague. The first three factors most likely curtailed the wealth of the rich. The fourth factor combined the selective extermination of the poor and simultaneously enabled the survivors to earn higher wages, thus redistributing economic resources towards the surviving poor. In consequence, inequality was reduced from the top and the bottom of the wealth distribution (Alfani et al. 2022). Bringing together many destructive factors and the long duration of the war probably let its effect spill over all of Germany. Yet at the same time military events remained limited to the German lands, concentrating the destruction of this monumental "European war" there (Burkhardt 1992). Historians argue that this combination of factors made the Thirty Years' War an exceptionally intense war, a universally destructive shock to the German economy. Proportionally it was even more destructive than the two world wars, and definitely more destructive than any other conflict in early modern Europe, at least in terms of its demographic impact (Wilson 2009: 787). This implies that one should not be too surprised to find that the Thirty Years' War's negative impact on inequality was not the historical norm. This is also suggested by Figure 3.8, which shows the inequality reduction the war caused (red box) in Augsburg, a case that has often been cited to exemplify the equalising effect of warfare (van Zanden 1995, Scheidel 2017, van Bavel 2020).

However, Augsburg was exposed to other conflicts during the early modern period (indicated by red lines): to battles of the War of the Succession of Landshut, the Schmalkaldic War, the Palatine Succession War and others. Yet contrary to what happened during the Thirty Years' War, after most other conflicts inequality in Augsburg increased. This suggests that the relationship that ran from conflict exposure to inequality was not always negative or positive. In Augsburg, the destructive effect might have outweighed the extractive effect only during one truly major war.

Outbreak of the Schmalkaldic War (1546-47)

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-

Figure 3.8: Inequality and Conflicts in Early Modern Augsburg



Notes: Own calculations. 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.

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. An open confrontation was postponed, due to the emperor's ongoing conflicts with the Ottoman Empire and France. When these were over, Charles V attacked the Schmalkaldic League in 1546, near Füssen in Southern Germany (Schilling 1994, Whaley 2012). Other conflicts took place in the Centre-South, the North-West and the East of Germany. Local governments reportedly increased inequality-promoting taxation to extract the necessary means to cover the costs of the Schmalkaldic War. For example, in Augsburg the city council decided to increase consumption taxes to cover the costs of war right the month after the conflict had ended (Roth 1928: 372).

Outbreak of the Palatine Succession War (1688-97)

The Palatine Succession War broke out when the Elector Palatine Carl II died in 1685 after a short reign without heir. The Protestant line of the Wittelsbach family had died out and several parties put forward claims to inherit land and title. The French King Louis XIV, eager to extend his territory in the North East, advanced a claim based on the hereditary title of Princess Liselotte of the Palatine, the last Elector's sister and wife of Louis' brother Philippe I Duke of Orléans. However, the Catholic emperor conferred the Electoral title of the Palatine to his ally Philipp Wilhelm of Pfalz-Neuburg, of the Catholic line of the Wittelsbach family. Philipp Wilhelm refused the French King's claim for land, which led Louis to invade the Empire in 1688 and start the war. Emperor Leopold I and the new English King Wilhelm III did not want Louis to get away with what they saw as an illegitimate raid, and led a broad defence alliance against France. After an initial phase of battle action in the South West of the Empire, the war soon spread through other parts of Europe and even reached the Americas (Schilling 1998).

3.7.2 Construction of the Conflict Database

The staring point for the conflict database employed in the main analysis was the existing database of Dincecco and Onorato (2016). Their database focuses on major conflicts for the whole European continent, leaving out several smaller conflicts that might have been relevant for specific regions. I have upgraded that database by adding information on battles and their location from several secondary sources, focusing specifically on the conflicts taking place within the Holy Roman Empire. I have consulted the following books and articles: Brandstätter 2009, Clodfelter 2008, Darby and Fullard 1970, Demandt 1980, Glasauer 2017, Isenmann 2014, Jahn 1997, Jaques 2007, Jensch 2015, Kirchgässner 1964, Kohn 2007, Kroener 2013, Leng 2012, Löbe 1895, Niederstätter 2011, Potthoff 1911, Press 1977, 1985, Schilling 1994, 1998, Schrörs 1878, Schubert 2011, Seyboth 2014, Stanelle 1982, Sternberg 2004, Voges 1988, Whaley 2012, Zeilinger 2015, Zemek 2014.

I have cross-checked against individual conflict entries on www.wikipedia.de. I have then georeferenced these conflicts using mapcoordinates.net. All conflicts fall within the wider Holy Roman Empire, using the borders indicated by Volckart (2020). 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.

3.7.3 Simulation of Personal Wealth Accumulation during Warfare

The predictions of the theoretical framework introduced in the main text can be simulated by plugging in reasonable values for the parameters. 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. 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 2022). As values for initial wealth W_t^L and W_t^H 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 2022). The measure of inequality is the ratio between the average wealth share between rich and poor households:

$$W^H/W^L.$$

This is a simple inequality indicator in the spirit of "Kuznets-shares" (see also Piketty 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 preindustrial Germany. It is at best possible to make an educated guess about the savings rates of poor and rich in relation to their wealth, because there exist no estimates for preindustrial times. Poor people have a lower savings rate than rich people, in preindustrial as in industrial times (Alfani and Di Tullio 2019, Saez and Zucman 2016). Most investments in public debt were made by wealthy individuals (Chilosi et al. 2018: 653). However, the definition of the poor in my 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 had a savings rate which was on average smaller than those of the rich, but nevertheless above zero. 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.[30] These parameters determine wealth accumulation in peace time.

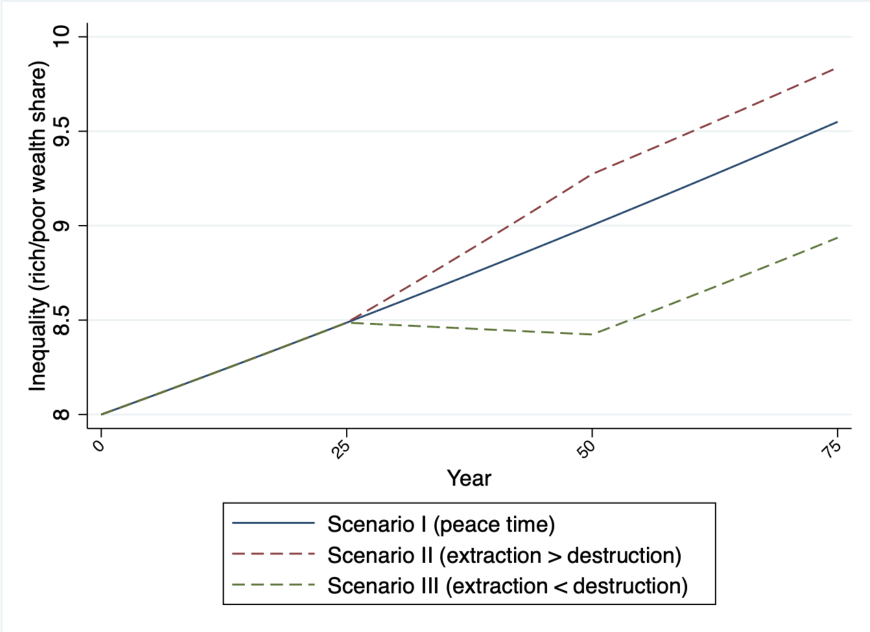
In wartime the extraction and destruction rates play in. It is again difficult to come up with absolute extraction and destruction rates, but to highlight the countervailing effects of the two factors it suffices to make an assumption about their relative importance. I assume that the extraction rate 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. These might seem arbitrary values, but the direction of inequality development remains qualitatively 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 3.9 shows how these assumptions affect the development of wealth inequality in a simple simulation. Three scenarios have been simulated: peace time (Scenario I), a war where extraction dominated (Scenario II), and a war where destruction dominated (Scenario III). 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 (years 0 to 25), there is no warfare and therefore neither extraction nor destruction. Inequality develops into the same direction in all three scenarios. It increases because of the higher savings rate of the rich.

In period two (years 25 to 50), warfare comes into play in scenarios two and three. The peace-time scenario represents the counterfactual inequality-development without warfare.

[30]The amount rich households saved in relation to their *income* was probably much larger than what they saved in relation to their capital stock. For example, in a similar simulation Alfani and Di Tullio (2019: 156) set the savings rate of the rich to 20-40 percent of their income. For keeping the model simple, and because there exist no income estimates for different socio-economic classes in preindustrial Germany, I calculate savings in relation to already accumulated wealth.

Figure 3.9: Simulated Wealth Inequality during Warfare



In Scenario II wealth inequality increases with respect to the counterfactual situation, the typical development during ordinary wars. Conversely, Scenario III shows that inequality may be reduced with respect to the counterfactual and even decline in absolute terms *if* the destructive effect is large enough to offset the extractive effect. Historically this happened in major wars only, such as the Thirty Years' War. In the third period (years 50 to 75) there is no warfare and inequality develops into the same direction in all three scenarios. However, the inequality levels of the three scenarios differ because warfare has permanently changed the wealth distribution in scenarios two and three.

3.7.4 Wars and Their Causes

Table 3.8 lists all wars that are part of the dataset and schematically summarises the causes why they broke out according to the historical literature, including references to the relevant literature. I also include a qualitative assessment of whether the war reasons included in inequality in the population, and thus whether the war was a plausibly exogenous event from the perspective of an individual locality. 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). However, in 84 out of 86 wars, economic inequality among the population did not play a role according to the historical literature, supporting the notion that these wars were plausibly exogenous events from the perspective of an individual town. In a later section I repeat the baseline analysis from the main text, dropping those potentially endogenous wars, which does not change the results.

Table 3.8: Wars and Their Causes

Date	War	Main Causes	Plausibly Exog.
1351-1401	Florentine-Milanese Wars	Sphere of influence (Jaques 2007: 918)	YES
1369-1389	Caroline War (100-Years' War)	Territorial gain (Wagner 2006: 86-87)	YES
1371-1379	1. Gueldarian Succession War	Succession (Sternberg 2004: 111)	YES
1376-1377	War of the Swabian League	Independence (Isenmann 2014: 322)	YES
1386	Sempach War	Territorial gain (Marchal 1987: 432)	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: 84)	YES
1400	Bergtheim War	Independence (Leng 2012)	YES
1401-1429	Appenzell Wars	Independence, payment of levy (Burmeister 2001)	YES
1409-1435	Kalmar War with Holstein	Territorial gain (Kohn 2007: 284)	YES
1410-1413	Austrian War	Strategic routes, territorial gain (Brandstätter 2009)	YES
1415-1453	Lancastrian War (100-Years' War)	Succession (Wagner 2006: 149-150)	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
1422-1435	Kalmar War with Hanseatic League	Strategic route, trade dispute (Kohn 2007: 283)	YES
1423-1444	2. Gueldarian Succession War	Succession (Ferber 1863: 14-15)	YES
1423-1454	Venetian-Milanese Wars	Power balance, strategic routes (Lane 1973: 225-230)	YES
1427	Mainz-Hesse War	Sphere of influence, territorial gain (Demandt 1980: 196)	YES

Date	War	Main Causes	Plausibly Exog.
1439-1450	Old Zurich War	Succession (Illi 2015)	YES
1444-1448	Soest Feud	Independence, territorial gain (Heimann 2009: 321-322)	YES
1446-1451	Saxon Fratricidal War	Succession (Löbe 1895: 157-161)	YES
1447-1450	Milanese War of Succession	Succession (Jaques 2007: 200)	YES
1449-1450	1. Margrave War	Territorial gain, revenues natural 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	Succession, ecclesiastical control (Sprenger 1999: 207-208)	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	Succession (Hollegger 2005: 42-45)	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
1501-1512	Danish-Lübish War	Territorial gain (Kohn 2007: 155)	YES
1503-1505	Landshut Succession War	Succession (Schmidt 2004: 7)	YES
1508-1516	War of League of Cambrai	Power balance (Clodfelter 2008: 8)	YES
1519	War of Swabian League against U.v. Württemberg	Territorial gain (Isenmann 2014: 322)	YES
1519-1523	Hildesheim Diocesan Feud	Territorial gain, payment of levy (Brüdermann 2000: 445)	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
1525-1544	Italian Wars Between Charles V. and Francis I.	Territorial gain (Kohn 2007: 261)	YES
1526-1533	1. Habsburg–Ottoman War (Hungary)	Succession (Schilling 1994: 224)	YES

Date	War	Main Causes	Plausibly Exog.
1531	Second War of Kappel	Confessional dispute (Clodfelter 2008: 12)	YES
1534	Battle of Lauffen	Restitution political order (Whaley 2012: 308)	YES
1534-1535	Muenster Rebellion	Confessional dispute, ecclesiastical control (Schilling 1994: 175)	YES
1542	Brunswick War	Confessional dispute, political alliance (Schilling 1994: 227)	YES
1543	3. Gueldarian Succession War	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
1554	Occupation of Bergedorf by Brunswick-Wolfenb.	Strategic route (Baasch 1905)	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
1609-1614	War of the Succession of Jülich	Herreditary Succession (Kohn 2007: 278)	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)	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
1651	Duesseldorf Cow War	Territorial gain (Schilling 1998: 202)	YES
1652-1654	1. Dutch War	Trade dispute (Press 1991: 390)	YES
1653	Swiss Peasant War	Social discontent (Suter 2010)	NO
1656	1. Villmergen War	Confessional dispute (Press 1991: 388)	YES

Date	War	Main Causes	Plausibly Exog.
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	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	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	Succession (Clodfelter 2008: 77)	YES
1740-1748	Austrian Succession War	Succession, territorial gain, power balance (Schilling 1998: 289-290)	YES
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 whether a territory became Protestant or remained Catholic. "Ecclesiastical control" means for example a dispute over which church or candidate gets to hold a bishop office. "Succession" means for example a dispute because a ruler dies without male heir. "Imperial status" means for example a dispute over receiving elector dignity. "Independence" means for example a dispute over political independence of a region from a ruler's rule. "Payment of levy" means for example a dispute over the payment of a rent. "Political alliance" means for example a dispute because a ruler leaves a previous political alliance. "Power balance" means for example a dispute to avoid that one polity gets too powerful. "Privilege restoration" means for example a dispute to obtain back an old privilege. "Restitution political order" means for example a dispute to reinstall a discharged ruler. "Revenues natural resource extraction" means for example a dispute over revenues from a mining monopoly. "Social discontent" means for example a dispute because of impoverishment of social classes. "Sphere of influence" means for example a dispute over who is the dominate power in a contested region. "Strategic routes" means for example a dispute over the control of a river. "Territorial gain" means for example a dispute because a ruler attempts to round off his territory. "Trade dispute" means for example a dispute over the right to trade within a region. In the column that indicates plausible exogeneity "YES" means that the war was clearly exogenous, "NO" means that it was likely endogenous.

3.7.5 Coding of Independent Variables

This Appendix describes how the control variables employed in the analysis in the main text and the Appendix 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 typically assumed for preindustrial German towns is 4.5 (Minns et al. 2020: 611).

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*. Epidemics indicated by the *Städtebuch* are for example smallpox, syphilis and different forms of plague. For those rural communities in the dataset that have no entry in the *Städtebuch* I had to make an assumption about plague occurrence. I assumed that the rural communities had the same plague occurrence as a town nearby for which an entry in the *Städtebuch* and information about the outbreak of epidemics is available. 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). For those villages that were under the administrative authority and were taxed by a nearby city that is part of the dataset, I have assumed the same occurrence of epidemics as in the city. For example, for the rural community of Niederwangen I assume the same plague occurrence as for the nearby city of Wangen. For those villages that were not under the administrative authority of a city in the dataset I have assumed the same occurrence of epidemics as in the closest town with an entry in the *Städtebuch*.

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 introduces 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 communities without entry in the *Städtebuch* usually 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).

Garrison. Variable that indicates whether a community had one or several garrisons in the previous period. Information has been taken from the *Städtebuch.* The information is not available for all communities. Some communities have no entry in the Städtebuch, in other cases the entry does not contain the section on garrisons, and again in other cases the relevant section exists but contains too imprecise information (for example, no dates).

Military construction. Variable that indicates one or several incidents of military construction in the previous period. Information has been taken from the *Städtebuch.* The information is not available for all communities. Some communities have no entry in the Städtebuch, in other cases the entry does not contain the sections on construction activity, and again in other cases the relevant sections exist but contain too imprecise information (for example, no dates). I have classified as military construction barracks, arsenals, stables for warhorses, gunmen houses (*Schützenhaus*), firing ranges or foundries for weapons. My classification scheme is analogous to the scheme of Cantoni et al. (2018).

City. A dummy that indicates whether a locality was a city. I consider all those localities as cities that have an entry in the *Städtebuch.*

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).

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.

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

Hanse member. A dummy that indicates whether a locality was member of the Hanseatic league, as indicated by Dollinger (1981: 68).

3.7.6 Additional Results

Plausibly Exogenous Wars

In this section I test whether the baseline results hold when dropping those conflicts that broke out because of reasons that suggest these events were not plausibly exogenous events from the perspective of an individual community (see Table 3.8). The results in Table 3.9 suggest that these few wars do not drive the main results of the chapter when taking different inequality indicators as dependent variable and across different time periods. All coefficients have a similar magnitude and significance compared to the baseline results reported in the main text.

Balanced Sub-Panel

In this Appendix I report the baseline specification for a balanced sub-panel. This reduces the number of observations substantially, so I limit the analysis to the period from 1500 to 1600 because that is the century with the highest data concentration. The results show that the inequality-increasing effect of wars holds even in this restricted panel, taking the Gini

Table 3.9: Plausibly Exogenous Wars

	1400-1600 & 1675-1800				1400-1800
	(1) Gini	(2) Gini	(3) Bot. 50%	(4) Top 10%	(5) Gini
Conflict exposure	0.032***	0.025**	-1.659**	1.411	0.026**
	(0.010)	(0.011)	(0.687)	(1.077)	(0.011)
Locality FE	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES
Imperial circle × time FE	NO	YES	YES	YES	YES
R^2	0.267	0.327	0.307	0.276	0.329
Observations	434	434	434	434	504

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

coefficient and the wealth share of the bottom 50 percent of the population as outcomes. The coefficients suggest that the results are not driven by the unbalanced structure of the panel used in the main analysis.

Table 3.10: Balanced Sub-Panel, 1500-1600

	1500-1600	
	(1) Gini	(2) Bot. 50%
Conflict exposure	0.027**	-2.196**
	(0.012)	(0.806)
Observations	166	166
R^2	0.303	0.298
Locality FE	YES	YES
Time FE	YES	YES

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

Inequality Estimates without the Propertyless

The inequality estimates in the main text are based on wealth distributions including propertyless households, as far as they were recorded in the tax registers. In this Appendix I repeat the main analysis, using only inequality estimates that are based on wealth distributions from which I have dropped the propertyless households. The principal hypothesised mechanism connecting warfare to inequality growth in this paper is regressive resource extraction, which by implication hit poorer strata harder. One would therefore expect that dropping a part of the poor strata — the propertyless — potentially leads to smaller and less precisely estimated coefficients, while still pointing towards inequality increase. That is exactly what the results in Table 3.11 suggest, where I estimate again the baseline specification.

Table 3.11: Conflict Exposure and Economic Inequality: No Propertyless Households

	1400-1600 & 1675-1800						1400-1800
	(1) Gini	(2) Bot. 50%	(3) Top 10%	(4) Gini	(5) Gini	(6) Gini	(7) Gini
Conflict exposure (200km)	0.020**	-1.256*	1.187	0.021**		0.016*	0.018**
	(0.010)	(0.628)	(1.103)	(0.010)		(0.009)	(0.009)
Conflict exposure (25km)				-0.005			
				(0.010)			
Conflict exposure (25-200km)					0.018*		
					(0.010)		
Locality FE	YES	YES	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES	YES	YES
All time-variant controls	NO	NO	NO	NO	NO	YES	NO
Observations	380	380	380	380	380	380	445
R^2	0.332	0.354	0.212	0.333	0.330	0.355	0.335

Notes: Estimation method is OLS. Standard errors clustered at locality level in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Time-variant controls include: log-population size, occurrence of epidemics, introduction of the Protestant Reformation, log-distance to the closest university.

Columns 1 to 3 suggest that conflict exposure led to higher overall inequality as measured with the Gini coefficient, a reduced wealth share of poor strata and a higher wealth share of the rich, although this result in Column 3 is statistically not significant (as in the main text). Columns 4 and 5 show that warfare also led to higher inequality when conflicts in the immediate surrounding of a locality are controlled for, and if one only considers the conflicts within the "donut" between 25 and 200 kilometres around a community. In Column 6 all time-variant controls have been added, and in Column 7 the whole period from 1400 until 1800, including the reportedly inequality-reducing Thirty Years' War (see Alfani et al. 2022), is considered. Still, there seems to be a positive relationship running from conflict exposure to inequality, as the estimates in the main text suggest. Overall, the results imply that including or excluding the propertyless from the analysis does not drive the results of this paper, which is reassuring. However, the fact that dropping the propertyless leads to smaller and less precise estimates is actually consistent with the hypothesis that regressive resource extraction was at least part of the reason why warfare increased inequality.

Spatial Dependence

Table 3.12 reports the baseline specifications when standard errors are adjusted for spatial autocorrelation (Conley 1999; see also Gibbons et al. 2015: 137). The Conley-correction for spatial dependence is considered sufficient to assess the significance of regression results (Voth 2021: 259). 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.

Table 3.12: Spatial Autocorrelation-Adjusted Std. Errors

	1400-1600 & 1675-1800				1400-1800	
	(1) Gini	(2) Gini	(3) Gini	(4) Gini	(5) Gini	(6) Gini
Conflict exposure	0.032***	0.032***	0.032***	0.032***	0.030***	0.030***
	(0.010)	(0.009)	(0.008)	(0.007)	(0.009)	(0.007)
Cutoff	50km	200km	500km	1000km	200km	1000km
R^2	0.027	0.027	0.027	0.027	0.024	0.024
Observations	434	434	434	434	504	504

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

Columns 1 to 4 show the results of the baseline specification (see Column 1 of Table 3.4 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. Again, the estimates of the main variable of interest remain highly significant regardless of the cutoff. 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.

One might still wonder whether there existed actual spatial autocorrelation among localities, which could inflate t-statistics in the regressions. For addressing this potential concern, Moran's I has been calculated to evaluate whether there is spatial autocorrelation in the regression residuals. This has been done for the respective cross-sections of the panel-dataset, as done by Kelly (2019).

Table 3.13: Spatial Dependence in Regression Residuals (Global Morans'I-Statistic)

	(1)	(2)	(3)
Year	I-statistic	Z-score	P-value
1400	-0.308	-1.308	0.095
1425	-0.270	-1.009	0.156
1450	-0.065	0.008	0.497
1475	-0.117	-0.780	0.218
1500	-0.106	-1.043	0.148
1525	-0.063	-0.568	0.285
1550	-0.031	-0.172	0.432
1575	-0.015	0.073	0.471
1600	-0.034	-0.248	0.402
1625	-0.089	-0.788	0.215
1650	-0.081	-0.643	0.260
1675	-0.141	-1.009	0.156
1700	-0.217	-1.728	0.042
1725	-0.246	-1.941	0.026
1750	-0.040	0.112	0.455
1775	-0.388	-3.194	0.001
1800	-0.316	-2.228	0.013

Following Voth (2021) I assess the presence of spatial dependence in the most saturated specification. It includes all controls of the main analysis, but without any of the fixed effects, due to the cross-sectional data structure. Instead I have added longitude, latitude and the interaction of longitude and latitude. Kelly (2019: 17, 22) considers positive z-scores of two and greater as indicative of relevant spatial autocorrelation. Table 3.13 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 would be a reason of concern, is not an issue for this study. Only in a few years at the end of the study period there is significantly 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 few periods. Spatial dependence, therefore, seems to be a negligible concern for this study. Moreover, note that more sophisticated methods to deal with spatial dependence are not feasible because of the highly unbalanced structure of the panel. For example, because a spatial weighting matrix cannot be calculated with the available data, estimating a spatial autoregressive (SAR) model is impossible (Gibbons et al. 2015: 128).

Conflict Distance Test

In Table 3.14 I report full regression results for the test of different conflict distances, reported graphically in the main text. Note, first, that the best fit of the model (highest R-squared) is obtained with the 200-kilometres threshold. Second, the 200-kilometres threshold does not have the lowest p-value. The 150-, 175- and 225-thresholds have the lowest p-value of 0.001.

Target Effect Test

To provide further evidence for the assumption that the positive association between warfare and inequality actually runs from the the former to the latter factor, I perform a 'target test": I regress the conflict exposure variable on lagged values of the Gini coefficient. More unequal places might have offered richer booty for attackers. If the relationship documented in the main text actually ran from conflicts to inequality, then conflicts should not be predicted by past inequality, evidenced by an insignificant coefficient. In other words, this approach is a test for the presence of reverse causality (Dincecco and Onorato 2016).

Table 3.15 shows that conflict exposure is not significantly predicted by past inequality. In Columns 1 and 2 I test the relationship in the full dataset. In subsequent specifications I limit the dataset to those communities that had city status, because one might suspect

Table 3.14: Conflict Distance Test

	1400-1600 & 1675-1800											
	(1) Gini	(2) Gini	(3) Gini	(4) Gini	(5) Gini	(6) Gini	(7) Gini	(8) Gini	(9) Gini	(10) Gini	(11) Gini	(12) Gini
Conflict exposure (25km)	-0.014											
	(0.009)											
Conflict exposure (50km)		0.003										
		(0.007)										
Conflict exposure (75km)			0.004									
			(0.006)									
Conflict exposure (100km)				0.011								
				(0.007)								
Conflict exposure (125km)					0.021***							
					(0.008)							
Conflict exposure (150km)						0.024***						
						(0.007)						
Conflict exposure (175km)							0.027***					
							(0.008)					
Conflict exposure (200km)								0.032***				
								(0.010)				
Conflict exposure (225km)									0.030***			
									(0.009)			
Conflict exposure (250km)										0.025***		
										(0.009)		
Conflict exposure (275km)											0.014	
											(0.009)	
Conflict exposure (300km)												0.011
												(0.009)
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	YES	YES	YES
R^2	0.249	0.247	0.247	0.252	0.264	0.264	0.262	0.267	0.265	0.259	0.250	0.248
Observations	434	434	434	434	434	434	434	434	434	434	434	434

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

Table 3.15: Target Effect Test

	1400-1600 & 1675-1800					
	(1) Conflict exp.	(2) Conflict exp.	(3) Conflict exp.	(4) Conflict exp.	(5) Conflict exp.	(6) Conflict exp.
Gini (1. lag)	0.310	0.276	0.081	-0.013	0.102	0.027
	(0.203)	(0.215)	(0.345)	(0.371)	(0.524)	(0.676)
Gini (2. lag)					0.348	0.173
					(0.492)	(0.416)
Cities only	NO	NO	YES	YES	YES	YES
All controls	NO	YES	NO	YES	NO	YES
R^2	0.512	0.519	0.558	0.576	0.463	0.604
Observations	369	368	220	219	191	190

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

that cities were particularly attractive targets. Their economies were more trade-oriented, characterised by riches held in more mobile assets compared to rural, agriculturally-oriented areas. In Columns 5 and 6 I also test whether the Gini in the more distant past (second lag) might explain conflict exposure. None of the coefficients is significant. This strengthens the argument that it was actually past conflict exposure that led to an increase in inequality, and not vice versa.

Alternative Conflict Measures

As an additional robustness check, alternative conflict measures are employed in Table 3.16. In the main analysis a simple dummy variable has been used, which has the advantage of mitigating potential measurement error. I have constructed an ordered categorical variable as an alternative (Columns 1 to 3), 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. Unsurprisingly, the coefficients are smaller, but point into the right direction and are mostly significant.

Additional alternative measures are reported in Column 4 to 6. Two dummy variables are employed: the first takes the value one when a community was exposed to exactly one conflict, the second takes the value one if the community was exposed also to at least two conflicts. Both coefficients are positive and significant across specifications, indicating that communities that were repeatedly exposed to warfare also experienced a higher inequality increase.

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.

Inequality of War-Losers during the Schmalkaldic War

This Appendix complements the case study of the Schmalkaldic War in the main text. Scheidel (2017: 199) hypothesises that premodern wars reduced economic inequality in a

Table 3.16: Alternative Conflict Measures

	1400-1600 & 1675-1800					
	(1) Gini	(2) Gini	(3) Gini	(4) Gini	(5) Gini	(6) Gini
Conflict exposure (ordered)	0.012**	0.011**	0.008			
	(0.005)	(0.005)	(0.005)			
Conflict exposure (singular exposure)				0.045***	0.042**	0.044**
				(0.015)	(0.016)	(0.017)
Conflict exposure (repeated exposure)				0.028***	0.026**	0.019*
				(0.010)	(0.010)	(0.010)
All controls	NO	YES	YES	NO	YES	YES
Locality FE	YES	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES	YES
Imperial circle × time FE	NO	NO	YES	NO	NO	YES
R^2	0.258	0.291	0.357	0.270	0.302	0.370
Observations	434	434	434	434	434	434

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

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 is one of the rare occasions to test this argument, because the war had a clear loser: the members of the defence alliance Schmalkaldic League (Schilling 1994).

In Table 3.17 I estimate the DD effect of the war, employing a single post-treatment indicator (instead of a set of time dummies, as in the main text). But I also augment the main interaction term of interest to a "triple-difference", adding an indicator for whether a locality was part of the Schmalkaldic League. A significantly negative coefficient would suggest that among treated localities, those that were part of losers experienced a differential effect of the war on inequality, compared to all other treated localities. The coefficient on the triple-interaction term in Column 1 cautiously suggests that being among the losers in fact might have reduced inequality compared to other treated localities. Yet when adding controls it is not significant anymore, while the main treatment coefficient remains just significantly positive. This result is interesting because it gives further support to the notion that inequality-promoting extraction likely outweighed inequality-reducing destruction, even in places that were among the war losers.

Table 3.17: The Effect of the Schmalkaldic War on War Losers' Inequality (Difference-in-Differences Estimates)

	1475-1600	
	(1)	(2)
	Gini	Gini
Conflict exposure×Post1546	0.032**	0.026*
	(0.014)	(0.014)
Conflict exposure×Post1546×League	-0.037*	-0.026
	(0.020)	(0.019)
All controls	NO	YES
R^2	0.375	0.429
Observations	231	231

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

Chapter 4

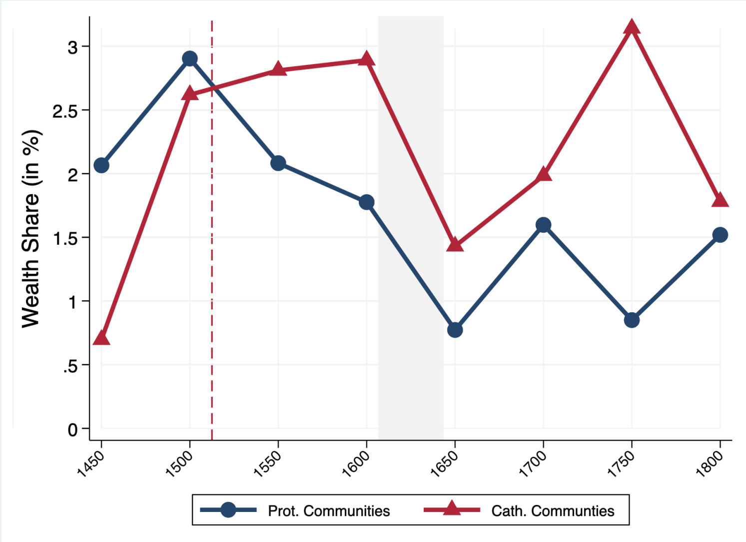
The Unequal Spirit of the Protestant Reformation: Particularism and Wealth Distribution in Early Modern Germany

4.1 Introduction

What is the impact of religious confession on the distribution of wealth and inequality? Since Max Weber's seminal book (1930) "The Protestant Ethic and The Spirit of Capitalism" scholars have debated the socio-economic impact of the Protestant Reformation. While considerable attention has been given to this event's effect on economic growth, its impact on inequality has only been studied marginally (see Becker et al. 2021). This lack of knowledge clashes with the key role recent work attributes to ideology and institutions in explaining how high levels of economic inequality came about and persisted over the long run of history (Abramitzky 2008, Piketty 2020, Alfani 2021a).

Much discussion about the Reformation's socio-economic impact centres on the expansion of public goods provision, especially in terms of social welfare including more generous poor relief (Kahl 2009, Dittmar and Meisenzahl 2020). This line of argumentation implies more redistribution of economic resources to poor people, and a more egalitarian distribution of income and wealth under Protestantism. And yet a key dimension of redistributive policies is how universal or particularistic the provision of social welfare is. Societies can redistribute only to people that are socially close or also to distant strangers (Enke et al. 2021, 2022), with obvious implications for inequality. This generates a trade-off: if Protestant redistribution did not go as far as marginal poor people (see Hartung 1989, Jütte 1994), it probably increased the gap between poor strata and the rest of society, thus increasing economic inequality. Figure 4.1 suggests that this might have been the case. It illustrates a divergence in the wealth share of poor strata — defined as the bottom 20 percent of the distribution (Dollar and Kraay 2002) — in Protestant and Catholic communities in my dataset. (The vertical red line represents the beginning of the Reformation period, the grey box the Thirty Years' War.)

Figure 4.1: Wealth Share of the Bottom 20% in Protestant and Catholic Communities



Notes: Values were were collapsed into 50-year intervals and represent half-century-averages. To avoid that communities with more observations dominate the trend, every community has the same weight in the average. Because of the uneven number of years and the low number of observations in 1400, I collapsed the values for the years 1400 until 1450 into one data point. The vertical red line represents the beginning of the Reformation period, the grey box the Thirty Years' War.

In this chapter I, first, model that trade-off theoretically. To test the predictions deriving from that informal model, I then construct a novel panel database on local religious confession, wealth distribution and inequality in German communities from 1400 to 1800, based on c.380,000 household wealth observations collected from archival tax records and secondary sources. The data make it possible to observe wealth in a highly disaggregated way across the distribution. The setup leads me to study inequality — that is, *relative* and not *absolute* wealth distribution — within Protestant and Catholic communities.

With its religious heterogeneity among communities, the Holy Roman Empire — Germany, for short — is the ideal testing ground for shedding light on the redistributive effect of the Protestant Reformation. The data structure and long time span of the analysis have the advantage that I can estimate this effect for the first time in a difference-in-differences setting, making actual before-and-after comparisons possible. To identify the causal impact of the Reformation, I exploit the fact that the event was practically imposed on the overwhelming majority of people by local rulers following the Peace of Augsburg (1555). Importantly, there is no statistically significant difference or a differential pre-Reformation trend in the wealth share of poor strata associated with the eventual adoption of Protestantism. To further support the claim that the effect of the Reformation can be interpreted causally, I also report flexible difference-in-difference estimates, which permit a more formal test of pre-trends. A natural concern with this setting might be selection bias. To address the potential endogeneity of confessional choice of communities and to account for the possibility that the Reformation broke out, for example, because of economic factors I employ a second identification strategy. To isolate exogenous variation in the adoption-of-Protestantism indicator, I use the distance to Wittenberg as instrument, as previously employed by Becker and Woessmann (2009), Cantoni (2015) and Becker and Pascali (2019). It exploits the fact that the Reformation was more likely to be adopted the closer a community was to the movements' starting point.

I find that the Reformation increased economic inequality, by making poor people poorer relative to the rest of the population. This result is not just statistically significant but also economically meaningful: the bottom fifth of the population lost about 39 percent of its pre-treatment wealth share. The finding is robust to a rich set of controls, including economic growth and warfare, and I do not find evidence of significant pre-trends. The main empirical pattern of poor strata losing significant wealth shares was not short-lived. It endured until the end of the early modern age when industrialisation began. Interestingly, I find no significant evidence that the Reformation had an effect on top wealth shares. This is contrary to what one would expect following a Weberian logic which implies that Protestantism fosters saving and capital accumulation. Similarly, I do not find evidence for significant changes to the wealth shares of middling classes. I interpret these results in the following way: there were concentrated losses at the bottom of the wealth distribution, but dispersed gains for the rest of society. The main OLS results are confirmed by alternative instrumental variable estimates: where the Reformation was introduced poor strata lost significantly, increasing inequality. The estimates are slightly larger than the main difference-in-difference results. However, I document that the instrument was not related to higher inequality before, but only after the beginning of the Reformation. This suggests that, conditional on several covariates, the exclusion restriction most likely holds. The instrumental variable results suggest that selection bias is unlikely to drive the main results.

I argue that the findings reflect the Reformation's shift towards a more particularistic provision of poor relief, which outweighed the expansion of social welfare available to some insiders. In Protestant places strangers and able-bodied but non-working poor were excluded from the local Christian community, and declared as non-eligible for receiving poor relief. The consequence was a reduction in the supply of poor relief to the excluded groups and a decrease of transfers from better-off to marginal poor people. These new low-redistribution policies left behind the bottom of the poor in Protestant society, while the Catholic Church stuck to its universal approach to charity during the Counter-Reformation. Moreover, the particularistic Protestant poor relief system came with a whole set of policies and implications that were also particularistic in nature, or direct consequences of the particularistic poor relief system: the prohibition of begging, the disincentivising of almsgiving, the stigmatisation of the poor in the labour market. These reduced the share of economic resources held by marginal poor people in a way that was analogous to poor relief in the strict sense and exacerbated its effect. In consequence, the Reformation reshuffled the bottom end of the income and wealth distribution, making some poor even poorer and increasing inequality.

I provide several pieces of evidence for the plausibility of this hypothesised mechanism. First, within the poor strata of society it was the bottom decile, the poorest of the poor, that lost the largest share in Protestant places. This finding is consistent with the notion that Protestant communities excluded people at the very margin of society from poor relief. Second, I document that Protestant places experienced higher population decline from epidemics, such as plague. This is exactly what one would expect if Protestantism made poor strata not just *relatively* worse off — that is, increasing inequality, the main focus of this paper — but also in *absolute* terms. Preindustrial epidemics were selective on socio-economic status. If Protestantism made the poor also absolutely worse off, then part of the effect of the Reformation on the economic resources of poor people should have shown up on the death margin, by selectively killing poorer people.

Third, I construct additional data on the closure of monasteries by territorial rulers during the Reformation period, which shut down a central part of the former Catholic social welfare system that had redistributed in a more universal manner economic resources to poor people. One would expect that the closure of redistributing monasteries closed an income channel for marginal poor people, making them comparatively worse off. I find that the monastery closures indeed reduced the wealth shares of poor strata. Fourth, I collect additional data to test the effect of the introduction of so-called "church ordinances" in many newly Protestant localities after the adoption of the Reformation. These laws regulated poor relief among other welfare tasks, and were the legal basis for the particularistic provision of Protestant poor relief. It were these laws that formally excluded community outsiders, but also restricted begging and introduced poor badges that would stigmatise paupers, preventing them from working their way out of poverty in the labour market. All these measures most likely redistributed economic resources away from marginal poor people. Accordingly, church ordinances are found to explain substantial parts of the negative effect of the Reformation on poor strata. The effect is particularly large in places where the laws explicitly restricted poor peoples' opportunities for begging. This suggests that the first-order effects of the Reformation on poverty and inequality were transmitted through the institutional change the religious divide brought about.

The chapter relates to several strands of literature. First, it contributes to the classical debate in the social sciences about the economic impact of the Reformation in the long run of history (see Becker and Woessmann 2009, Cantoni 2015, Rubin 2017, Spenkuch 2017, Cantoni et al. 2018, Kersting et al. 2020, Henrich 2020).[1] Much emphasis has been

[1]This literature is closely related to a larger literature on the economic consequences of Christianity and religion (see Barro and McCleary 2003, Squicciarini 2020, Drelichman et al. 2021, Bryan et al. 2021).

put on the Reformation leading to an expansion of public goods provision, especially in terms of social welfare and poor relief (Dittmar and Meisenzahl 2020). This implies more redistribution to the poor and less inequality. My chapter theoretically shifts this focus, taking into account a second dimension of redistributive policies (see Enke et al. 2021, 2022), namely that social welfare was provided in a more particularistic way under Protestantism. I show that the new Protestant trade-off between generosity and universal or particularistic provision of social welfare increased inequality, by making poor people economically worse off compared to the rest of society. I also show that the Reformation and the subsequent closure of monasteries did not only redistribute economic resources between rulers and the Church (Cantoni et al. 2018), but also affected the distribution among individuals. These findings connect to studies employing contemporary outcomes with the aim to show that Protestants have lower preferences for redistribution and experience higher income inequality today (Guiso et al. 2003, Alesina and Giuliano 2010, Basten and Betz 2013). My results suggest that today's "Protestant inequality" might have its historical origins in the sixteenth century and persisted until at least the nineteenth century. Moreover, the absence of any significant evidence for the Reformation having an impact on the wealth share of the richest or middling parts of the population is hard to square with Weber's hypothesis about Protestant capital accumulation.

More generally the chapter improves our understanding of the deep historical causes and persistence of economic inequality and poverty. Recent research has revealed a striking empirical pattern: contrary to a conventional "Kuznetsian view", inequality did not start to grow with the beginning of industrialisation in the eighteenth or nineteenth centuries, but increased almost constantly since about the sixteenth century. Much of the high levels of inequality observed in the early twentieth century might have preindustrial roots (Alfani 2021a, Alfani et al. 2022). The leading explanations for this preindustrial inequality rise have stressed the role of economic growth (van Zanden 1995, Puga and Trefler 2014), demographic expansion (Milanovic 2016, Pfister 2020a) and increasing fiscal capacity of emerging states (Alfani and Di Tullio 2019). Instead, my results emphasise the role of ideological and institutional change (see also Abramitzky 2008, Piketty 2020). They suggest that the emergence of a new, more particularistic poor relief system, triggered by the novel religious confession, influenced redistribution, and consequently increased poverty and inequality.

Ultimately, the chapter contributes to research on how systems of redistribution within society have been shaped through history (Alesina and Fuchs-Schündeln 2007, Abramitzky 2008, Alesina et al. 2012, Saleh 2018). First, I explore the mechanisms that led to a low-redistribution equilibrium among Protestants. I provide empirical evidence for the hypothesised role played by new social welfare institutions that were introduced by the Reformation, including new laws regulating poor relief and begging. Second, contemporary data on religious confession, redistribution and inequality leave room for pre-trends and selection bias plaguing the estimated effect of Protestantism. My difference-in-differences and instrumental variable approaches take these identification problems into account as much as possible.

The next section models theoretically the trade-off between generosity and particularistic provision of poor relief under Catholicism and Protestantism, and formulates a hypothesis about the implications of the Reformation for wealth distribution and inequality. Section 4.3 describes in detail the data. Section 4.4 presents the empirical strategies to test the main hypothesis, and reports the main results of the chapter. Section 4.5 attempts to disentangle some of the mechanisms at work, and Section 4.6 concludes.

4.2 Redistribution under Catholicism and Protestantism: Conceptual Framework and Historical Evidence

To structure my analysis of the implications of Protestantism for the distribution of wealth and inequality, I model the Reformation as a shift in the trade-off between per capita redistribution and the social distance towards those people that were eligible to receive support. Next I outline the Catholic economy of salvation, and the Old Church's universal provision of poor relief. I then describe the Reformation's ideological and organisational innovations to welfare provision, which led to an expansion of social welfare, but also a more particularistic provision of poor relief. Finally, I discuss the implications of these new policies for the distribution of wealth and inequality. I argue that the Reformation was quite ambiguous in its redistributive implications, but overall it reshuffled the bottom part of the wealth distribution to increase the gap between poor people and the rest of society. I formulate a hypothesis that will be tested in the empirical analysis.

4.2.1 Overview

Prior work has emphasised that the Reformation led to an expansion of the provision of public goods by secular authorities, especially in terms of social welfare including poor relief (Dittmar and Meisenzahl 2020). Protestant communities began taking systematic care of their needy members. This extension of communities' tasks to replace and systematise the Catholic Church's provision of poor relief was a comparatively generous treatment of fellow Christian and community members (Kahl 2009). However, for understanding the Reformation's implications for redistribution and inequality we do not just have to focus on how generous Catholic and Protestant systems of poor relief were with *some* poor individuals, but also on how universal or particularistic this provision was with the *whole* poor strata. The decision to redistribute only to people that are socially close or also to distant strangers is a key variable in many policy decisions that involve redistribution today and in history.[2] Interestingly, religion has been found to be a key determinant of the degree to which a society tends to be more universal or particularistic in its welfare provision and redistribution (Enke et al. 2021, 2022). I argue that the Protestant Reformation was highly ambiguous in its implications for redistribution and inequality: on the one hand it provided increased welfare to some people compared to the Old Church's system, on the other hand it did not do so universally, that is, not for everybody who needed support. This new trade-off had implications for the distribution of economic resources and inequality.

In Figure 4.2 I develop a simple theoretical framework, loosely following arguments made

[2] For example, evidence from contemporary surveys suggest that communitarian respondents oppose federal welfare and redistributive programs like universal health care. Universalists, instead, do not mind that federal redistribution is impersonal and is extended to socially distant people (Enke et al. 2021).

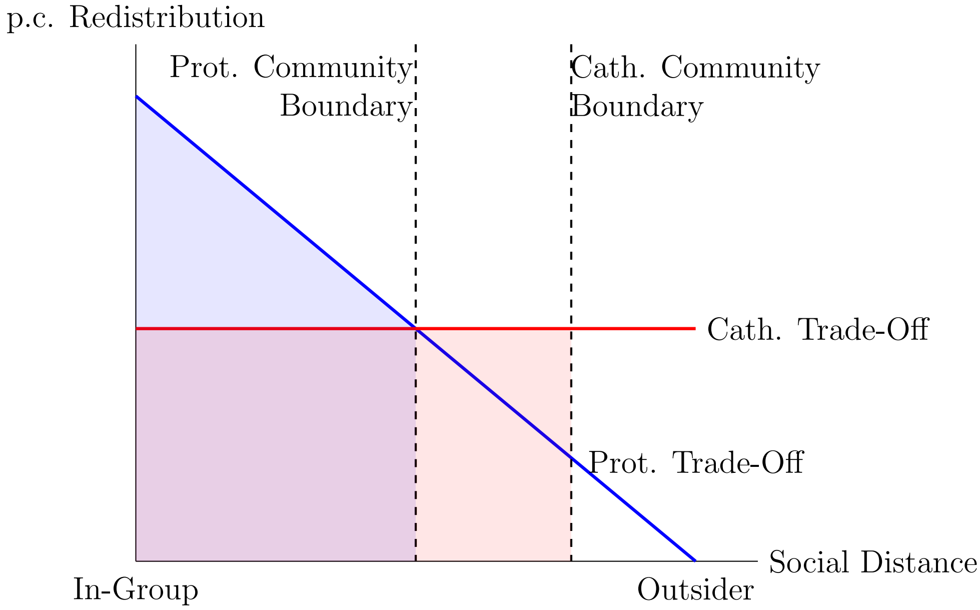


Figure 4.2: Framework: Redistribution under Catholicism and Protestantism

by Dittmar and Meisenzahl (2020) on welfare expansion due to the Reformation, and by Enke et al. (2021, 2022) on the universalism-particularism dimension of redistributive policies. The framework refers to all poor or needy people living in a community, but not all inhabitants had membership, that is, citizenship. The redistributive impact and thus inequality, either under Catholicism or Protestantism, was the result of a trade-off: between, first, the generosity of redistribution (y-axis) and, second, the social distance between the providers of poor relief and those needy people that were eligible to receive support, that is the universalism-particularism dimension (x-axis). Under the assumption that more socially distant people were also poorer, then the more the correlation between these two factors tends towards zero, or even a positive value, the more economic resources are redistributed towards poor people. The result is a higher share of economic resources in the hands of poor people, and lower inequality. Conversely, the more negative the correlation is, the lower is redistribution to needy people, lowering their share of economic resources and increasing inequality. It is important to recognise that this trade-off refers to the entire poor relief system coming with one or the other confession, including the provisions from ecclesiastical and secular authorities.

The Catholic Church was arguably the biggest redistributor in human history, channelling money to its own elites (see Schilling 1994: 97), but most importantly also to poor people. It played that charitable role via its local organisations, such as monasteries, churches or confraternities (Hsia 1996: 366, Kahl 2009: 269-271). It provided a medium amount of per capita redistribution through poor relief. Yet it did so in a universal manner, as indicated by the flat slope of the red line: poor people from in- or outside the local Christian community received support. The boundary of the Catholic community was relatively wide and not limited to local community insiders. The total amount of redistribution, indicated by the red and purple areas, was distributed over a large group of needy people.

The advent of the Reformation shifted this trade-off in newly Protestant communities, as indicated by the declining blue line.[3] Per capita redistribution through poor relief may have been more generous for some people under Protestantism, but it was provided in a particularistic manner, as represented by the tighter community boundary: only members of the local Christian community, to whom the social distance was short, received support. Outsiders, such as strangers or those that were considered religiously "undeserving" or "bad Christians" did not receive poor relief. The total amount of redistribution under Protestantism, indicated by the blue and purple areas, was thus more concentrated compared to Catholicism. It left out a fairly large portion of the poor population (see Hartung 1989, Jütte 1994: 108). This would imply a lower share of economic resources held by some part of the poor population, and higher economic inequality. It is important to recognise that this redistributive effect was not just the result of poor relief in the narrow sense. Protestantism also restricted begging, disincentivised private almsgiving and stigmatised poor people, making it harder for them to enter the labour market. All these factors came with the Protestant poor relief system and had analogous distributive implications. Given that preindustrial Germany was a relatively poor society, part of the effect of Protestantism on the distribution of economic resources could also have shown up on the death margin, by selectively

[3]The aspects mentioned in this section represent "on average characterisations" of differences between Protestant and Catholic approaches to poor relief. Neither were the differences always sharp, nor did such generalisations necessarily apply to all communities in the same manner, but they are nevertheless useful points of reference. Notwithstanding certain commonalities between Protestant and Catholic poor relief systems, historians agree that the Reformation introduced undeniable differences (Bog 1986: 984, Grell 2002: 49).

killing poorer people. Figure 4.2 also suggests that some poor community members may actually have received more resources under Protestantism than under Catholicism. Note further that the framework is not making a strong claim about the total amount of resources employed for poor relief either under Protestantism or Catholicism, as there is no evidence that could credibly support one or the other claim. I therefore assume that the total amount of redistribution was approximately equal under both systems. The crucial aspect is that the available resources were distributed differently.

In what follows I fill this abstract framework with historical evidence. After that I will return to the framework's predictions for the distribution of economic resources among the poor strata and inequality.

4.2.2 The Catholic Economy of Salvation

In the Catholic economy of salvation, the poor played a central role. They represented Jesus Christ, and were glorified as "God's best friends" and the inheritors of the Kingdom of Heaven. Helping the poor was a "good work", a moral duty that erased sinful behaviour on behalf of the donor. If at death accumulated good works outweighed sins, the good Christian could be saved from purgatory and went to heaven (Kahl 2009: 270). This ideology created powerful incentives for Catholics and church organisations to make taking care of the poor a priority.

Poor relief practice reflected this appreciation of the poor. It was based on private almsgiving and poor relief by a variety of mostly uncoordinated ecclesiastical organisations, such as monasteries, hospitals (*Spital*), churches and confraternities. Poor relief was among their principal tasks and between one third and one fourth of church income went to support the poor (Hsia 1996: 366, Kahl 2009: 269-271). A poor person could make ends meet, through a combination of payments and offerings from several charitable organisations, private charity, and begging. Catholic welfare provision was thus relatively inclusive, embracing a variety of needs indiscriminately and providing relief to the poor universally (Ackels 1984: 100, Jütte 1994: 138). The poor continued to have this elevated role in the Catholic worldview also after the Reformation began. The Catholic Church was critical with what it saw as Protestant stigmatisation of the poor and expressly stuck to its universal approach to charity during the Counter-Reformation (Jütte 1994: 125-138, Kahl 2009: 279-280).

Where the Reformation was introduced, the Catholic Church and its welfare provision literally disappeared. For example, the monasteries through which the Old Church had provided poor relief were simply closed. Confiscated assets were freed from the Catholic Church for secular use, but often went into the coffers of local rulers, for instance to build palaces or wage war (Cohn 1987, Cantoni et al. 2018).[4] Monasteries and other ecclesiastical organisations could not fulfil their redistributive function anymore. Poor relief had to be replaced by an entirely new system in Protestant places.

4.2.3 The Reformation: Poor Relief as a Public Responsibility and the Expansion of Social Welfare

Where the Reformation was introduced, the Christian approach to poor relief changed drastically, because it involved a new trade-off between per capita redistribution and social distance to the recipient. Luther's and other reformers' reinterpretation of Christianity provided a radically new vision of how to deal with poverty, organisationally and ideologically.

First of all, reformers envisioned poor relief to be a secular task now, performed by communities instead of ecclesiastical organisations. This followed directly from Luther's "doctrine of the two kingdoms", which postulated the complete separation of the spiritual from the secular realm (Scribner and Dixon 2003: 36). In 1521 Luther and Karlstadt formulated the first Protestant poor law, for the city of Wittenberg, which was the legal basis for a new poor relief system. This first poor law subsequently served as a blueprint that was adopted throughout Protestant Germany when *secular* rulers decreed the introduction of new poor relief systems in their territories. The centrepiece of the new policy was to create for the first time a budget for welfare tasks in Protestant towns and villages, the so-called "poor

[4]For instance, the Duke of Saxony closed almost all monasteries in his territory, through which he obtained the immense sum of about 150,000 florin (Wolgast 2014: 141-142).

chest" (*Armenkasten*),[5] to give systematic support to the poor. The chest was envisaged to be financed with a mix of endowments of secularised church property, and voluntary contributions from the community. Luther himself was involved in practically setting up the new institutions of poor relief, for example, in the small Saxon community of Leisnig in 1523. Such community-based systems of poor relief remained in place until about the late eighteenth or early nineteenth centuries (Jütte 1994: 106-109, 1996: 392, Kahl 2009: 272).

Under this new system, poor relief became for the first time a public responsibility: it became society's duty to take systematically care of fellow Christians, and the poor were entitled to receive a minimum standard of living from the chest. It usually included foodstuffs, fuel, clothing and other goods, and up to several florin of cash payment (Wüst 2017: 114-115). For this reason, the minimum amount of redistribution in Figure 4.2 was higher for a Protestant community insider than for any poor relief recipient under Catholicism. It was also common that communities gave more generous alms (*reiche Almosen*) to certain poor groups that had a higher social status within the community,[6] which is reflected in the declining slope of the Protestant trade-off line.[7] In this sense, the new Protestant poor relief system was a generous expansion of social welfare (Kahl 2009, Dittmar and Meisenzahl 2020).

4.2.4 Particularistic Provision of Poor Relief under Protestantism

Generously expanding social welfare was not the only novelty introduced by Luther and his fellow reformers. Protestantism also provided poor relief in a less universal way. It introduced two mechanism that placed *part of* the poor population outside the local community of "good Christians", therewith excluding them from receiving social welfare. The first mechanism was to exclude all people that were not legally part of the local community — the non-residents — as it was now the community's responsibility to deliver poor relief. Poor people without citizenship or *Beisitz* were denied access to communal poor relief and were turned

[5]Note that it were not just cities that provided public poor relief. Villages fulfilled that task in an analogous way, and had for example "poor chests" (see Dussel 1995: 228, Blickle 2015: 160).

[6]For example in the merchant-city Nuremberg, those that received more generous alms were impoverished merchants (Hartung 1989: 170).

[7]In reality, the Protestant line consisted probably of several downward steps, but for clarity I represent it as linearly declining.

away (Ogilvie 1997: 45-57, Minns et al. 2020: 605, Battenberg 1991: 51-55).[8] This exclusion was made legally quite explicit. For example, in the chest law that introduced the new Protestant poor relief system in Leisnig in 1523 — under the guidance of Luther himself — a whole chapter was dedicated to "Rejection of the Burden from Strangers" (my translation) (Sehling 1902: 601). This particularistic approach meshed well with the material interests of communities. The exclusion of poor strangers from receiving secular poor relief enabled communities to keep low the number of recipients that burdened the communal budget (Präger 1997: 20, Ogilvie 1997: 50-51).

The second mechanism by which Protestantism excluded a certain sub-group of the poor from poor relief was by declaring them as undeserving or "bad Christians", and therefore non-eligible for social welfare. This sub-group were the non-working but able-bodied poor, such as those that could not find a job.[9] Their poverty was "not a misfortune to be pitied and relieved, but a moral failing to be condemned" (Tawney 1926: 230). Only poor individuals that were strictly unable to work, such as invalids, children or old people, were considered "deserving" of community relief under the new Protestant welfare system. Instead, able-bodied but non-working poor were by default considered unwilling to work and hence undeserving. "Who does not work shall not eat" was the guiding principle, and able-bodied "fake beggars" were excluded from communal help (Laube 1981: 134-135, Jütte 1996: 392, Kahl 2009: 271). This particularistic approach resulted directly from the reformed Christian ideology. Luther, the son of an affluent family, stated that work was pleasing to God, and that idleness was to be disdained.[10] That logic implied that able-bodied but non-working

[8] Communities in preindustrial times were often hostile to outsiders in general, and did not allow them to participate politically, practice an occupation, or use other public goods. Becoming member was difficult, especially for poor people who did not fulfil the minimum wealth criterion (Ogilvie 1997: 45-57, Minns et al. 2020: 605-608, 615).

[9] The exclusion of able-bodied unemployed from poor relief was likely based on a flawed understanding of the labour market. To presume that every able-bodied non-working poor person was unwilling to work amounts to assuming an unlimited supply of jobs. Yet the economy in sixteenth-century Germany was declining in terms of per capita output (Broadberry et al. 2015: 423), and burdened with all kinds of entry barriers that made it hard to enter the labour market, such as having connections to guild masters (Ogilvie 2019: 93-138). In consequence, unemployment was in actuality widespread (Schilling 1994: 378).

[10] Religious reformers like Luther, Zwingli, Karlstadt Bugenhagen, Hyperius and Bucer differed on many points in their theology, but they were surprisingly united in their negative views on poverty (Jütte 1996: 392).

poor now represented the exact opposite of the ideal member of Christian society, placing them outside the Christian community. In Figure 4.2 the two mechanisms that placed part of the poor population outside the local community of "good Christians", making Protestant poor relief less universal, is reflected in a tighter Protestant community boundary compared to Catholicism.

It is not possible to quantify the number of poor people excluded from public relief in Protestant communities systematically, but historians have long conjectured that their numbers were substantial (Bog 1986: 990). Scattered evidence comes from a few cases where community-level estimates of the share of poor relief recipients in relation to the total population are available (see Table 4.1). The evidence is patchy, but across four centuries after the beginning of the Reformation, the share of public poor relief recipients was lower in almost all Protestant cases compared to Catholic ones. The difference in the average share of recipients is considerable, suggesting a substantial divergence in how exclusionary the two confessions were.

4.2.5 Beyond Poor Relief: Second Order Effects of Protestant Particularism

The particularistic approach of Protestantism to poor relief had further welfare implications that were related to the new poor relief system and that exacerbated its distributive effect. One of these implications was the restriction and often outright prohibition of begging (von Hippel 2013: 48-50). Since communities took adequate care of "their" poor, the reasoning went, those that were still begging must have been undeserving outsiders. Moreover, it was assumed that beggars could just take up a job if they wanted and simply needed a strong enough incentive to do so (Kahl 2009: 274-278). Prohibiting begging was therefore not illogical. Yet it closed an important income channel for many poor people, reducing their share of economic resources.

Additionally, restrictions to begging went hand in hand with the elimination of the incentives for donors to engage in private almsgiving. Protestantism denied private almsgiving its theological function of avoiding purgatory and declared it as outright wasteful. Instead,

Table 4.1: Population Share of Recipients of Public Poor Relief

Community	Year	Confession	Recipients as % of Population
Trier	1591	Catholic	8.3%
Trier	1600	Catholic	34.6%
Trier	1625	Catholic	27.1%
Trier	1649	Catholic	16.9%
Solothurn (villages)	1768	Catholic	22.5%
Würzburg	1794	Catholic	4.3%
Cologne	1799	Catholic	8.2%
Cologne	1816	Catholic	38.0%
Average Share in Catholic Communities:			20.0%
Nuremberg	1531	Protestant	1.2%
Ulm	1531	Protestant	2.8%
Frankenberg	1533-42	Protestant	4.2%
Frankfurt a.M.	1539	Protestant	3.6%
Augsburg	1550	Protestant/Mixed	5.5%
Augsburg	1574	Protestant/Mixed	4.3%
Zürich (villages)	1590	Protestant	4.5%
Augsburg	1610	Protestant/Mixed	7.2%
Berlin	1665	Protestant	2.0%
Berlin	1799	Protestant	7.2%
Berlin	1860	Protestant	6.0%
Average Share in Protestant Communities:			4.4%

Notes: Data on poor relief recipients for Cologne and Berlin from Fischer (1982: 58, 83) and Jütte (1994: 54), for Trier from Ackels (1984: 94), for Augsburg from Clasen (1984: 89) and Röck (1989: 169), for Nuremberg and Ulm from Hartung (1989: 168, 172), for Würzburg and Frankenberg from Jütte (1996: 388) and for the villages of Solothurn and Zürich, and Frankfurt a.M. from Jütte (1994: 54). For Solothurn and Zürich the data refer to the villages surrounding the towns.

better-off individuals were urged to donate only to the common chest, in order to redistribute money more "efficiently" to deserving community members (Jütte 1996: 396-397, Scribner and Dixon 2003: 58). In other words, there was a shift in norms about almsgiving, which was associated with lower private charity. The disincentivising of almsgiving amounted to a new equilibrium that, too, closed an income channel and reduced the share of economic resources held by poor people.

Ultimately, the particularistic Protestant approach stigmatised poverty. In many Protestant communities poor relief recipients had to identify themselves wearing a poor badge, putting them in the same social category of non-trustworthy people like prostitutes or lepers (Jütte 1994: 161-161; see Figure 4.3). Stigma deriving from discriminatory symbolic policies can prevent the poor from entering the labour market and improving their economic situation through work. This redistributes income and wealth away from the poor (Bartik 2001, Dewan and Wolton 2019). For example, in Nuremberg and Augsburg the poor badges reportedly discredited the poor with potential employers, while no beggar emblem was necessary in Catholic Trier. It was enough that the priest attested one's need to receive poor relief (Hartung 1989: 169, 171, 174, Ackels 1984: 80), thus avoiding discrediting stigmatisation, giving the poor a better chance in the labour market and channelling a larger share of income and wealth to them.

Figure 4.3: Poor Badge from Sixteenth-Century Nuremberg

Source: Germanic National Museum Nuremberg, reproduced in Endres (1993).

4.2.6 Hypothesis: Implications of the Reformation for the Distribution of Economic Resources and Inequality

The shift in the trade-off between generosity of redistribution and the universalism-particularism dimension of who is eligible to receive social welfare suggests the following hypothesis regarding redistribution and inequality following the Reformation.

While Protestantism expanded welfare provision for few insiders (Kahl 2009, Dittmar 2019), it excluded strangers and able-bodied but non-working and non-resident poor from the local

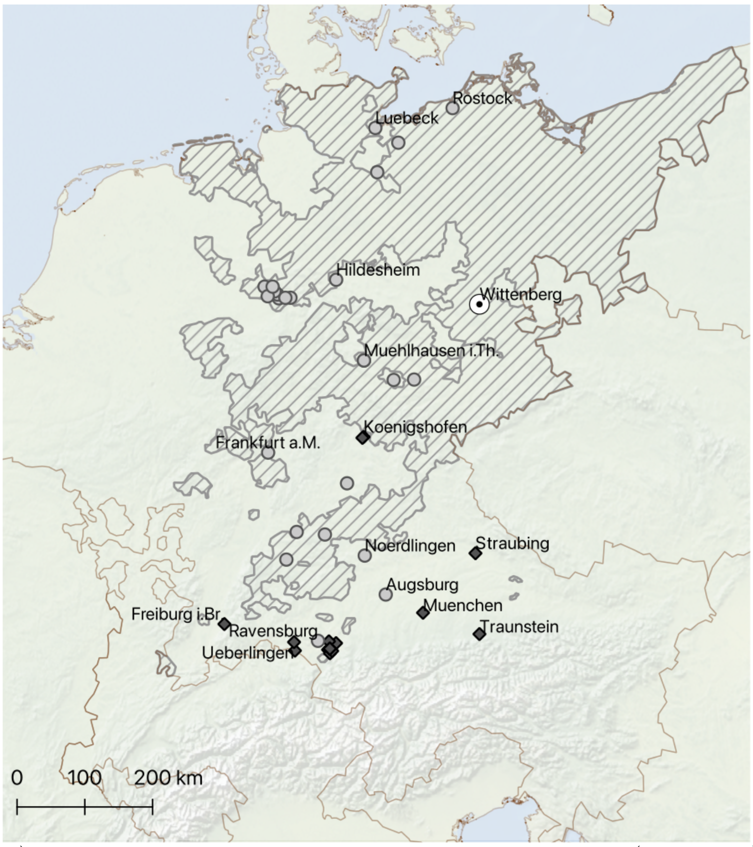
Christian community, and declared these individuals non-eligible for receiving poor relief. This was a shift away from universalism in the provision of social welfare (see Enke et al. 2021, 2022). It implied a reduction in the supply of poor relief to the excluded groups and a decrease of transfers from better-off to poor strata. These new low-redistribution policies left the bottom of the poor behind in Protestant society. From this particularistic system derived relevant second order effects — the prohibitions of begging, the disincentivising of almsgiving, the stigmatisation of the poor in the labour market — which had analogous distributive effect. In consequence, the Reformation reshuffled the bottom end of the income and wealth distribution, making some poor even poorer. The inequality data employed in the empirical analysis comes from property taxes,[11] which implies that any income above subsistence is covered. Given these data, I would expect to observe a reduction in the wealth shares of poor strata, particularly among the poorest of the poor in Protestant places. This reshaping of the bottom end of the wealth distribution could have also increased the overall gap between the poor and the rest of the population, thus increasing inequality. Additionally, it could be that part of the negative effect of the Reformation on poor people's income and wealth showed up on the death margin. This hypothesis will be tested in the empirical analysis.

4.3 Data

Figure 4.4 provides an overview of the 18 Catholic and 25 (eventually) Protestant communities included in the analysis, and the major Protestant areas in the mid sixteenth century. As can be seen from examining the geographical distribution of communities, there was a concentration of Protestant places closer to the town of Wittenberg in the north-east and centre of Germany, while Catholic communities tended to lie in the south. There were Catholic places in the north and north-west, such as Cologne or Münster, but they are less frequent and considering the north of Königshofen, none is part of the dataset (see the Appendix for

[11] The tax base usually included real estate, crops, animals, cash money, loans, household goods and other property. Property is a stock, but it also gives information about income flows in so far as not all income has been used for subsistence.

Figure 4.4: Protestant and Catholic Communities in the Dataset



Notes: Dots (light grey) correspond to Protestant communities and squares (dark grey) to Catholic communities, by 1600. Wittenberg is not part of the dataset. Borders of the Holy Roman Empire around 1545 and Protestant areas around 1559 from Volckart (2020). Selected cities labelled. Not all communities are visible because of visual overlap on the map, and not all, sometimes tiny, Protestant areas are visible because of the scale of the map.

results that take concerns about spatial dependence into account).

I use a subset of the community-level inequality data introduced in Chapter 2. For the 43 rural and urban communities that are covered, at least one observation before and one after the beginning of the Reformation is available, resulting in an unbalanced panel. Overall, a total of 368 observations between 1400 and 1800 are available, based on information about more than 380,000 households. To address potential concerns about the small sample size causing uncertainty of statistical inference, the Appendix reports p-values and confidence intervals obtained with Wild Cluster Bootstrapping. I also report results obtained from an extended sample, including localities that were observed only before or after the beginning of the Reformation. The panel-regression setup makes it possible to limit the analysis to within-community variation, and account as good as possible for the possibility that differences in taxation practices between localities could systematically influence the measured wealth distribution. As mentioned, the wealth distributions have a potential bias towards the middle, as some privileged individuals could obtain tax exemptions. These were for example clergymen or members of the nobility. It is likely that most tax-exempt households belonged to these groups, but not all noblemen or clergymen were exempt. For instance, lower-level clerics were usually included in the tax registers. Note that German tax registers have been found to cover poor households very well (Alfani et al. 2022). Potentially missing parts of the distribution are likely to bias my estimates of the impact of the Reformation downwards. The Appendix shows that the *potential* exclusion of parts of the poor from the wealth distributions does not change the trend of poor peoples' wealth shares, which is reassuring given that my regressions include unit- and time-fixed effects. (Additionally, the Appendix reproduces examples of the archival sources, showing also how households with zero wealth were typically recorded.)

A potential concern could be the presence of "hidden wealth", perhaps differentially among confessional groups. Authorities implemented several measures to fight tax evasion, such as obliging individuals to swear an oath on the correctness of their tax payment, checks of tax estimates and payments, and severe penalties for evaders, such as confiscation of one's property and public announcement of evaders' names. These measures were likely imperfect, again very similar to today, but it is reasonable to assume that they increased an individual's cost of tax evasion substantially (Isenmann 2014: 539–541). To my best knowledge there is no study providing evidence of differentially hidden wealth among confessional groups, and a potential bias would most likely work against the result I find.[12]

More generally, one might be concerned about Protestants applying different taxation principles, which could influence the recorded distribution of wealth. Again, to my best knowledge, no study demonstrating differential taxation record-keeping among confessional groups

[12] If anything, one might conjecture that top wealth holders in Catholic communities were more inclined to hide wealth, because richness was morally sanctioned by the Old Church, while it was implicitly encouraged by Protestantism (Weber 1930). This could bias estimates of wealth concentration in Catholic places negatively. However, the results reported below provide no evidence that Catholics had significantly lower wealth concentration.

in preindustrial Germany exists. The reason is most likely that taxation in the localities of the Holy Roman Empire had a common legal basis, which contained the possibility of local variations. The basic procedure for levying wealth taxes goes back to King Rudolf I, who decreed in 1287 that local taxes should be levied by applying a uniform tax rate to the entire property owned by every household (Isenmann 2014: 522). (In the Appendix I show archival tax registers from Catholic and Protestant places, which visually exhibit a high degree of homogeneity.) Similarly, one might wonder whether local bureaucracies in Protestant places recorded people more rigorously in the tax registers. This would be particularly relevant if poor strata were recorded more thoroughly. If that were the case one would expect, for example, a higher number of taxpayers in Protestant compared to Catholic places. To address this concern I check whether there are any discontinuities in the number of taxpayers during the early phase of the Reformation, and I do not find any significant differences between the two confessions.

Information about whether the Reformation was introduced in a community has been obtained from standard secondary sources, mainly from the *Deutsches Städtebuch* and the *Historisches Lexikon der deutschen Länder* (see the Appendix for details about coding of variables; see Cantoni 2012, 2015, Dittmar and Meisenzahl 2020 for a similar approach). Few communities in the dataset reverted to Catholicism after introducing the Reformation. I have coded those few places depending on whether the Reformation was partially or completely reversed. To exemplify my coding approach, consider the cases of Augsburg and Konstanz. In Augsburg Catholicism co-existed with Protestantism after the Schmalkaldic War (1546-48), but Reformation legislation was not genuinely taken back. Konstanz, instead, was entirely re-catholicised and Reformation legislation was reversed. Consequently, Augsburg was coded as Protestant and Konstanz as Catholic. I will show that the results do not depend on these switching cases.

What were the characteristics of those communities that eventually became Protestant? As shown in Table 4.2, there were some differences (estimated with bivariate regressions) between them and Catholic places in the pre-Reformation period. Column 1 shows by how much

Table 4.2: Community Characteristics before the Reformation

	(1) β Protestant	(2) SE	(3) Mean
Bottom 50% Wealth Share pre Reformation	-0.91	(1.84)	12.55
Bottom 20% Wealth Share pre Reformation	0.37	(0.48)	2.18
Middling 40% Wealth Share pre Reformation	-0.11	(2.33)	21.76
Top 10% Wealth Share pre Reformation	-3.24	(3.78)	45.92
Gini Coefficient pre Reformation	-0.01	(0.04)	0.59
Population Size (Log) Coefficient pre Reformation	0.31	(0.36)	7.51
Occurence of Epidemics pre Reformation	-0.07	(0.10)	0.38
Urban Community	-0.07	(0.11)	0.61
Soil Quality	0.00	(0.03)	0.72

Notes: Column 1 shows the estimates on an indicator for a community being Protestant before the Reformation began in bivariate regressions. Columns 2 displays standard errors in parentheses. Columns 3 provides the mean of the dependent variable in the whole population. *** p<0.01, ** p<0.05, * p<0.1

communities that became Protestant after 1517 differed from the mean values of all communities (Column 3). Column 2 shows the standard errors. However, all these differences, in terms of wealth shares of different parts of the population (Bottom 50%, Bottom 20%, Middling 40%, Top 10%), overall inequality (Gini), economic and demographic characteristics (population size, occurrence of epidemics) and time-invariant community characteristics (city-status, soil quality) were not statistically significant in my sample.

An important question is how much property the poor actually owned. Table 4.2 indicates that the bottom fifth of the population owned on average 2.18 percent of the total wealth in my sample before the Reformation. But how much is this really worth? We do not have information about poor peoples' wealth at the macro-level at the time, but we can combine information about taxable wealth, the number of households, local currencies and real wages in an individual community to get a rough estimate. For instance, in Augsburg, just before the onset of the Reformation, 2.18 percent of total wealth would have implied that a household in the poorest fifth of the population owned property corresponding approximately to 17.14 daily wages of an unskilled worker (see the Appendix for details of the calculation).

One might also wonder what the composition of this property was, and who the poor actually were. These questions are even harder, if not impossible, to answer systematically, because tax registers do not usually record the different types of household property, but only indicate a total sum. Information about occupation and other individual characteristics, too, were rarely recorded systematically. The available evidence suggests, however, that poor people owned a variety of assets. For example, they typically possessed household goods, such as textiles and furniture, agricultural produce, such as foodstuffs, small sums of cash, and sometimes little amounts of savings and real estate. Their individual characteristics were diverse too. Among the poor we find small peasants, shepherds, craftsmen (for example, bakers, shoemakers, carpenters or weavers), small retailers, journeymen, day labourers, construction workers, apprentices, servants, but also the so-called "dishonest occupations", such as hangmen. Many poor people were also just old, sick, or female, especially after their breadwinning husband had died (Jütte 1994: 71-72, Fischer 1982: 13-25).

4.4 Empirical Analysis

4.4.1 Econometric Methodology

In order to identify the effect of the Protestant Reformation on economic inequality, I employ the difference-in-differences (DD) setup in Equation 4.1:

$$I_{i,t} = \alpha_i + \pi_t + \theta Prot_i \times PostReform_t + \boldsymbol{\gamma' X_{i,t}} + \epsilon_{i,t} \tag{4.1}$$

$I_{i,t}$ is some measure of wealth inequality in locality i in year t (t = 1400, 1425,... until 1800).[13] I am mostly interested in wealth percentiles of poor strata, but also consider other inequality measures, such as the Gini coefficient. The difference-in-differences estimate is given by the interaction of post-treatment indicator ($PostReform_t$) and treatment status ($Prot_i$).[14] The post-indicator is coded as "one" after 1517, which follows the coding

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

[14] The treatment indicator does not differentiate between various forms of Protestantism. This is common practice in the published literature on the topic (see Cantoni 2015, Spenkuch 2017), for two reasons: first, differences existed between the branches of the reformed faith and within them. However, taken together the different Protestant confessions as inspired by Martin Luther, Huldrich Zwingli, Johannes Calvin and others were more similar to each other than to the theology and religious practices of the Old Church (see Dixon 2002: 47-51, Blickle 2015: 71-74). Second, Lutheranism, by far the most dominant branch of Protestantism in Germany, is the only reformed faith in the dataset. It was probably less "Weberian" than Calvinism.

of Cantoni (2015: 562) and Dittmar and Meisenzahl (2020). As outlined in the historical background section in the Appendix, there was variation in treatment timing. Since many communities *de facto* implemented reformist ideas before the official legal introduction, for example, through the substitution of the Catholic priest with a Protestant preacher, it seems prudent to place the beginning of treatment when the Reformation movement began, and Lutheran ideas started to spread throughout Germany. In the Appendix I repeat the baseline specification using the official legal introduction date, which leads to almost identical results. I assigned communities treatment status based on whether they had become Protestant until 1600.[15] While the year 1600 is arguably arbitrary, it is historically a reasonable choice. The Peace of Augsburg in 1555 established for the first time the right of authorities to convert to Protestantism and impose the new religion on subjects. Several polities did officially introduce the Reformation in the following years, but it is sensible to assume that the conversion process had reached a steady state until about 1600. This coding is a standard approach in the published literature (Rubin 2014, Cantoni 2015, Dittmar and Meisenzahl 2020), and alternative coding of the treatment variable does not alter the results (see Appendix). Theta (θ) is the main coefficient of interest, providing an estimate of how the introduction of the Reformation affected economic inequality.

To further mitigate the possibility of the main variable of interest being correlated with the error term, I account for several locality-level time-variant observable demographic, economic and institutional characteristics included in vector $\boldsymbol{X_{i,t}}$. These controls are included in most but not all specifications, since they could be "bad controls" or collider variables (Angrist and Pischke 2009, Schneider 2020). I include the log population size of a locality because demographic expansion could have played a role in determining the preindustrial wealth distribution (Milanovic 2016, Pfister 2020a). As population size is a sensible marker of productivity growth, it is also a frequently used proxy for economic development. Economic growth has often been considered a potential driver of inequality (Kuznets 1955, van

[15] Another historically sensible cut-off year would be 1624, the reference date established by the Peace of Westphalia. Only few territories changed their religious denomination between 1600 and 1624, and none of the places in the dataset did so.

Zanden 1995: 649, 656-658, Deaton 2015: 1-5), though it could have also driven the introduction of the Reformation: in more entrepreneurial environments, with vibrant markets, rich merchants and high overall prosperity, Protestantism, with its less hostile view on profit-seeking, was particularly appealing (Ekelund et al. 2002). I also include the local occurrence of epidemics, which could have impacted inequality but also the probability of the adoption of the Reformation (Alfani 2015, Dittmar and Meisenzahl 2020). One might be concerned that wars and in particular the German Peasants' War (1524-25) might have been an alternative treatment impacting Protestant and Catholic places differentially. This war just happened few years after the Reformation began and might have impoverished peasants in the affected areas, thus influencing inequality. To account for such a possibility, I control for the occurrence of battle action nearby. Ultimately a variable is included that indicates the log distance of a community to its nearest university. Universities might have had an impact on inequality (see van Zanden 1995: 658-661, Dittmar 2019), but their role in the distribution of ideas and provision of trained theologians could have influenced the introduction of reformist ideas as well (Scribner and Dixon 2003: 28). The Appendix provides further details about the coding of variables, and I test the robustness of my results to the inclusion of several time-invariant controls interacted with time-dummies. I consider variables that could have had an impact on inequality and Reformation adoption: agricultural potential, seaside location, whether a locality was an urban community, an Imperial city, a Hanse city or belonged to a bishopric.

Unobserved factors are captured with the random error term $\epsilon_{i,t}$. The standard errors are robust and clustered at the locality level to account for the possibility of serial correlation in the error term. The Appendix reports the baseline results with spatial autocorrelation-adjusted standard errors, and Morans' I test for spatial dependence. The results indicate that spatial dependence is a negligible issue for this study.

4.4.2 Difference-in-Differences Estimates

Motivation

There exists a rich literature in the social sciences on the causes of the Reformation (for a review, see Becker et al. 2016), and an obvious concern when studying its socio-economic effects is the potentially endogenous choice of religious confession by political elites: selecting into the Reformation could have been correlated with determinants of economic inequality, hereby creating selection bias. For example, Ekelund et al. (2002) discuss the possibility that inequality was related to the adoption of the Reformation, although they explicitly aim to explain the diffusion of the Reformation at the country- and not the community-level.

Some historical facts suggest that the possibility of communities choosing their confession because of inequality is most likely a minor concern (see the Appendix for more historical background on the introduction of the Reformation). First, for the overwhelming majority of people the decision to adopt the Reformation was taken by their territorial lords, namely the competence conferred on them in the Peace of Augsburg of 1555. According to Bob Scribner, the religious confession was practically *imposed* on the population living in the communities of the sixteenth century, not to speak of the generations born into a confessionally settled environment in later centuries. Only about 10 percent of the population in sixteenth-century Germany ever showed an active interest in the ideas of the Reformation, but up to about 80 percent of the population was Protestant during that time. The difference must have been due to the confessional choices of magistrates and princes (Scribner and Dixon 2003: 34). Second, many historians argue that the Reformation was above all a response to widespread dissatisfaction with theological and church-matters at the time (Pohlig 2017). People were frustrated about the growing contradictions between preached ideals and lived reality of clerics, the abuses of the church, deficient pastoral support, corruption, exaggerated interference in people's daily lives, and the sale of indulgences. All these developments fuelled anti-clericalism, and the Reformation picked up that sentiment (Blickle 2015: 29-40). Third, detailed case studies of the social groups, that locally advocated the adoption of the Reformation, found no clear patterns that could suggest a connection between social structure and adoption. This is unsurprising because the new version of Christian faith had something to offer for everyone. Hence in some places it were political and economic elites, while in others it were lower classes that demanded the adoption of the new confession.[16] These historical reasons make it unlikely that a community's religious confession depended on local pre-Reformation wealth shares of the poor and inequality.

Yet the main motivation for the difference-in-differences analysis comes from Figure 4.1, which plots the raw data. It shows a divergence in the wealth shares of poor strata in newly Protestant places, but similar trends compared to Catholic localities before the Reformation began (for a similar approach to identification, see for example Cantoni 2015, Cantoni et al. 2018, Becker and Pascali 2019, Dittmar and Meisenzahl 2020). Moreover, Table 4.2 shows that there were no statistically significant differences in my sample between eventually Protestant and Catholic communities, in terms of poor peoples' wealth shares or overall inequality. Flexible DD estimates will provide further evidence for the assumption that the religious confession of localities did not depend on the wealth shares of poor strata. I will then show that the main results are confirmed when exploiting exogenous variation in treatment allocation in an instrumental variable strategy, which accounts for the possibility that the Reformation was adopted, for example, because of local economic factors.

Baseline Results

According to the theoretical model presented in Section 4.2 the Protestant Reformation implied a reduction in the supply of poor relief to the excluded groups, and a decrease of transfers from better-off to poor strata. These new low-redistribution policies left the bottom of the poor behind in Protestant society. One would expect to observe a reduction in the wealth shares of poor strata, particularly among the poorest of the poor in Protestant places.

Table 4.3 reports DD results for the effect of the Reformation on the distribution of wealth

[16] For example, in Nuremberg the city council introduced the Reformation after the insurrection of peasants and craftsmen. But in nearby Kitzingen the council demanded the Reformation initially, later supported by wealthy city dwellers, and eventually it was the territorial lord to introduce the new confession (Blickle 2015: 96, 102-109, Arnold 1976).

in communities. The coefficients represent an average post-treatment difference in wealth shares or inequality. The focus is on the wealth percentiles of the lower classes of the population. In order to evaluate the hypothesis derived from the theoretical model, I consider a wide definition of the poor, that is, the wealth shares of the bottom 50 percent (Piketty 2020), but also narrower definitions, namely the wealth shares of the bottom 20 percent (Dollar and Kraay 2002) and of the bottom 10 percent. While the effect on the bottom 50 percent is insignificant (Column 1), the other two estimates are negative and highly significant (Columns 2 and 3). They are also economically sizeable if one considers that lower deciles of the population owned minuscule shares of overall wealth in communities: the bottom 20 percent in Protestant places lost about one wealth share percentage point, which corresponds to 39.4 percent of their pre-treatment wealth share of about 2.6 percentage points (compare regression coefficients with pre-treatment wealth shares, reported in the summary statistics in the Appendix). This suggests that the Reformation had a sizeable impact on inequality if measured as the wealth percentiles of lower classes. It did not have a significant impact on the bottom half but made specifically people in the poorest fifth of society comparatively poorer.

Table 4.3: Wealth Share Changes of Poor Strata (Difference-in-Differences Estimates)

	(1) Bot. 50%	(2) Bot. 20%	(3) D1	(4) Bot. 50%	(5) Bot. 20%	(6) D1	(7) D2	(8) Gini	(9) D1/Top 90%
Protestant×Post-Reform.	-1.094	-1.004**	-0.431***	-1.330	-1.023***	-0.428***	-0.595**	0.031	-0.004***
	(1.369)	(0.383)	(0.144)	(1.292)	(0.360)	(0.137)	(0.247)	(0.023)	(0.001)
Controls	NO	NO	NO	YES	YES	YES	YES	YES	YES
Locality FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Observations	368	368	368	368	368	368	368	368	368
Communities	43	43	43	43	43	43	43	43	43
R^2	0.255	0.165	0.147	0.306	0.228	0.235	0.194	0.318	0.235

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

Columns 4 to 9 introduce all demographic and economic, and institutional time-variant controls, to make the comparison between Protestant and Catholic communities as close to *ceteris paribus* as possible. The results do not change. Since the controls contain a proxy for economic development, a community's population size, the results indicate that the distributional, relative losses of the poor strata hold even if changes in absolute prosperity are accounted for.[17] The results also hold for the second decile of the distribution (Column 7). Comparing Columns 6 and 7, the coefficients indicate that the bottom decile (D1), the poorest of the poor, lost the largest wealth share among the poor: 44.0 percent, compared to the 37.8 percent loss of the second decile (D2) (see summary statistics in the Appendix for pre-treatment wealth shares). This is indicative of Protestantism hurting economically most severely people at the margins of society, as the theoretical model predicts.

I also consider indicators of wealth inequality in the whole population. The Gini coefficient (Column 8) points towards inequality increase, but it is not significantly different from zero. This does not mean that there were no changes to overall inequality in Protestant places. It only means that the Gini coefficient does not capture the changes, possibly because the indicator is more sensible to changes in the middle of a distribution rather than its extremes. The share of the first decile in relation to the rest of the population (Column 9), another indicator of wealth inequality, suggests a highly statistically significant increase in overall inequality.

One might be wondering whether the wealth share reductions of poor strata are simply the mechanical consequence of a higher wealth concentration at the top or middle of the wealth distribution. To address this concern I also estimate whether the Reformation had an effect on top and middling wealth shares, reported in Table 4.4. Columns 1 and 2 show the changes for top wealth shares. The coefficients are positive but small in magnitude. The coefficient in Column 1 corresponds to 6.8 percent of the top 10 percent wealth share and the coefficient in Column 2 to 1.1 percent of the top 1 percent wealth share in the pre-treatment period in eventually Protestant places (see summary statistics in the Appendix for pre-treatment wealth shares). Yet none of the estimates indicates wealth concentration at the top significantly different from zero. In other words, these estimates provide no evidence for

[17] In the Appendix I test the robustness of my results to the inclusion of several time-invariant controls interacted with time-dummies. I consider variables that could have had an impact on inequality and Reformation adoption: agricultural potential, seaside location, whether a locality was a urban community, an Imperial city or a Hanse city. None of these variables changes the main result.

significant wealth concentration in the upper strata of society in Protestant communities.[18] However, since the coefficients are not precisely zero, one cannot rule out with certainty that the Reformation led to limited redistribution to the rich.

Table 4.4: Wealth Share Changes of Top and Middling Strata (Difference-in-Differences Estimates)

	(1) Top 10%	(2) Top 1%	(3) Mid. 40%	(4) Top 10%	(5) Top 1%	(6) Mid. 40%
Protestant×Post-Reform.	3.072	0.123	-0.161	3.493	0.340	-0.514
	(2.334)	(1.829)	(1.608)	(2.504)	(1.849)	(1.580)
Controls	NO	NO	NO	YES	YES	YES
Locality FE	YES	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES	YES
Observations	368	368	368	368	368	368
Communities	43	43	43	43	43	43
R^2	0.196	0.071	0.286	0.237	0.109	0.304

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

To check whether the Reformation had any impact on the wealth share of the "middle class", Column 3 reports the effect of the Reformation on the arithmetic middle (deciles four to seven) of the population. This coefficient, too, is statistically not distinguishable from zero, suggesting that the Reformation did not alter the wealth share of the middling groups in the population either. Columns 4 to 6 indicate that all these results hold when controls are added to the specifications. One might ask how it is arithmetically possible that the lower strata of the population lost wealth shares, while the middle and upper parts did not gain. Note that the difference in the wealth share of middle and upper classes as indicated in Table 4.4 is just not *statistically significant*, which does not rule out that there was any redistribution. It seems that the wealth share lost in a concentrated fashion at the bottom, was distributed in a dispersed way, not benefiting any other part of the population in particular.

Could it be that the insignificant results are only the result of a too low statistical power

[18] This result is in line with the recent findings of Kersting et al. (2020), who do not find evidence for Protestant capital accumulation in modern Prussia.

of a relatively small dataset? While this possibility cannot be entirely excluded, Table 4.3 suggests that the dataset is not too small to capture significant distributional changes caused by the Reformation on lower strata of the population.

Another concern might be whether the Reformation led to migration between Protestant and Catholic places, which might imply geographical spillover effects and could have an impact on the results. This type of externality would amount to a violation of the stable unit treatment value assumption (SUTVA). While theoretically possible,[19] it is historically unlikely to be a major issue, especially since the Holy Roman Empire was the most fragmented political entity in early modern Europe consisting of more than 300 polities (Ogilvie 2019: 27). There was no general right to migrate between the territories of the Empire. To the contrary, subjects had to ask permission from their authorities, had to be free from feudal bonds, often had to pay high fees for the right to leave, needed to obtain a passport for moving around, and host communities could simply refuse to take in immigrants they did not want, such as poor people. These economic and institutional barriers restricted mobility considerably given that migrants had to cross many different jurisdictions when covering even short distances. The barriers forced in particular the undesired poor to stay put, because police ordinances outright criminalised their migration, and they probably could not afford the high costs of leaving (Gerteis 1998, Scholz 2020: 24-25, 135-136, Blickle 2015: 189-190). Additionally, because the religious divide was such an incisive cultural dividing line at the time, individuals from the other confession were usually met with hostility and, for example, excluded from receiving welfare benefits such as poor relief from hosting communities (Battenberg 1991: 60). Ultimately, note that spillover effects would bias my estimates against finding a significant effect of Protestantism. If Protestantism made poor people worse off, one would expect that the poor tried to leave these places, thus reducing their number and measured inequality. Then the actual effect would be even larger than what my estimates suggest.

[19]In theory, people could leave their territories for religious reasons after 1555. In practice, the legal and economic hurdles made emigration at the very least a ruinous endeavour, thwarting the economic motives for migration (Blickle 2015: 189-190).

Differential Population Decline from Epidemics

As a check for the plausibility of the result that poor people were made economically worse off in newly Protestant places, I study population decline during local epidemics. The main aim of the paper is to investigate whether the Reformation made the poor poorer in *relative* terms, that is increased inequality. Studying population decline gives us a hint whether this negative effect might have also been an *absolute* one.

In preindustrial times epidemics were most likely exogenous events for individual localities (Jedwab et al. 2022), but selective by socio-economic status within a town or village: the poor were more likely to die (Cummins et al. 2016, Alfani 2021b). If the poor strata were made comparatively poorer by Protestantism, as implied by the theoretical framework, one would expect this effect to show up not only in lower population stratas' wealth shares, but also on the death margin. There should be a higher mortality in Protestant compared to Catholic places when an epidemic hit.

Table 4.5: Mortality from Epidemics (Difference-in-Differences Estimates)

	(1) ln-Population	(2) ln-Population
Protestant × Post-Reform. × Epidemics	-0.191***	-0.188***
	(0.069)	(0.068)
Protestant × Post-Reform.	YES	YES
Post-Reform. × Epidemics	YES	YES
Epidemics	YES	YES
Controls	NO	YES
Locality FE	YES	YES
Time FE	YES	YES
Observations	368	368
Communities	43	43
R^2	0.268	0.279
Mean of dependent variable	7.509	7.509

Notes: Estimation method is OLS. Controls include conflict exposure and distance to the nearest university. Standard errors clustered at locality level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 4.5 tests this idea. The set-up is similar to Equation 4.1, but I take the local population size (in logarithm) as outcome. The coefficient of interest comes from the interaction of a place being Protestant and the outbreak of local epidemics in the previous period. I control for the lower order interaction between being Protestant and the Post-Reformation period, to account for the possibility that the Reformation itself had an impact on population levels. I also control for the interaction between being in the Post-Reformation period and the occurrence of epidemics, to pick up potential changes in the strength of epidemics after the Reformation began.

The results in Columns 1 and 2 confirm the logic of the theoretical framework. Epidemics reduced the population size in Protestant places significantly more than in Catholic places. This suggests that the Reformation made the poorest strata not just relatively worse off in economic terms, but it had very tangible consequences for the lives of people: it made them more vulnerable to the lethality of epidemics, which were a defining characteristic of preindustrial times. This empirical finding is consistent with the theoretical framework and the results in the main analysis.

Robustness: Alternative Samples

Given the limited nature of the dataset, one might be wondering whether the results are driven by idiosyncratic characteristics of groups of communities in the dataset. Figure 4.6 shows that the results hold when certain localities are dropped. A first concern could be that Northern communities were peripheral and not well integrated in the governance structures of the Holy Roman Empire. They therefore differed from other places in the centre and south of Germany and it might be that the Reformation-variable just picks up this otherness. In Column 1 the four communities high in the north (see Figure 4.4) are dropped, which does not change the results.

I also drop the five largest cities in the dataset in 1500 (Lübeck, Augsburg, Erfurt, Munich and Frankfurt a.M.), to examine whether these exceptionally large and economically dynamic places drive the results. Column 2 indicates that this is not the case. Another concern could be that the geographical clustering of places, evident in Figure 4.4, around Wangen (10 communities) in the south and Lippe (8 communities) in the centre-west, could drive the

Table 4.6: Alternative Samples (Difference-in-Differences Estimates)

	(1) Bot. 20%	(2) Bot. 20%	(3) Bot. 20%	(4) Bot. 20%	(5) Bot. 20%	(6) Bot. 20%
Protestant×Post-Reform.	-0.995**	-1.137***	-0.604*	-0.825**	-1.130***	-0.979**
	(0.392)	(0.410)	(0.339)	(0.380)	(0.398)	(0.384)
Northern localities excl.	YES	NO	NO	NO	NO	NO
5 largest cities in 1500 excl.	NO	YES	NO	NO	NO	NO
Lippe localities excl.	NO	NO	YES	NO	NO	NO
Wangen localities excl.	NO	NO	NO	YES	NO	NO
Confessional reversal localities excl.	NO	NO	NO	NO	YES	NO
Bishopric localities excl.	NO	NO	NO	NO	NO	YES
Observations	340	317	320	285	338	339
Communities	39	38	35	33	40	39
R^2	0.253	0.292	0.182	0.239	0.250	0.235

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

results. Columns 3 and 4 show that although losing this relatively large number of places reduces the power of the dataset considerably, the main results qualitatively hold. A fifth concern could be that localities that switch their confession more than once could drive the results. Column 5 shows that the results hold when those places are dropped that, after having introduced the Reformation, partially or completely reverse to Catholicism. Finally, one might wonder about localities that belonged to a bishopric. Column 6 suggests that the results hold when dropping these places. These checks reassure that the main results are robust to the composition of the sample.

Continuity in the Number of Taxpayers

One might wonder whether the lower wealth shares of poor strata in Protestant places were the result of different taxation principles in localities that introduced the Reformation, which could have had an impact on the recorded distribution of wealth. For example, it could be possible that local bureaucracies in Protestant places recorded poor people more rigorously in the tax registers. Alternatively, one could conjecture that Protestant places recorded serfs, which on average were most likely poorer than the rest of the population, more thoroughly than Catholic administrations. If that were the case, one would expect a higher number of taxpayers in Protestant compared to Catholic places.

In Table 4.7 I check whether there are any discontinuities in the number of taxpayers during the early phase of the Reformation. The set-up is again similar to Equation 4.1, but I take the local number of taxpayers (in logarithm) as dependent variable, which can be easily calculated from the tax registers that were used to calculate inequality measures. Moreover, I restrict the dataset to three different periods, from the time just before the Reformation began until three different cutoff years in the early phase of the Reformation: 1500-1550, 1500-1575 and 1500-1600. All coefficients are close to zero and slightly negative, but not statistically significant. This suggests that if anything there were slightly less taxpayers in Protestant places. These results provide no evidence for the idea that there existed differential recording of taxpayers in Protestant compared to Catholic places. Of course, this does not rule out entirely that Protestant bureaucracies recorded poor people more thoroughly, but it makes it unlikely.

Table 4.7: Number of Taxpayers (Difference-in-Differences Estimates)

	(1) ln-Taxpayers	(2) ln-Taxpayers	(3) ln-Taxpayers
Protestant×Post-Reform.	-0.029	-0.032	-0.002
	(0.082)	(0.084)	(0.097)
Period	1500-1550	1500-1575	1500-1600
Observations	127	163	196
Communities	43	43	43
R^2	0.221	0.211	0.208
Mean of dependent variable	5.575	5.601	5.615

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

Flexible Difference-in-Differences Estimates

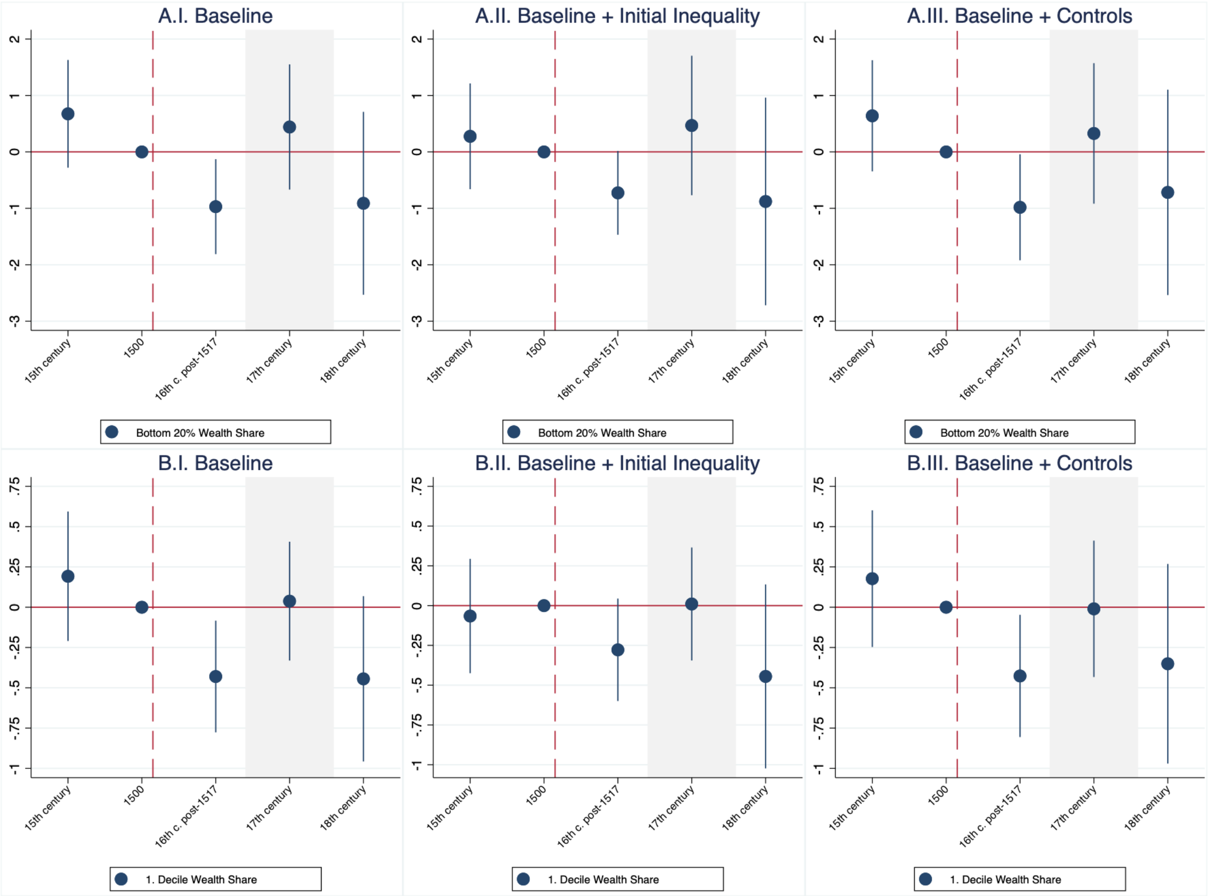
The relative impoverishment of poor strata in Protestant places and consequently the increase of inequality from the bottom of the distribution is the principal insight of the analysis so far. In Figure 4.5 I plot the estimation results of a flexible DD model, taking the wealth share of the bottom 20 percent and the first decile as dependent variables. This setup allows me to evaluate, first, whether the relationship between Protestantism and the wealth share of the lower classes of society changed over time and whether short- and long-term effects were similar. Second, it allows for a formal test of the presence of differential pre-Reformation trends, and to control for such trends to some extent. The specification is almost identical to Equation 4.1:

$$I_{i,t} = \alpha_i + \pi_t + \sum_{t=1}^{4} \beta_t (Prot_i \times Century_t) + \boldsymbol{\gamma' X_{i,t}} + \epsilon_{i,t} \tag{4.2}$$

The main difference is the inclusion of an interaction term between the treatment status indicator ($Prot_i$) and a set of four century dummies ($Century_t$) covering the pre- and post-treatment periods. The beta (β) coefficients are the main coefficients of interest. Unfortunately, the small dataset does not have enough power for estimating a fully flexible model in 25-year intervals. I therefore follow the example of Cantoni et al. (2018) and cluster observations into larger intervals, to increase the number of observations for each interval. I take the year 1500 as reference category and divide the remaining period of observation into four intervals corresponding to centuries: from 1400 until about 1475; from 1525 just after the Reformation began until 1600, from 1625 until 1700; and from 1725 until the end of the period of observation in 1800. Note that this is still a demanding specification, considering the size of the dataset, especially towards the end of the period of study when the coverage of the dataset is less dense.

In Panel A I take the wealth share of the bottom 20 percent and in Panel B of the first decile as dependent variable. Both patterns are almost identical. In Frame I the coefficients on the pre-Reformation period indicates that Protestant communities may not have been significantly different from Catholic ones. This is an important result because it supports the common-trend assumption. Yet the indicators of the first part of the treatment period (1525-1600) point to a significant widening in outcomes between Protestant and Catholic communities once the Reformation began. The poor strata of the population were worse off in Protestant places, and the difference is of a similar magnitude as the simple DD estimates in Table 4.3 suggest.

Figure 4.5: Wealth Share of Bottom 20% and D1 in Protestant vs. Catholic Communities (Flexible Difference-in-Differences Estimates)



Notes: Regression estimates of the wealth share of the bottom 20 percent of the population (Panel A) and of the first decile of the population (Panel B) in eventually Protestant communities before and after the Reformation (vertical red line), with respect to Catholic communities (horizontal red line). The omitted reference year is 1500. The estimation method is OLS. All specifications have a full set of community fixed effects and time fixed effects. Standard errors clustered at locality level. Confidence intervals indicate significance at the 95-percent level. The vertical red line represents the beginning of the Reformation period, the grey box the Thirty Years' War.

The coefficients for the period 1625 to 1700 indicate that the effect seems to disappear during the crisis-ridden seventeenth century, the century of the Thirty Years' War (1618-48; approximately indicated by the grey box). The coefficients in both panels indicate that there was no significant difference between Protestant and Catholic communities. This result might seem puzzling, but it is actually precisely what one would expect if poor relief policies were one of the mechanisms causing lower wealth shares of the poor strata in Protestants communities and considering the historical events of the period. The war was exceptionally destructive and the prolonged macroeconomic recession that followed had long lasting negative effects on the local budgets available for poor relief. This was particularly relevant in Catholic communities, which might have had more generous poor relief provision (Ogilvie 1992, Jütte 1994: 131). For example, it has been shown for Catholic Trier that the war led to a cessation of poor relief for decades. While hundreds of poor households had received support in the sixteenth and early seventeenth centuries, the number of recipients dropped to zero during the war (Ackels 1984). In other words, the universal and comparatively generous poor relief in Catholic communities during peace time could not have its redistributive effect in time of war and crisis. This might have reduced temporarily the difference in the wealth share of poor strata in Protestant compared to Catholic communities.

If the war and the crisis of the seventeenth century were an exceptional shock to poor relief, it is not surprising that the coefficients for the eighteenth century are clearly negative again. The new Protestant poor relief institutions remained substantially unchanged until at least the late eighteenth or early nineteenth centuries (Jütte 1994: 106-109). However, this result is not statistically significant. The large confidence intervals suggest that this is probably the result of the poorer coverage of the later early modern period in the dataset. From 1725 to 1800 the dataset only contains 50 observations. Of course, it is also possible that the insignificant result indicates that the effect of the Reformation was short-lived.

In Frame II I interact every community's inequality level at the beginning of observation with a full set of year dummies. These interactions absorb a large amount of the pre-Reformation variation in the wealth shares of poor strata. The benefit of this demanding specification is to make communities even more comparable in the post-treatment period. Reassuringly, the patterns of redistribution observed in Frame I are qualitatively preserved, regardless of whether the bottom 20 percent or the first decile is employed as dependent variable. In Frame III all controls are added to the baseline specification. In this demanding saturated specification, too, the picture of significantly lower wealth shares of poor strata in Protestant communities is preserved in Panel A and B. That is an important result because one could argue that it was population decline leading to higher wages of lower strata, not the cessation of poor relief, that led to the vanishing difference between Catholic and Protestant communities during the seventeenth century. Since population change is controlled for in this specification, it cannot drive the results.

A general concern with the results presented in this section could be that Protestantism made poor people only relatively worse off, but richer in absolute terms due to positive effects on economic growth. This possibility cannot be entirely excluded, because the only data systematically available refer to the distribution of wealth but cannot be used to analyse wealth levels.[20] Though it is an unlikely scenario as I control for proxies of economic development in most of the regressions. The results indicate that although I effectively hold economic growth constant, the poor parts of the population move significantly further and further away in terms of their wealth share from the bulk of the population in Protestant places.

4.4.3 Endogenous Adoption of the Reformation: Instrumental Variable Analysis

The historical reasons for the advent of Protestantism and the flexible DD estimates support the assumption that the Reformation did not break out because of factors that were also related to inequality in the localities. Moreover, the flexible DD estimates did not provide evidence for significant pre-trends. Nonetheless, one might still be sceptical about selection bias, and ask whether the Reformation was really adopted because of religious reasons in the sixteenth century, and not also because of unobserved economic factors, which could in turn be related to inequality. For example, one could argue that more entrepreneurial places were more prone to the Protestant interpretation of Christianity but were also more likely to have higher inequality. Then the OLS results could overstate the actual effect. Moreover, one could be concerned about measurement error in the treatment variable. To rule out

[20]The main problem is the lack of comparability of wealth in different communities, a consequence of preindustrial Germany's political fragmentation that led to a plethora of local currencies. This is not a problem for calculating the distribution of wealth, but it makes the a comparison of wealth levels impossible. The wealth information expressed in different local currencies cannot be converted into a common one, given our current historical knowledge. Even the most comprehensive accounts of currencies and conversion rates do not even come close to making it possible to make wealth estimates comparable across communities and time (Alfani et al. 2022).

such potential remaining concerns I follow an approach that has been employed in several previous studies (see Becker and Woessmann 2009, Cantoni 2012, 2015, Becker and Pascali 2019), and use the distance to the city of Wittenberg — the place where Luther started the Reformation — as an instrumental variable (IV). The distance-to-Wittenberg instrument has been criticised (see Edwards 2017). Since we cannot ultimately test the assumptions on which the method builds, the instrument *might* be imperfect. Hence, I view the results of the IV-analysis as a useful addition to the main DD results.

The IV isolates exogenous variation in the main variable of interest, the adoption-of-Protestantism indicator. It does so by exploiting that the Reformation was more likely to be adopted the closer a community was to the movements' starting point, possibly because of geopolitical considerations in the sixteenth century. There were numerous reasons why a prince or magistrate could have been in favour or against the Reformation, some of which have been mentioned above. In addition to these reasons it was outright dangerous for princes and magistrates to officially introduce the new religious confession. Legally, Luther was banned, and his ideas and writings were prohibited in the Empire before the Peace of Augsburg. A military intervention by the Catholic Emperor and his allies or similar dire political consequences were an imminent threat. Therefore, allegiances among polities were important when introducing the Reformation (Scribner and Dixon 2003: 42-43). In that situation, having a powerful neighbouring polity made adoption less risky. Wittenberg was a centre of the first state within the Holy Roman Empire to adopt the Reformation, which was also a powerful polity compared to many others: the Electorate of Saxony (Cantoni 2012). Additionally, being closer to Wittenberg and Saxony might have made it easier for imitators to observe how the Reformation was practically implemented (Becker and Woessmann 2009: 557-558). Moreover, having contact with Luther himself was immensely important for the spreading of his new interpretation of Christianity. He had most frequent and intense contacts — either through writing or personal visits — with places close to Wittenberg (Becker et al. 2020: 868-869).

Table 4.8 shows the correlations between the potentially endogenous variable, the binary

Table 4.8: Correlates of the Adoption of Protestantism until 1600

	(1) Protestant	(2) Protestant	(3) Protestant	(4) Protestant	(5) Protestant	(6) Protestant	(7) Protestant	(8) Protestant	(9) Protestant
Dist. to Wittenberg	-0.003***	-0.002**	-0.002***	-0.003***	-0.003***	-0.003***	-0.003***	-0.003***	-0.002**
	(0.000)	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)
Dist. to Zurich		0.001							
		(0.000)							
Dist. to Geneva			0.000						
			(0.000)						
Soil Quality				0.318					
				(0.423)					
ln-Pop. in 1500					0.105				
					(0.078)				
Top 1%						0.016			
						(0.014)			
Urban Community					-0.198	-0.059	0.150	0.113	
					(0.269)	(0.207)	(0.148)	(0.111)	
Hanse City							-0.105		
							(0.200)		
Imperial City								0.116	
								(0.124)	
Longitude									-0.241
									(0.783)
Latitude									0.052
									(0.181)
Longitude×Latitude									0.003
									(0.015)
Constant	1.534***	1.086**	1.165**	1.338***	0.835	1.428***	1.527***	1.511***	-0.268
	(0.135)	(0.453)	(0.552)	(0.226)	(0.570)	(0.168)	(0.193)	(0.148)	(9.577)
Observations	43	43	43	43	43	43	43	43	43
R^2	0.496	0.509	0.501	0.503	0.547	0.536	0.514	0.521	0.600

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

indicator of the adoption of Protestantism in a community until 1600, and several possible predictors, including the distance to Wittenberg. The set-up is conceptually equivalent to a first-stage regression.[21]

The results indicate that distance to Wittenberg is indeed a strong predictor of Reformation adoption, suggesting that the instrument-relevance condition is met. The further a community was away from Wittenberg, the less likely it was to be Protestant in 1600. This relationship is robust to the inclusion of other theoretically plausible predictors, such as the distance to other centres of the Reformation in Europe, Zurich and Geneva (Columns 2 and

[21]Note that the first-stage varies when the complete panel of communities is employed to estimate the causal effect of the Reformation on the wealth distribution over time. The set-up of the main analysis is slightly different, containing various fixed effects and the instrumented variable interacted with the post-treatment indicator. The relationship between the instrument and the instrumented variable is nevertheless strong.

3). Similarly, several economic indicators — soil quality, the population size of a community in 1500, the top 1% wealth share in a community in 1500 — do not eliminate the significance of the distance to Wittenberg (Columns 4 to 6).[22] The inclusion of whether a city had a special constitutional status, that is, was member of the Hanse or an independent Imperial city (Columns 7 and 8), does not change the picture either, as do geographic coordinates (Column 9). Cantoni (2012, 2015) performs similar analyses and reports analogous results.

The exclusion restriction in this setting amounts to assuming that, conditional on locality and time fixed effects and several covariates, the distance to Wittenberg affected inequality only through influencing the probability of a community of becoming Protestant. One theoretically possible threat could be that being closer to Wittenberg was somehow related to better pre-existing economic development, for example through regional effects (Edwards 2017), which might have also implied higher inequality. Then the instrument would not be independent and violate the exclusion restriction.[23] However, Wittenberg was not at all an economic hub. Quite to the contrary, it was an economically backward and remote place, or to put it the words of Luther "on the edge of civilisation" (Iserloh et al. 1980: 19).

Figure 4.6 provides additional evidence supporting the assumption that the instrument is as good as randomly assigned. Several wealth shares and inequality indicators in 1500 are plotted against the distance to Wittenberg. If the instrument really provides variation that is exogenous to pre-existing determinants of wealth distribution, then one would expect to find no significant correlation between the distance to Wittenberg and distributional outcomes in 1500. This is precisely what the results of this placebo test indicate. The correlations are practically zero, and not statistically significant (see additional results in the Appendix). If anything, there is a slight tendency towards lower inequality closer to Wittenberg. That is the exact opposite of what one would expect if one were concerned about closeness to

[22]Note that when regressing Protestantism on the population size and top wealth shares one has to control for whether a community was a city. The reason being that cities had almost mechanically a larger population, and also higher top wealth shares, but cities were also more likely to adopt the Reformation (Scribner 1994: 7-14).

[23]Note that the assumption of treatment being assigned as good as randomly ("independence restriction") is strictly speaking conceptually different, but commonly subsumed under the exclusion restriction (Angrist and Pischke 2009: 117, 153).

Figure 4.6: Instrument Exogeneity: Wealth Distribution in 1500

Wittenberg causing higher inequality independently of the Reformation.

More evidence for Wittenberg's economic insignificance comes from Becker and Woessmann (2009: 559-563). They test whether the distance to Wittenberg was related to a battery of pre-Reformation economic and human-capital indicators, such as urban population or the presence of a school in 1517, but do not find any significant relationship. This further suggests that the distance to Wittenberg did not matter for economic outcomes and thus for economic inequality via other channels than the Reformation in preindustrial Germany. Moreover, in most of the regressions presented below I control for several observable economic but also demographic and institutional time-variant characteristics of communities, in addition to time-invariant characteristics captured by the fixed effects. If these saturated specifications still produced significant results, one could be relatively confident that the exclusion restriction holds, conditional on covariates, and that the instrument is as good as randomly assigned, that is, independent of potential outcomes.

My instrumental variable setup is similar to Cantoni (2015). Compared to the previous DD setup, the interaction term of interest including the indicator of the adoption of the Reformation is now instrumented by an interaction term containing the IV instead. This is conceptually equivalent to a two-stage least squares setup. The first-stage is:

$$Prot_i \times P_t = \alpha_{1i} + \pi_{1t} + \phi DistanceWittenberg_i \times P_t + \epsilon_{1i,t} \tag{4.3}$$

Equation 4.4 represents the second-stage:

$$I_{i,t} = \alpha_{2i} + \pi_{2t} + \delta \widehat{Prot_i \times P_t} + \epsilon_{2i,t} \tag{4.4}$$

Panel A of Table 4.9 shows the results of the IV estimates (reduced-form estimates are reported in the Appendix, showing the same pattern as the second stage results). The coefficients again represent an average post-treatment difference in wealth shares or inequality. The estimates are local average treatment effects (LATE), as is usually the case in IV analyses. In other words, they report the causal impact of the introduction of the Reformation on communities which for reasons of geographical positioning became Protestant. The coefficients are slightly larger than the OLS estimates (Panel B). The difference most likely is the result of proxy measurement error in the Reformation-indicator. The IV might recover a cleaner measure of the intensity of treatment, which is lost in the simple binary treatment variable employed in the OLS regressions. It might also be that the IV removes some of the endogeneity that plagues and thus attenuates the OLS estimates, for example due to unobserved community characteristics. The IV identification relies on compliers, and if these places are less likely to have characteristics that attenuate the effects of the Reformation, this would lead to larger IV estimates compared to OLS estimates. Again, weakness of the instrument does not seem to be an issue: Kleibergen-Paap F-statistics are well above the conventional cutoff of 10.

The picture of distributional differences revealed by the IV estimates is in essence the same as in Tables 4.3 and 4.4. Columns 1 to 3 show a negative, substantial, and highly significant effect of the Reformation on the wealth share of lower classes of society. This shows that there are inequality-differences as measured with the wealth percentiles of poor classes. Importantly, this relationship also holds when controlling for several community-level covariates, including proxies for economic development. The highly significant estimates in

Table 4.9: Instrumental Variable Estimates

	(1) Bot. 20%	(2) Bot. 20%	(3) D1	(4) Gini	(5) Top 10%	(6) Mid. 40%
Panel A: IV-Estimates						
Protestant×Post-Reform.	-1.995***	-1.896***	-0.744***	0.052*	2.899	-0.391
	(0.483)	(0.502)	(0.211)	(0.028)	(3.097)	(2.034)
Panel B: OLS-Estimates						
Protestant×Post-Reform.	-1.004**	-1.023***	-0.428***	0.031	3.493	-0.514
	(0.383)	(0.360)	(0.137)	(0.023)	(2.504)	(1.580)
R^2	0.165	0.228	0.235	0.318	0.237	0.304
F-Stat. IV	75.68	65.35	65.35	65.35	65.35	65.35
Controls	NO	YES	YES	YES	YES	YES
Locality FE	YES	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES	YES
Observations	368	368	368	368	368	368
Communities	43	43	43	43	43	43

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

the saturated specifications including covariates (Column 2 to 6) indicate that the estimates are robust to potential exclusion and independence restriction violations. Column 4 shows that the overall inequality changes in Protestant communities are now also captured by the Gini coefficient. Yet Columns 5 and 6 show that there is again no evidence of a significantly positive effect of the Reformation on top wealth shares, or on the wealth of the arithmetic middle of the population.

Overall, the IV results confirm the main DD results. The IV-approach de facto randomises treatment, which suggests that the DD results are unlikely to be driven by selection bias. The results also confirm the hypothesis derived from the theoretical model. We observe a reduction in the wealth shares of poor strata in Protestant places, consistent with the notion that the new low-redistribution policies left the bottom of the poor behind in Protestant society.

4.5 Mechanisms: Particularistic Protestant Poor Relief Policies

The analysis has shown so far that the Reformation made poor people relatively poorer in Protestant communities, thus increasing the gap between them and the rest of the population. What are the likely mechanisms behind this relationship? One of them being that inequality and relative poverty grew in Protestant places due to potential economic expansion, for example fuelled by upper-tail human capital growth. These could have been outcomes of the Reformation and could have led to social differentiation. But on closer examination this conclusion is unlikely to explain the results: first, because in most regressions economic development is controlled for; second, because growth and higher human capital would benefit the middling and upper classes of society disproportionately, leading to wealth concentration at the very top of the distribution (Ray 1998: 209-211). But as we have seen, no other part of the population gained significant wealth shares, neither the middling parts nor the percentiles at the very top. Another explanation might be that it was not the Reformation, but rather the German Peasants' War (1524-25) that impoverished poor strata and happened around the time when the Reformation began. This is an unlikely explanation too, because I also control for exposure to warfare in most regressions.

The theoretical model in Section 4.2 suggests a different hypothesis to explain the empirical patterns: under Protestantism welfare provision was expanded for few insiders (Kahl 2009, Dittmar 2019). Yet it excluded strangers and able-bodied but non-working and non-resident poor from the local Christian community. These individuals were consequently non-eligible for receiving poor relief. This was a shift away from the Catholic universalism in the provision of social welfare (see Enke et al. 2021, 2022). The Reformation, therefore, brought about a reduction in the supply of poor relief to the excluded groups and a decrease of transfers from better-off to poor strata. The new low-redistribution policies left the bottom of the poor behind in Protestant society. The particularistic new social welfare system also had second order effects, such as the prohibitions of begging, the disincentivising of almsgiving and the stigmatisation of the poor in the labour market. These had analogous distributive effect. This is why the difference-in-differences and instrumental variable results in the previous section have found that the Reformation reshuffled the bottom end of the income and wealth distribution to make some poor even poorer.

4.5.1 Econometric Evidence

Many of the elements of the low-redistribution policies in Protestant places are impossible to measure, given the available historical evidence for the pre-statistical age. Ideally, one would like to have data on effective redistribution to poor people. Unfortunately, such data are not available. For that reason, one has to rely on indirect evidence. Some results from previous sections are consistent with the notion that Protestantism brought about new low-redistribution policies that excluded some people at the margin of society from social welfare. It was found that it was the bottom decile, the poorest of the poor, that lost the largest wealth share in Protestant places. Moreover, I have documented that Protestant places experienced higher mortality from epidemics, such as plague. This is exactly what one would expect if Protestantism made poor strata worse off.

In order to further disentangle the hypothesised mechanisms leading from Protestantism to a lower wealth share of poor strata I study, first, monastery closures during the Reformation and, second, the legal changes brought about by the Reformation in cities and territories.

The Disappearance of Catholic Social Welfare: Monastery Closures

I proxy for one facet of the disappearance of the universal social welfare system of the Catholic Church during the Reformation: the expropriation and closure of monasteries by local rulers, thus confiscating valuable assets of the Old Church. Monasteries used to redistribute a substantial part of the Church's income — probably between one third and one fourth — to the poor through almsgiving and were an important element of the Catholic poor relief system. Moreover, were often also hospitals that provided rudimentary healthcare to poor people. Reformers had envisaged that monasteries' expropriated assets would be reemployed in Protestant communities to provide relief for needy people from the poor chest (Hsia 1996:

366, Kahl 2009: 270).

In reality, these assets often went into the coffers of local rulers and were not employed for welfare but for waging wars, building roads, hiring bureaucrats, or building palaces. In Catholic territories these assets continued to be employed to benefit the poor (Cohn 1987, Cantoni et al. 2018). One would expect that this disappearance of Catholic social welfare *ceteris paribus* led to lower strata of society losing wealth shares.

Table 4.10: Evidence on Monastery Closures (Difference-in-Differences Estimates)

	1400-1600			1400-1800		
	(1) Bot. 20%	(2) Bot. 20%	(3) D1	(4) Bot. 20%	(5) Bot. 20%	(6) D1
Monasteries closed×Post-Reform.	-0.137**	-0.167**	-0.056**	-0.076	-0.127**	-0.050*
	(0.062)	(0.062)	(0.027)	(0.054)	(0.056)	(0.026)
Controls	NO	YES	YES	NO	YES	YES
Locality FE	YES	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES	YES
Observations	252	252	252	368	368	368
Communities	43	43	43	43	43	43
R^2	0.113	0.211	0.227	0.148	0.218	0.222

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

I gather information on the location and, if applicable, the closure date, of 3,094 monasteries from Cantoni et al. (2018) and Jürgensmeier and Schwerdtfeger (2005). For each locality in my dataset, I calculate the number of monasteries nearby that were closed during the Reformation period until 1600. Since the closure was a one-off reallocation of resources, usually happening when a ruler converted to Protestantism, it is likely that an effect could be observed most clearly in the early phase of the Reformation period. For that reason, I provide estimates covering a shorter period until about 1600 and estimates for the complete period until 1800.

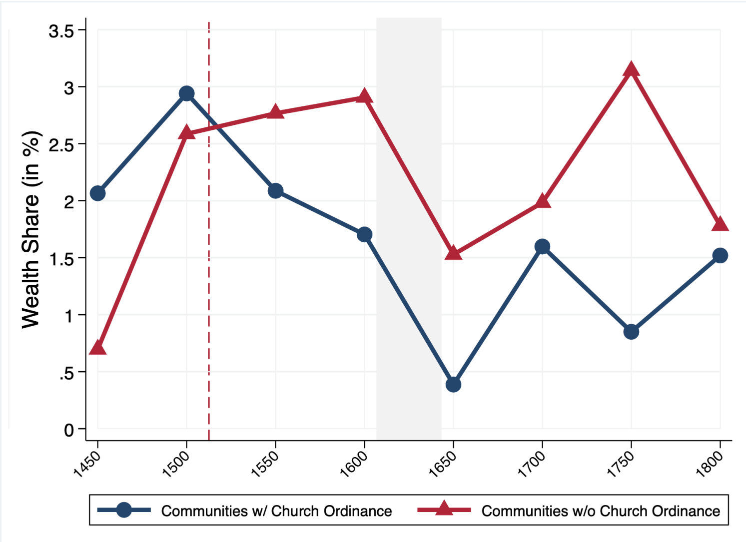
I estimate regressions analogous to the DD setup in Equation 4.1, taking monastery closures as treatment variable. The results in Table 4.10 are consistent with the predictions of the theoretical model. Column 1 indicates that the closures of monasteries significantly reduced the wealth share of the bottom fifth of the population. For every monastery closed, people at the bottom of the population lost 0.137 percentage points of their wealth share until 1600. Columns 2 and 3 show that this result holds when controls are introduced. Columns 4 to 6 show that, as expected, the effect is somewhat smaller and less precisely estimated if we consider the entire period until 1800 but is qualitatively preserved. These results suggest that the Reformation and the subsequent closure of monasteries did not just redistribute economic resources between rulers and the Church (Cantoni et al. 2018), but also affected the distribution among individuals. One might wonder whether rulers' propensity to close and expropriate monasteries was driven by an increase in the macro-level frequency of warfare in the Reformation period. However, such a macro effect should be captured fairly well by time fixed effects.

New Poor Relief Institutions: Protestant Church Ordinances

Often, some years after the adoption of the Reformation, Protestant city magistrates and territorial rulers introduced laws in their communities, which regulated the areas of public life that used to be regulated by the Catholic Church, namely education, church governance and, importantly, the secular provision of poor relief (Hsia 1996: 366, Dittmar and Meisenzahl 2020). These "church ordinances" (*Kirchenordnungen*) were the legal basis for such poor relief institutions as the common chest or the prohibition of begging. They also often put the particularistic provision of poor relief into written law, by declaring non-working and non-resident poor as non-eligible for receiving support. Ordinances also conferred communities the task of managing poor relief locally. The texts of the laws together with their dates of introduction have been edited in a multi-volume series by Sehling (1902). I employ the introduction of a church ordinance in a community as a proxy variable for the establishment of the new systems of Protestant welfare and poor relief, similar to the study of Dittmar and Meisenzahl (2020).

In addition to the mere presence of a church ordinance, the editions of Sehling make it possible to code a variable indicating whether the content of the laws regulated begging in a community. The regulation of begging took different forms: begging could be limited to

Figure 4.7: Wealth Share of the Bottom 20% in Communities with and without Protestant Church Ordinances



Notes: Values were were collapsed into 50-year intervals and represent half-century-averages. To avoid that communities with more observations dominate the trend, every community has the same weight in the average. Because of the uneven number of years and the low number of observations in 1400, I collapsed the values for the years 1400 until 1450 into one data point. The vertical red line represents the beginning of the Reformation period, the grey box the Thirty Years' War.

certain days of the week, the right to beg could be denied to specific groups (for example foreigners), begging could be restricted to certain areas of the locality or prohibited altogether. The important aspect that unites these forms of regulation is that they restricted the opportunities for begging, thus denying needy people an important source of income and limiting voluntary transfers from better-off to poor individuals.[24]

Figure 4.7 report the wealth shares of poor strata (calculated analogously to Figure 4.1) for communities with and without ordinances. They show a clear pattern: a decline of the wealth shares of poor people from the sixteenth to the eighteenth century relative to the initial value in places with church ordinances. At the same time wealth shares of the poor stagnate in places without ordinances over the centuries. Instead, before the beginning of the Reformation both groups seem to have followed similar developments.

In order to test the presence of these different patterns more formally, I estimate again a DD

[24]It has to be kept in mind that the text of the lengthy ordinances has not always been published entirely in the Sehling-volumes (see Dittmar and Meisenzahl 2020: Appendix). Hence the variable has potential measurement error because it is possible that some communities that were coded as not regulating begging actually did so. The estimates therefore give only a lower bound estimate of the actual effect of begging regulation.

setup that is similar to Equation 4.1. I augment the main interaction term of interest to a triple-difference-in-differences, including the ordinance indicator:[25]

$$
\begin{aligned}
I_{i,t} = \alpha_i + \pi_t + \theta_1 Prot_i \times PostReform_t \times Ordinance_i \\
+\theta_2 Prot_i \times PostReform_t + \boldsymbol{\gamma' X_{i,t}} + \epsilon_{i,t}
\end{aligned}
\tag{4.5}
$$

Columns 1 and 2 of Table 4.11 document that the introduction of an ordinance led to a sizeable and significant reduction of the wealth share of the bottom 20 percent of the population. The effect of Protestantism loses its statistical and economic significance. This is indicative of church ordinances and the poor relief institutions they introduced having large explanatory power and driving a major part of the observed differences in the wealth shares of poor people in Protestant and Catholic communities.

Table 4.11: Evidence on Laws Regulating Poor Relief (Difference-in-Differences Estimates)

	(1) Bot. 20%	(2) Bot. 20%	(3) Bot. 20%	(4) Bot. 20%	(5) D1
Protestant×Post-Reform.×Ordinance	-1.476***	-1.639***	-0.532	-0.843**	-0.478**
	(0.383)	(0.286)	(0.441)	(0.412)	(0.212)
Protestant×Post-Reform.×Ordinance×Begging			-1.443***	-1.181***	-0.594***
			(0.430)	(0.438)	(0.185)
Protestant×Post-Reform.	0.375	0.518	0.212	0.349	0.313
	(0.428)	(0.339)	(0.460)	(0.361)	(0.189)
Controls	NO	YES	NO	YES	YES
Locality FE	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES
Observations	368	368	368	368	368
Communities	43	43	43	43	43
R^2	0.174	0.239	0.200	0.256	0.283

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

In Columns 3 and I augment the interaction term of interest with the begging restriction indicator. The results show that, as expected, regulating begging had a sizeable and negative association with the wealth share of poor strata. As one would expect, the presence of a church ordinance still shows a negative effect, most likely because ordinances restricted

[25] Since the first ordinance dates from 1522, the treatment variable is interacted with a post-treatment indicator that switches on in 1522.

poor relief in unmeasured ways other than limiting begging, for instance by excluding able-bodied but non-working people from welfare or by disincentivising almsgiving. Note that the effect on the first decile (Column 5) being more than half as large as the effect on the bottom 20 percent (Column 4) suggests that poorer households were hit harder by a begging regulation. Intuitively, this is exactly what one would expect because households were more likely to receive money from begging the poorer they were. In general one has to interpret these results keeping in mind that regulating begging was likely correlated with other unmeasured characteristics of the poor relief system that limited redistribution to poor people in Protestant communities.

Overall, the evidence is consistent with the hypothesis that particularistic Protestant poor relief institutions were an important reason for why poor strata were worse off in Protestant places. The insignificant coefficients on the Protestantism indicator across all specification do not imply that what mattered about the Reformation's impact on inequality were only institutional changes as opposed to ideological ones — this would be impossible to disentangle completely with the available data — but the results point into that direction. In the Appendix I report flexible difference-in-differences estimates for the effect of church ordinances on the wealth shares of poor people. The results show an analogous pattern to Figure 4.5 and support the conclusions of this section.

A final note on international comparisons: one might object that poor relief inspired by Protestantism in England, especially the Old Poor Law, was very effective (Kelly and Ó Gráda 2011). However, there are at least three reasons why the English and German experiences with Protestant poor relief are not readily comparable. First, England's version of Protestantism was different from German Lutheranism — so different that English Protestantism was termed "Catholicism without the Pope" (Dixon 2002: 26). Second, the institutional context differed. England had a strong central state that could effectively force communities to provide poor relief and levy taxes to pay for it. In politically decentralised Germany that was not the case (Jütte 1994: 140). Third, England and Germany followed different economic trajectories. The former was a winner, the latter a loser in the so-called

"Little Divergence" (Broadberry et al. 2015: 423). This might have influenced the amount of resources available to redistribute and the setup of poor relief institutions.

4.6 Conclusion

This chapter has investigated whether religious confession has an impact on the distribution of wealth and inequality, studying the emblematic case of the Protestant Reformation. I have modelled the Reformation's inherent trade-off between, first, the expansion of public goods provision, especially in terms of social welfare, and, second, a more particularistic provision of poor relief to in-group members only. The informal model suggests that the Reformation was quite ambiguous in its redistributive implications and for inequality. I have then argued that the Reformation overall reshuffled the lower part of the wealth distribution to increase the gap between poor strata and all others.

This hypothesis has then been tested empirically. Employing a DD and IV strategy, the analysis finds strong evidence of a negative causal effect of the Reformation on the wealth share of lower classes of the population in Protestant communities between 1400 and 1800. It seems that the Reformation increased inequality, by making the poor relatively poorer. This effect can be traced back through the early modern period, which suggests that the Reformation had a lasting impact that only began in the sixteenth century. Yet I do not find evidence for significantly higher wealth shares of the rich or middling parts of the population. This result is confirmed by a variety of specifications.

The empirical picture is consistent with a historical characterisation of the Reformation as the trigger of new low-redistribution policies in Protestant places, which embodied the particularistic character of the new religious confession towards parts of the poor. Evidence for the plausibility of this hypothesised mechanism comes from information about the closure of monasteries, the introduction of church ordinances and begging prohibitions. The results suggest that what mattered about the Reformation for its effect on poverty and inequality was the institutional change that the religious divide brought about. The inequality-promoting character of Protestantism, typically observed in the modern day, seems to have deep historical roots. Moreover, the case of the Reformation exemplifies that a key dimension of redistributive policies is how universal or particularistic societies provide social welfare (Enke et al. 2021, 2022), today as in the past.

4.7 Appendices

4.7.1 Historical Background: the Introduction of the Reformation in Early Modern Germany

The introduction of the Reformation took place over several decades. This long process can be divided into three phases: from 1517 until 1525, from 1525 until 1555, and from 1555 onward. An overview of the religious-political developments during this period is helpful for appreciating the identification strategy of the analysis in the main text.[26]

The first phase begins with the formulation and circulation of Martin Luther's famous 95 theses in the Saxon town Wittenberg in 1517. They were a sharp critique of the Catholic Church and the Pope, especially of the practice of selling indulgences. Luther wanted to call attention to theological misconceptions and provoke a debate, but not split the Church. He was soon accused of heresy. Frederick the Wise, the Elector of Saxony, protected Luther, who was a theology professor at Frederick's newly established university in Wittenberg. The spread of Luther's critique in the Holy Roman Empire and beyond was rapid. It was made possible by the diffusion of Gutenberg's printing technology a few decades earlier, and by the fact that authorities in the Empire were distracted by political struggles and warfare. In these early years the Reformation was characterised by the absence of a legal provision in the Empire about whether or how to introduce the new faith. Several polities introduced it nevertheless, such as Electoral Saxony or Hesse. Some localities started to follow the reformatory vision of Christianity before their hesitant rulers officially introduced it, for example by substituting their Catholic priest with a Protestant preacher. However, this happened in a legal vacuum, and the Peasants' War in 1525 ended this early phase of the

[26] The following historical account is based on Schilling (1994: 85-116, 193-254, 445-464), Reinhard (2004) and Blickle (2015: 165-191).

Reformation. A coalition of princes, Imperial cities and the Emperor crushed the revolt.

The war began the second phase of the Reformation, from 1525 until 1555, during which its introduction fell in the hands of political authorities. This was, at least implicitly, confirmed by the Imperial Diet in Speyer in 1526: as no Empire-wide agreement could be found yet, the decision to introduce the new faith was left to individual territorial rulers and magistrates. Still, this happened under great legal and political uncertainty.

The Peace of Augsburg was the beginning of the third phase of the Reformation, from 1555 onward. Only at this Imperial Diet a treaty between Lutherans, by far the largest branch of Protestantism in Germany, and Catholics was brokered. It officially confirmed the existence of a second protected religious confession in Germany. A principle later termed *cuius regio, eius religio* ("Whose rule, his religion") certified worldly rulers' monopolistic right to determine the confession in the communities of their territories and of their subjects, a right which they had practically exercised since 1526.[27] The treaty made the Reformation a project of the slowly emerging "states" within the Holy Roman Empire. Following the Peace of Augsburg in 1555, more polities became formally Protestant.

Even the Thirty Years' War (1618-1648) did not change the fact that two confessions existed in Germany. The Peace of Westphalia further consolidated the religious divide: it fixed polity's confessional denomination *ex post* to its status in 1624 and established that rulers could change their personal confession, but not force their territories' subjects to convert anymore.[28] As a consequence, for the overwhelming majority of subjects the religious denomination of the polities they were living in did not change until at least the nineteenth century.

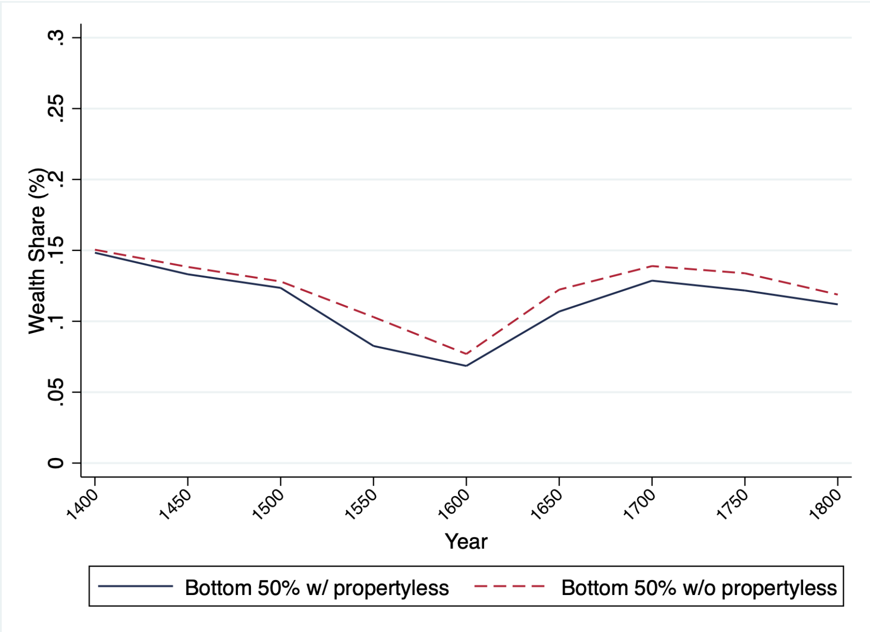
[27] In the case of the many ecclesiastical polities in the Empire, not even the rulers could choose the religious confession (*geistlicher Vorbehalt*). They were legally obliged to stick to the old confession or otherwise loose their dominion and possessions (Blickle 2015: 189-190).

[28] Arguably another important provision was the legalisation of Calvinism.

4.7.2 Additional Information on the Propertyless in the Dataset

The community-level inequality panel used in this Chapter and presented in Chapter 2 provides all local inequality estimates including the propertyless households — the only sub-category among the poor that was sometimes missing from the tax registers — where these were recorded in the tax registers. Of course, what constituted propertylessness was dependent on local understanding of the concept, and even if people without property were reported, we cannot exclude with absolute certainty that some poor people were not captured by the tax registers.

Figure 4.8: Poor Wealth Shares in Germany with Propertyless (Bottom 50%)

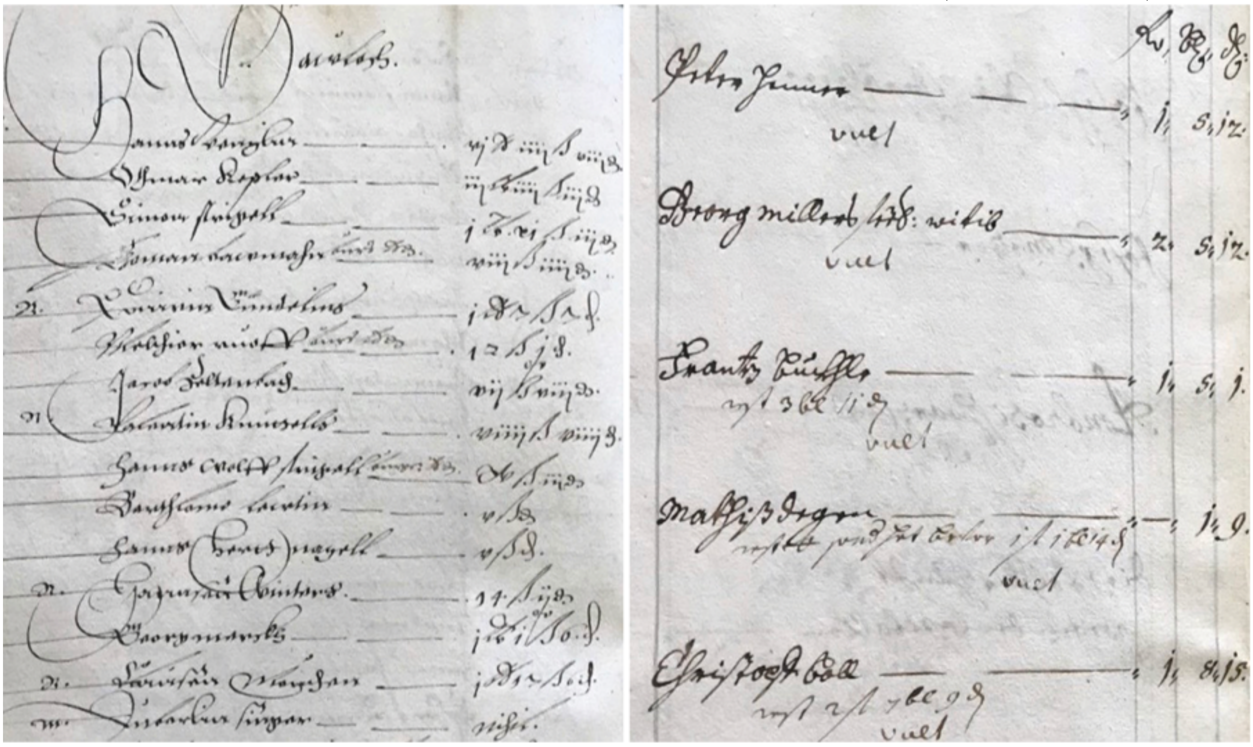


Source: See the text.

The propertyless were a small part of the total population in preindustrial Germany. Their share ranged between 0.81 percent in 1400 and a high of 8.4 percent in 1550. Reassuringly, Figure 4.8 shows that the hypothetical exclusion of all the propertyless from the wealth distributions reduces the wealth share of the poor, as measured by the wealth share of the bottom 50 percent, only marginally in preindustrial Germany (data from Alfani et al. 2022). This implies that inequality estimates where some of the poor are missing should be interpreted as lower bound estimates. Most importantly for my analysis, that is, regressions with unit- and time-fixed effects, including or dropping the propertyless does not change the trend of the wealth share of the poor. Even more reassuringly, the propertyless were missing from the tax registers of one town in my sample only, Heilbronn. As shown below, dropping Heilbronn from the sample does not change the results.

Comparison of Sources from Protestant and Catholic Communities

Figure 4.9: Archival Sources: Tax Registers from Überlingen (1575 and 1750)



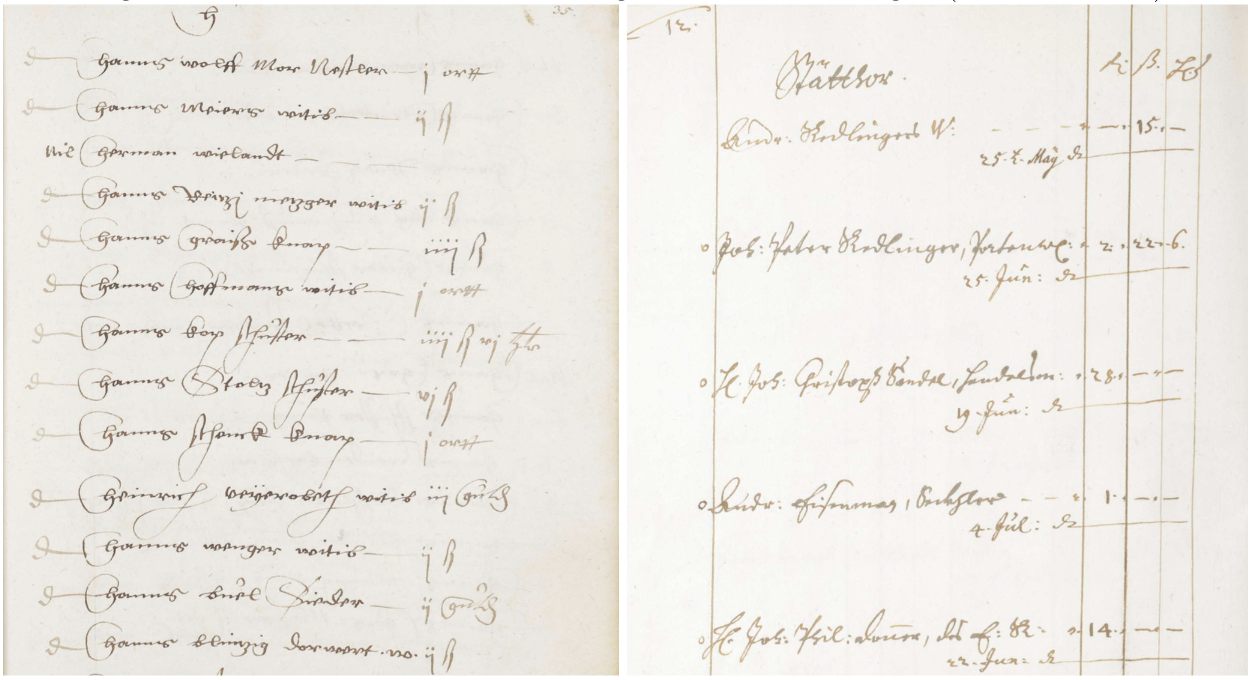
Source: Stadtarchiv Überlingen.

Figure 4.9 shows excerpts from two representative tax registers, for the medium-size town Überlingen in the German south. Note that the last entry on the page states no amount but instead states "nothing" (*nihil*). This household was propertyless.

Überlingen was a Catholic city. For comparison, Figure 4.10 shows tax registers from Protestant Nördlingen. Simply eye-balling the documents shows a high degree of similarity between the sources, although both cities were different Imperial estates. The tax registers list household heads and their respective tax estimate. The third entry of the register on the left shows a propertyless household. Similar to Überlingen, it was marked as having "*nil*". The high degree of homogeneity of the tax registers in (Catholic) Überlingen and (Protestant)

Nördlingen does not provide any visual evidence for the notion of differential Protestant tax record-keeping.

Figure 4.10: Archival Sources: Tax Registers from Nördlingen (1575 and 1725)



Source: Stadtarchiv Nördlingen.

Summary Statistics

Table 4.12 provides summary statistics by confessional groups of the main variables of interest, divided into the time before and after the beginning of the Reformation. Importantly, Catholic and eventually Protestant communities appear balanced in terms of pre-treatment inequality levels. For example, the Gini coefficient was 0.587 in Catholic, and 0.580 in eventually Protestant communities. The population size, soil quality and the share of urban communities are also similar in the two samples. The differences in coordinates reflect the fact that Catholic communities tended to be more concentrated in the south of Germany.

Real Wealth Calculation for Poor Strata in Augsburg

This Appendix describes the calculation of the real wealth per poor household — understood as a household in the bottom 20 percent of the wealth distribution — in Augsburg around the beginning of the Reformation. In 1512 the total amount of wealth tax paid in Augsburg

Table 4.12: Community Characteristics by Confessional Group

	1400-1500 (pre-treatment period)		1525-1800 (treatment period)	
	N	Mean	N	Mean
ALL COMMUNITIES:				
Log-Population Size	99	7.664	269	7.452
Soil Quality	99	0.704	269	0.726
Longitude	99	10.07	269	10.03
Latitude	99	50.28	269	49.21
Urban Communities	99	0.626	269	0.606
Wealth Share Top 1%	99	11.70	269	12.19
Wealth Share Top 10%	99	45.87	269	45.93
Wealth Share Middle 40%	99	21.87	269	21.72
Wealth Share Bottom 20%	99	2.433	269	2.089
Wealth Share D2	99	1.593	269	1.473
Wealth Share D1	99	0.840	269	0.616
Gini	99	0.582	269	0.592
CATHOLIC COMMUNITIES:				
Log-Population Size	31	7.454	149	7.139
Soil Quality	31	0.701	149	0.717
Longitude	31	10.33	149	10.19
Latitude	31	48.08	149	48.06
Urban Communities	31	0.677	149	0.577
Wealth Share Top 1%	31	12.59	149	10.76
Wealth Share Top 10%	31	48.10	149	42.85
Wealth Share Middle 40%	31	21.94	149	23.72
Wealth Share Bottom 20%	31	2.177	149	2.406
Wealth Share D2	31	1.631	149	1.768
Wealth Share D1	31	0.546	149	0.638
Gini	31	0.587	149	0.559
PROTESTANT COMMUNITIES:				
Log-Population Size	68	7.760	120	7.840
Soil Quality	68	0.705	120	0.738
Longitude	68	9.952	120	9.829
Latitude	68	51.29	120	50.65
Urban Communities	68	0.603	120	0.642
Wealth Share Top 1%	68	11.29	120	13.95
Wealth Share Top 10%	68	44.86	120	49.76
Wealth Share Middle 40%	68	21.83	120	19.24
Wealth Share Bottom 20%	68	2.549	120	1.694
Wealth Share D2	68	1.576	120	1.106
Wealth Share D1	68	0.973	120	0.588
Gini	68	0.580	120	0.632

Notes: The variables “Soil Quality”, “Longitude”, “Latitude” and “Urban Communities” are time-invariant. Differences in the means between the two periods are due to changes in the composition of the sample.

was 17,990 fl., and there were 5,479 households recorded including the propertyless (Hartung 1898: 191-192). The tax rate in that year was 0.38 percent (Gierok 2022: Appendix), which implies that the total (taxable) wealth was 47,342.11 fl. Calculations reported in the main text suggest that the average wealth share of the bottom 20 percent was 2.18 percent in my sample, which would correspond to 1,032.06 fl. held by all poor households in Augsburg, or 0.94 fl. per household. Note that this value is strikingly close to the real wealth poverty threshold of 1 fl. set by many studies of poverty in preindustrial Germany (see Dirlmeier 1978, Fischer 1982).

This number does not say much about the real value of poor households' property, so I decided to calculate the corresponding number of day wages of an unskilled labourer. An unskilled labourer earned 1.51 grams of silver per day in 1513 Augsburg (Pfister 2017: Appendix). We know that 1 guilders corresponded to 64 kreuzer in Augsburg in the period 1525–1530, and 1 kreuzer contained de jure 0.43 grams of silver (Geffcken and Häberlein 2014: 10-5-152, Weisenstein 2002: 106, Newald 1883: 6). That implies that the average taxable wealth of a household in the bottom 20 percent in Augsburg corresponded to 25.90 grams of silver, or 17.14 day wages of an unskilled worker.

4.7.3 Coding of Independent Variables

This Appendix describes how the main independent variables employed in the analysis in the main text and the other appendices have been coded.

Protestant Reformation. A dummy that indicates whether the Protestant Reformation has been introduced in a locality by a town council or local ruler after 1517. I have also recorded the official year of introduction, and when no precise year is indicated I took as alternative date the appointment of a protestant priest by the town council. When no introduction is mentioned, or the source indicates that the Reformation had "no substantial impact", I code the locality as 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 communities without entry in the *Städtebuch* usually 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-taxpayers. The number of taxpayers comes from the same local tax registers that provide information about the distribution of wealth.

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 typically assumed for preindustrial German towns is 4.5 (Minns et al. 2020: 611).

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*. Epidemics indicated by the *Städtebuch* are for example smallpox, syphilis and plague. For those rural communities in the dataset that have no entry in the *Städtebuch* I had to make an assumption about plague occurrence. I assumed that the rural communities had the same plague occurrence as the nearest town for which an entry in the *Städtebuch* and information about the outbreak of epidemics is available. These assumptions are based 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). For those villages that were under the administrative authority and were taxed by a nearby city that is part of the dataset, I have assumed the same occurrence of epidemics as in the city. For example, for the rural community of Niederwangen I assume the same plague occurrence as for the nearby city of Wangen. For those villages that were not under the administrative authority of a city in the dataset I have assumed the same occurrence of epidemics as in the closest town with an entry in the *Städtebuch*.

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

Conflict exposure. A dummy that indicates whether a locality was exposed to battle action or a siege within a radius of 25 km. The data are from Schaff (2020).

Urban community. A dummy that indicates whether a locality was a city. I consider all those localities as cities that have an entry in the *Städtebuch*.

Soil quality. 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 itself, but also climatic conditions.

Distance to Wittenberg. Distance of a locality to the city of Wittenberg, measured in kilometres (own calculations).

Distance to Geneva. Distance of a locality to the city of Geneva, measured in kilometres (own calculations).

Distance to Zurich. Distance of a locality to the city of Zurich, measured in kilometres (own calculations).

Number of Monasteries closed. A continuous variable indicating the number of monasteries within 20km from a locality, that were closed during the Reformation period (beginning in 1517) until 1600. Data on the geographical position, opening, and, if applicable, closure date of 3094 monasteries in Germany (as reported in Jürgensmeier and Schwerdtfeger (2005)) was taken from Cantoni et al. (2018). For those single entries without geographical position in the Cantoni et al. (2018) dataset, I cross-checked against the entries in Jürgensmeier and Schwerdtfeger (2005) (from where Cantoni et al. (2018) have obtained the information) to locate and geo-reference the single monasteries.

Church ordinance. A dummy that indicates whether a locality had a so-called church ordinance (*Kirchenordnung*) from 1522 onward. The information come from Sehling (1902), who provides word-by-word editions of the ordinances. I count a community as having a church ordinance if it introduced the ordinance itself, which was usually the case for urban communities, or if the Imperial estate to which the locality belonged introduced an ordinance.

Church ordinance with begging restriction. A dummy that indicates whether a locality had a church ordinance that includes restrictions of begging. These restrictions could be, for instance, denying specific groups the right to beg, restricting begging to certain areas of the locality, or prohibiting begging altogether. The information come from Sehling (1902).

Seaside locality. A dummy that indicates localities that lie within 10 kilometres of the seaside.

Imperial city. A dummy that indicates whether a locality was an Imperial city. This information has been obtained from the *Städtebuch*. Information about the Imperial rank of each estate (that is, elector, bishop, Imperial city, and so on) have been obtained from Zeumer (1913).

Hanse city. A dummy that indicates whether a city was member of the Hanseatic league, as indicated by Dollinger (1981: 68).

4.7.4 Additional Results

Communities with Propertyless Households in the Wealth Distributions

In this section I estimate the effect of Protestantism on the wealth share of the poor, including only those localities in the sample that record the propertyless population in the tax registers. Luckily, only one community in the sample does not record the propertyless, Heilbronn. Intuitively one would expect that dropping this community increases the negative effect of the Reformation on the wealth share of the poor. The results in Table 4.13 show that this is the case. The effect of Protestantism on the wealth share of the lowest fifth of the population is slightly larger and more significant, compared to the baseline results in the main text. This result holds regardless of whether controls are added or not, and suggests that the baseline results are to be interpreted as lower-bound estimates.

Table 4.13: Communities with Propertyless in the Wealth Distributions

	(1)	(2)
	Bot. 20%	Bot. 20%
Protestant×Post-Reform.	-1.076***	-1.108***
	(0.386)	(0.361)
Localities w/o propertyless dropped	YES	YES
Controls	NO	YES
Locality FE	YES	YES
Time FE	YES	YES
Observations	354	354
Communities	42	42
R^2	0.163	0.230

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

Alternative Treatment Variable Coding

In this section I employ an alternatively coded treatment variable. In the main text I employ a treatment variable that switches on in the year 1517, the year in which the Reformation period begins with the publication of Luther's 95 theses, if a community eventually became Protestant until 1600. This is the standard approach in the published literature (Rubin 2014, Cantoni 2015, Dittmar and Meisenzahl 2020). The reasoning behind this coding is that communities might have lived *de facto* as if it were Protestant before the official introduction, but often political authorities delayed the official adherence to Protestantism because of geo-political considerations. Here I estimate the baseline specification employing a treatment variable that switches on differentially for each community, in the year it became officially Protestant.

Table 4.14 shows that the alternative coding of the treatment variable does not change the DD results. There is still no significant effect of becoming Protestant on top and middling wealth shares. Instead, there is a negative and sizeable effect on the wealth shares of the poor strata of the population. These results reinforce the findings in the main text.

Table 4.14: Alternative Treatment Variable Coding

	(1) Top 10%	(2) Mid. 40%	(3) Gini	(4) Bot. 20%	(5) D1
Protestant×Post$_{i,t}$	1.366	0.599	0.012	-0.761**	-0.356**
	(2.006)	(1.378)	(0.019)	(0.364)	(0.150)
Locality FE	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES
Observations	368	368	368	368	368
Communities	43	43	43	43	43
R^2	0.189	0.287	0.257	0.159	0.144

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

Spatial Dependence

Table 4.15 reports the baseline specifications when standard errors are adjusted for spatial autocorrelation (Conley 1999; see also Gibbons et al. 2015: 137). The Conley-correction is considered sufficient to assess the significance of regression results in spatial datasets (Voth 2021: 259). 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 is assumed to decrease to zero, from 50 to 1000 kilometres.

Table 4.15: Baselines Estimates: Spatial Autocorrelation-Adjusted Std. Errors

	(1) Bot. 20%	(2) Bot. 20%	(3) Bot. 20%	(4) Bot. 20%
Protestant×Post-Reform.	-1.004***	-1.004***	-1.004***	-1.004***
	(0.293)	(0.306)	(0.277)	(0.285)
Cutoff	50km	100km	500km	1000km
Observations	368	368	368	368
R^2	0.025	0.025	0.025	0.025

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

Columns 1 to 4 show the results of the baseline specification in the main text, taking the wealth share of the bottom 20 percent of the population as dependent variable. The difference-in-difference estimates remain unchanged and significance remains very high, regardless of the cutoff distance. 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.

One might still wonder whether there existed actual spatial autocorrelation among localities, which could inflate t-statistics in the regressions. For addressing this potential concern, Moran's I has been calculated to evaluate whether there is spatial autocorrelation in the regression residuals. This has been done for the respective cross-sections of the panel-dataset, as done by Kelly (2019: 16-17, 27).

Table 4.16: Spatial Dependence in Regression Residuals (Global Morans'I-Statistic)

	(1)	(2)	(3)
Year	I-statistic	Z-score	P-value
1400	-0.218	-0.868	0.193
1425	-0.156	-0.308	0.379
1450	-0.014	0.423	0.336
1475	-0.373	-3.442	0.000
1500	-0.124	-1.277	0.101
1525	0.001	0.334	0.369
1550	0.092	1.734	0.041
1575	-0.061	-0.87	0.192
1600	0.011	0.612	0.270
1625	0.466	6.518	0.000
1650	0.726	9.243	0.000
1675	-0.059	-0.195	0.423
1700	-0.052	-0.045	0.482
1725	-0.075	-0.272	0.393
1750	0.071	1.252	0.105
1775	0.064	1.111	0.133
1800	0.009	0.684	0.247

The specification includes all controls of the main analysis, but without any of the fixed effects, due to the cross-sectional data structure. Instead I have added longitude, latitude and the interaction of longitude and latitude. Kelly (2019: 17, 22) considers positive z-scores of two and greater as indicative of relevant spatial autocorrelation. Table 4.16 shows that the z-score for almost all years of the analysis is considerably below the critical value of two. This indicates that positive spatial autocorrelation, which would be a reason of concern, is not an issue for this study. Only in two years, 1625 and 1650, are there signs of significant spatial autocorrelation, that is, a tendency that localities with similar values cluster. However, the degree of spatial-autocorrelation is quite low. It can only be speculated why spatial dependence occurs precisely in this period. One possibility could be that the Thirty Years' War (1618-50) played a role. Since this war was a global negative shock to inequality across Germany, affecting practically all communities (Alfani et al. 2022), a shock that due to the cross-sectional setting cannot be absorbed by time fixed effects, it could be that it induced spatial dependence in this particular period. Beyond this historically exceptional period and considering the results using the Conley-correction of standard errors, spatial dependence seems to be a negligible concern for this analysis. Moreover, note that more sophisticated methods to deal with spatial dependence are not feasible because of the highly unbalanced structure of the panel. For example, because a spatial weighting matrix cannot be calculated with the available data, estimating a spatial autoregressive (SAR) model is impossible (Gibbons et al. 2015).

Wild Cluster Bootstrap Statistics

To address concerns about uncertainty of inference because of the small sample size, I calculate p-values and confidence intervals obtained with Wild Cluster Bootstrapping — a re-sampling technique aimed at obtaining statistics and distributional properties that reflect the true underlying distribution of the data when we have a small number of cluster — for the baseline DD estimates in the main text. I follow the routine of Roodman et al. (2019).

To assess whether the small number of clusters in the dataset causes significant uncertainty, one can compare the normal significance levels (as indicated by asterisks) and the Wild Cluster Bootstrap p-values and confidence intervals reported in Table 4.17. None of the significance levels change when considering the alternative statistics. The effect of the intro-

Table 4.17: Baseline Estimates: Wild Cluster Bootstrap Statistics

	(1) Top 10%	(2) Mid. 40%	(3) Gini	(4) Bot. 20%	(5) D1
Protestant×Post-Reform.	3.072	-0.161	0.027	-1.004**	-0.431***
	(2.334)	(1.608)	(0.023)	(0.383)	(0.144)
Wild Cluster Bootstrap p-value	0.195	0.921	0.268	0.019	0.006
Wild Cluster Bootstrap CI (lower bound)	-1.835	-3.351	-0.021	-1.783	-0.739
Wild Cluster Bootstrap CI (upper bound)	8.034	3.092	0.078	-0.219	-0.147
Communities	43	43	43	43	43
Observations	368	368	368	368	368
R^2	0.196	0.286	0.262	0.165	0.147

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

duction of the Reformation remains insignificant when taking top or middling wealth shares or the Gini coefficient as dependent variables. However, there is a negative effect at the same significance level when considering the wealth shares of poor strata of the population. These results suggest that the small sample size and number of clusters in the data do not cause relevant uncertainty of inference.

Extended Sample

As a further robustness check, I employ an extended sample to test the effect of Protestantism, including all communities that are part of the dataset of Schaff (2022), also those for which only information before or after the beginning of the Reformation is available. For example, I include also information about the cities of Koblenz (Catholic) and Hersfeld (Protestant), although they are only observed after the Reformation began. This implies that I am interested not only in estimating differences within localities, but also differences between localities. For that reason I follow the random effects approach (Wooldridge 2002, Greene 2012). This approach is often considered inferior to the fixed-effects approach, but it has the advantage that it allows me to increase the size of the sample considerably, by 22 localities, and capture between-unit variation. The results should therefore be interpreted with caution.

The coefficients in Table 4.18 suggest that the main results in the main text hold, even in

Table 4.18: Extended Sample: Random Effects Estimates

	(1) Top 10%	(2) Mid. 40%	(3) Gini	(4) Bot. 20%	(5) D1
Protestant×Post-Reform.	2.101	0.290	0.018	-0.673**	-0.240**
	(2.097)	(1.410)	(0.020)	(0.313)	(0.121)
Random effects	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES
Observations	504	504	504	504	504
Communities	75	75	75	75	75
R^2	0.198	0.273	0.258	0.118	0.116

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

this extended dataset using between and within-variation of units. There is no statistically significant effect of the Reformation on top or middling wealth share. Yet there is a significant reduction of the wealth shares of the poor parts of the population.

Time-Invariant Controls

The evidence reported in the main text suggests that the main results of the chapter are robust to the inclusion of some economic, demographic and institutional observable time-variant characteristics. In this section I assess the robustness of the main results to the inclusion of several time-invariant locality-specific characteristics. Because of the panel-structure of the dataset I interact these time-invariant characteristics with time dummies. This is a demanding specification given the small size of the dataset, because the interaction terms consume many degrees of freedom. I test for variables that might have influenced inequality and the adoption of the Reformation, and might have therefore induced selection bias. In particular I focus on variables related to economic fundamentals and city status: agricultural potential, seaside location, whether a locality was an urban community, an Imperial city or a member of the Hanseatic League. All regressions include a full set of locality and time fixed effects. The results in Table 4.19 show that the main finding is robust to the inclusion of all these controls: the introduction of the Reformation led to a reduction of the wealth share of poor strata. The coefficient of interest remains negative, sizeable and significant.

Table 4.19: Time-Invariant Controls

	(1) Bot. 20%	(2) Bot. 20%	(3) Bot. 20%	(4) Bot. 20%	(5) Bot. 20%
Protestant×Post-Reform.	-1.126***	-1.034**	-0.724*	-1.355***	-1.011**
	(0.327)	(0.401)	(0.379)	(0.345)	(0.413)
Soil quality	YES	NO	NO	NO	NO
Seaside location	NO	YES	NO	NO	NO
Urban community	NO	NO	YES	NO	NO
Imperial city	NO	NO	NO	YES	NO
Hanse city	NO	NO	NO	NO	YES
Observations	368	368	368	368	368
Communities	43	43	43	43	43
R^2	0.211	0.215	0.389	0.303	0.220

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

Instrument Exogeneity

In the instrumental variable analysis the exclusion restriction amounts to assuming that, conditional on locality and time fixed effects and several covariates, the distance to Wittenberg affected inequality only through influencing the probability of a community of becoming Protestant. The assumption would be violated if the distance to Wittenberg caused higher inequality independently of the Protestant Reformation. While this assumption is itself untestable, Table 4.20 provides further evidence that the exclusion restriction might hold. I have regressed several wealth share and inequality indicators in 1500 on the distance to Wittenberg.

An instrument that provides exogenous variation to preexisting determinants of unequal wealth distribution should not produce coefficients that point towards significantly lower inequality the further a locality is away from Wittenberg in 1500. This is what the coefficients indicate. They are statistically not different form zero, and almost zero. This result supports the exclusion restriction of the distance-to-Wittenberg instrument.

Table 4.20: Distance to Wittenberg and Wealth Distribution in 1500

	(1) Top 10%	(2) Mid. 40%	(3) Bot. 20%	(4) Gini
Dist. to Wittenberg	0.018	0.002	-0.003	0.000
	(0.020)	(0.013)	(0.003)	(0.000)
Observations	43	43	43	43
R^2	0.020	0.001	0.037	0.003

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

Reduced Form Estimates

Table 4.21 reports reduced form or "intention to treat" estimates of the effect of the instrument for Protestantism on wealth distribution and inequality. Intuitively, one can think of these estimates as the effect of "encouraging" a locality to adopt the Protestant Reformation on inequality. Significant reduced-form estimates make the causal interpretation of instrumental variable estimates more credible (Angrist and Pischke 2009: 213).

Table 4.21: Reduced Form Estimates

	(1) Top 10%	(2) Top 1%	(3) Mid. 40%	(4) Gini	(5) Bot. 20%	(6) D1
Dist. to Wittenberg$\times P_t$	-0.008	-0.003	0.001	-0.000*	0.006***	0.002***
	(0.008)	(0.005)	(0.006)	(0.000)	(0.001)	(0.001)
Locality FE	YES	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES	YES
Observations	368	368	368	368	368	368
Communities	43	43	43	43	43	43
R^2	0.191	0.073	0.286	0.268	0.185	0.166

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

The results are in line with the pattern of the second stage and the DD results in the main text. While there was no significant effect on top or middling wealth shares, inequality as measured with the wealth shares of the poor strata got larger the closer a locality was to Wittenberg after the Reformation began. This result reinforces the plausibility of the findings in the main text.

Effect of Church Ordinances (Flexible Difference-in-Differences Estimates)

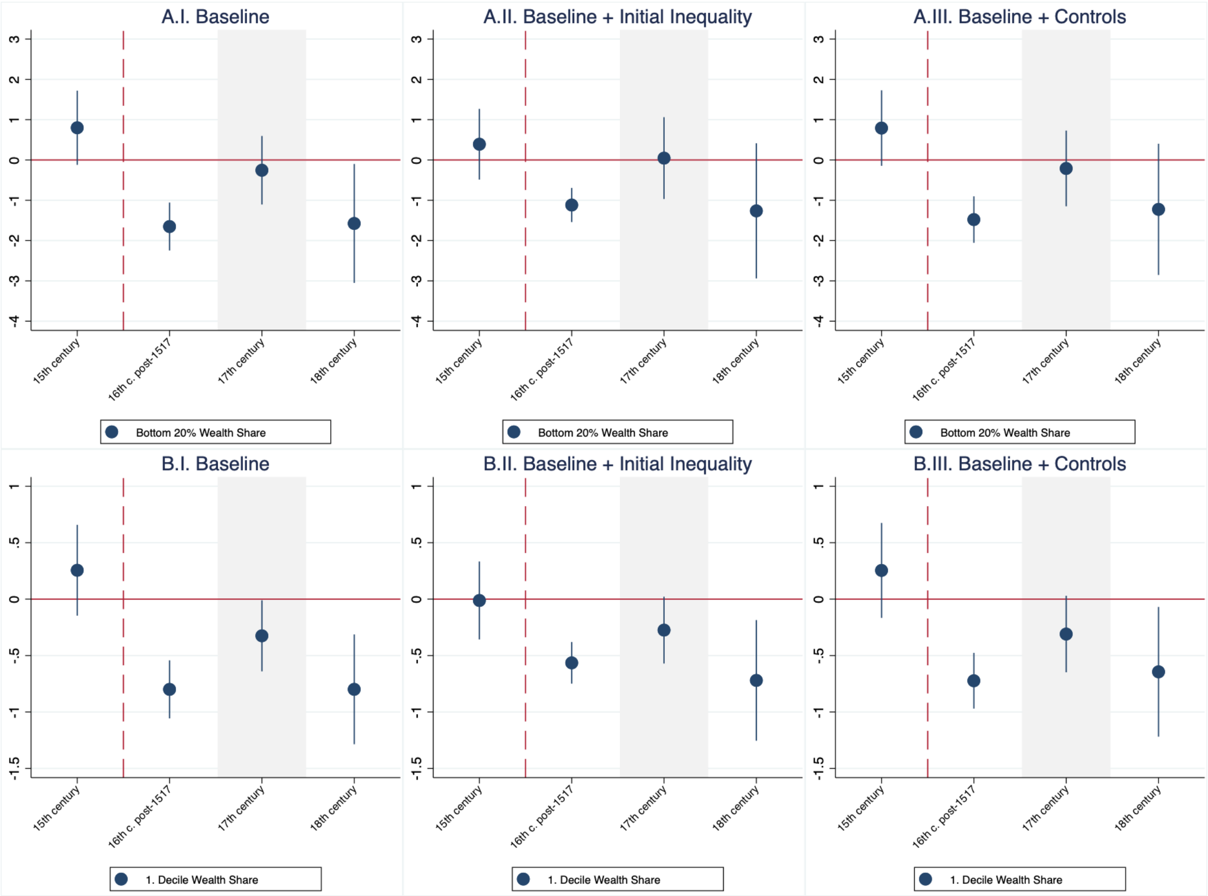
In Figure 4.11 I plot the estimation results of a flexible DD model, taking the wealth share of the bottom 20 percent and the first decile as dependent variables, and the introduction of a church ordinance in Protestant places as treatment. I estimate variants of the following specification, which is similar to the triple difference-in-difference specification in the main text:

$$\begin{aligned} I_{i,t} = \alpha_i + \pi_t + \sum_{t=1}^{4} \beta_t Prot_i \times Century_t \times Ordinance_i \\ + \theta Prot_i \times PostReform_t + \boldsymbol{\gamma' X_{i,t}} + \epsilon_{i,t} \end{aligned} \tag{4.6}$$

The main difference is the inclusion of an interaction term between the treatment status indicator and a set of four century dummies ($Century_t$) covering the pre- and post-treatment periods. The beta (β) coefficients are the main coefficients of interest. This setup allows me to evaluate, first, whether the relationship between the introduction of a church ordinance and the wealth share of the lower classes of society changed over time and whether short- and long-term effects were similar. As for the flexible estimates of the effect of the Reformation in the main text, I take the year 1500 as reference category and divide the remaining period of observation into four intervals corresponding to centuries: from 1400 until about 1475; from 1525 just after the Reformation began until 1600, from 1625 until 1700; and from 1725 until the end of the period of observation in 1800.

The results show that the introduction of church ordinances in Protestant places led to a reduction of the wealth share of the bottom fifth and the first decile of the population. The crisis-ridden seventeenth century interrupted that pattern, possibly because of the impact of the exceptionally destructive Thirty Years' War, as argued in the main text. The coefficient for the fifteenth century indicates insignificant larger values in eventually Protestant communities. However, in Frame 2 such initial wealth share differences are at least partially

Figure 4.11: Wealth Share of Bottom 20% and D1 in Communities with and without Church Ordinance (Flexible Difference-in-Differences Estimates)



Notes: Regression estimates of the wealth share of the bottom 20 percent of the population (Panel A) and of the first decile of the population (Panel B) in communities with church ordinance before and after the Reformation (vertical red line), with respect to communities without church ordinance (horizontal red line). The omitted reference year is 1500. The estimation method is OLS. All specifications have a full set of community fixed effects and time fixed effects. Standard errors clustered at locality level. Confidence intervals indicate significance at the 95-percent level. The vertical red line represents the beginning of the Reformation period, the grey box the Thirty Years' War.

controlled for, leaving still a sizeable and negative effect of church ordinances on the wealth shares of poor strata. Overall, the results are similar to the flexible DD estimates of the effect of Protestantism, and confirm the DD estimates of the effect of church Ordinances in the main text.

Chapter 5

Urban Political Structure and Inequality: Political Economy Lessons from Early Modern German Cities

5.1 Introduction

What was the impact of preindustrial political structure on the long-run development of economic inequality? It has often been argued that feudalism was a cause of high inequality in rural societies (Weber 1978, Piketty 2020). However, much less is known about the effect of the political structure of cities on inequality.

In this chapter, I investigate the effect of urban political structure on economic inequality in preindustrial times. Most cities in early modern Europe had an "oligarchic" or closed political structure (Pirenne 1958). Urban governments usually lacked "input legitimacy", that is, elections coming close to modern democratic standards. Yet according to a common historical narrative this did not matter, because governments were capable of achieving "output legitimacy": cities were run well and social peace preserved, because the political elites were highly civic-minded rulers. They were guided by strong norms to govern for the common good. Political elites set their personal interests aside and employed their valuable time and economic resources for the city (Weber 1958, 1978, Isenmann 1997, 2014). For economic inequality, this narrative implies that the political structure of preindustrial cities probably did not contribute to concentrating wealth in the hands of elites (Rogge 1996, Bátori 2007), or increasing inequality. But is this argument justified?

I address that question by constructing, first, a city-level dataset registering aggregate wealth inequality and participative local democracy in 35 early modern German cities. This dataset makes it possible to study the broad association between closed and open urban political structure and inequality. To understand better the mechanisms behind that relationship, I then construct a second, individual-level panel-dataset for the Southern-German city-state of Nördlingen,[1] between 1579 and 1700. I will make use of unique micro data — containing c.27,000 observations of linked individuals from property tax registers — to study how personal wealth and wealth distribution changed when individuals entered a political office in an oligarchic system. While the data are available for a single city-state only, I argue that its political structure was typical of urban Germany, and indeed of much of early modern Europe. Moreover, the granularity of the data generate results that could not be obtained through cross-city analyses, but are consistent with those from the city-level analysis.

To investigate the impact of becoming part of the political elite on personal wealth and inequality, I employ a difference-in-differences research design. I also report flexible difference-in-differences results, to check for pre-trends. Additionally, I exploit the Thirty Years' War as a shock to the potential for rent-seeking by political elites, and as an occasion to observe their behaviour in a period of severe socio-economic crisis.

In the cross-city analysis I find that, conditional on a rich set of covariates, cities without council elections displayed substantially greater wealth concentration. For example, the top 1 percent wealth share was about five percent higher, and the top 5 percent share about six percent higher in places without elections. I interpret these results in the following way: more oligarchic governments seem to have failed more in providing equality of wealth. This is, of course, only one dimension of governments' output. Yet it is an important one, as many aspects of welfare are likely to have been correlated with wealth.

My individual-level analysis for Nördlingen then suggests the mechanisms by which more oligarchic city governments may have failed to provide equality. I find, first, that political

[1] Within the Empire, there existed 87 Free and Imperial Cities in the sixteenth century (Schilling 1998). These cities enjoyed considerable self-government privileges, and their magistrates acted in the same way as territorial rulers did (Press 1991: 124-126). For simplicity, I refer to these cities also as "city states", like Stasavage (2011).

elites increased their personal wealth on average by 0.787 to 0.855 log wealth points after they entered office. This increase was not driven by pre-trends. Second, individuals with greater political power — mayors — increased their personal wealth even more, on average by 1.531 log wealth points. Third, this increase in the personal wealth of political elites contributed to higher wealth concentration and inequality in the city-state overall. City council members climbed up in the wealth distribution by 4.5 percentiles, and were 23.9 percent more likely to be part of the top five percent of the wealth distribution — that is, to be members of the economic elite.

Fourth, all these effects were particularly large in the period of the Thirty Years' War. This suggests that political elites exploited a shock to municipal finances and a period of socio-economic crisis as an opportunity to enrich themselves even further. Fifth, magistrates themselves were not the only group getting significantly richer, but were also joined by city clerks (for example, tax collectors or secretaries), by on average 0.81 to 1.31 log wealth points. These office-holders assisted the magistrates in the operations of the city government. This suggests that political elites, and their helpers, used their privileged position to seek rents, most likely through manipulating the regulatory and fiscal system from inside the city government. Sixth, merchants who were also city council members did not experience any differential increase in their personal wealth during the war relative to non-merchants. This suggests that the enrichment of city council members was not driven by a "merchant banking effect".

The chapter contributes, first, to the literature on the drivers of preindustrial inequality (van Zanden 1995, Milanovic 2016, Alfani and Di Tullio 2019). A number of studies have hypothesised that access to political power facilitated personal enrichment and mattered for explaining inequality growth in preindustrial times (Alfani and Ryckbosch 2016, Scheidel 2017, Piketty 2020, Alfani 2021a). Yet the empirical evidence on this topic has not been as systematic as one would desire. Most studies are conjectural, usually not going beyond case studies of individuals or families. To my best knowledge, there does not exist a single study that investigates the closed politics-inequality nexus systematically, while addressing questions of causality, most likely because it is very difficult to measure rent-seeking accurately, today as in the past. Moreover, it remains an open question which types of political systems were more or less conducive to inequality, and through what mechanisms. My study shows systematically that more closed, or oligarchic, urban political systems led to greater personal enrichment by holders of political office and contributed to higher inequality. I also show that closed systems were particularly vulnerable to personal enrichment during times of crisis, such as wars and epidemics, which were frequent phenomena at the time. Moreover, I provide evidence that points towards the manipulation of the regulatory and fiscal system by magistrates and city clerks as driving wealth accumulation and inequality (for arguments about the importance of the fiscal system in explaining preindustrial inequality, see Alfani 2015, Alfani and Di Tullio 2019, Alfani 2021a).

The chapter contributes, second, to a wide theoretical and empirical literature on the private returns from public office (Besley 2004, Caselli and Morelli 2004, Eggers and Hainmueller 2009, Fisman et al. 2014). My chapter is probably closest to recent work by Belloc et al. (2021), who show how political elites in medieval Florence accumulated personal wealth. My results suggest that the dynamics they study hold in a panel setting, were not driven by pre-trends, were present in other geographic, political and historical contexts as well, and contributed to higher inequality. My chapter is also related to Querubin and Snyder (2013), who document how politicians used the US Civil War as an occasion to enrich themselves. Preindustrial warfare and epidemics might have been beneficial for growth (Voigtländer and Voth 2013), but my results suggest that they could also be exploited by political elites for personal enrichment, making society more unequal. My chapter also goes beyond previous studies in documenting the returns to office for a closed political system without any elections.

The chapter speaks, third, to an unresolved debate in the urban history of Europe: did urban political elites act as self-interested rent-seekers, or were they civic-minded guardians of the common good (Weber 1958, Quarthal 1987, Boockmann 1998, Isenmann 2014)? This is by no means a "German debate", but an issue that concerns the urban history of Europe in general (see Pirenne 1958, Grubb 1986, Puga and Trefler 2014). It connects to the wider question about why many once prosperous cities declined across early modern Europe. My data suggest that oligarchic political elites enriched themselves when they could, by manipulating the regulatory and fiscal system from inside the government, for example, during times of crisis. It is likely that this manipulation did not just contribute to increasing inequality, but also inflicted deadweight losses on the city economy. These results are hard to square with the "civic-mindedness narrative" of urban political elites. But they are in line with a stream of literature that characterises the preindustrial German economy as burdened by inequality-promoting rent-seeking by various special-interest groups (Ogilvie 1997, 2019, 2021, Volckart 1998, 2002c).

The chapter proceeds as follows. In Section 5.2 I discus theories of urban political elites, explore what they imply for inequality, and provide historical background information. Section 5.3 introduces the two datasets I have constructed for the empirical analysis. Section 5.4 describes the econometric strategies and results for the city-level and individual-level analyses. Section 5.5 concludes.

5.2 Historical Framework and Background

5.2.1 Theories of Oligarchic Governments and Rent-Seeking, and Inequality

In modern societies it is commonly believed that unchecked or "oligarchic" governments use their power to feather their own nests: they engage in rent-seeking, thus enriching themselves. This political economy dynamic has been widely studied, theoretically and empirically (Diermeier et al. 2005, North et al. 2009, Querubin and Snyder 2013). It is likely that politicians who enrich themselves nowadays increase economic inequality in the societies they govern (Acemoglu 2008, Milanovic 2019). Figure 5.1 summarises this logic.

Closed political institutions $\rightarrow$ Rent-seeking by political elites $\rightarrow$ Political elites accumulate wealth $\rightarrow$ Inequality in governed society $\Uparrow$

Figure 5.1: Oligarchic Political Structure and Economic Inequality

Several scholars investigating inequality specifically in preindustrial societies have made similar arguments. The basic idea is that if political power is monopolised by a small elite because the governmental system is closed — for example by an oligarchic city government — then those elites will use their power in a way to enrich themselves. This personal enrichment by elites then contributes to higher inequality (Puga and Trefler 2014, Alfani and Ryckbosch 2016, Scheidel 2017, Piketty 2020, Alfani 2021a). For instance, Alfani (2021a: 26) writes that "in a preindustrial context, political power could be a crucial tool in building a fortune. [...] This might explain a significant part of the tendencies affecting the top rich [...] leading, in fact, to growing polarization". This hypothesis is entirely plausible. And yet there remain two open questions: One the one hand, we have to ask whether there is sufficient systematic empirical evidence for this hypothesis. Unfortunately, the answer is no. The available evidence is usually piecemeal, anecdotal or indirect. For instance, Scheidel (2017: 83-84) provides figures on the bequeathed fortunes of few individual government officials in Spanish America and France to document wealth accumulation. Puga and Trefler (2014: 796) use economic theory and employ marriage networks as a proxy for inequality, but do neither measure the actual wealth of political elites nor the distribution of wealth or income. This is not meant to criticise the cited scholars, as their approaches are the result of the fact that systematic data on personal enrichment of politicians and inequality is very hard to get, in preindustrial and industrial societies. The exceptional sources available for Nördlingen make it possible to obtain such micro-data.

On the other hand, it is still an open question what the precise mechanisms were that connected closed politics with personal enrichment of elites and inequality growth. A number of plausible hypotheses exist. The first possibility is that political office gave those in power profitable opportunities for their own businesses, for example because they used their power for anti-competitive practices. For instance, officeholder could try to exclude non-officeholders from particularly profitable activities, such as long-distance trade (Puga and Trefler 2014). A second possibility is patronage and clientelism: regulating the local economy and society meant that politicians had to weigh up potentially conflicting economic interests of different groups, such as craftsmen and merchants. There is abundant historical evidence that special interest groups, especially guilds, lobbied governments in preindustrial times to obtain privileges, often in return for some material benefit. The financial gain from this lobbying activity most likely enriched individual officeholders (see Ogilvie 1996a, 2019, 2021). Third, there could have been simple embezzlement and theft of public money. This type of enrichment was facilitated by the physical challenges of monitoring immense quantities of diverse coins in a context of relatively primitive administration and monitoring (Quarthal 1987). I use "rent-seeking" as an umbrella term for these three potential mechanism, that is, for the behaviour of people who hold an office, seeking personal benefit from it. Some forms of rent-seeking were considered legitimate at the time, such as better business opportunities or receiving "gifts" (*honoraria*) in return for privileges ((Ogilvie 1997: 372-373)). Other forms, such as stealing, were prohibited also at the time (van Klaveren 1957: 318). It is true that that contemporaries expected officholders to engage in tthese different forms of rent-seeking (Engels 2014: 60), but this does diminish the importance of rent-seeking for explaining the observed wealth accumulation by officials. Unfortunately, evidence for these mechanisms of personal enrichment is even harder to provide, again today as in the past (Boockmann 1998: 367). Below I will provide indirect econometric evidence and historical examples which suggest that certain mechanisms might have been more plausible than others.

While for inequality scholars the expected relationship of closed politics and inequality seems to be theoretically clear, urban history has another, more fundamental unanswered question: were urban political elites benevolent oligarchs? In preindustrial cities and towns local authorities were usually more important than central authorities in matters of commerce, work, and life in general (Schmoller 1896: 7-12, Minns et al. 2020). Most cities in Germany and Europe had an oligarchic political structure (Pirenne 1958), that is, without substantial formal checks on the rulers through popular participation in elections and possibilities for holding politicians accountable. For example, as late as 1800 more than 80 percent of German cities had no elections for the city council in which the population could participate, leaving co-optation by sitting members the principal way of becoming a magistrate (Friedrichs 2000:

13, Wahl 2019: 202).

Many historians argue that notwithstanding this lack of formal checks, oligarchic urban elites governed successfully, running the city well and implementing policies to preserve social harmony. The reason was that elites' actions were guided by their civic-mindedness, making these responsible rulers the guardians of the common good of the city. For example, urban historian Eberhard Isenmann states that "Taking care of the common good (public interest), the pursuit of the city's existential interests [...] were the genuine areas of action and central guiding principles of the city council, and legitimated its power" (my translation) (Isenmann 2014: 330). Magistrates' strong norms about what it meant to govern a city responsibly ensured "that personal interest would not prevail over the common good" (my translation) (Isenmann 2014: 331; see also Isenmann 1997: 213). These norms would have broken the link between closed political institutions and rent-seeking in Figure 5.1,[2] which implies that political elites' behaviour would probably not increase their personal riches and inequality. In fact, Max Weber has famously argued along the lines of the civic-mindedness narrative, stating that being wealthy was the condition for — not the result of — individuals participating in urban politics in preindustrial times. First, because sufficient personal wealth was required to enable them to spare valuable time from their own economic activities (*Abkömmlichkeit*), and, second, because at the time an important component of the prevailing norms was that political elites would cover certain obligations of the city through their own economic means (Weber 1958: 121-6, 1978: 290-291). This would imply that political elites would even tend to lose wealth because of holding an office (see Bátori 2007: 90),[3] potentially contributing to *equality* of wealth.

Not all historians subscribe to that romantic narrative (see Quarthal 1987, Boockmann 1998, Friedrichs 2000, and it is also in contradiction with a more pessimistic, economic interpretation of what political and economic elites in history potentially were: self-interested

[2]Similar conclusions have been drawn for a number of cities and towns in Germany and other areas of preindustrial Europe (see Grubb 1986, Rogge 1996, Di Tullio 2018).

[3]For example, Bátori (2007: 89-90) provides evidence for several of Nördlingen's city council members who lost substantial parts of their wealth while holding office. She suggests that the reason lies in long periods of travel for the city, which inflicted economic losses on their businesses, consequently reducing their wealth.

rent-seekers that enriched themselves at the expense of the rest of the population when they could. In that view, oligarchic or closed governments are a small distributional coalition. Political institutions are the result of a conflict over resources, in which some powerful individuals dominate and extract resources from the rest of the population. At the individual level such extractive behaviour would have probably led to personal enrichment of elites and inequality — the exact opposite of what the civic-mindedness narrative postulates — and for the city it would have probably implied institutional sclerosis and deadweight losses on the local economy (Olson 1982, Ogilvie 2007, 2019, 2021, Acemoglu 2008, Acemoglu and Robinson 2012). My data make it possible to shed light on whether the more romantic, historical or more pessimistic, economic interpretation of political elites is more plausible. I do so quantitatively, by studying their personal wealth and the inequality of wealth in the cities in which they lived.

5.2.2 The Free Imperial City Nördlingen and its Oligarchic Political System

Some historical background information is essential for appreciating the analysis below. In early modern Germany roughly 4,000 cities existed. The majority were territorial cities, that is, under the control of a territorial overlord. Most territorial cities had nevertheless wide-ranging self-governance rights — a characteristic feature of the politically highly fragmented Holy Roman Empire — for example in fiscal and military affairs (Schilling and Ehrenpreis 2015). Nördlingen was one of the 87 Free Imperial Cities that existed in the sixteenth century.[4] These were usually the largest and economically most vibrant cities in Germany, such as Cologne, Frankfurt a.M., or Augsburg, and they had even wider self-governance rights. They had a special constitutional status in the Empire, because the Emperor was their only formal overlord, who, however, intervened only very rarely in the cities' administration (Schilling 1994: 108). *De facto* Free Imperial Cities were independent city-states, not dissimilar from Italian city states. Their magistrates acted in the same way as territorial rulers did (Press 1991: 124-126). Yet many of these Free Imperial Cities were in economic and

[4] 11 out of 35 places in the city-level dataset are Free Imperial Cities, not counting Konstanz, which was a territorial city for almost the whole period of study.

political decline over the early modern period: at the end of the eighteenth century only 37 had been able to maintain their independent status (Schilling 1994: 108). In the early modern period, when the medieval concept of "municipal law families" (*Stadtrechtsfamilien*) had lost its importance, all territorial and city states in the Empire were subject to the "Imperial Police Ordinance" (*Reichspolizeyordnung*). This legislation, not to be confounded with the modern notion of police, provided a common legal framework for all Imperial Estates for how to regulate, for instance, commerce, crime punishment or other spheres of public life. But every polity had large discretion over how to fill that framework with local legislation (Isenmann 2014: 192-195, Härter 2010). Of course, there were other factors that had an influence on how the political institutions of a city were set up, such as its economic orientation. For instance, while merchants were very influential in the city politics of the trade-oriented towns, such as the Hanseatic towns, craftsmen were often part of the government in manufacturing-oriented towns (see Ogilvie 2011: 11, Isenmann 2014: 807).

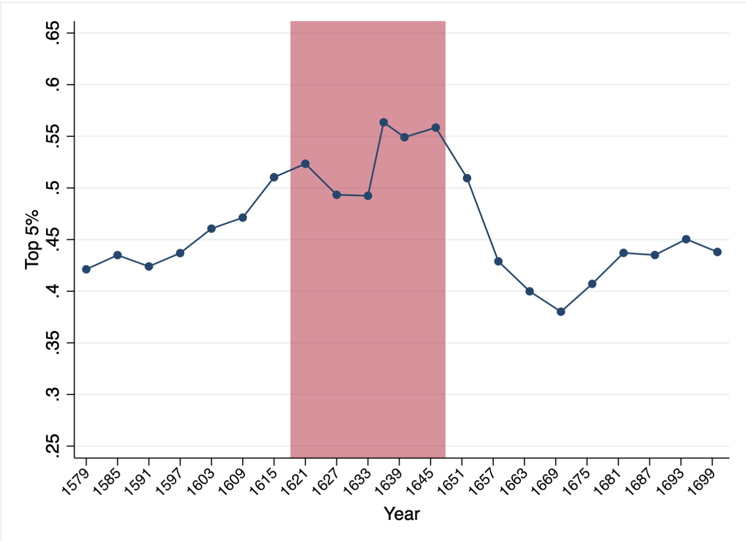
Nördlingen, like many other Free Imperial Cities, was well known for its manufacturing sector, especially woolen textile production, and long-distance trade. Two great medieval trade routes intersected in Nördlingen, a northeast-southwestern one that connected Bohemia and the area around Lake Constance, and one that went from the Low Countries to Frankfurt a.M. and then southward to Switzerland and Italy. Moreover, the city hosted an annual fair. About half of the citizenry participated in the production for the export oriented sectors of the economy. However, already from the fifteenth century onwards trade routes and fair started to decline in importance (Friedrichs 1979: 6-7, 79). Nevertheless, Nördlingen continued to grow, at least in terms of population size, during the first part of the early modern period. It had approximately 6,300 inhabitants around 1500, and 8,400 inhabitants around 1600. But it experienced a decline during the crisis-ridden seventeenth century, to around 5,700 inhabitants in 1700, a decline from which the city could not recover until 1800, when it had roughly 6,000 inhabitants.[5]

How did wealth inequality develop in Nördlingen over the period under study? Figure

[5] Population figures from 1500 until 1700 are based on my own calculations, and for 1800 on Bairoch et al. (1988).

5.2 shows the Top 5 percent wealth share, a frequently used measure in the preindustrial context that reflects inequality from the top of the distribution (see Alfani 2023). Wealth concentration at the top increased almost constantly, from about 42 percent at the beginning of observation until about 56 percent in 1646. Wealth concentration only declined for about two decades right after the end of the Thirty Years' War, a period that was associated with intense inequality decline almost everywhere in Germany (Alfani et al. 2022: 104). This decline was caused, among other factors, by a decline in trade, a pillar of Nördlingen's economy (Wilson 2009: 806). One might even wonder why wealth concentration did not decline already during the war. The answer comes from the research presented in Chapter 3, showing that wars have an inequality-reducing destructive effect, but also an inequality-promoting extractive effect, and that historically the latter often outweighed the former. Nördlingen experienced massive resource extraction for the duration of the war (see below). It is therefore entirely plausible that inequality increased during the war while resource extraction was in full swing, but declined afterwards when the financial state of emergency of the war was over.

Figure 5.2: Wealth Share of the Top 5% in Nördlingen (1579-1700)



Notes: The red box represents the period of the Thirty Years' War.
Sources: See the main text.

What was the political structure of Nördlingen? The Holy Roman Empire was, as mentioned, characterised by a high degree of political fragmentation. Similarities existed, but there was no common governance model. Every of the several hundred independent territories, and indeed, every city, had its own customs for how to chose public officials, leading to a staggering variety of constitutional forms (Friedrichs 2000: 13). Nevertheless, Nördlingen's political system was in some central aspects similar to the typical oligarchic political structure of cities in preindustrial Germany (see Scribner 1996), and wide swathes of Europe (Pirenne 1958). In 1552, the Holy Roman Emperor Charles V imposed on a number of Imperial cities including Nördlingen a new, more closed political constitution. Similar developments, towards more closed political systems in the early modern period, could be observed in other parts of Germany but also, for example, in Italy and the Dutch Republic (Andermann 2001: 379, Alfani 2023: ch. 5, De Vries and Van der Woude 1997: 586-596). The Emperor had just won the Schmalkaldic War (1546-47) and was at the peak of his power. Many Imperial cities had sided with the Schmalkaldic League to fight against him. Charles was convinced that part of the reason why the cities opposed him were their governments, which were characterised by forms of popular participation, such as elections for the city council. This had induced the cities to support the Protestant Schmalkaldic League. Charles wanted to set up political structures that were more stable and predictable for him. The city councils that were set up due to his intervention were called "*Hasenräte*", named after the Emperor's delegate Heinrich Has who oversaw the implementation of the new political constitution in the cities (Naujoks 1985).

Under this new system, the council of Nördlingen consisted of 15 magistrates who were appointed by co-optation, that is, new members were selected by the sitting council members. Note that the introduction of the new system also broke the formal political power of the city's 12 guilds that had constituted the city government so far. The craft organisations that succeeded the guilds remained nevertheless among the principal special interest groups in Nördlingen (Friedrichs 1979: 18). Co-optation was the common way of choosing a new city administration in early modern Free Imperial Cities, but also in many territorial cities (Press 1991: 125, Friedrichs 2000: 14, Wahl 2019: 202). Once selected a new magistrate was appointed for lifetime in Nördlingen, and a total of 108 appointments were made between

1580 and 1720. No protocols or other evidence survive which record the deliberations of the council members on how they selected a new member. Yet we know that the chosen individuals usually had high socio-economic status (see also Table 5.1), a typical feature of magistrates in preindustrial European cities (Friedrichs 2000: 18). Almost all of them came from the top 20 percent of the wealth distribution. Their occupations, however, were heterogeneous: merchants, lawyers, craftsmen, officials and many other professions. Magistrates did not receive a salary, but only a minimal annual expense allowance, of 2.5 to 8 florins (Voges 1988: 27, Friedrichs 1979: 171-179). The fact that most magistrates were already rich before entering office creates a potential endogeneity problem for the analysis. However, my difference-in-differences identification strategy will take this concern into account (see discussion below).

Magistrates had a high degree of discretion over most aspects of urban life,[6] especially economically relevant ones. The imperial constitution left large room for Nördlingen's urban rulers to regulate, for example, trade and commerce, including limits to competition. The office-holders also approved craft bylaws, set fiscal policy, and borrowed money on behalf of the city. Shaping the regulatory and fiscal system obviously involved weighing up different economic interests (Voges 1988: 26-28, Friedrichs 1979: 144-148, 199-206). This system endured in Nördlingen and in many other German cities until the end of the Holy Roman Empire at the beginning of the nineteenth century (Bátori 2007: 88).

5.2.3 A Shock to the Potential for Rent-Seeking: the Thirty Years' War

The Thirty Years' War provides an occasion to observe the behaviour of political elites in a period of a severe socio-economic crisis. Moreover, this episode is also the closest we can get to an experimental setting that increased in a plausibly exogenous way political elites' potential to engage in rent-seeking.

[6]The council even specified the number of guests permitted at a wedding and the number of courses permitted at the wedding feast. Note that that these powers were, however, basically restricted to the area within the city walls. The city could claim territorial rulership over only two villages, because the surrounding area was mostly controlled by the house of Oettingen (Friedrichs 1979: 19-21, 201)

It is a well-known fact that Nördlingen was subject to regular "visits" by soldiers of different camps during the war, due to its geographical location at the crossroads of two principal marching routes. Beginning in 1619, these soldiers regularly threatened to plunder or burn down the city if it did not pay the sums they demanded. This led to an extraction of monetary resources from Nördingen's population of about 2.3 million florins during the war, not counting in the numerous but not quantifiable non-monetary payments (Friedrichs 1979: 28, Voges 1988: 257-258, Schilling and Ehrenpreis 2015: 16). This was an immense sum for a city with 1,000-2,000 taxpayers, and considering that the total median wealth of a household was only about 190 florins just before the war in 1615, and about 90 florins around its end in 1646.[7] How the city would come up with the demanded sums was left to its city council. The council had to decide through which channels to obtain the sums, on whom to put the burden, also for non-cash benefits such as quartering soldiers, and how to punish tax evaders (Friedrichs 1979: 118, 152-158, Voges 1988: 258).[8] It also had to administer money collection and the storage of cash money, and had to hand it over to soldiers. In short, the council was a kind of bottleneck through which all money, goods and services passed which the population of Nördlingen rendered up to various groups of soldiers.

But it is also important to keep in mind the multifaceted crisis Nördlingen faced during the war. Apart from the constant passing of soldiers and subjugation by Swedish, Imperial and French troops, the city saw two major battles taking place in its vicinity, in 1634 (including a siege) and in 1645. Moreover, the town was struck by plague in 1634, possibly brought in by soldiers (Voges 1988: 241, Zipperer 1979: 123, 130). Consequently, the number of households declined during the war by about 49 percent, and real wealth declined *per capita* declined by about 34 percent (Friedrichs 1979: 42, 113).

Two scenarios are possible during the Thirty Years' War. On the one hand, in line with a standard political economy logic, it might have been an opportunity for political elites to engage in rent-seeking, thus increasing their personal wealth and the town's overall inequality.

[7]The tax rate applied to peoples' property was between 0.5 and one percent during the sixteenth and seventeenth centuries (Friedrichs 1979: 158).

[8]The council of Nördlingen reportedly used the threat of the death penalty to increase their peoples' willingness to pay taxes during the Thirty Years' War (Friedrichs 1979: 217)

This because the war increased the quantity of resources the council had to administer, and because the socio-economic chaos of the war might have been an ideal veil for covering rent-seeking activities in a context of few checks on government activity (see for an example from modern times Querubin and Snyder 2013). On the other hand, it might have also been an episode in which civic-minded magistrates acted responsibly to protect the common good, possibly even spending their own resources for the needs of the city (see Weber 1978, Bátori 2007: 90, Isenmann 2014: 330-331). In that scenario one would expect to see no increase either in the personal wealth of political elites nor in wealth concentration.

5.3 Data

I construct two different datasets for the empirical analysis, the first at the level of German cities, the second at the level of individual taxpayers in a specific city.

For constructing the first dataset I take panel-data on top wealth shares (top 10, top 5 or top 1 percent) in 35 early modern German cities introduced in Chapter 2. As mentioned, it contains well-known cities like Augsburg, Frankfurt a.M., Rostock, Munich or Nördlingen, but also several others. The panel is unbalanced, as not all cities are observed over the whole period of study (1500-1800).

Additionally, I have collected information about a defining characteristic of the closedness of cities' political systems from the *Deutsches Städtebuch*:[9] whether entry into the city council was governed by elections in which the population participated. Based on this information I have constructed a dummy variable registering 1 if there were participative elections in that city in a given year, and 0 otherwise (see Wahl 2019 for an analogous approach). Given available information, it is unfortunately only possible to differentiate whether places did or did not have elections, but not other characteristics of the election, such as how fair they were or which groups of the population could vote. The introduction of elections was not always a one-off change. In some places elections were introduced, abolished at some point

[9]For more information about the *Deutsches Städtebuch* and the construction of variables, see the Appendix.

and later re-introduced. The alternative to elections was usually co-optation (Schlotterose 1953), as in Nördlingen. Under that system, the council recruited itself through sitting members selecting new members. This is interpreted as a higher degree of closedness of the political structure (Wahl 2019: 197).

The second panel-dataset is based on the property tax registers of Nördlingen, between 1579 and 1700.[10] These registers cover all of the city's citizens, giving information about name and surname of a taxpayer, property tax payment, gender — on average 16.64 percent of the taxpayers were women between 1579 and 1700 (see Friedrichs 1979: 321) — and occupation. Importantly, individuals were ordered alphabetically by name and surname in the registers, and a new page was dedicated to every combination of initials, that is, A.A., A.B., A.C. and so on. For the time, this was an extraordinarily systematic way of creating tax registers, and I am not aware of any other city in preindustrial Germany which kept such orderly kept tax registers. The disadvantage of this structure is that the registers are very voluminous, covering several hundreds of pages per year, because only a handful of individuals usually had the same initials. The main advantage is that they make it possible to link individuals over time easily. The transcription and linking of individual taxpayers' records was done over several years by the urban historian Christopher Friedrichs in the 1970s (see Friedrichs (1979) for more information on the original project).

I have hand-digitised Friedrichs' c.27,000 paper-based taxpayer-year records, and double checked several hundred of them with the original sources. They cover c.6,000 individuals, in steps of three to six years, for a total of 22 periods. However, I have only kept those observations for which the first appearance in the tax registers occurred during my period of study, to be able to control for age. This somewhat reduces the number of observations. Considering the time period under study, the data are exceptionally granular. I have then

[10]Note that the population of Nördlingen had to pay not just property taxes, but also several consumption taxes, such as the ones on beer and wine, and also tolls. Additionally, in periods of warfare special war taxes (*Anlagen*) were levied. The regular property tax only had a relatively small share in the receipts of the city treasury, of about 19.7 percent in 1700, while the share of beer and wine taxes was almost twice as large. The exact rates for all the different taxes are not known. But we know, for example, that in 1634 the property tax was levied 11.5 times, and in 1643 the *Anlagen* was levied 33 times, that is, each citizen had to pay 11.5 or 33 times the amount assigned to them (Friedrichs 1979: 157-162).

combined these raw tax-payment data with information about the applied tax rate, to calculate the total wealth of each taxpayer-year record, and built an unbalanced panel of personal wealth from 1579 until 1700. Note that because each year of the panel includes all taxpayers in a given year, I could also obtain the complete wealth distributions of taxpayers for every year. This is crucial because it makes it possible to analyse not just the development of personal wealth, but also wealth concentration and inequality, as I can observe individuals moving up or down in the wealth distribution between years. I have then added information on which individuals were magistrates, that is, had a seat on the city council, who was mayor (*Bürgermeister*), and for which years these offices were held by that taxpayer. I collected this information from the secondary literature, based on the original lists of city council members in the city archive of Nördlingen. The Appendix provides more information about how the dataset and its main variables were constructed.

A first question about these property tax data is which categories of assets were taxed. The general rule in Nördlingen was that all mobile and immobile property inside and outside the city was taxed, but there exist no lists of taxable items for the period of study. We know that real estate was usually assessed at its most recent purchase price, but it is not entirely clear whether different assessment criteria were applied to other asset categories. To ensure tax compliance, the administration of Nördlingen put in place severe legal and social controls, which can be assumed to have been quite effective in a small community of about 1,000 to 2,000 taxpayers. For example, individuals had to swear an oath on the correctness of their tax payment, transactions of real estate were recorded and witnessed by the city council, and fines for tax evaders were heavy, sometimes reaching a multiple of one hundred times and more of the evaded amount (Friedrichs 1979: 98-101).

What were the characteristics of those individuals that became city council members in Nördlingen, compared to all taxpayers? As shown in Table 5.1, there were systematic differences between them and the rest of the population (estimated with bivariate regressions). The right Column displays the mean of the variables in for all taxpayers. The left Column shows by how much taxpayers that became magistrates in the next period of observation

Table 5.1: Taxpayer Characteristics Compared to Magistrates before Council Membership

	(1) β Council	(2) SE	(3) Mean
Wealth (Log) pre Council	2.56	(0.14)	7.52
Wealth Percentile pre Council	43.46	(1.88)	46.48
Top 5 Percent pre Council	0.33	(0.01)	0.05
Nr. of Tax Payments pre Council	-0.93	(0.14)	3.70
City Clerk pre Council	0.16	(0.01)	0.02
Merchant pre Council	0.12	(0.01)	0.01
Writing Occupation pre Council	0.01	(0.00)	0.00
Without Occupation (Wealth > 1 fl.) pre Council	0.05	(0.01)	0.02
Wool Weaver pre Council	-0.15	(0.02)	0.18

Notes: Column 1 shows the estimates on an indicator for being a council member before taking office in bivariate regressions. All coefficients presented are statistically significant at the 1% level. Columns 2 displays standard errors in parentheses. Columns 3 provides the mean of the dependent variable in the whole population.

differed from the whole population, before entering the council. The central Column shows the standard errors. These findings confirm that individuals who would subsequently become council members were richer and ranked higher in the local wealth distribution, than those who would not become council members. About a third was even part of the top five percent of the wealth distribution, which may be interpreted as the economic elite. They had also made fewer tax payments, which is indicative of them being younger than the average taxpayer at the time they entered the council.

In terms of occupation, city council members were more often city clerks, merchants, or had a writing occupation, such as being a notary. They were also more often without occupation while being not poor, that is, had more than one florin of total wealth. This probably reflects the fact that wealthy individuals could live from the rents they received, for example from real estate property. It was rare but not impossible to find a wool weaver on the city council. Wool weaving was usually associated with low socio-economic class at the time. Yet it was the most frequent occupation in Nördlingen, which is unsurprising given that the city was well known for its cloth trade.

5.4 Empirical Analysis

5.4.1 City-Level Evidence: Wealth Inequality and Elections

To obtain a first impression of the impact of oligarchic urban political institutions on inequality, I first analyse the city-level dataset to investigate the relationship between the presence of participative elections into the city council and top-level wealth concentration (that is, the percentage of taxable wealth held by the top 1, 5 and 10 percent of taxpayers). Whether the presence of elections had an impact on inequality probably depended on many unmeasured factors, such as who exactly was eligible to vote, and how large the group of eligible voters was in relation to the total population. Yet one would expect that the presence of elections *ceteris paribus* meant more checks on political elites, that is, a less closed system. If the "civic-mindedness narrative" were true, then one would expect to find no substantial inequality differences between politically more closed and more open cities.

I estimate the following econometric specification:

$$I_{i,t} = \alpha_i + \pi_t + \beta Election_{i,t} + \boldsymbol{\gamma' X_{i,t}} + \epsilon_{i,t} \tag{5.1}$$

$I_{i,t}$ is wealth inequality of locality i in year t (t = 1500, 1525,... until 1800), measured as the wealth shares of the top 1, 5 and 10 percent of the population. $Election_{i,t}$ is the measure of elections that takes the value one if locality i held elections for membership on the city council, and zero otherwise. The modelling approach accounts for unobserved factors that might have had an impact on the dependent and the independent variable of interest. α_i are a full set of locality fixed effects, which account for characteristics that are time-invariant and locality-specific, such as geographical location. π_t are time fixed effects (years), which account for shocks that might have had an impact on inequality in all localities, such as macroeconomic trends. Hence, the estimated correlations are identified from time variation within communities, and the coefficients will pick up the effect of introducing elections. In this set up the treatment can switch on and off. $\boldsymbol{X_{i,t}}$ is a vector of locality-level controls.

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.

To mitigate further the possibility for omitted variable bias, I account for several observable economic, demographic and institutional characteristics. These factors have been regarded as alternative explanations for inequality change, and could also be related to a city's political structure. I include the log-population size of a city, the occurrence of local epidemics, whether a city introduced the Protestant Reformation, the occurrence of warfare nearby, and the log distance of a city to its nearest university (Stasavage 2011, Milanovic 2016, van Zanden 1995, Deaton 2015, Alfani 2015, Ekelund et al. 2002, Alfani et al. 2022, Dittmar 2019). The construction of control variables is explained in more detail in the Appendix.

Table 5.2 reports the results. They show a clear pattern. In cities that had participative elections to the council, the concentration of wealth at the top of the population and thus inequality was significantly lower, regardless of which wealth percentile is employed as the dependent variable. The quantities are sizeable, especially for the the top 5 and 1 percent, which is unsurprising given that council members were often part of this very rich group. For example, the coefficient in Column 5 indicates that the top 1 percent held 5.2 percentage points less of the total wealth in places with elections, which corresponds to about 36.2 percent of the mean wealth held by this section of the population.

Overall, these results are in line with the argument of Alfani and Ryckbosch (2016), who have conjectured that more open political institutions were likely to result in lower wealth inequality in preindustrial European states. Needless to say, one should be cautious in interpreting these conditional, city-level correlations as causal. Local political structure was highly endogenous, and it might still be that fixed effects and controls do not adequately account for all omitted variables. Yet the picture points into a clear direction: closed political institutions are associated with higher wealth concentration and greater inequality. In what follows I study the micro-level mechanisms behind this relationship. I identify the effect of closed political institutions on individual wealth accumulation and inequality, in a more

Table 5.2: Wealth Inequality and Elections in Early Modern German Cities

	(1) Top 10%	(2) Top 10%	(3) Top 5%	(4) Top 5%	(5) Top 1%	(6) Top 1%
Council elections	-4.429***	-4.457**	-5.908***	-6.097***	-5.198**	-5.468**
	(1.482)	(1.704)	(2.001)	(2.181)	(2.238)	(2.228)
Controls	NO	YES	NO	YES	NO	YES
Locality FE	YES	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES	YES
Observations	243	243	243	243	243	243
Cities	35	35	35	35	35	35
R^2	0.209	0.215	0.139	0.142	0.100	0.108
Mean of dependent variable	51.95	51.95	36.91	36.91	14.38	14.38

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

robust way using highly granular individual-level data from Nördlingen.

5.4.2 Individual-Level Evidence: Political Elites' Wealth and Inequality

In order to study the effect of holding a political office in a closed political system on wealth concentration, I now turn to the individual-level wealth data from Nördlingen. I estimate variants of the following specification:

$$W_{i,t} = \alpha_i + \pi_t + \beta(Council_i \times Post_{i,t}) + \boldsymbol{\gamma' X_{i,t}} + \epsilon_{i,t} \quad (5.2)$$

$W_{i,t}$ is an indicator of an individual's personal wealth (in logarithm),[11] his percentile in the wealth distribution, or whether he was part of the top 5 percent of the wealth distribution, measured in intervals of three to six years between 1585 and 1700. $Council_i$ is a dummy variable that takes the value one if an individual is a member of the city council, and zero otherwise. $Post_{i,t}$ is another dummy variable that takes the value one in the years after an individual has joined the city council, and zero otherwise. $X_{i,t}$ is a vector of taxpayer controls, including dummies for the 60 occupational categories Friedrichs (1979) created

[11] Those individuals that had zero wealth received the wealth value 0.1 before log-transforming the wealth variable.

based on information in the tax registers (see the Appendix for a complete list). I also control for gender, and proxy for age and and age-squared by including the years (and years-squared) since a taxpayer was first listed in the registers. For that reason I limit the analysis to individuals who enter the registers in 1585 or later. α_i and π_t are taxpayer and year fixed effects. The standard errors are again robust, clustered at the taxpayer level in order to account for the possibility of serial correlation in the error term.

Table 5.3 reports the effect of city council membership on taxpayer wealth, and how having a political office contributed to wealth concentration. The coefficients represent average differences (ATT) between council members and the rest of the population. Columns 1 and 2 show that the effect on personal wealth was positive, highly significant and very large. If we read the change in log points as a lower bound estimate of the percentage change, then council members increased their personal wealth by 78.7 to 85.5 percent after they entered office. Column 3 indicates that becoming member of the council enabled an individual to climb up about 4.5 percentiles in the wealth distribution. Similarly, Column 4 suggests an increase of about 24 percent in the likelihood of being in the top 5 percent of the wealth distribution. In other words, becoming a council member did not just enrich those specific individuals, but also contributed to a greater economic polarisation of society, that is, greater inequality.

In Columns 5 to 7 I disentangle this average and look at whether those city council members who also served a year as mayors experienced a differential change in their wealth when they held office. If mayors were really civic-minded responsible rulers, we would not expect to find substantial enrichment. If, instead, greater political power was associated with greater rent-seeking, personal enrichment and inequality (see Alfani 2021a: 25-26), we would expect to see significant differences. The coefficients suggest that those individuals with more political power indeed enriched themselves even more. Again, if we read the change in log points as a lower bound estimate of the percentage change, then mayors (who were always also council members) increased their personal wealth on average by 153.1 percent compared to non-council members. Mayors also ranked higher in the wealth distribution and were even more likely to be part of the top five percent, holding ordinary council membership constant.

Table 5.3: Political Office and Wealth (Difference-in-Differences Estimates)

	(1) ln-Wealth	(2) ln-Wealth	(3) Percentile	(4) Top 5%	(5) ln-Wealth	(6) Percentile	(7) Top 5%
Council member × Post	0.855***	0.787***	4.455***	0.239***	0.753***	4.172***	0.232***
	(0.116)	(0.153)	(1.557)	(0.048)	(0.152)	(1.568)	(0.048)
Council member × Post × Mayor					0.778***	6.527**	0.167*
					(0.287)	(2.830)	(0.085)
Controls	NO	YES	YES	YES	YES	YES	YES
Age & age-sq.	YES	YES	YES	YES	YES	YES	YES
Locality FE	YES	YES	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES	YES	YES
Observations	21,579	21,579	21,579	21,579	21,579	21,579	21,579
Taxpayers	4,490	4,490	4,490	4,490	4,490	4,490	4,490
Mean of dependent variable	7.523	7.523	46.48	0.0484	7.523	46.48	0.0484
R^2	0.089	0.105	0.071	0.041	0.106	0.071	0.042

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

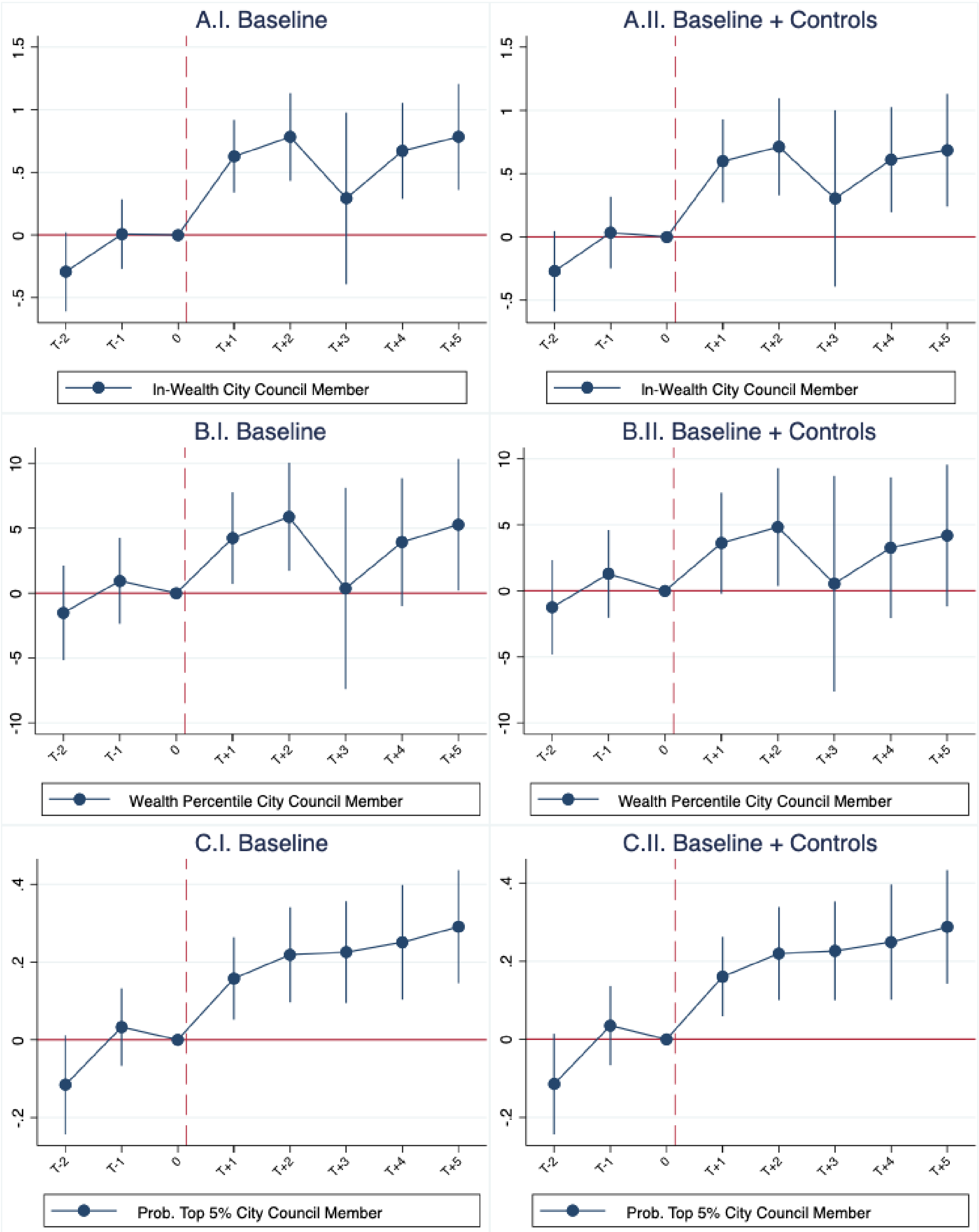
The results open up two further questions. First, were there differential pre-trends before a taxpayer became a city council member. In fact, some degree of a pre-trend would be historically unsurprising, given that magistrates often selected their peers based on whether they could potentially lend money to the city state (Stasavage 2011). Second, how did the effect of holding a political office on personal wealth and wealth concentration evolve over time? To address these issues more formally I estimate the following flexible difference-in-difference model:

$$W_{i,t} = \alpha_i + \pi_t + \sum_{t=-2}^{7} \beta_t (Council_i \times PeriodsToCouncil_{i,t}) + \boldsymbol{\gamma' X_{i,t}} + \epsilon_{i,t} \tag{5.3}$$

The main difference compared to the previous specification is the inclusion of an interaction term between the treatment status indicator ($Council_i$) and a set of seven period dummies ($PeriodsToCouncil_{i,t}$) covering the individual pre- and post-treatment periods. The beta (β) coefficients are the main coefficients of interest.

In Figure 5.3 I plot the estimates of this flexible specification, taking ln-wealth (Panel A), the wealth percentile (Panel B) and the probability of being part of the top 5 percent (Panel

Figure 5.3: Political Office and Wealth (Flexible Difference-in-Differences Estimates)



Notes: Regression estimates of ln-wealth before and after becoming city council member (vertical red line), with respect to all other taxpayers (horizontal red line). The omitted reference year is the period just before becoming council member. The estimation method is OLS. All regressions include a full set of taxpayer and time fixed effects, and controls for age and age-squared of the taxpayer. Standard errors clustered at the household level in parentheses. Confidence intervals indicate significance at the 95-percent level.

C) as outcomes. Across all three panels the picture is similar, regardless of whether controls are included. While there are signs of an insignificant increase in t-2, the point estimates in t-1 are all almost zero and not significant, which indicates parallel trends. However, when council members' terms begins, a significant and large increase in personal wealth and in the individual's position in the wealth distribution is observable.

One might wonder whether there was heterogeneity in the effect depending on when the treatment began during the period of observation (1585-1700). This is also important in light of recent critiques of difference-in-differences designs with staggered treatment (see De Chaisemartin and D'Haultfœuille 2020, Goodman-Bacon 2021). It has recently been proposed to use the two-way-fixed-effects estimator in a more flexible way to explore the heterogeneity in treatment timing (Wooldridge 2021: 49). For that reason, I interact in Table 5.4 the treatment indicator with dummies for three subperiods of the analysis. They indicate the period of observations before (1585-1615), during (1621-46) and after (1651-1700) the Thirty Years' War, corresponding to "early", "middle" and "late" treatment timing. In this way I account to some extent for heterogeneity in treatment timing, which is a major concern of the recent critiques of difference-in-differences designs (see Wooldridge 2021).

Table 5.4: Heterogeneity: Political Office and Wealth (Difference-in-Differences Estimates)

	(1) ln-Wealth	(2) ln-Wealth	(3) Percentile	(4) Percentile	(5) Top 5%	(6) Top 5%	(7) ln-Wealth	(8) ln-Wealth
Council member × Post × Subperiod 1585-1615	0.555**	0.386	3.813*	1.496	0.134*	0.132*		
	(0.258)	(0.321)	(2.189)	(2.464)	(0.081)	(0.079)		
Council member × Post × Subperiod 1621-1646	1.339***	1.297***	6.904***	6.083***	0.226***	0.229***		
	(0.140)	(0.159)	(2.119)	(2.214)	(0.068)	(0.065)		
Council member × Post × Subperiod 1651-1700	0.562***	0.477***	5.208***	3.843**	0.267***	0.271***		
	(0.130)	(0.166)	(1.466)	(1.714)	(0.062)	(0.060)		
Council member × Post × Low initial wealth							1.964***	1.863***
							(0.499)	(0.520)
Council member × Post × Med.-low initial wealth							0.967***	0.881***
							(0.202)	(0.258)
Council member × Post × Med.-high initial wealth							1.009***	0.906***
							(0.182)	(0.226)
Council member × Post × High initial wealth							0.385**	0.382**
							(0.179)	(0.193)
Controls	NO	YES	NO	YES	NO	YES	NO	YES
Age & age-sq.	YES	YES	YES	YES	YES	YES	YES	YES
Locality FE	YES	YES	YES	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES	YES	YES	YES
Observations	21,579	21,579	21,579	21,579	21,579	21,579	21,579	21,579
Taxpayers	4,490	4,490	4,490	4,490	4,490	4,490	4,490	4,490
Mean of dependent variable	7.523	7.523	46.48	46.48	0.0484	0.0484	7.523	7.523
R^2	0.090	0.106	0.052	0.071	0.034	0.041	0.089	0.106

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

It is reassuring that the coefficients in Columns 1 to 6 in Table 5.4 all point in the same direction as the average treatment effects in Table 5.3, most of them at high levels of statistical significance. The results also hold regardless of whether time-variant covariates are included. The coefficients for the time of the Thirty Years' War are particularly large, which will be analysed in more detail in the next section.

In Columns 7 and 8 I investigate another form of heterogeneity: whether the effect of becoming a magistrate on personal wealth depended on an individual's initial wealth. For that purpose I differentiate whether an individual had a low (until 1000 fl.), medium-low (1,001-10,000 fl.), medium-high (10,001-25,000 fl.) or high (more than 25,000 fl.) wealth level when he or she appeared for the first time in the tax registers.[12] The results indicate that the financial benefit of holding an office was beneficial for individuals of all wealth levels, but the benefit declined in relative terms the richer an individual was. This is plausible, and is also in line with the argument that rent-seeking was the channel through which enrichment happened. Doubling one's riches via rent-seeking (as opposed to investments) is intuitively harder the larger one's initial wealth level is.

So far, the results of this section suggest that holding a political office led to higher concentration of wealth in the hands of the oligarchic political elite, thus increasing inequality. But what were the mechanisms through which this enrichment happened? As mentioned, at least three not entirely exclusive hypotheses exist. First, political office could have given those in power profitable opportunities for their own businesses. Second, patronage and clientelism, that is, receiving money for regulating the local economy in a way that was more favourable to some than to other interest groups, such as guilds. Third, simple embezzlement or theft of public money. All three scenarios imply that political elites would have derived some personal benefit or rent from holding their office, and possibilities two and three would most likely be called "corruption" in modern societies (van Klaveren 1957: 289). Systematic evidence for any of the three possibilities is extremely difficult to provide, especially for

[12] Another theoretical possible alternative to estimate the effect of becoming a council member on different parts of the distribution of a continuous variable like wealth would be quantile regression (Angrist and Pischke 2009: 269-270). However, currently available estimators require a large time dimension relative to the number of units (n/T below ten) to be valid. Otherwise, confidence intervals are poorly covered (Machado and Santos Silva 2019: 151). Since the time dimension is small relative to the number of units in my data (see number of observations and taxpayers in Table 5.3), I do not employ quantile regression.

things like patronage or embezzlement that were often suspected but impossible to prove for contemporaries (Boockmann 1998: 367). One therefore has to rely on indicative evidence.

Table 5.5: Mechanisms: Wealth of Merchants and City Clerks (Difference-in-Differences Estimates)

	(1) ln-Wealth	(2) ln-Wealth	(3) ln-Wealth	(4) ln-Wealth
Council member × Post × Merchant	0.263	0.298		
	(0.359)	(0.385)		
Council member × Post	0.817***	0.741***		
	(0.122)	(0.172)		
City clerk × Post			0.807*	1.308**
			(0.463)	(0.543)
Merchant	YES	YES	NO	NO
Controls	NO	YES	NO	YES
Age & age-sq.	YES	YES	YES	YES
Locality FE	YES	YES	YES	YES
Time FE	YES	YES	YES	YES
Observations	21,579	21,579	21,579	21,579
Taxpayers	4,490	4,490	4,490	4,490
R^2	0.089	0.105	0.090	0.104
Mean of dependent variable	7.523	7.523	7.523	7.523

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

To provide such evidence I investigate the effect on personal wealth of being a council member while at the same time working as a merchant, and of being a city clerk. Merchants were a well represented group on the council of Nördlingen and they had broad opportunities to conduct business (Friedrichs 1979: 175). If hypothesis one, that is, the enrichment of council members was the result of emerging profitable business opportunities, explained the personal enrichment of magistrates, then one would expect this effect to show up especially among merchants. In Column 1 and 2 of Table 5.5 I test this possibility, by interacting council membership with being a merchant, while holding council membership and being a merchant separately constant. The coefficients are insignificant, regardless of whether controls are added. This means that being a merchant did not make a significant difference for the increase of the personal wealth of magistrates. In the Appendix I report additional results for other well represented professional groups — retailers, taverners and foodproducers — on the city council that might have benefited from better business opportunities, but none of the results is statistically different from zero. These findings make it unlikely that hypothesis one — better business opportunities — can account for the observed wealth increase of political elites. To be fair, the coefficient on the interaction of being a council member while also being a merchant is not precisely zero, so it cannot be excluded that better business opportunities did play a minor role in the enrichment of magistrates, or that simply all occupations benefited to a similar extent from better business opportunities. However, the large and highly significant coefficients of being a council member suggest that the bulk of wealth increase likely came from sources other than business opportunities, such as patronage, embezzlement or theft of public money (Hypotheses 2 and 3).

This leads us to the results in Columns 3 and 4, where I test the effect of being a city clerk on personal wealth. The group of clerks included city secretaries, city administrators or individuals working as various kinds of tax collectors. In other words, they were the magistrates' helpers in running the city administration. Being a clerk was usually a full-time occupation, so they could not be at the same time also merchants, like the magistrates. Historians that have studied "corruption" in preindustrial times have argued that patronage, clientelism, embezzlement or theft of public money by magistrates and officials were very frequent phenomena in cities. The revenues were often taken in by lower-level officials, because they were in more direct contact with the population and physically administered the difficult-to-monitor money, and then shared it with their superiors, that is, members of the political elite (Schmoller 1922: 232, van Klaveren 1957: 299, 322, Quarthal 1987: 41-42). If this is true, then one would expect that the city clerks experienced an increase in their personal wealth too. Columns 3 and 4 of Table 5.5 suggest that being a city clerk in the individual post period,[13] instead of being a council member, led to substantial personal enrichment, by 81 to 131 percent if one reads the change in log points as lower bound percentage change. I

[13] To avoid that once treated units are later counted as non-treated, for example because a city clerk stops working, I consider all those individuals that ever were city clerks as treated as long as they are in the tax registers. To avoid problems of multicollinearity, I drop from the set of controls the occupation-dummies for city clerks, that is of city secretaries, city administrators and tax collectors.

interpret this as evidence indicative of patronage, clientelism, embezzlement or theft of public money — or simply manipulation of the regulatory and fiscal system — by magistrates and officials playing a role in their personal enrichment.

Is it possible to support this claim with more direct evidence? As mentioned, patronage, clientelism, embezzlement or theft of public money were and continue to be very hard to proof. The reason is most likely that magistrates and other officials often did such things in secrecy, probably because these actions were at least partially illegal, even in preindustrial times (van Klaveren 1957: 315-318). But there exists some qualitative indirect evidence. For instance, in the seventeenth century the municipal court of Nördlingen — a public institution — enforced the weavers craft organisation's[14] — a private organisation of businessmen — internal rules aiming at excluding craftsmen that employed different techniques and produced higher quantities than the ones prescribed by the craft organisation (see for example Kluge 2007).[15] Craft organisations were politically disempowered in Nördlingen, at least formally in terms of seats on the city council. But they could still influence city politics. Yet this would have been supposedly more expensive without formal representation, as it would have required the craft organisations to lobby officeholders. This actually happened. Ogilvie (2019) reports in her "Qualitative Guilds Database" a telling incident from the year 1620 — just after the Thirty Years' War had begun and the first soldiers had visited Nötdlingen — where the weaver craft organisation incurred (an unfortunately unspecified amount of) costs to lobby Nördlingen's political authorities, to enforce entry barriers against an outside individual.[16] In other words, craft organisations got privileges from the town government, and political elites got material favours in return. At the time these favours were considered "gifts", but they were effectively a bribe (Ogilvie 1997: 372-373). It is probably not far-fetched to believe that this form of patronage or clientelism contributed to the personal

[14]The surviving archival documentation and research about guilds in Nördlingen is very sparse (Voges 1988: 121-122).

[15]In one case the municipal court sentenced a weaver to death for having produced cloth in a way that deviated from the organisation's rules, and for selling more than the quantity he was allowed to sell (Kluge 2007: 296).

[16]In the specific case the weaver craft organisation wanted to keep an individual out of their organisation whose father also had connections to individuals working with leather, that is, competitors of the weavers (see Stuart 1999: 96).

richness of the political elites involved.

Embezzlement or theft of public money is even harder to demonstrate, and for Nördlingen the evidence seems to be entirely lost. However, this does not mean that it did not happen. An episode from the history of the nearby city Ehingen demonstrates this.[17] In 1681 it was discovered that several thousand florins were missing in the treasury. The mayor and another high official were accused of having misappropriated the money, but the case could never be solved. In 1688 the city had suffered heavily from attacks of the French army during the Palatine Succession War (1688-98). The year after the beginning of the war another check of the treasury was conducted. Again, a huge (but unspecified) amount was missing. Again, mayor and other high officials were suspected of having taken the money, but also this time it was impossible to solve the case. Six years later — the war was still ongoing — another check was conducted, and for the third time in less than two decades money was missing. This time the large amount of 297.830 florin had disappeared, but it was not possible to solve the case (Quarthal 1979: 8). This historical account demonstrates several things. First, embezzlement or theft of public money by offiholders was a frequent phenomenon at the time and in the region, and it is entirely plausible that it explains part of the enrichment of Nördlingen's political elites. Second, the fact that in all three instances no offender could be identified, notwithstanding the large amounts of money involved, suggests that political elites were not subject to higher scrutiny because they checked each other (for this possibility see North et al. 2009: 191). Instead, it seems more plausible that officeholders acted as distributional cartel that enriched themselves at the expense of the population (see van Klaveren 1957, Olson 1982, Acemoglu 2008; also North et al. 2009: 18-21). Third, two of the three incidents of theft of public money happened in chaotic times of warfare, that saw attacks, burning of houses and plundering by soldiers in Ehlingen, similar to what happened in Nördlingen during the Thirty Years' War. This supports the argument that the chaos of war could be an ideal veil for rent-seeking activities of political elites (see also Querubin and Snyder 2013). We will return to this issue in the next section.

[17] Around 1800 Ehingen had a population size of approximately half the size of Nördlingen (Keyser 1962: 346).

But before proceeding two remarks are due. First, note that the results of this section do not support the civic-mindedness narrative of urban political elites. The city-level results in Section 5.4.1 have already suggested that there existed a broad pattern of a more closed or oligarchic political structure being conducive to higher economic inequality. The micro-level results of this section suggest that the mechanism driving this relationship was most likely that urban administrations were vulnerable to personal enrichment by elites. Second, one might be concerned about the external validity of the findings from Nördlingen. To address this issue one would ideally have individual-level data about personal wealth and office holding for a larger sample of early modern cities. Unfortunately, the necessary data are currently not available, and may never become available in the same exceptional quality as in Nördlingen. This is a result of the extraordinary orderliness with which Nördlingen's records were kept, and the fact that they survived over time. Yet Nördlingen's political structure was not dissimilar to that of many other cities in Germany and Europe (see Scribner 1996, Wahl 2019, Pirenne 1958), which suggests that the conclusions for Nördlingen may well hold more broadly.

5.4.3 Rent-Seeking and Wealth Concentration in Times of Crisis

While the key assumption of the difference-in-differences research design of common trends holds, the possibility of reverse causality biasing the results cannot be entirely excluded. One could, for instance, argue that existing council members chose new members not just based on their past wealth accumulation performance, but also based on the *expected* future wealth accumulation of potential candidates. In that case, selection bias could lead to reverse causality bias. Unfortunately, there is no way of empirically accounting for council members' expectations about future wealth accumulation of membership candidates. The only way to get around this identification problem is to exploit a setting that provides plausibly exogenous variation.

In this section I exploit such variation in the potential for political elites to engage in rent-seeking, deriving from the shock to urban life and municipal finances brought about by the

Thirty Years' War. Moreover, the war provides an occasion to observe the behaviour of political elites in times of crisis. The pressure on elites to act responsibly was probably greatest in this period. But also the temptation to engage in rent-seeking — given the extraordinary amount of resources involved — and to use the war as a veil for covering these activities were considerable.

I estimate regressions of the following form, which is again similar to Equation 5.2:

$$W_{i,t} = \alpha_i + \pi_t + \beta_1(Council_i \times Post_{i,t} \times 30YearsWar_t) + \beta_2(Council_i \times Post_{i,t}) + \boldsymbol{\gamma' X_{i,t}} + \epsilon_{i,t} \tag{5.4}$$

The principal difference is that I add to the interaction term a variable $30YearsWar_t$ that takes the value one after the beginning of the war in 1618, when Nördlingen started to be regularly visited by soldiers, was besieged and experienced battles in its vicinity, and zero before 1618. β_1 is the coefficient of interest, which captures wealth and inequality changes of council members during the Thirty Years' War. I hold ordinary council membership in the individual post-period constant, so that I capture only the *additional* effect of the war. I limit the analysis to individuals that are observed before and after the beginning of war. The results should be interpreted as reduced form or intention-to-treat estimates, because we cannot observe rent-seeking — through one of the three potential channels discussed above — as such. We only observe the "invitation" to engage in rent-seeking, that is, office-holding.

Table 5.6 provides estimates of how being a council member increased private wealth, and contributed *ceteris paribus* to a greater economic polarisation and inequality during the war with respect to the pre-war period. For all three outcome variables the results point towards greater enrichment and inequality, with or without controls. For example, if we read the coefficients in Columns 1 and 2 as lower bound percentage changes of wealth, then the war increased personal wealth by 103 to 105 percent relative to the rest of the population, on top of council members' ordinary wealth accumulation. Controlling for occupational categories is particularly important here, because one could argue that council members got richer

Table 5.6: Political Office and Wealth During the 30-Years' War (Difference-in-Differences Estimates)

	(1) ln-Wealth	(2) ln-Wealth	(3) Percentile	(4) Percentile	(5) Top 5%	(6) Top 5%
Council member × Post × 30-Years' War	1.048***	1.032***	5.975**	6.069**	0.204*	0.190*
	(0.270)	(0.270)	(2.997)	(3.057)	(0.120)	(0.114)
Council member × Post	YES	YES	YES	YES	YES	YES
Controls	NO	YES	NO	YES	NO	YES
Age & age-sq.	YES	YES	YES	YES	YES	YES
Locality FE	YES	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES	YES
Observations	6,289	6,289	6,289	6,289	6,289	6,289
Taxpayers	1,155	1,155	1,155	1,155	1,155	1,155
Mean of dependent variable	7.460	7.460	48.01	48.01	0.0596	0.0596
R^2	0.112	0.141	0.059	0.095	0.029	0.047

Notes: Estimation method is OLS. The period of analysis is 1603-1646. Standard errors clustered at the household level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

because they were often merchants who could have benefited from increased demand for the goods they traded during the war, such as military equipment and provisions. These results are consistent with the hypothesis that the war increased potential and actual rent-seeking of political elites.

Can we get a better sense of how large the estimated effects are? In Appendix I repeat the above analysis, but taking actual wealth as outcome variable. In the baseline specification an individual magistrate gained about 33,794 florin due to the war. Consider that there were 15 magistrates, and that reported military exactions from the population amounted to 2.3 million florin (not counting in non-monetary payments). Then a back-of-the-envelope calculation suggests that the total enrichment of magistrates during the war might have corresponded to about 22.04 percent of the amount extracted by soldiers.

How does the effect of office-holding on personal wealth and economic polarisation evolve over time? To answer that question I estimate the following flexible difference-in-differences model:

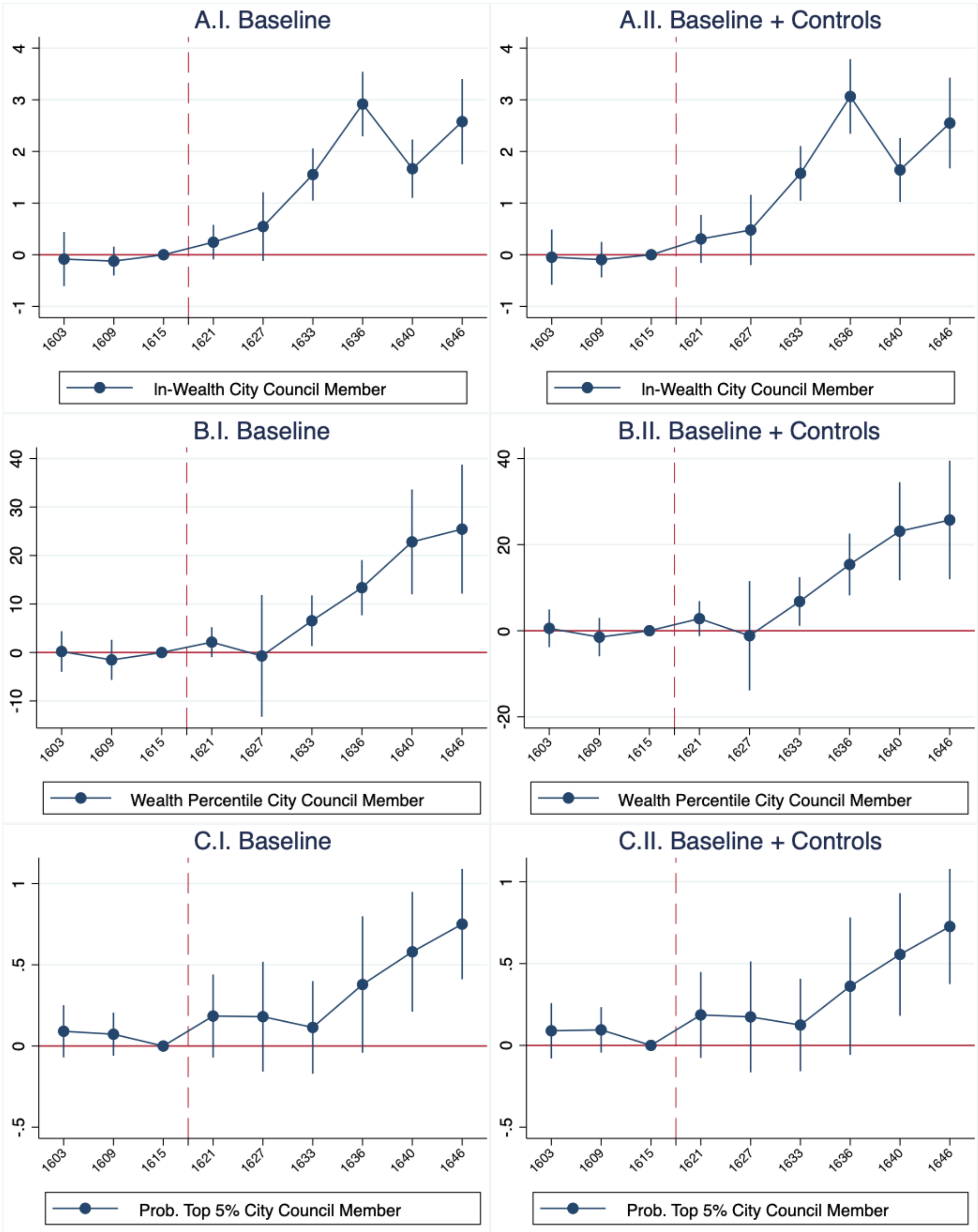
$$W_{i,t} = \alpha_i + \pi_t + \sum_{t=-2}^{8} \beta_t (Council_i \times Post_{i,t} \times Year_t) + \delta (Council_i \times Post_{i,t}) + \boldsymbol{\gamma' X_{i,t}} + \epsilon_{i,t} \quad (5.5)$$

Being a council member in the individual post-period is now interacted with a set of dummies covering time before and during the war. In Figure 5.4 the insignificant coefficients in the pre-treatment period suggest that the common trends assumption holds. Sitting city council members did not get significantly richer before the war. Yet they got substantially richer once the war began (Panel A), they climbed up in the wealth distribution (Panel B), and they were more likely to be part of the top 5 percent (Panel C). Given that during the Thirty Years' War it was not just troops and war that came to Nördlingen, but also plague (in 1634), one might wonder whether the results in Table 5.6 were also driven by the epidemic. But the results in Figure 5.4 suggest that this is unlikely, since the increasing trend in personal wealth and wealth concentration of political elites is clearly visible before the arrival of plague. In the Appendix I repeat the analysis of magistrates' personal wealth, but restrict the sample even further, to those magistrates that were not just part of the dataset before the war began, but had also entered office before. The results are analogous to the ones reported here, which suggests that selection of richer magistrates into the council does not drive the observed patterns.

So far, the results of this section suggest that the increase in the potential for rent-seeking due to the circumstances of the war led to higher concentration of wealth in the hands of the oligarchic political elite, thus increasing inequality. The fact that it was precisely during this crisis that a small and powerful elite enriched itself is hard to square with the notion that political elites acted as civic-minded guardians of the common good. The evidence is suggestive of elites being instead self-interested rent-seekers who feathered their nests at the expense of the population they were governing, especially when threats by soldiers, battles and plague gave them an opportunity to do so.

But one might ask again what the precise mechanisms were through which this enrichment

Figure 5.4: Political Office and Wealth During the 30-Years' War (Flexible Difference-in-Differences Estimates)



Notes: Regression estimates of ln-wealth of city council members before and during the Thirty Years' War (vertical red line), with respect to all other taxpayers (horizontal red line). The omitted reference year is the last year of measurement (1615) before the beginning of the war. The estimation method is OLS. All regressions include a full set of taxpayer and time fixed effects, and controls for age and age-squared of the taxpayer. Standard errors clustered at the household level in parentheses. Confidence intervals indicate significance at the 95-percent level.

Table 5.7: Mechanisms: Merchants and City Clerk During the Thirty-Years' War (Difference-in-Differences Estimates)

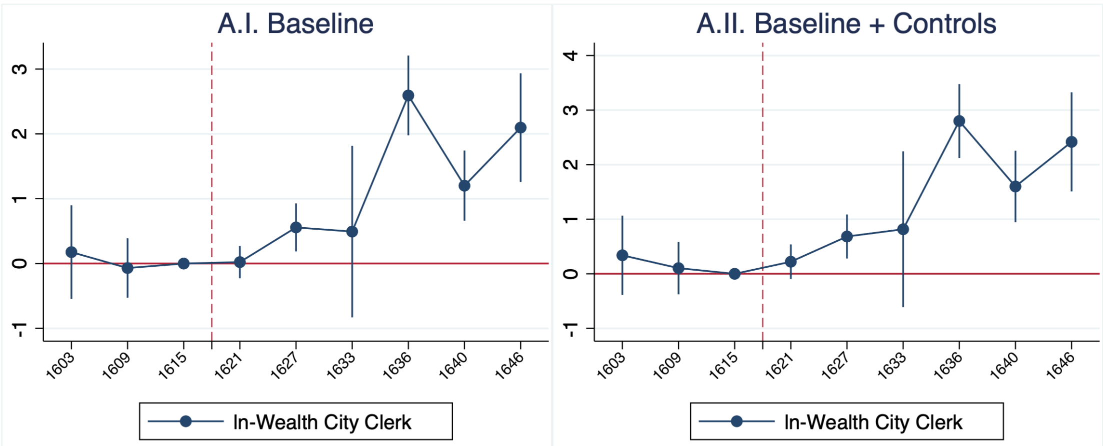
	(1) ln-Wealth	(2) ln-Wealth	(3) ln-Wealth	(4) ln-Wealth
Council member × Post × 30-Years' War × Merchant	-0.104	-0.145		
	(0.464)	(0.479)		
Council member × Post × 30-Years' War	1.015***	1.005***		
	(0.292)	(0.291)		
City clerk × Post × 30-Years' War			0.591*	0.654*
			(0.338)	(0.341)
30-Years' War × Merchant	YES	YES	NO	NO
Council member × Post	YES	YES	NO	NO
City clerk × Post	NO	NO	YES	YES
Controls	NO	YES	NO	YES
Age & age-sq.	YES	YES	YES	YES
Locality FE	YES	YES	YES	YES
Time FE	YES	YES	YES	YES
Observations	6,289	6,289	6,289	6,289
Taxpayers	1,155	1,155	1,155	1,155
Mean of dependent variable	7.460	7.460	7.460	7.460
R^2	0.112	0.141	0.110	0.139

Notes: Estimation method is OLS. The period of analysis is 1603-1646. Standard errors clustered at the household level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

happened. In principle, the same three not entirely exclusive possibilities discussed in the previous section are plausible: better business opportunities, patronage and clientelism, embezzlement and theft of public money. To get at least some indicative evidence about the likely mechanisms, I investigate in Columns 1 and 2 of Table 5.7 again the wealth of merchants that were also part of the city council, in their individual post-period, during the Thirty Years' War. I hold the relevant components of the interaction term constant. Merchants presumably had the broadest opportunities to conduct business, that is, they could derive potentially a large benefit from their office. Moreover, merchants were among the principal lenders of money to their own cities in times of war (Stasavage 2011). Meeting an increased demand for war finance could have driven up the personal wealth of city council members further. The insignificant and even negative coefficients suggest that this was not the case, making it again unlikely that better business opportunities can explain magistrates' wealth increase.

What about the wealth of city clerks during the war, that is, those full-time officials that assisted the magistrates in running the city, and that according to some historians were in a particularly advantageous position to engage in patronage and clientelism, embezzlement or theft of public money, or simply corrupt conduct (Schmoller 1922: 232, van Klaveren 1957: 299, 322, Quarthal 1987: 41-42)? The coefficients on the interaction term including city clerks in Columns 3 and 4 suggest that this group benefited substantially from the opportunity of the war. Note that being a city clerk in the individual post-period is held constant, so I capture only the *additional* effect of the opportunities for personal enrichment of the war.

Figure 5.5: City Clerks and Wealth During the 30-Years' War (Flexible Difference-in-Differences Estimates)



Notes: Regression estimates of ln-wealth of city clerks before and during the Thirty Years' War (vertical red line), with respect to all other taxpayers (horizontal red line), and conditional on being council member. The omitted reference year is the last year of measurement (1615) before the beginning of the war. The estimation method is OLS. All regressions include a full set of taxpayer and time fixed effects, and controls for age and age-squared of the taxpayer. Standard errors clustered at the household level in parentheses. Confidence intervals indicate significance at the 95-percent level.

Figure 5.5 plots flexible estimates (estimated analogously to Equation 5.5) for the effect of being a city clerk during the war on personal wealth, holding council membership constant. Similar to council members, one would expect city clerks to experience an increase of wealth after 1618, but not before. The estimates clearly point into that direction. Overall, I interpret the evidence in Table 5.7 and Figure 5.5 as making patronage and clientelism, embezzlement or theft of public money more plausible mechanisms than simply better business opportunities.

Several historical facts about the Thirty Years' War and the specific context in Nördlingen make this a credible hypothesis. For what concerns patronage and clientelism, we have already seen in the previous section that there were incidents of lobbying in the early years of the war, where the government granted craft organisations privileges in return for material favours (see Ogilvie 2019). But this could have happened at any time, so what was special about the war period? As mentioned, in Nördlingen magistrates were a kind of bottleneck through which not less than 2.3 million florin exerted by soldiers plus the numerous but not quantifiable non-monetary payments went, to avoid plundering and burning by soldiers. This opened up new opportunities for officeholders to use their discretion over how to allocate the huge material burden. They could decide which taxes to raise and on which groups to put the burden, in which areas of the city and in which households to quarter hundreds of soldiers, which tax evaders to punish, and from whom to confiscate the non-monetary resources necessary to fulfil soldiers' demands, such as food or equipment. All this opened the door for patron-client relationships which could easily benefit magistrates economically.

Embezzlement and theft of public money are at least equally likely to have played a role. For one thing, the chaotic years of the war made it difficult to maintain minimum standards of justice and prevent a total collapse of law and discipline (Parker 1984: 177-178). This context of lawlessness, coercion and predation facilitated the personal enrichment of certain individuals, especially of mercenaries, military entrepreneurs and officials. The war was a time of large-scale dispossession of ordinary people to the benefit of who was legitimised to do so by "the state". This is a well-established fact in the historiography (Press 1991: 245-250, Schilling 1994: 437-443, Stier and von Hippel 1996: 234, Asch 1997: 158; for examples see Redlich 1959). Moreover, wars were generally occasions that were reportedly exploited by officials to steal public money — the history of Ehlingen discussed above already provided vivid examples — and the Thirty Years' War was no exception to that. One of the most spectacular incidents during that war was General Albrecht von Wallentein's theft of 96,000 thalers from the treasury of the Bohemian estates in 1619 (Mann 1987: 138-142). Ultimately, the Thirty Years' War was not just a time of chaos in terms of physical violence, but also of recurring plague, in Nördlingen, Germany and almost all of Europe. Epidemics, too, were frequently exploited by officials to defraud and steal public money and other assets. Ogilvie (2022: ch.3) provides a series of examples for this fact, specifically for the 1630-31 plague wave.[18]

As mentioned, in Nördlingen all this happened in a context where officials were a kind of bottleneck through which not less than 2.3 million florin went that the city paid to soldiers, to avoid plundering and burning. It does not require much imagination to see the temptation this amount likely created. Future research will hopefully provide the necessary data to support this historically plausible hypothesis in a more systematic fashion.

A final question is how the above results relate to work showing that the Thirty Years' War reduced inequality, as measured for example with the Gini coefficient (van Zanden 1995, Scheidel 2017, Alfani et al. 2022). My results do not contradict that work. They suggest that although certain macro shocks can reduce inequality overall, there can at the same time exist powerful forces pushing for higher inequality.

5.5 Conclusion

This chapter has investigated the relationship between urban political structure and inequality. I constructed and analysed two different datasets. At the macro city-level, I found that cities with a more oligarchic or closed political structure, that is, without participative elections, had distribution of wealth that was substantially more unequal. The individual-level analysis for Nördlingen then suggested an important mechanism by which cities with more oligarchic governments may have failed more to provide equality: political elites, and those individuals that assisted them in administering the city, enriched themselves significantly. Those with more political power enriched themselves the most. An individual who entered a political office, therefore, contributed to a more unequal wealth distribution. The time of the Thirty Years' War, a period that saw military action, immense extraction of resources

[18] For example, in one case in 1630/31 a high city clerk working in a local pesthouse defrauded the public health funds by claiming more money than he had actually paid out in grave-diggers' wages (Ogilvie 2022: 104; see also Henderson 2019: 242-3, 267-8).

and death by epidemics, accelerated politicians' wealth accumulation.

These empirical results have four main implications. First, access to political power and the degree of closedness of the political system was an important explanation of preindustrial inequality, as hypothesised by Alfani and Ryckbosch (2016), Scheidel (2017), Piketty (2020) and Alfani (2021a). This chapter provides systematic evidence for that view. The oligarchic or closed political systems of many cities both in Germany and in the rest of Europe were probably a relevant driver of preindustrial inequality. Second, urban political elites were almost certainly not the civic-minded, responsible rulers who guarded the common good under great personal sacrifice (see Weber 1978, Bátori 2007, Isenmann 2014). When they could, they enriched themselves, contributing to inequality. In this sense, political oligarchs in the early modern period were probably not much better than nowadays (see Querubin and Snyder 2013, Milanovic 2019).

Third, moments of socio-economic crisis, such as warfare and epidemics — which were very frequent in the preindustrial world — could be a veil for political elites' rent-seeking efforts at the expense of the wider population, again contributing to higher inequality. Ultimately, the manipulations of the regulatory and fiscal system from inside the government,[19] through which political elites enriched themselves, almost certainly inflicted deadweight losses on the economy. This may go a long way in explaining the economic decline of many once prosperous cities in early modern Europe. If it is true that 'the city drove the countryside" in the process of industrialisation (Allen 2014), then the importance of this urban decline can hardly be overestimated.

Future research will hopefully make more individual-level data available, of the kind that I have constructed for Nördlingen, to provide a broader empirical basis for these conclusions.

[19]For arguments about the importance of the fiscal system for explaining preindustrial inequality, see Alfani 2015, Alfani and Di Tullio 2019, Alfani 2021a.

5.6 Appendices

5.6.1 Independent Variables in the City-Level Dataset

This Appendix describes how the independent variables employed in the city-level analysis have been coded.

Most of the information was taken from the *Deutsches Städtebuch* (Keyser 1939, 1941, 1952, 1954, 1956, 1957, 1959, 1962, 1964, Keyser and Stoob 1971, 1974, Baltzarek et al. 1973).

Council elections. A dummy that indicates whether (at least part of) the local population could participate in politics through "participative elections", that is, electing the city council or the mayor. I followed the approach of Wahl (2019). Information was taken from the *Städtebuch*.

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 typically assumed for preindustrial German towns is 4.5 (Minns et al. 2020: 611).

Epidemic. A dummy that indicates whether there was an outbreak of an epidemic in a locality in the preceding period. Information on major outbreaks of epidemics has been taken from the *Städtebuch*. Epidemics indicated by the *Städtebuch* are for example smallpox, syphilis and plague. For those rural communities in the dataset that have no entry in the *Städtebuch* I had to make an assumption about plague occurrence. I assumed that the rural communities had the same plague occurrence as the nearest town for which an entry in the *Städtebuch* and information about the outbreak of epidemics is available. These assumptions are based on the regular interaction between village and town inhabitants via urban markets in preindustrial times. Towns were daily markets in which peasants from surrounding villages regularly sold agricultural products and bought goods that they could not produce themselves (Isenmann 2014: 673). For those villages that were under the administrative authority and were taxed by a nearby city which is part of the dataset, I have assumed the same occurrence of epidemics as in the city. For example, for the rural community of Niederwangen I assume the same plague occurrence as for the nearby city of Wangen. For those villages that were not under the administrative authority of a city in the dataset I have assumed the same occurrence of epidemics as in the closest town with an entry in the *Städtebuch*.

Protestant Reformation. A dummy that indicates whether the Protestant Reformation was been introduced in a locality after 1517. I have taken as introduction date when a town council or local ruler officially introduced the Reformation. However, when no precise year is indicated I took as alternative date the appointment of a Protestant priest by the town council. When no introduction is mentioned, or the source indicates that the Reformation had "no substantial impact", I code the locality as Catholic. Information was taken from the *Städtebuch*. For communities without entry in the *Städtebuch* usually the relevant Imperial Estate introduced the Protestant Reformation. This information has been taken from the *Städtebuch* and the *Historisches Lexikon der deutschen Länder* (Köbler 2007).

Warfare. A dummy that indicates whether a locality was exposed to battle action or a siege within a radius of 25 km. The data are from Schaff (2020).

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

5.6.2 Construction of the Nördlingen Dataset

The dataset contains all households (6,557) living in Nördlingen between 1579-1700, in 3 to 6-year intervals (22 points of observation). Since the tax registers were meticulously ordered by alphabetical order of names of household heads, it is possible to connect and trace individuals over time. This has been done by Christopher Friedrichs in the early 1970s, and led to the publication of a book (see Friedrichs 1979). Professor Friedrichs generously made available his paper-based records which I then digitised them to create a panel-dataset.

For every household I collected the following information:

- abbreviated family name
- abbreviated first name
- gender
- tax payment in the year concerned, expressed in florin (fl.) or fractions thereof
- occupation

Importantly, tax payments could be easily converted into actual wealth levels because the uniform tax rate is known. Over the whole period of study the tax rate was 0.5 percent of an individual's total wealth.

I then converted the abbreviated family and first names into full names. For that I used lists of the abbreviations in the city archive of Nördlingen. Only the full names made it possible to determine which individuals were at some point part of the city government. I took information about who was a magistrate, who was a mayor and for which years these offices were held from Friedrichs (1979: Appendix). In the analysis I only use information about those individuals for which I observe the year in which they made their first tax payment. This made it possible to calculate for how long an individual had been paying taxes at any point in time. I used this information as a proxy for an individual taxpayer's age.

The original tax registers give very detailed information about occupation of taxpayers. Friedrichs (1979) sorted all individuals into 60 occupational categories. I created a dummy variable for every category, which I have then employed in the empirical analysis. The categories are the following:

wool weaving, fine cloth weaving, linen weaving, other basic textile producing trades (e.g. dyer), tailoring, other clothing and rare textile producing trades (e.g. silk maker), furs, tanning, shoemaking, other leather working trades (e.g. saddler), masons, other construction trades (e.g. bricklayer), cabinet maker, other woodworking trades (e.g. barrel maker), smiths weaponry, smiths riding gear, smiths tools, fixtures, etc. (e.g. locksmith), smiths domestic equipment, metal casters, goldsmith, rope maker, brushes and baskets, pottery and glass, complex instruments (e.g. clock maker), books and paper (e.g. printer), artists (e.g. painter), miscellaneous craftsmen, baker, confectioners, butchers, fishmongers, brewers and distillers, millers, merchants, retailers, taverners, learned occupations religious, learned occupations legal, learned occupations medical, learned occupations pedagogical (e.g. schoolmaster), learned occupations writing (e.g. notary), non-municipal administrator (e.g. of religious foundation), learned occupations other, city secretaries, high city administrative positions (e.g. legal counsel, armaments superintendent), city offices related to tax collections, city offices related to inspection of products, watchmen, mounted and forest officials, municipal servants, carters, messengers, musicians, healthy and hygiene workers, agricultural workers, menial workers (e.g. day labourers), soldiers, miscellaneous non-craftsmen, no occupation wealth above 1 gulden (e.g. only city council member), no occupation wealth below 1 gulden (e.g. unemployed).

5.6.3 Additional Results

Wealth Changes of Main Occupational Groups on the City Council

In the main text I have shown that being a merchant while at the same time being a city council member does not explain the substantial enrichment of Nördlingen's political elites. I have interpreted this result as evidence that better business opportunities were probably not a major channel through which the magistrates enriched themselves when entering office. One might ask whether other occupational groups might have instead benefited significantly from holding an office. Besides merchants, retailers, taverners and foodproducers were among the most frequent occupational groups on the city council Friedrichs (1979: 175).[20]

In Columns 1 to 6 of Table 5.8 I interact these occupational groups with being a city council member. As for merchants, none of these groups had a significant differential wealth share increase. Yet the coefficient on being a council member in the individual post period remains almost unchanged and highly statistically significant in all specifications. This is even the

[20] "Foodproducer" is an umbrella category for several food-producing trades: bakers, confectioners, butchers, brewers and millers.

Table 5.8: Mechanisms: Wealth of Other Occupational Groups (Difference-in-Differences Estimates)

	(1) ln-Wealth	(2) ln-Wealth	(3) ln-Wealth	(4) ln-Wealth	(5) ln-Wealth	(6) ln-Wealth	(7) ln-Wealth
Council member × Post × Retailer	-0.064	-0.035					0.030
	(0.367)	(0.383)					(0.425)
Council member × Post × Taverner			-0.232	-0.155			-0.066
			(0.271)	(0.309)			(0.353)
Council member × Post × Foodproducer					0.246	0.398	0.441
					(0.632)	(0.658)	(0.684)
Council member × Post × Merchant							0.314
							(0.426)
Council member × Post	0.861***	0.790***	0.901***	0.816***	0.842***	0.766***	0.725***
	(0.123)	(0.164)	(0.132)	(0.179)	(0.117)	(0.157)	(0.247)
Retailer	YES	YES	NO	NO	NO	NO	YES
Taverner	NO	NO	YES	YES	NO	NO	YES
Foodproducer	NO	NO	NO	NO	YES	YES	YES
Merchant	NO	NO	NO	NO	NO	NO	YES
Controls	NO	YES	NO	YES	NO	YES	YES
Age & age-sq.	YES	YES	YES	YES	YES	YES	YES
Locality FE	YES	YES	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES	YES	YES
Observations	21,579	21,579	21,579	21,579	21,579	21,579	21,579
Taxpayers	4,490	4,490	4,490	4,490	4,490	4,490	4,490
Mean of dependent variable	7.523	7.523	7.523	7.523	7.523	7.523	7.523
R^2	0.090	0.105	0.089	0.105	0.089	0.105	0.105

Notes: Estimation method is OLS. The period of analysis is 1603-1646. Standard errors clustered at the household level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

case when I include all three occupational groups and also merchants in the same regression (Column 7). I interpret these results as further evidence that channels other than business opportunities have to be taken into account for explaining the large increase in personal wealth of officeholders.

Wealth Changes of Magistrates in Office before the Thirty Years' War

One concern about the results reported in the main text could be that the wealth increase of magistrates during the Thirty Years' War was driven by the selection of richer individuals into the city council. The analysis was already restricted to all individuals for which there is at least one observation before and one after the beginning of the the war. Here I make the comparison even more stringent, by dropping all magistrates that were recorded in the tax registers before the war began, but that entered their office afterwards.

The results in Table 5.9 show that even those magistrates that were already in office before

Table 5.9: Political Office and Wealth During the 30-Years' War (Difference-in-Differences Estimates)

	(1) ln-Wealth	(2) ln-Wealth
Council member × Post × 30-Years' War	0.639**	0.603**
	(0.264)	(0.267)
Council member × Post	YES	YES
Controls	NO	YES
Age & age-sq.	YES	YES
Locality FE	YES	YES
Time FE	YES	YES
Observations	6,202	6,202
Taxpayers	1,142	1,142
Mean of dependent variable	7.420	7.420
R^2	0.114	0.143

Notes: Estimation method is OLS. The period of analysis is 1603-1646. Standard errors clustered at the household level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

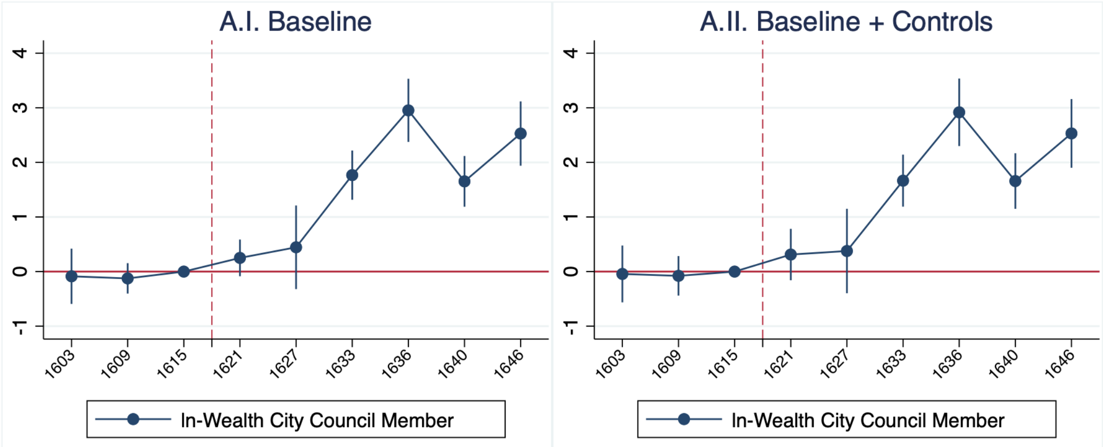
the war began experienced large personal wealth increases. The coefficients are somewhat smaller, but nevertheless substantial and statistically significant, notwithstanding the smaller number of treated units.

In Figure 5.6 I use the same restricted dataset to estimate the flexible difference-in-differences model. Again we find a pattern that is almost identical to the results obtained with the complete dataset. As soon as the war began, sitting magistrates' wealth grew substantially, but not before. Confidence intervals are larger, which is most likely the result of the smaller number of treated units. These results hold even when controls are added.

Actual Wealth Changes of Magistrates during the Thirty Years' War

The analysis in the main text suggests that the magistrates of Nördlingen gained substantially in terms of log-wealth points compared to the rest of the population during the Thirty Years' War. To get a better sense of the magnitudes of the effect, I repeat the baseline analysis below, but taking actual wealth measured in florin as outcome. The coefficients suggest that a city council member on average gained between 33,794 and 35,770 florin dur-

Figure 5.6: Political Office and Wealth During the 30-Years' War (Flexible Difference-in-Differences Estimates)



Notes: Regression estimates of ln-wealth of city clerks before and during the Thirty Years' War (vertical red line), with respect to all other taxpayers (horizontal red line), and conditional on being council member. The omitted reference year is the last year of measurement (1615) before the beginning of the war. The estimation method is OLS. All regressions include a full set of taxpayer and time fixed effects, and controls for age and age-squared of the taxpayer. Standard errors clustered at the household level in parentheses. Confidence intervals indicate significance at the 95-percent level.

ing the war, with respect to the rest of the population. These effects are highly statistically significant.

Table 5.10: Actual Wealth of City Clerks During the 30-Years' War (Difference-in-Differences Estimates)

	(1) Wealth (fl.)	(2) Wealth (fl.)
Council member × Post	33,793.932***	35,770.239***
	(10,719.840)	(11,107.365)
Council member × Post	YES	YES
Controls	NO	YES
Age & age-sq.	YES	YES
Locality FE	YES	YES
Time FE	YES	YES
Observations	6,289	6,289
Taxpayers	1,155	1,155
Mean of dependent variable	10451	10451
R^2	0.036	0.052

Notes: Estimation method is OLS. The period of analysis is 1603-1646. Standard errors clustered at the household level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Chapter 6

Conclusion

The main aim of the preceding chapters was to address a major puzzle in economic history: why was economic inequality already high when industrialisation and modern economic growth began, contrary to what one would expect adopting the "Kutznetsian" paradigm? To start shedding light on this question, Chapter 2 established the "historical facts" for the area and period under study. It introduced the main dataset on which the thesis is built, and mapped the trajectory of wealth inequality in German towns and villages, and at a hypothetical "national" level, over the long run. It provided a more nuanced understanding of inequality during the period from 1350 until 1850, by expanding the dataset compiled by Alfani, Gierok and Schaff (2020). I find that, consistent with the literature, inequality rose and fell across four broad periods: Germany followed a secular trend of inequality growth between the fourteenth and nineteenth centuries, interrupted by two major shocks, the Black Death (1350) epidemic and the Thirty Years' War (1618-48).

In the second part of the thesis, I focused on explanations for the patterns observed in the data. The investigation concentrated on the causal effects of three facets grounded in the political economy of early modern Europe: warfare, the Protestant Reformation and oligarchic governmental institutions.

The evidence presented in Chapter 3 showed a strong and positive relationship running from warfare to economic inequality. During ordinary conflicts wealth shares shifted from the lower and middle classes of society to the rich. As a consequence, economic inequality increased. 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, most likely because it was exceptionally destructive. I argued that the seemingly puzzling empirical results are due to two countervailing effects of conflicts on inequality: egalitarian destruction, and inequality-promoting extraction, mostly via regressive taxation. Usually the latter factor outweighed the former one to increase inequality.

Chapter 4 analysed the impact of Protestantism on inequality. I find that the Reformation increased economic inequality, by making poor people poorer relative to the rest of the population. This empirical pattern endured until the end of the early modern age when industrialisation began. The finding reflects the Reformation's shift towards a more particularistic provision of poor relief, which outweighed the expansion of social welfare available to some insiders. The consequence was a reduction in the supply of poor relief to the excluded groups and a decrease of transfers from better-off to marginal poor people. These new low-redistribution policies left behind the bottom of the poor in Protestant society, while the Catholic Church stuck to its universal approach to charity. The findings also reflect that the particularistic Protestant poor relief system had relevant second order effects: the prohibitions of begging, the disincentivising of almsgiving and the stigmatisation of the poor in the labour market reduced marginal poor peoples' share of economic resources. In consequence, the Reformation reshuffled the bottom end of the income and wealth distribution, making some poor even poorer and increasing inequality. Moreover, I documented that Protestant places experienced higher population decline from epidemics, as one would expect if Protestantism made poor strata worse off.

The final Chapter 5 found in a cross-city analysis that cities without council elections — interpreted as more closed or oligarchic governments — had a considerably higher wealth concentration. An individual-level analysis for the city-state Nördlingen then suggested the mechanisms by which more oligarchic city governments may have failed to provide equality: political elites enriched themselves after they entered office, which contributed to higher wealth concentration and inequality in the city-state overall. Moreover, political elites exploited the Thirty Years' War, a shock to municipal finances and a period of socio-economic crisis, as an opportunity to enrich themselves even further. They used their privileged position to seek rents, most likely through manipulating the regulatory and fiscal system from inside the city government.

How do these findings fit together? How can they contribute to explain the secular trend of inequality growth — except for the shocks of the Black Death and the Thirty Years' War, and their aftermaths — that Germany followed between the fourteenth and nineteenth centuries. This trend of inequality to grow is particularly surprising because the German economy was in decline over the early modern period (Pfister 2011: 15). There is a straightforward connection between the main findings in Chapter 3 and 5. On the one hand, the prevailing unequal legal structure of society favoured closed governments by small privileged elites. These enriched themselves when they could, putting the financial burden on the bulk of a less-privileged population. This happened in a context of a highly fragmented political structure in preindustrial Germany and Europe: many rivalling polities often fought costly wars, putting the burden again on the bulk of the population. The result of this interaction between deeply rooted legal and political structures was a tendency for inequality to grow.

More broadly, the findings of the three analytical papers suggest that growing inequality was in many instances the result of the actions of (usually unchecked) governments. They took the decision to begin a war (Chapter 3), to introduce a new confession such as Protestantism (Chapter 4), and their members individually decided to enrich themselves (Chapter 5). As we have seen, all these were forces that pushed up economic inequality in preindustrial Germany. This leads us to the contribution to the literature.

The main contribution of Chapter 2 was to provide the largest single database on preindustrial inequality compared to all other published studies, putting Germany on the map of inequality studies. Chapter 3 contributes mainly by showing that the exclusive focus of the existing literature on the impact of major wars on inequality is misleading. Instead, it suggested that we need to consider the indirect effects of the many ordinary conflicts that happened, because they generated a negative externality in the form of increasing economic inequality. The principal contribution of Chapter 4 was to shift the focus in the debate about the economic impact of the Protestant Reformation in the long run of history, away from the focus on the expansion of public goods provision. Protestantism came with a whole new *trade-off* with implications for redistribution and inequality: between generosity and universal or particularistic provision of social welfare. The result was an increase of inequality, by making some poor people economically worse off compared to the rest of society. Chapter 5's contribution was mainly to show for the first time systematically what has been an intuitive and long-standing hypothesis, but one that was lacking empirical evidence: access to political power facilitated personal enrichment and mattered for explaining inequality growth in preindustrial times, and closed, or oligarchic, urban political systems led to greater personal enrichment by holders of political office.

But if we look at the whole picture there is an additional contribution. As mentioned, the observed pattern of growing and ultimately high inequality long before industrialisation began has been hard to explain with the conventional "Kutznetsian" model. This puzzle has even become more complicated by recent research showing that the "usual suspects" to explain preindustrial inequality changes — economic expansion and demographic dynamics — have at best weak explanatory power (see the synthesis by Alfani (2021a), and the results in Chapter 2). The above findings suggest that political economy factors, that is, institutions, policies and cultural factors, might be a very promising area to search for the causes of preindustrial inequality change.

Ultimately, a note on what could be done in terms of future research. First, there are of course many political economy forces that were not covered in this thesis. A prime candidate would be to study the effect of inheritance practices on preindustrial inequality, a factor to which many authors have ascribed large importance since at least De Tocqueville (1835). At the heart of such a study would lie the idea that inheritance practices, arguably the most important resource allocation mechanism outside markets, can be inequality-promoting or reducing. These practices are historically so persistent that the same customs prevail in many areas of Germany since the Middle Ages until today. Yet the impact of inheritance practices on preindustrial inequality has not been tested systematically yet.[1] This would be particularly interesting because their effects could probably be identified most clearly in a preindustrial, mostly agricultural, developing economy like Germany.

Second, a large stream of literature has been interested in understanding whether there are any persistent impacts of historical forces on modern economic outcomes, such as growth. One could apply the same approach to modern inequality, to find out whether there exist any historically persistent causes of inequality, which would have obvious policy implications. For example, it would be interesting to know whether regions that are dominantly Protestant, that had more exposure to preindustrial warfare, or that had for a longer time an oligarchic government, are still more unequal today, what mechanisms led to this persistence, or whether such connections are mere fiction. Thirdly, we need of course more data, at the aggregate community level, but also at the individual level, of the kind that I have constructed for Nördlingen. These data would provide a broader empirical basis for the insights of this study, would make it possible to refine and if necessary correct the findings presented here.

[1] For the effect of inheritance practices on inequality in *industrial* times see Hager and Hilbig (2019) and Bartels et al. (2020).

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