

Urban Political Structure and Inequality: Political Economy Lessons from Early Modern German Cities

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What was the impact of urban political structure on preindustrial wealth inequality? I document that more closed political institutions were associated with higher inequality in a panel of early-modern German cities. To investigate the mechanisms behind that macro-relationship, I construct a unique individual-level panel dataset on personal wealth and political office-holding in the city-state of Nördlingen (1579–1700). I employ a difference-in-differences setting to show that political elites enriched themselves substantially, increasing inequality. To address endogeneity concerns, I exploit the Thirty-Years' War as a shock to elites' potential for enrichment from public office. Officials manipulated this crisis to enrich themselves further.

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 (Piketty 2020). However, much less is known about the effect of the political structure of cities on inequality.

In this paper, 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

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coming close to modern democratic standards. Many scholars have hypothesized that a closed political structure leads to the enrichment of political elites and higher inequality, now and in the past (Acemoglu 2008; Puga and Treffer 2014; Milanovic 2019; Piketty 2020; Alfani 2021, 2023). And yet, systematic empirical evidence for this plausible hypothesis is almost non-existent.

Additionally, according to a common historical narrative, closed political structures did not even matter because governments were capable of achieving “output legitimacy”: cities were run well and social peace was preserved because the political elites were highly civic-minded rulers. They set their personal interests aside and employed their valuable time and economic resources for the city (Weber 1958; Isenmann 2014). For economic inequality, this romantic historical narrative implies that the political structure of preindustrial cities probably did not contribute to concentrating wealth in the hands of elites or to increasing inequality. But is this argument justified?

I address these two debates by constructing, first, a city-level dataset registering aggregate wealth inequality and participative local democracy in 33 early modern German cities. This dataset makes it possible to study the broad association between closed and open urban political structures and inequality. To better understand the mechanisms behind that relationship, I then construct a second, individual-level panel dataset for the southern-German city-state of Nördlingen between 1579 and 1700. I 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 suggest that its political structure was in some key aspects 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 analyzes 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 plausibly exogenous shock to the potential for personal enrichment from public office by political elites and as an occasion to observe their behavior in a period of severe socio-economic crisis. Estimates are obtained employing the heterogeneous treatment effect-robust estimators recently developed by De Chaisemartin and D’Haultfœuille (2020, 2022).

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 5 percentage points higher, and the top 5 percent share was about 6 percentage points higher in places without elections. I interpret these results in the following way: More oligarchic governments seem to have failed more at providing equality of 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 elites increased their personal wealth on average by 0.44 to 0.55 log wealth points after they entered office. Event-study estimates confirm these estimates but also suggest that later officeholders experienced faster wealth growth a few years before becoming part of the government. This was probably the result of future magistrates holding powerful and potentially profitable positions on the city court before entering government. Second, individuals with greater political power—mayors—increased their personal wealth even more, on average by an additional 0.30 to 0.34 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 3.29 to 4.18 percentiles and were 6.5 to 10.2 percent more likely to be part of the top 5 percent of the wealth distribution.

Fourth, these effects were particularly large in the period of the Thirty Years' War, and they were not driven by pre-trends. 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 (e.g., tax collectors or secretaries), by on average 0.55 to 1.63 log wealth points. The channels of enrichment were most likely increasing fees magistrates received from the Treasury for holding their honorary office, but also patronage and possibly embezzlement of public money. I support this hypothesis with suggestive qualitative historical evidence.

The paper contributes, first, to the literature on the drivers of preindustrial inequality (van Zanden 1995; Alfani 2021; Schaff 2023). A number of studies have hypothesized that access to political power facilitated personal enrichment and mattered in explaining inequality growth, especially in preindustrial times (Acemoglu 2008; Puga and Treffer 2014; Scheidel 2017; Piketty 2020; Alfani 2021, 2023). 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 the best of my knowledge, there is not a single study that investigates the closed politics-inequality nexus systematically. 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 toward increasing compensation through fees but also patronage and potentially even embezzlement of public money by magistrates and city clerks as driving wealth accumulation and inequality.

The paper contributes, second, to a wide theoretical and empirical literature on private returns from public office (Eggers and Hainmueller 2009; Fisman, Schulz, and Vig 2014). My paper is probably closest to recent work by Belloc et al. (2022), 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 present in other geographic, political, and historical contexts as well, and contributed to higher inequality. My paper is also related to Querubin and Snyder (2013), who document how politicians used the U.S. 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 (Schaff 2023). My paper also goes beyond previous studies in documenting the returns to office of a closed political system without any elections.

The paper speaks, third, to an unresolved debate in the urban history of Europe: were urban political elites civic-minded guardians of the common good or self-interested actors (Weber 1958; Boockmann 1998; Friedrichs 2000; 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; Puga and Treffer 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. It is likely that this personal enrichment also inflicted deadweight losses on the city economy. These results are hard to square with the “civic-mindedness narrative.” But they are in line with a stream of literature characterizing political elites in history as members of distributional coalitions that caused institutional sclerosis and economic decline with their extractive behavior (Olson 1982; Ogilvie 2007, 2019; Stasavage 2014; Wahl 2019).

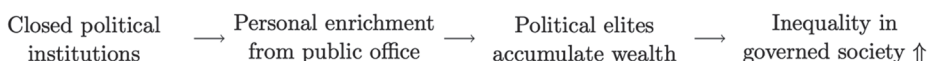


FIGURE 1
OLIGARCHIC POLITICAL STRUCTURE AND ECONOMIC INEQUALITY

Sources: Author's own elaboration.

HISTORICAL FRAMEWORK AND BACKGROUND

Theory and Evidence of Oligarchic Governments, Personal Enrichment from Public Office, and Inequality

In modern societies, it is commonly believed that unchecked or “oligarchic” governments use their power to feather their own nests; they enrich themselves personally through their public offices. It is likely that politicians who enrich themselves nowadays increase economic inequality in the societies they govern (Acemoglu 2008; Querubin and Snyder 2013; Milanovic 2019). Figure 1 summarizes this logic.

Scholars investigating *preindustrial* inequality have made similar arguments: if political power is monopolized by a small elite—for example, by a closed oligarchic city government—then those elites will use their power to enrich themselves, contributing to higher inequality (Puga and Trefler 2014; Scheidel 2017; Piketty 2020; Alfani 2021, 2023). For instance, Alfani (2021, p. 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. Unfortunately, the available empirical evidence is usually piecemeal, anecdotal, or indirect. For instance, Scheidel (2017, pp. 83–84) provides figures on the bequeathed fortunes of a few individual government officials in Spanish America and France. Puga and Trefler (2014, p. 796) use economic theory and employ marriage networks as a proxy for inequality, but neither measure the actual wealth of political elites nor the distribution of wealth or income. This is not meant to criticize the cited scholars; their approaches are the result of systematic data on the personal enrichment of politicians and inequality being very hard to get for preindustrial and industrial societies. The exceptional sources available for Nördlingen make it possible to obtain such micro-data.

Additionally, it is still an open question what the precise mechanisms were that connected closed politics with the personal enrichment

of elites and inequality growth. The first possibility is the potentially generous compensation that magistrates might have received for their activities. Serving in the government of a preindustrial German city was in principle an honorary office without compensation, held by rich individuals that could afford it (Friedrichs 2000, p. 19). In reality, however, political elites often obtained substantial fees for their activities (see Pomerantz 1965; Friedrichs 1979, pp. 177–78). Second, political office could have given those in power profitable opportunities for their own businesses. For instance, officeholders could try to exclude non-officeholders from particularly profitable activities, such as long-distance trade (Puga and Treffer 2014). A third possibility is patronage. Regulating the local economy and society meant that politicians had to weigh up the potentially conflicting economic interests of different groups, such as craftsmen and merchants. Special interest groups often lobbied governments to obtain privileges in return for material benefits. These financial gains likely enriched individual officeholders (see Ogilvie 2019). Fourth, there could have been simple embezzlement of public money in a context of primitive administration and monitoring (Quarthal 1987).

Some forms of personal enrichment from public office were considered legitimate at the time, such as better business opportunities or receiving “gifts” (*honoraria*) in return for privileges (Ogilvie 1997, pp. 372–73). Other forms, such as stealing, were also prohibited at the time, that is, not legitimate (van Klaveren 1957, p. 318). This becomes evident in the many popular urban revolts in early modern Germany that centered on accusations of rulers’ wasteful extravagance and financial abuse, and that people saw as a betrayal of trust (Friedrichs 1982, p. 30). Unfortunately, evidence for these mechanisms of personal enrichment is even harder to provide, today than in the past (Boockmann 1998, p. 367). I provide indirect econometric evidence and historical examples that suggest that certain mechanisms might have been more plausible than others.

Urban historiography has another, more fundamental, unanswered question: were urban political elites benevolent oligarchs? In preindustrial towns, local authorities were usually more important than central authorities in matters of commerce, work, and life in general (Minns et al. 2020). Most cities in Germany and Europe had an oligarchic political structure (Pirenne 1958), without substantial formal checks on the rulers through popular participation in elections and possibilities for holding politicians accountable. 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, p. 13; Wahl 2019, p. 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. Urban 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, p. 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, p. 331). These norms would have broken the link between closed political institutions and enrichment from public office in Figure 1. Max Weber has famously argued along the lines of the civic-mindedness narrative 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, pp. 121–26). This would imply that political elites would even tend to lose wealth because of holding office (see Bátori 2007, p. 90; Friedrichs 2000, p. 19), potentially contributing to *equality* of wealth.

This romantic narrative is in contradiction with a more pessimistic, economic interpretation of what political and economic elites in history were: small, self-interested, powerful distributional coalitions that dominated and extracted resources from the rest of the population in a political system that allowed them to do so. Such extractive behavior 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 dead-weight losses on the local economy (Olson 1982; Ogilvie 2007; 2019; Acemoglu 2008; Stasavage 2014). My data make it possible to shed light on whether the more romantic and historical or the more pessimistic and 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.

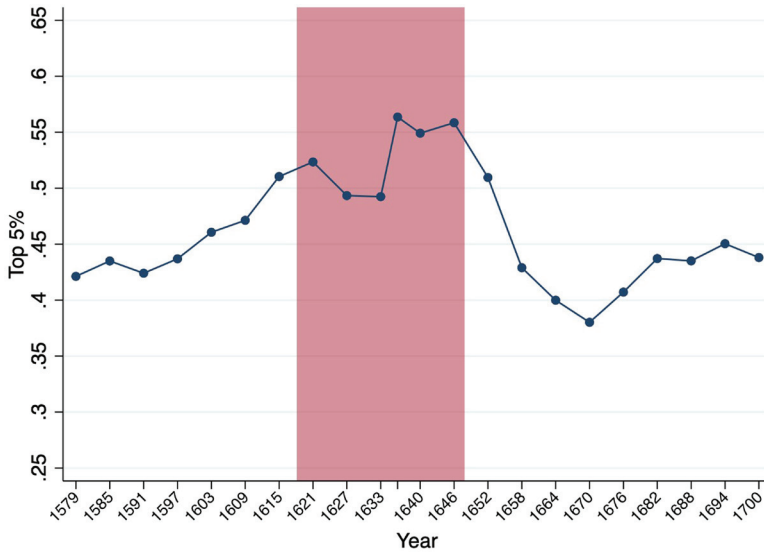


FIGURE 2
WEALTH SHARE OF THE TOP 5 PERCENT IN NÖRDLINGEN (1579–1700)

Notes: The shaded box represents the period of the Thirty Years' War.

Sources: See the main text.

The Free Imperial City of Nördlingen and Its Oligarchic Political System

Nördlingen was one of the 87 Free Imperial Cities—*independent city-states, not dissimilar to Italian city-states*—that existed in the sixteenth century. It was well known for its manufacturing sector, especially woolen textile production, and long-distance trade. Two great medieval trade routes intersected in Nördlingen, and the city hosted an annual fair. From the fifteenth century onward, trade routes and fairs started to decline in importance (Friedrichs 1979, pp. 6–7, 79). However, wealth concentration as measured by the top 5 percent share increased almost constantly (see Figure 2), 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 associated with inequality decline almost everywhere in Germany (Alfani, Gierok, and Schaff 2022, p. 104). This decline was caused, among other factors, by a decline in trade, a pillar of Nördlingen's economy (Wilson 2009, p. 806).

In the highly politically fragmented Holy Roman Empire, every one of the several hundred independent territories, and indeed, every city,

had its own customs for choosing public officials, leading to a staggering variety of constitutional forms (Friedrichs 2000, p. 13). But similarities existed, and 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 a new, more closed political constitution on a number of imperial cities, including Nördlingen. The Emperor had just won the Schmalkaldic War (1546–1547). 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 characterized by forms of popular participation, such as elections for the city council. Charles wanted to set up political structures that were more stable and predictable for him. The city councils that were set up as a result of his intervention were called "*Hasenräte*," named after the Emperor's delegate Heinrich Has, who oversaw the implementation (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. The introduction of the new system also broke the formal political power of the city's 12 guilds, which had constituted the city government until then. The craft organizations that succeeded the guilds nevertheless remained among the principal special interest groups in Nördlingen (Friedrichs 1979, p. 18). Once selected, a new magistrate was appointed for a lifetime; a total of 108 appointments were made between 1580 and 1720. No protocols or other evidence survive that 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 1), a typical feature of magistrates in preindustrial European cities (Friedrichs 2000, p. 18). Almost all of them came from the top 20 percent of the wealth distribution. The fact that most magistrates were already rich before entering office creates a potential endogeneity problem for the analysis, which my difference-in-differences strategy takes into account.

Magistrates had an enormous breadth of concerns and a high degree of discretion over most aspects of urban life. The council had four broad areas of activity: it was at the same time the legislator of Nördlingen (e.g., regulating commerce), its highest court (e.g., deciding over property rights disputes), its administration (e.g., inspecting manufactured goods), and was in charge of foreign affairs (e.g., deciding over defense

issues; see Online Appendix 1 for historical details). This system endured in Nördlingen and many other German cities until the end of the Holy Roman Empire at the beginning of the nineteenth century (Friedrichs 1979; Bátori 2007).

What compensation did government members receive? At the beginning of the early modern period, magistrates only got a minimal annual expense allowance for attending council meetings (of 2.5 florins) and for city-related travel (Friedrichs 1979, pp. 170–79; Bátori 1990, p. 623). According to an expert on Nördlingen’s political elites, it was expected that “magistrates would not become rich from their service to the city.” It was an honorary office of civic responsibility, “without remuneration.” Only rich people were appointed to the council who were wealthy enough to be able to afford this potentially burdensome office. Magistrates were usually “rich pensioners [...] idle honourable men” (my translation) (Bátori 2007, pp. 89–90, 105). Beyond satisfying one’s civic duty, the only personal economic benefit was thought to come indirectly through the general prosperity of the city (Bátori 2007, p. 118). This was not just true for Nördlingen. Referring to early modern German cities in general, Friedrichs (2000, p. 19) writes: “Cities had salaried bureaucrats, but municipal office-holding was mostly volunteer work: council members were usually paid little or nothing for doing their job. [...] Generally council members received scant remuneration, and holding a high municipal office could actually become a financial burden.”

So far, the expectation, but what compensation did magistrates actually get? In mid-sixteenth century Nördlingen, the costs of magistrates’ compensation have been estimated to be around 3 percent (or about 12 florins per magistrate and year) of the treasury’s budget for officeholders’ compensation. This seems in line with the idea of an honorary office that was scantily remunerated. However, from about the time when the Emperor imposed on Nördlingen and other cities the “*Hasenräte*,” basically an unchecked form of government, we observe a notable increase in magistrates’ compensation. The payments to magistrates increased to 13.5 percent of the annual budget in 1652 and to 18.1 percent in 1700. This corresponded, respectively, to 77 and 126 florins per magistrate and year, which was a considerable sum. Magistrates increased existing and introduced new forms of compensation for their service to the city. First, they more than tripled their expense allowance to 8 florins, which was still modest. Second, they introduced additional fees, paid from the city treasury, to compensate themselves for all sorts of activities: sitting on sub-councils, being mayor, managing city finances, inspecting locally produced manufacturing goods, collecting taxes, judicial, bureaucratic,

and several other activities (Friedrichs 1979, pp. 177–78). These new fees were legal, as they were covered by magistrates' discretion over how to spend the city's money. One could even argue that they were adequate compensation for people of high status. But these new fees were certainly a break with the tradition of government being an honorary office.

In other early modern cities and countries, political elites received generous fees directly from the population, for example, in eighteenth-century New York or France (Pomerantz 1965, pp. 38–39; Ertman 1997, pp. 100–2). In Nördlingen and other German cities (see Isenmann 2014, pp. 398–402), the historical record suggests that officeholders received, if at all, fees from the city treasury. However, it cannot be excluded that they also obtained some additional fees directly from the population.

*A Shock to the Potential for Personal Enrichment from Public Office:
The Thirty Years' War (1618–1648)*

The Thirty Years' War provides an occasion to observe the behavior of political elites in a period of a severe socio-economic crisis. This episode is also the closest we can get to an experimental setting that increased political elites' potential to enrich themselves from their offices in a plausibly exogenous way.

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 Nördlingen's population of about 2.3 million florins during the war, not counting the numerous but not quantifiable non-monetary payments. This was an immense sum for a city with 1,000–2,000 taxpayers, 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. How the city would come up with the demanded sums was left to the discretion of the city council. The council had to decide through which channels to obtain the sums, on whom to put the burden, and also for non-cash benefits such as quartering soldiers and how to punish tax evaders. The council of Nördlingen reportedly used the threat of the death penalty to increase people's willingness to pay taxes during the Thirty Years' War (Friedrichs 1979, pp. 28, 118, 152–58, 217; Voges 1988, pp. 257–58). The council 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 1645. Moreover, the town was struck by plague in 1634, most likely brought in by soldiers (Voges 1988, p. 241; Zipperer 1979, pp. 123, 130). Consequently, the number of households declined during the war by about 49 percent, and real wealth declined *per capita* by about 34 percent (Friedrichs 1979, pp. 42, 113).

Two scenarios are possible during the Thirty Years' War. On the one hand, it might have been an opportunity for political elites to enrich themselves even further, thus increasing their personal wealth and the town's overall inequality. This was because the war increased patronage opportunities for political elites and the quantity of resources they had to administer. The socio-economic chaos of the war might have been an ideal veil for covering personal enrichment from public office in a context of few checks on government activity (see Quarthal 1979; Querubin and Snyder 2013). On the other hand, it might also have 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 1958; Bátori 2007, p. 90; Isenmann 2014, pp. 330–31). In that scenario, one would expect to see no increase either in the personal wealth of political elites or in wealth concentration.

DATA

I construct two different datasets for the empirical analysis, the first at the level of German cities and the second at the level of individual taxpayers in a specific city.¹ To construct the first dataset, I take panel data on top wealth shares in 33 early modern German cities from Schaff (2022). The panel is unbalanced, as not all cities are observed over the whole period of study (1500–1800). Inequality is measured based on the entire wealth distribution of the taxpaying population. A data point refers, for example, to the top 10 (or top 5 or top 1) percent wealth share of Augsburg in 1500, another one to Augsburg in 1525, and so on. Additionally, I have collected information about a defining characteristic of the closedness of cities' political systems from the *Deutsches Städtebuch* (for more information about the dataset see Online Appendix 2): whether entry

¹ All data and replication files can be accessed on openICPSR (see Schaff 2024).

onto 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 participatory elections in a city in a given year and 0 otherwise (see Wahl (2019) for an analogous approach). Participative elections mean that at least a part of the citizenry that was not sitting on the government voted directly or through an electoral college for new members of the government. Elections must have been a regular and not an exceptional means of selecting government members, and in many places, government elections were held annually. Given the 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 large the share of the citizenry was that was eligible to vote, which groups of the population could vote, or whether certain groups of the population had a quota of government seats. The alternative to elections was usually co-optation, as in Nördlingen. Under that system, the council recruited itself through sitting members selecting new members. This is interpreted as a higher degree in closedness of the political structure (Friedrichs 2000, p. 13; Wahl 2019, p. 197).

The second panel dataset is based on the property tax registers of Nördlingen, available from 1579 until 1700. These registers cover all of the city's citizens, giving information about the name and surname of a taxpayer, property tax payment, gender, and occupation. The beginning of taxation was usually when an independent household was set up. 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 (an example is provided in Online Appendix 3). 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 that kept such orderly tax registers. They make it possible to easily link individuals over time. 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).

I have hand-digitized Friedrichs' c.27,000 paper-based taxpayer-year records and double-checked the correctness of the transcription of several hundred of them with the original sources. These cover c.6,000 individuals, usually in steps of six years, but between 1633 and 1640, the year 1636 is also available, 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 proxy for age. This somewhat reduces the number of observations. Considering the time period

under study, the data are exceptionally granular. I have then 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. 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 analyze 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 from the secondary literature on which individuals were magistrates, that is, who had a seat on the city council, who was mayor (*Bürgermeister*), and for which years these offices were held by that taxpayer. Online Appendix 3 provides more information about how the dataset and its main variables were constructed.

The general rule was that all mobile and immobile property inside and outside the city was taxed, but there were no lists of taxable items for the period of study. We know that real estate is usually assessed at its most recent purchase price, but it is not entirely clear whether different assessment criteria were applied to other asset categories. Note that neither the Friedrichs records nor the original archival sources record the composition of the property. It cannot, therefore, be entirely excluded that changes in reported wealth reflect a shift in the composition of an individual's property. However, in practice, this is unlikely to be a major issue for two reasons. First, as mentioned earlier, "all" property was in principle subject to taxation. Second, if such shifts toward more valuable assets were possible, they would only bias my results if they happened after an individual became part of the government. 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, and fines for tax evaders were heavy, sometimes reaching a multiple of one hundred times and more of the evaded amount (Friedrichs 1979, pp. 98–101).

In Nördlingen, as in most German communities, propertyless individuals were included in the tax registers. About 3.1 percent of the observations used in the analysis are people with zero taxable property. Additionally, it is important to keep in mind that this panel dataset records property or wealth, not only income. If it were just (pre-tax) income, one could argue that council members might have spent their earnings for charitable purposes, thus redistributing them to the community. I have not found any evidence for such civic-minded spending on

TABLE 1
TAXPAYER CHARACTERISTICS COMPARED
TO MAGISTRATES BEFORE COUNCIL MEMBERSHIP

	(1) β Council	(2) SE	(3) Mean
Wealth (Log) pre council	2.49	(0.12)	5.29
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
Share of women pre council	-0.03	(0.01)	0.03
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. Column (2) displays standard errors in parentheses. Column (3) provides the mean of the dependent variable in the whole population.

Sources: See the main text.

a systematic scale. But more importantly, even if there were charitable spending in time t , then the property estimate in time $t + 1$ would be net of this spending.

Table 1 shows systematic differences between later council members and the rest of the population (estimated with bivariate regressions). Column (1) shows how much taxpayers who became magistrates in the next period of observation differed from the whole population (Column (3)) before entering the council. The figures confirm that later council members were richer and ranked higher in the local wealth distribution than those who would not become council members. 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 not poor, that is, had more than one florin of total wealth. This probably reflects the fact that wealthy individuals could live on 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 the most frequent occupation in Nördlingen but was usually associated with a low socio-economic class.

CITY-LEVEL EVIDENCE: WEALTH INEQUALITY AND ELECTIONS

To obtain a first impression of the impact of oligarchic urban political institutions on inequality, I first analyze the city-level dataset to

investigate the relationship between the presence of participative elections to the city council and top-level wealth concentration. Whether the presence of elections had an impact on inequality probably depended, as mentioned, on many unmeasured factors. Without a doubt, the democratic character of elections in the preindustrial era was very limited. Many groups, such as women, individuals without full citizenship, who were unmarried, born illegitimately, simply poor, or Jews, were usually excluded from voting. Even in places with elections, it was a small minority, usually with higher-than-average socio-economic status, that was eligible to participate in the elections (see Isenmann 2014, pp. 350–52). This has to be kept in mind when interpreting the results. Still, one would expect that the presence of regular elections in which only a small part of the population could vote, *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 use top wealth shares as dependent variables in the analysis, first because they are the preferred metric in the literature to measure inequality changes from the top of the distribution, which is what this paper is primarily interested in. Second, top wealth shares make orders of magnitude clearer (Piketty 2020, p. 26). In Online Appendix 4, I also report results using inter-percentile shares as the dependent variable, which are analogous to the ones reported here.² I estimate the following econometric specification:

$$I_{i,t} = \alpha_i + \pi_t + \beta Election_{i,t} + \gamma' X_{i,t} + \varepsilon_{i,t} \quad (1)$$

where $I_{i,t}$ is wealth inequality of locality i in year t ($t = 1500, 1525, \dots$ until 1800), measured as the wealth shares of the top 1, 5, and 10 percent of the population, and $Election_{i,t}$ 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 modeling approach accounts for unobserved factors that might have had an impact on the dependent and independent variables of interest. A full set of city fixed effects (α_i) account for characteristics that are time-invariant and city-specific, such as geographical location. Time fixed effects (years; π_t), which account for shocks that might have had an impact on inequality in all cities, such as macroeconomic trends. Hence,

² The Gini coefficient, another possible indicator, is particularly sensitive to changes in the middle of the income or wealth distribution. It is, therefore, not a good indicator for this study, which is interested in inequality from the top of the distribution.

TABLE 2
WEALTH INEQUALITY AND ELECTIONS IN EARLY MODERN GERMAN CITIES
(TOP-WEALTH SHARES)

	(1)	(2)	(3)	(4)	(5)	(6)
	Top 10%	Top 10%	Top 5%	Top 5%	Top 1%	Top 1%
Council elections	-4.429*** (1.484)	-4.457** (1.706)	-5.908*** (2.003)	-6.097*** (2.184)	-5.198** (2.240)	-5.468** (2.231)
Controls	No	Yes	No	Yes	No	Yes
City FE	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	241	241	241	241	241	241
Cities	33	33	33	33	33	33
R2	0.209	0.215	0.139	0.142	0.100	0.108
Mean of dependent variable	51.94	51.94	36.89	36.89	14.37	14.37

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

Sources: See the main text.

the estimated correlations are identified from time variation within cities, and the coefficients will pick up the effect of introducing elections, where treatment can switch on and off and on again.³

Locality-level controls are included in vector $X_{i,t}$. Unobserved factors are captured with the random error term $\varepsilon_{i,t}$. The standard errors are robust,⁴ clustered at the city level in order to account for the possibility of serial correlation in the error term. To mitigate further the possibility of omitted variable bias, I account for several observable economic, demographic, and institutional characteristics. 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 (van Zanden 1995; Alfani, Gierok, and Schaff 2022; Schaff 2023).

Table 2 reports the results. In cities that regularly held participatory elections to the council, the concentration of wealth at the top of the population and thus inequality were significantly lower. The quantities are sizeable, especially for the top 5 and 1 percent, which is unsurprising given that council members were often part of this very rich

³ The data structure of the city-level analysis does not correspond to a conventional difference-in-differences (DiD) setup because elections could be introduced, then abolished, then reintroduced again, and so on. Treatment could also happen before the period of analysis. Given these *multiple* shocks with non-treatment periods in between, it is impossible to define pre- and post-treatment periods. The new DiD literature (see De Chaisemartin and D’Haultfœuille 2020) has proposed solutions for conventional DiD settings with a single shock. To the best of my knowledge, there is currently no option for the unconventional case of multiple shocks.

⁴ Results with bootstrap standard errors are reported in Online Appendix 4.

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.

Overall, these results are in line with the arguments of Puga and Trefler (2014), Alfani (2021), and others, who have conjectured that more closed political institutions were likely to result in higher inequality in preindustrial European polities. Needless to say, one should be cautious when 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 in a clear direction: closed political institutions are associated with higher wealth concentration and inequality. In what follows, I study the micro-level mechanisms behind this relationship. I document in a more robust way the effect of closed politics on wealth accumulation and inequality, using highly granular individual-level data from Nördlingen.

INDIVIDUAL-LEVEL EVIDENCE: POLITICAL ELITES' WEALTH AND INEQUALITY

Main Analysis

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:

$$W_{i,t} = \alpha_i + \pi_t + \beta(\text{Council}_i \times \text{Post}_{i,t}) + \gamma' X_{i,t} + \varepsilon_{i,t} \quad (2)$$

where $W_{i,t}$ is an indicator of an individual's personal wealth (in logarithm),⁵ 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; $\text{Post}_{i,t}$ is another dummy variable that takes the value one in the years after an individual has joined the city council and zero otherwise; and $X_{i,t}$ is a vector of taxpayer controls, including dummies for the 60 occupational categories Friedrichs (1979) created based on information in the tax registers. I also control for gender and proxy for age

⁵ Those individuals who had zero wealth received a wealth value of 0.1 before the log-transformation. In Online Appendix 5, I construct an alternative variable where I add the value one to all values before the log-transformation.

and age-squared by including a linear and quadratic time trend to capture individuals' position in the life cycle. This is admittedly an imperfect proxy, but the best available one. In order to capture all individuals from the beginning of their tax payments, I limit the analysis to individuals who enter the registers in 1585 or later. Taxpayer and year fixed effects are α_i and π_p , respectively. Specifications with linear and quadratic time trends are the baseline, but I will also report results with taxpayer and year fixed effects only. The standard errors have been obtained by block bootstrapping and are clustered at the taxpayer level to account for the possibility of serial correlation in the error term.

Recent developments in the estimation of staggered difference-in-differences (DiD) designs have shown that conventional two-way fixed-effects specifications (TWFE) can lead to severely biased estimates of average treatment on the treated (ATT) effects. This happens because of heterogeneity in treatment effects across time and units (De Chaisemartin and D'Haultfœuille 2020). The main results are, therefore, obtained using the De Chaisemartin and D'Haultfœuille (2020, 2022) estimator, which is robust to heterogeneous treatment effects. Essentially, it produces a re-weighted average of all single treatment group-period DiD estimates and compares outcomes across time and units in a more restrictive way to avoid bias. This comes at the cost of lower statistical precision because the estimator uses not all observations when comparing treated and untreated units.⁶ In Online Appendix 5, I report results with TWFE specifications.

Table 3 reports the effect of city council membership on taxpayer wealth and how having a political office contributed to wealth concentration. Column (3) reports ATTs, Column (4) standard errors, and Columns (5) and (6) confidence intervals at the 95-percent level.⁷ Results in Rows 1, 2, and 3 show that the effect on personal wealth was positive, highly significant, and 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 44.2 to 55 percent after they entered office. Rows 4 and 5 indicate that an individual climbed up 3.3 to 4.2 percentiles in the wealth distribution. Similarly, Rows 6 and 7 suggest an increase of 6.5 to 10.2 percent in the likelihood of being in the top 5 percent of the

⁶ To calculate the simple DiD estimates, I use the Stata command “did multiplgt,” developed by De Chaisemartin and D'Haultfœuille (2020, 2022). In some saturated specifications with few observations, the command does not account for all controls.

⁷ Another possible alternative to estimate effects on different parts of a distribution would be quantile regression. However, currently available estimators require a large time dimension relative to the number of units (n/T below ten) to be valid (Machado and Santos Silva 2019, p. 151), which is not the case here. Moreover, to the best of my knowledge, there is no treatment heterogeneity estimator like De Chaisemartin and D'Haultfœuille (2020, 2022) that follows a quantile regression approach.

TABLE 3
POLITICAL OFFICE AND WEALTH (DE CHAISEMARTIN AND D'HAULTFŒUILLE
(2020, 2022) DIFF-IN-DIFF ESTIMATES)

Nr	(1) Y	(2) Specification	(3) ATT	(4) SE	(5) Up. 95% CI	(6) Lo. 95% CI	(7) N
<i>Treatment: Council member × Post</i>							
1	In-Wealth	Only FE	0.550	0.162	0.868	0.232	16315
2	In-Wealth	Baseline	0.526	0.162	0.843	0.210	16315
3	In-Wealth	Baseline + Controls	0.442	0.183	0.802	0.083	16315
4	Percentile	Baseline	4.183	1.467	7.059	1.307	16315
5	Percentile	Baseline + Controls	3.290	1.725	6.672	-0.092	16315
6	Top 5%	Baseline	0.102	0.049	0.198	0.007	16315
7	Top 5%	Baseline + Controls	0.065	0.056	0.176	-0.045	16315
<i>Treatment: Mayor × Post</i>							
8	In-Wealth	Baseline	0.302	0.152	0.601	0.004	9727
9	In-Wealth	Baseline + Controls	0.339	0.184	0.700	-0.021	9727

Notes: Regression estimates of the effect of becoming a council member on ln-wealth, wealth percentile, and the probability of being part of the Top 5% of the wealth distribution, following De Chaisemartin and D'Haultfœuille (2020, 2022). This consists of estimating aggregate impacts by comparing all changers with non-changers when treatment begins. All regressions include a full set of taxpayer and time fixed effects. The baseline specification also includes linear and quadratic time trends. In the specifications in Rows 8 and 9, I also control for Council member × Post. Block-bootstrapped standard errors are clustered at the household level. Confidence intervals indicate significance at the 95-percent level.

Sources: See the main text.

wealth distribution. In other words, becoming a council member did not just enrich those specific individuals but also contributed to a greater economic polarization of society, that is, greater inequality.

In Rows 8 and 9, I 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 personal enrichment and inequality, 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 increased their personal wealth by 30.2 to 33.9 percent on top of the gains of ordinary council membership, which is held constant. These results already give us a first hint about the potential mechanisms at work.⁸

⁸ In Online Appendix 5, I repeat the main analysis with only the males in the dataset, which does not change the results.

Were there differential pre-trends before a taxpayer became a city council member? That is, did individuals who later became magistrates experience more rapid wealth growth before treatment began?⁹ In fact, some degree of a pre-trend would be historically unsurprising, given that magistrates often were part of the city court for a few years before entering the government in Nördlingen, which was already a powerful and potentially profitable position. Additionally, one might ask how the effect of holding a political office on personal wealth and wealth concentration evolved over time. To address these issues, I estimate the following flexible difference-in-differences model, using again the treatment effect heterogeneity-robust estimator of De Chaisemartin and D'Haultfœuille (2020, 2022):¹⁰

$$W_{i,t} = \alpha_i + \pi_t + \sum_{t=-2}^7 \beta(\text{Council}_i \times \text{PeriodsToCouncil}_{i,t}) + \gamma' X_{i,t} + \varepsilon_{i,t} \quad (3)$$

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 period dummies ($\text{PeriodsToCouncil}_{i,t}$) covering the individual pre- and post-treatment periods. The betas (β) are the main coefficients of interest.

In Figure 3, I plot the estimates, taking ln-wealth (Panel A), the wealth percentile (Panel B), and the probability of being part of the top 5 percent (Panel C) as outcomes. Across all three panels, the picture is similar: we see substantial increases in the three outcomes after treatment begins, regardless of whether controls are included. Lower significance levels are most likely the result of fewer available observations and of the alternative estimator not using all available observations. However, the pre-treatment estimates require close attention. Panels B and C report results that are not statistically different from zero. Instead, in Panel A, the pre-treatment coefficients follow a parallel trend in periods minus two and three, but they are significantly negative. This implies a positive pre-trend between the period minus two and the omitted base period (minus one), that is, just before treatment began.¹¹

⁹ There can be selection on traits that are correlated with wealth levels, such as kinship. As long as the trends are the same, the average treatment effect of those treated (ATT)—a change in trends, not levels—can be calculated (see Angrist and Pischke 2009, p. 230).

¹⁰ To calculate the flexible DiD estimates, I again use the Stata command “did multiplgt,” with the “robust dynamic” option (De Chaisemartin and D'Haultfœuille 2020, 2022). It makes it possible to calculate “placebo” estimates, which mimic estimates of dynamic effects in the pre-treatment period.

¹¹ In Online Appendix 5, I report a placebo test where treatment begins one period later. The test suggests that the results reported here do not depend on the reference category chosen.

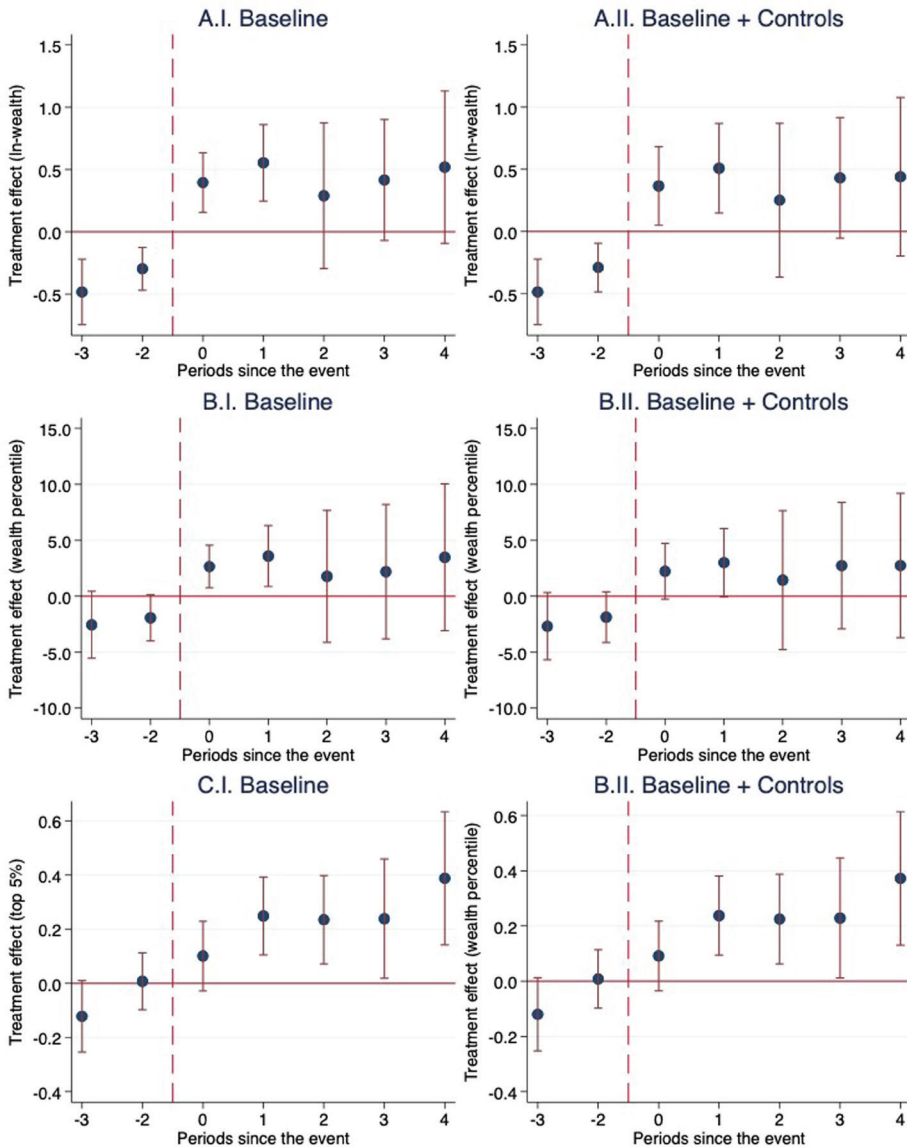


FIGURE 3
POLITICAL OFFICE AND WEALTH (DE CHAISEMARTIN AND D'HAULTFŒUILLE
(2020, 2022) FLEXIBLE DIFF-IN-DIFF ESTIMATES)

Notes: Regression estimates of ln-wealth (Panel A), wealth percentile (Panel B), and the probability of being part of the Top 5% of the wealth distribution (Panel C) before and after becoming a city council member (vertical dashed line), following De Chaisemartin and D'Haultfœuille (2020, 2022). This consists of estimating aggregate impacts by comparing all changers with non-changers in the respective period. The omitted base is period -1. All regressions include a full set of taxpayer and time fixed effects, a linear and a quadratic time trend. Block-bootstrapped standard errors are clustered at the household level in parentheses. Confidence intervals indicate significance at the 95-percent level.

Sources: See the main text.

What could have been the historical reasons for political elites' growing wealth just before they entered office? As mentioned, this result is not entirely unexpected. One possible explanation is that later magistrates were often members of the city court for a few years before being selected for the city council. The court was below the council in terms of power and prestige, as its jurisdiction was limited to certain types of civil cases, especially disputes over debts. Yet the court was a "testing ground" for later magistrates. About two-thirds of council members might have served on the court before the council, but precise information about who was part of the city court and when is unfortunately not available (Friedrichs 1979, pp. 13, 173). It is not implausible that court members used their jurisdictional power for personal enrichment in Nördlingen—court members were often suspected of enriching themselves through patronage in seventeenth-century Germany (Grüne 2011, pp. 303–04)—therefore causing a positive pre-trend in magistrates' wealth. These flexible results might not be ideal in terms of precisely estimating the causal effect of council membership on personal wealth, but they seem in line with the historical process of becoming a magistrate.

Mechanisms and Discussion

In this section, I investigate through which mechanisms enrichment by the oligarchic political elite might have occurred.

A very first possibility could be that enrichment had nothing to do with the political office as such, but with inheritance. Council membership was for a lifetime, that is, until death, and certain elite families had a "council tradition," meaning that sons followed their fathers onto the council (Friedrichs 1979, pp. 170–72). The wealth increase could simply be the result of some individuals inheriting the (potentially high) wealth of their deceased fathers while following them on the council at about the same time. Then the observed effect could have nothing to do with personal enrichment from officeholding. To investigate this possibility, I have constructed a variable that proxies this "council-inheritance" effect, using information about surnames. The variable indicates whether within one period (six years) prior to a new council member entering office, a magistrate with the same surname exited from the council, which usually indicates the end of a lifetime. These coincidences most likely capture the inheritance of the father's wealth and seat on the council fairly well. In my data, about 18 percent of new council members had a person with the same name exiting the council within one period. In Columns (1) and (2) of Table 4, I interact this time-invariant variable with council membership

TABLE 4
MECHANISMS: INHERITANCE, AND THE WEALTH OF MERCHANTS AND
CITY CLERKS (DE CHAISEMARTIN AND D'HAULTFŒUILLE
(2020, 2022) DIFF-IN-DIFF ESTIMATES)

Nr	(1) Y	(2) Specification	(3) ATT	(4) SE	(5) Up. 95% CI	(6) Lo. 95% CI	(7) N
<i>Treatment: Council member × Post × Council inherit</i>							
1	ln-Wealth	Baseline	0.264	0.291	0.835	-0.307	9148
2	ln-Wealth	Baseline + Controls	0.360	0.356	1.058	-0.338	9148
<i>Treatment: Council member × Post</i>							
3	ln-Wealth	Nr. 1 + Council inherit	0.526	0.162	0.843	0.210	16315
4	ln-Wealth	Nr. 2 + Council inherit	0.442	0.183	0.802	0.083	16315
<i>Treatment: Council member × Post × Merchant</i>							
5	ln-Wealth	Baseline	0.537	0.443	1.405	-0.331	7036
6	ln-Wealth	Baseline + Controls	0.565	0.459	1.465	-0.335	7036
<i>Treatment: City clerk × Post</i>							
7	ln-Wealth	Baseline	0.550	0.297	1.133	-0.032	12782
8	ln-Wealth	Baseline + Controls	1.632	0.557	2.723	0.541	12782

Notes: Regression estimates of the effect of becoming a council member that “inherited” the seat on the government (Rows 1 and 2), of becoming a council member (Rows 3 and 4), of becoming a council member while also being a merchant (Rows 5 and 6), and of being a city clerk (Rows 7 and 8) on ln-wealth following De Chaisemartin and D’Haultfœuille (2020, 2022). This consists of estimating aggregate impacts by comparing all changers with non-changers when treatment begins. All regressions include a full set of taxpayer and time fixed effects, and linear and quadratic time trends. In the specifications in Rows 1, 2, 5, and 6, I also control for Council member × Post. In the specifications in Rows 3 and 4, I also control for Council member × Post × Council-inherit. In the specifications in Rows 5 and 6, I also control for being a merchant. Block-bootstrapped standard errors are clustered at the household level. Confidence intervals indicate significance at the 95-percent level.

Sources: See the main text.

in the individual post-period. The coefficient is not precisely zero, but far away from statistical significance. More importantly, in Rows 3 and 4, I run the same regressions, but I use council inheritance interacted with council membership in the individual post-period as a control and not as a treatment variable to evaluate whether it substantially reduces the main treatment effect of interest. The ATTs and confidence intervals are practically identical to the ones in Rows 2 and 3 of Table 3. This suggests that a council-inheritance effect is unlikely to play a significant role in explaining magistrates’ enrichment.

One-fifth of council members presumably “inheriting” the seat in the government from a relative has another implication. It indicates how little churn there was in the political elite. It implies that the enrichment of a magistrate did not just contribute to that individual’s upward social mobility, which would be the case in a system where the old guard

is constantly replaced with new members. In Nördlingen, becoming a magistrate made a restricted circle of people richer and richer, contributing to wealth concentration. This dynamic fits well with the increasing wealth concentration in Figure 2, and the practice of co-optation.

If one accepts the premise that the observed enrichment has something to do with the opportunities offered by the political office as such, then there exist, as mentioned, at least four not entirely exclusive hypotheses. First, enrichment might have been driven by the increasingly generous compensation officeholders received for their activities. We can almost be certain that increasing compensation mattered to some extent because we know that Nördlingen's magistrates paid themselves increasing fees from the treasury (see Friedrichs 1979, pp. 177–78). Second, political office could have given those in power profitable opportunities for their own businesses. Third, patronage could have played a role, that is, receiving money for regulating the economy and society in a way that was more favorable to some than to other interest groups, such as guilds. Fourth, there could have been embezzlement of public money. All four mechanisms imply that political elites would have derived some personal material benefit from holding public office. Systematic evidence for any of the four possibilities is extremely difficult to provide, especially for things like patronage or embezzlement that were often suspected but were usually impossible to prove for contemporaries (Boockmann 1998, p. 367). Therefore, one has to rely on indicative quantitative and qualitative evidence.

The most systematically testable one is the possibility that enrichment of officeholders was the result of better business opportunities. I investigate the effect on personal wealth of being a council member while at the same time working as a merchant. Merchants were a well-represented group on the council of Nördlingen, and they had broad opportunities to conduct business (Friedrichs 1979, p. 175). If emerging profitable business opportunities explained the personal enrichment of magistrates, then one would expect this effect to show up, especially among merchants. In Rows 5 and 6 of Table 4, I test this possibility by interacting council membership with being a merchant, while holding council membership and being a merchant are separately constant. The coefficients are again not precisely zero, but are far away from statistical significance, regardless of whether controls are added or not. To be fair, the non-zero coefficients allow for better business opportunities playing a minor role in the enrichment of magistrates, or simply for all occupations benefiting to a similar extent from better business opportunities. In Online Appendix 5, I report additional results for other well-represented professional groups—retailers, taverners, and food producers—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 better business opportunities can account for the observed wealth increase of political elites. The bulk of the increase in wealth likely came from other sources, such as increasingly generous compensation, patronage, or embezzlement.

In Rows 7 and 8, 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 tax collectors. They were the magistrates' helpers in running the city administration. Being a clerk was usually a full-time occupation, so they could not be merchants at the same time, like the magistrates. Historians studying officeholding in preindustrial times have argued that personal enrichment of officials through patronage, but sometimes also embezzlement of public money, was very frequent in cities. The revenues were often taken in by lower-level officials because they were in more direct contact with the population, physically administered the difficult-to-monitor money, and then shared it with their superiors, that is, the political elite (van Klaveren 1957, pp. 299, 322; Quarthal 1987, pp. 41–42). If this is true, then one would expect that the city clerks experienced an increase in their personal wealth too. Rows 7 and 8 of Table 4 suggest that being a city clerk in the individual post period,¹² instead of being a council member, led to substantial personal enrichment, by 55 to 163 percent if one reads the change in log points as a lower bound percentage change. Without controls, the estimate is just insignificant, with controls being highly significant. The flexible estimates in Figure 4 confirm this result. City clerks did not have significant wealth differences before entering their office, but only after their individual post-period began. I interpret this evidence as indicative of patronage and embezzlement of public money by magistrates and clerks, likely playing a role in their personal enrichment.

There also exists qualitative evidence suggesting that patronage, but possibly also embezzlement, were part of the mechanism behind political elites' private enrichment. For instance, Ogilvie (2019) reports in her "Qualitative Guilds Database" a telling incident from the year 1620 where the weaver craft organization incurred (an unfortunately unspecified amount of) costs to lobby Nördlingen's political authorities, to enforce entry barriers against an outside individual. In other words, craft organizations obtained privileges from the town government, and political elites received "gifts" in return, which were effectively bribes (Ogilvie 1997, pp. 372–73).

¹² To avoid once treated units being later counted as non-treated, for example, because a city clerk stops working, I consider city clerks to be treated as long as they are in the tax registers.

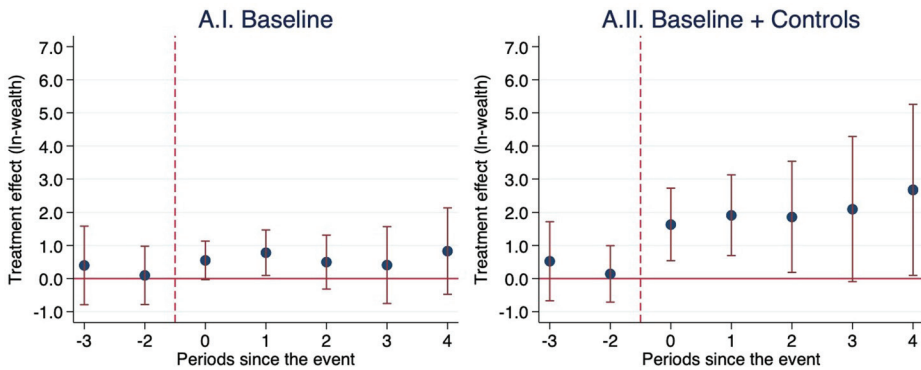


FIGURE 4

MECHANISMS: WEALTH OF CITY CLERKS (DE CHAISEMARTIN AND D'HAULTFŒUILLE (2020, 2022) FLEXIBLE DIFF-IN-DIFF ESTIMATES)

Notes: Regression estimates of ln-wealth before and after becoming a city clerk (vertical dashed line), following De Chaisemartin and D'Haultfœuille (2020, 2022). This consists of estimating aggregate impacts by comparing all changers with non-changers in the respective period. The omitted base is period -1 . All regressions include a full set of taxpayer and time fixed effects, a linear and a quadratic time trend. Block-bootstrapped standard errors are clustered at the household level in parentheses. Confidence intervals indicate significance at the 95-percent level. *Sources:* See the main text.

Embezzlement of public money is even harder to demonstrate. Rarely do we have evidence as clear as, for instance, late medieval Florence, where it was possible to document how the city's political leaders "raided" the public budget, "returning back home [from the city treasury] carrying sacks full of money" thanks to false invoices and other forms of embezzlement (Brown 1998, pp. 518–20). For Nördlingen, evidence of this kind seems entirely lost. However, this does not mean that it did not happen. For example, in the nearby city of Ehingen, the mayor and high officials were accused of having stolen several hundred thousand florins before and during the Palatine Succession War (1688–1698) (Quarthal 1979; see Online Appendix 1 for more historical details). It is probably not far-fetched to believe that these cases of patronage and embezzlement contributed to the personal wealth of the political elites involved.

The previous discussion of mechanisms can detect only some possible ones, but not others. Another hypothesis to explain why council members became richer could be that they were subject to more scrutiny and less able to evade taxes once they entered office. This hypothesis is not testable with the available data, but it is not very plausible for two reasons. First, tax evasion was, as mentioned, punished very severely and even religiously sanctioned in Nördlingen. Constantly hiding property by the amounts reported in Rows 1 to 3 of Table 3 would have been extremely risky. Second, increased scrutiny of council members assumes that

magistrates checked each other's behaviors. This is theoretically possible, but it is equally possible that magistrates acted as an enrichment cartel (see Olson 1982; North, Wallis, and Weingast 2009, pp. 18–21, 191). Given that magistrates were essentially the absolute rulers of Nördlingen, holding legislative, executive, and judiciary power, and given that they were in office for a lifetime without the possibility of being held accountable, it is hard to see any incentives for these elites to control each other instead of acting like an enrichment cartel.

PERSONAL ENRICHMENT FROM PUBLIC OFFICE AND WEALTH CONCENTRATION IN TIMES OF CRISIS

Main Analysis

The results in the previous section suggest that future magistrates experienced differential wealth growth just before entering office. The reason could lie in later magistrates previously serving on the city court. In addition to this first endogeneity problem, it could also be 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 expectations about future wealth accumulation of membership candidates. The only way to get around these identification problems is to exploit a setting that provides plausibly exogenous variation.

In this section, I exploit such variation in the potential for political elites to enrich themselves, 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 behavior 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 enrich oneself—given the extraordinary amount of resources involved—and to use the war as a veil for covering that enrichment was considerable.

I estimate regressions of the following form, similar to Equation (2), using again the estimator of De Chaisemartin and D'Haultfœuille (2020, 2022).¹³

$$W_{i,t} = \alpha_i + \pi_t + \beta_1(\text{Council}_i \times \text{Post}_{i,t} \times 30\text{YearsWar}_t) + \beta_2(\text{Council}_i \times \text{Post}_{i,t}) + \gamma' \mathbf{X}_{i,t} + \varepsilon_{i,t} \quad (4)$$

¹³ In Online Appendix 5, I report results with TWFE specifications for the analysis of the Thirty Years' War period.

TABLE 5
POLITICAL OFFICE AND WEALTH DURING THE 30-YEARS' WAR
(DE CHAISEMARTIN AND D'HAULTFÈUILLE (2020, 2022) DIFF-IN-DIFF ESTIMATES)

Nr.	(1) Y	(2) Specification	(3) ATT	(4) SE	(5) Up. 95% CI	(6) Lo. 95% CI	(7) N
<i>Treatment: Council member × Post × 30-Years' War</i>							
1	In-Wealth	Only FE	0.405	0.153	0.704	0.105	3354
2	In-Wealth	Baseline	0.445	0.143	0.726	0.164	3354
3	In-Wealth	Baseline + Controls	0.564	0.179	0.916	0.213	3354
4	Percentile	Baseline	3.584	0.976	5.498	1.671	3354
5	Percentile	Baseline + Controls	4.678	1.460	7.540	1.816	3354
6	Top 5%	Baseline	0.185	0.117	0.414	-0.044	3354
7	Top 5%	Baseline + Controls	0.212	0.121	0.449	-0.025	3354

Notes: Regression estimates of the effect of being a council member during the Thirty Years' War on In-wealth, wealth percentile, and the probability of being part of the Top 5% of the wealth distribution, with respect to all other taxpayers, following De Chaisemartin and D'Haultfœuille (2020, 2022). This consists of estimating aggregate impacts by comparing all changers with non-changers when treatment begins. The period of analysis is 1603–1646. All regressions include a full set of taxpayer and time fixed effects, and I control for Council member × Post. The baseline specification also includes linear and quadratic time trends. Block-bootstrapped standard errors are clustered at the household level. Confidence intervals indicate significance at the 95-percent level.

Sources: See the main text.

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. Wealth and inequality changes of council members during the Thirty Years' War are captured by β_1 . 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 who were observed before and after the beginning of the war. The results should be interpreted as reduced form or intention-to-treat estimates because we cannot observe personal enrichment from public office—through one of the potential channels discussed previously—as such. We only observe the “invitation” to do so, that is, office-holding during the war.

For all three outcome variables, the results in Table 5 point toward greater enrichment and inequality, with or without controls. The results in Rows 6 and 7, where the outcome is being in the top 5% of the wealth distribution, are not statistically significant. The reason is probably that council membership (which is controlled for) already increases that probability considerably, so the remaining variation that the war can generate is small. If we read, for example, the coefficients in Rows 1 to 3 as lower bound percentage changes of wealth, then the war increased personal

wealth by 40.5 to 56.4 percent relative to the rest of the population, on top of council members' ordinary wealth accumulation.

Can we get a better sense of how large the estimated effects are? In Online Appendix 5, I repeat the analysis but take actual wealth as an outcome variable. In the baseline specification, an individual magistrate gained about 1,997 to 2,151 florins due to the war, about 22 to 24 times the median household wealth at the end of the war. Consider that there were 15 magistrates, and the reported military exactions from the population amounted to 2.3 million florins. Then a back-of-the-envelope calculation suggests that the total additional enrichment of magistrates due to the war might have corresponded to about 1.30 to 1.40 percent of the amount extracted by soldiers. It would also be informative to compare the effects reported here to those found in similar studies. To the best of my knowledge, there are no studies that use individual-level panel data to estimate the effect of holding an office—or of similar factors—on personal enrichment and inequality in preindustrial times. The closest study is a recent working paper by Belloc et al. (2022) that investigates the costs of corruption for the municipal budget by officeholders belonging to Cosimo de Medici's network in medieval Florence. Their calculations suggest that “the total amount of resources diverted by the Medici's political machine was between a lower bound of 2.75 percent and an upper bound of 10.6 percent of the total amount of total direct taxes in Florence” (Belloc et al. 2022, p. 5).

To see how the effect of office-holding evolves over time, I estimate again a flexible difference-in-differences model (see Equation (3)), employing the estimator of De Chaisemartin and D'Haultfœuille (2020, 2022).

The only difference to Equation (4) is that 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, the insignificant coefficients in the pre-treatment period suggest that the common trends assumption holds. Sitting city council members did not become significantly richer before the war. Yet they became substantially richer once the war began (Panel A), and they climbed up in the wealth distribution (Panel B). Given that during the Thirty Years' War it was not just troops and war that came to Nördlingen but also the plague (in 1634), one might wonder whether the results were also driven by the epidemic. This is possible, but the results in Figure 5 clearly show that the increasing trend in political elites' wealth started before the arrival of the plague.¹⁴ The

¹⁴ In Online Appendix 5, I restrict the sample even further to those magistrates who were not just part of the dataset before the war began but had also entered office earlier. The results are analogous to the ones reported here, which suggests that the selection of richer magistrates onto the council does not drive the observed patterns.

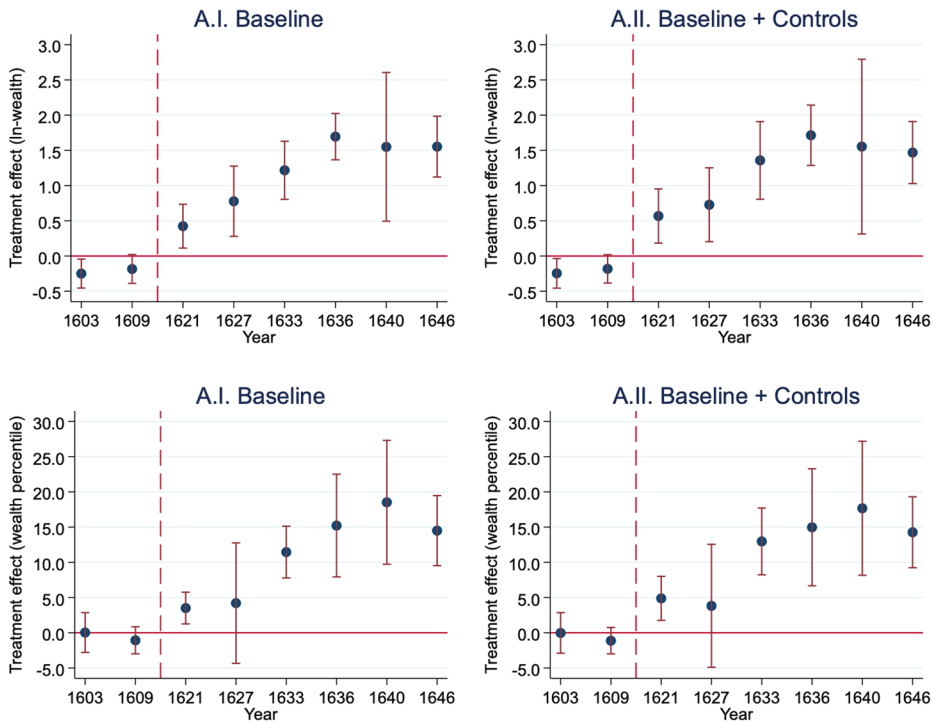


FIGURE 5
POLITICAL OFFICE AND WEALTH DURING THE 30-YEARS' WAR (DE CHAISEMARTIN AND D'HAULTFŒUILLE (2020, 2022) FLEXIBLE DIFF-IN-DIFF ESTIMATES)

Notes: Regression estimates of ln-wealth (Panel A) and wealth percentile (Panel B) of being a council member during the Thirty Years' War (vertical dashed line), following De Chaisemartin and D'Haultfœuille (2020, 2022). This consists of estimating aggregate impacts by comparing all changers with non-changers in the respective period. The omitted base is the year 1615. All regressions include a full set of taxpayer and time fixed effects, a linear and a quadratic time trend, and I control for Council member \times Post. Block-bootstrapped standard errors are clustered at the household level in parentheses. Confidence intervals indicate significance at the 95-percent level. Sources: See the main text.

results are again hard to square with the notion that political elites acted as civic-minded guardians of the common good in times of crisis. The evidence suggests elites were feathering their own nests at the expense of the population when threats from soldiers, battles, and plague gave them an opportunity to do so.

Mechanisms and Discussion

One might ask again what the precise mechanisms were through which this enrichment occurred. In principle, three of the possibilities discussed in the previous section are again plausible: better business opportunities,

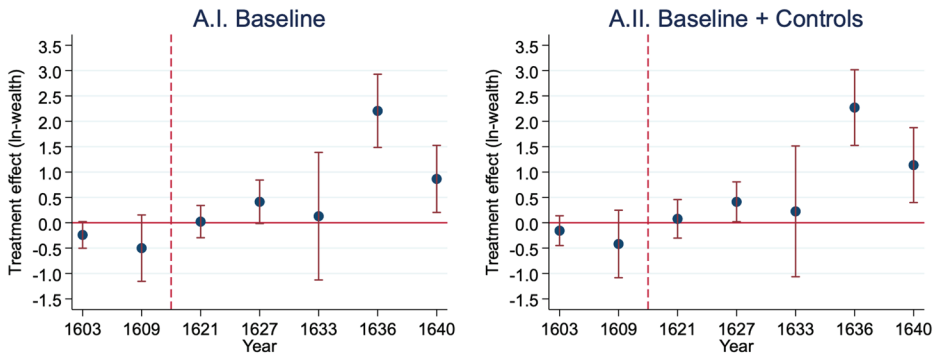


FIGURE 6

CITY CLERKS AND WEALTH DURING THE 30-YEARS' WAR (DE CHAISEMARTIN AND D'HAULTFŒUILLE (2020,2022) FLEXIBLE DIFF-IN-DIFF ESTIMATES)

Notes: Regression estimates of ln-wealth of being a city clerk during the Thirty Years' War (vertical dashed line), following De Chaisemartin and D'Haultfœuille (2020,2022). This consists of estimating aggregate impacts by comparing all changers with non-changers in the respective period. The omitted base is the year 1615. All regressions include a full set of taxpayer and time fixed effects, a linear and a quadratic time trend, and I control for being a city clerk. Block-bootstrapped standard errors are clustered at the household level in parentheses. Confidence intervals indicate significance at the 95-percent level.

Sources: See the main text.

patronage, and embezzlement of public money. Generous compensation of magistrates is less likely to play a role in this *additional* war effect since compensation is captured by ordinary council membership in the individual post-period. The results already suggested that better business opportunities played no important role and are even less likely to have mattered during the war, which destroyed trade routes and commercial opportunities. Yet the estimates in Figure 6 show an interesting pattern; although the results are noisy, they show a clear tendency for city clerks' wealth to grow during the war, but not before, conditional on being a city clerk. In later periods, that enrichment might have been quite substantial. I interpret these estimates as making patronage and embezzlement plausible mechanisms.

The available qualitative evidence for the time of the Thirty Years' War also suggests that patronage might have played a key role in political elites' private enrichment. Fritz (2004, pp. 120–21) found c.1,100 investigations against officials that had taken personal material advantage of their office in late medieval and early modern southwest Germany, for example, by accepting “gifts” in return for administrative favors. His figures suggest that recorded cases more than doubled from the half-century before the Thirty Years' War (1551–1600) to the half-century including the war (1601–1650). Moreover, we have already seen in the earlier discussion that there were incidents of lobbying in the early years of the war, where

the government granted craft organizations privileges in return for material favors (see Ogilvie 2019). The war opened up many new opportunities for officeholders to use their discretion over how to allocate the huge material burden of the event. As mentioned, in Nördlingen, magistrates were a kind of bottleneck through which not less than 2.3 million florins were exerted by soldiers, plus the numerous but not quantifiable non-monetary payments, to avoid plundering and burning by soldiers. The council 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 (often plague-infected) soldiers and in which not, which tax evaders to punish, and from whom to confiscate the non-monetary resources necessary to fulfill soldiers' demands and to defend the city, such as food or equipment. All these war-related activities opened the door for more patronage, which most likely benefited magistrates economically.

But we can also not exclude that embezzlement of public money played some role in magistrates' enrichment during the war and the epidemic. These chaotic years made it difficult to maintain minimum standards of justice. The context of lawlessness, coercion, and predation facilitated the large-scale dispossession of ordinary people to the benefit of those who were legitimized to do so by "the state," such as officials, mercenaries, and military entrepreneurs (Parker 1984). We know of some spectacular incidents, for example, General Albrecht von Wallentein's theft of 96,000 thalers from the treasury of the Bohemian estates in 1619 (Mann 1987, pp. 138–42). But also minor delicts have been documented, for example, city clerks' defrauding public health funds during the 1630/01 plague (Henderson 2019, pp. 242–43, 267–68; more historical examples in Online Appendix 1). It does not require much imagination to see how such embezzlement *could* have also contributed to officials' enrichment in Nördlingen.

A final question is how the results discussed previously 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, Gierok, and Schaff 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 (see Schaff 2023).

CONCLUSION

This paper has investigated the relationship between urban political structure and inequality. I constructed and analyzed two different datasets. At the macro-city level, I found that cities with a more oligarchic

or closed political structure, that is, without participatory elections, had a 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 who 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, and death by epidemics, accelerated politicians' wealth accumulation. I have provided indicative econometric and historical evidence suggesting that this personal enrichment from public office was likely the result of the increasing compensation magistrates received for their activities, but also of patronage and, possibly, of embezzlement of public money. Inheritance or simply better business opportunities for officeholders are unlikely to have played a major role in political elites' enrichment.

These empirical results have four main implications. First, access to political power and the degree of closedness of the political system were important explanations of preindustrial inequality, as hypothesized by Acemoglu (2008), Puga and Trefler (2014), Alfani (2021, 2023), and others. This paper 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 1958; Bátori 2007; Isenmann 2014). When they could, they enriched themselves, contributing to inequality. This enrichment was facilitated by a political system without any checks, especially after the intervention of the Holy Roman Emperor in the sixteenth century.

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' personal enrichment at the expense of the wider population, again contributing to higher inequality. Ultimately, personal enrichment from public office almost certainly inflicted dead-weight 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 industrialization (Allen 2014), then the importance of this urban decline can hardly be overestimated. It is to be hoped that future research

will make more individual-level data available, of the kind that I have constructed for Nördlingen, to provide a broader empirical basis for these conclusions.

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