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

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Abstract

What was the impact of urban political structure on economic inequality in preindustrial times? I document that more closed political institutions were associated with higher economic 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, containing c.27,000 observations on personal wealth and political office-holding in the city state of Nördlingen from 1579 to 1700. I employ a difference-in-differences setting to show that political elites enriched themselves substantially after entering office. Individuals with higher political power enriched themselves more. These private gains from public office contributed to economic inequality. To mitigate concerns about reverse causality, I exploit the Thirty Years' War (1618-1648) as a plausibly exogenous shock to elites' potential for rent-seeking. Political office-holders manipulated this crisis to enrich themselves further, contributing to an unequal wealth distribution. The results are hard to square with a common historical narrative suggesting that urban political elites were "civic-minded" guardians of the common good.

JEL Codes: D31, D72, H20, N43, N93, P48

Keywords: Wealth, Inequality, Elites, Political Economy, Rent-Seeking, Cities

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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 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 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 did 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

¹I use the term “Germany” as a shorthand to refer to the Holy Roman Empire. 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).

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 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 paper 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 2021). 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 2021).

The paper 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 paper 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 paper 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 paper also goes beyond previous studies in documenting the returns to office for a closed political system without any elections.

The paper 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 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, 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, 2002).

The paper proceeds as follows. In Section 2 I discuss theories of urban political elites, explore what they imply for inequality, and provide historical background information. Section 3 introduces the two datasets I have constructed for the empirical analysis. Section 4 describes the econometric strategies and results for the city-level and individual-level analyses. Section 5 concludes.

2 Historical Framework and Background

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 1 summarises this logic.

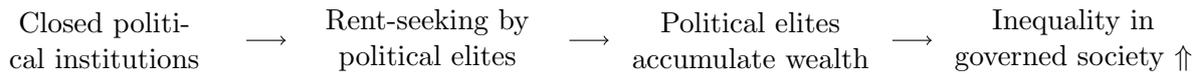


Figure 1: Oligarchic Political Structure and Economic Inequality

However, in preindustrial societies things are postulated to have been very different, especially in 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 preindustrial 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 (Wahl 2019: 202). In political science terms, preindustrial urban governments usually lacked “input legitimacy” (for example, an election coming close to modern democratic standards) (Scharpf 1999). But many historians argue that this did not matter, as oligarchic political elites were capable of “output legitimacy”: the city was run well and policies to preserve social harmony were implemented successfully. The reason that oligarchic political elites governed successfully was that their actions were guided by their civic-mindedness, making these responsible rulers the guardians of the common good of the city. I will refer to this as the “civic-mindedness narrative”. It can be found in its clearest form, for example, in the *Opus Magnum* of the eminent urban historian Eberhard Isenmann, when he 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). In other words, magistrates were guided by strong norms about what it meant to govern a city responsibly, “so 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 1. 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).

Acting in the interest of the common good implies that political elites did not engage in rent-seeking to enrich themselves at the expense of the general population (Rogge 1996: 37). It also implies that their behaviour would probably not increase economic polarisation 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 Bátorí 2007: 90),² potentially contributing to *equality* of wealth. The question I address in this paper is whether there is systematic empirical evidence for the civic-mindedness narrative of urban political elites. I do so quantitatively, by studying their personal wealth and the inequality of wealth in the cities in which they lived.

2.2 The Oligarchic Political System of Nördlingen

Nördlingen’s political system is in many ways representative of 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.³ 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.⁴ 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. Once selected the appointment was for lifetime, and a total of 108 appointments were made between 1580 and 1720 in total. 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 1). 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).

Magistrates had a high degree of discretion over most aspects of urban life,⁵ 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

²For example, Bátorí (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.

³Similar 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).

⁴In Nördlingen, this event broke the formal political power of the city’s 12 guilds that had constituted the city government. The craft organisations that succeeded the guilds remained nevertheless among the principal special interest groups in Nördlingen (Friedrichs 1979: 18).

⁵The 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)

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).

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.

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 Nördlingen'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 1646.⁶ 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).⁷ 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. 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

⁶The tax rate applied to peoples' property was between 0.5 and one percent during the sixteenth and seventeenth centuries (Friedrichs 1979: 158).

⁷The 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)

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.

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 in 35 early-modern German cities from Schaff (2022), where the data are discussed in great detail. This is an extended version of the dataset of Alfani et al. (2022), in which benchmark years have been extended to 25-year intervals. 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). 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*.⁸ 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 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 tax registers of Nördlingen, between 1579 and 1700. 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

⁸For more information about the *Deutsches Städtebuch* and the construction of variables, see the Appendix.

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 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 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 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

Table 1: Taxpayer Characteristics Compared to Magistrates before Council Membership

	(1)	(2)	(3)
	β Council	SE	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. Column 2 displays standard errors in parentheses. Column 3 provides the mean of the dependent variable in the whole population.

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.

4 Empirical Analysis

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:

$$I_{i,t} = \alpha_i + \pi_t + \beta Election_{i,t} + \gamma' \mathbf{X}_{i,t} + \epsilon_{i,t} \quad (1)$$

$I_{i,t}$ is wealth inequality of locality i in year t ($t = 1500, 1525, \dots$ until 1800),⁹ 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. The set up is analogous to a difference-in-difference setting where treatment can switch on and off. $\mathbf{X}_{i,t}$ 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 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

⁹Inequality measures have been clustered around their closest reference year.

Table 2: Wealth Inequality and Elections in Early-Modern German Cities

	(1)	(2)	(3)	(4)	(5)	(6)
	Top 10%	Top 10%	Top 5%	Top 5%	Top 1%	Top 1%
Council elections	-4.429*** (1.482)	-4.457** (1.704)	-5.908*** (2.001)	-6.097*** (2.181)	-5.198** (2.238)	-5.468** (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

behind this relationship. I identify the effect of closed political institutions on individual wealth accumulation and inequality, in a more robust way using highly granular individual-level data from Nördlingen.

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:

$$W_{i,t} = \alpha_i + \pi_t + \beta(Council_i \times Post_{i,t}) + \gamma' \mathbf{X}_{i,t} + \epsilon_{i,t} \quad (2)$$

$W_{i,t}$ is an indicator of an individual's personal wealth (in logarithm),¹⁰ 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. $\mathbf{X}_{i,t}$ is a vector of taxpayer controls, including dummies for the 60 occupational categories Friedrichs (1979) created based on information in the tax registers (see the Appendix for a complete list). I also control for gender, and proxy for age 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 3 reports the effect of city council membership on taxpayer wealth, and how having a political

¹⁰Those individuals that had zero wealth received the wealth value 0.1 before log-transforming the wealth variable.

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 2021: 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 3: Political Office and Wealth (Diff-in-Diff Estimates)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	ln-Wealth	ln-Wealth	Percentile	Top 5%	ln-Wealth	Percentile	Top 5%
Council member \times Post	0.855*** (0.116)	0.787*** (0.153)	4.455*** (1.557)	0.239*** (0.048)	0.753*** (0.152)	4.172*** (1.568)	0.232*** (0.048)
Council member \times Post \times Mayor					0.778*** (0.287)	6.527** (2.830)	0.167* (0.085)
Controls	NO	YES	YES	YES	YES	YES	YES
Age & age-sq.	YES						
Locality FE	YES						
Time FE	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 (or event-study) model:

$$W_{i,t} = \alpha_i + \pi_t + \sum_{t=-2}^7 \beta_t(Council_i \times PeriodsToCouncil_{i,t}) + \gamma' \mathbf{X}_{i,t} + \epsilon_{i,t} \quad (3)$$

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 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 2 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 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.

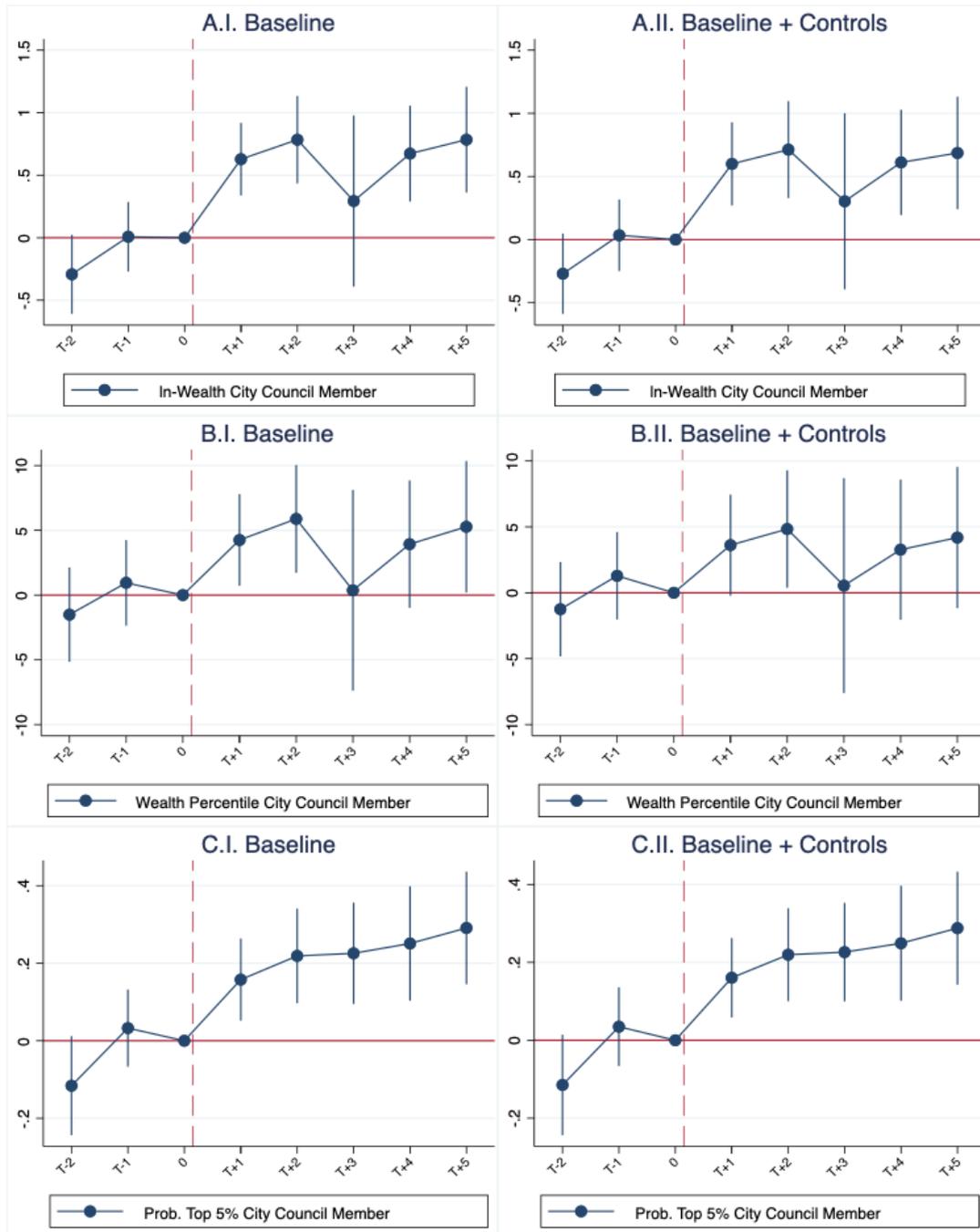
One might wonder whether there was heterogeneity in the effect depending on when the treatment began during the period of observation (1585-1700). This is also important in light of recent critiques of difference-in-differences designs with staggered treatment (see De Chaisemartin and D'Haultfœuille 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 4 the treatment indicator with dummies for three subperiods of the analysis. They indicate the period of observations before (1585-1615), during (1621-1646) 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).

It is reassuring that the coefficients in Columns 1 to 6 in Table 4 all point in the same direction as the average treatment effects in Table 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.¹¹ The results indicate that the financial benefit of holding an

¹¹Another 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

Figure 2: Political Office and Wealth (Flexible Diff-in-Diff 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.

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 the time dimension is small relative to the number of units in my data (see number of observations and taxpayers in Table 3), I do not employ quantile regression.

Table 4: Heterogeneity: Political Office and Wealth (Diff-in-Diff Estimates)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	ln-Wealth	ln-Wealth	Percentile	Percentile	Top 5%	Top 5%	ln-Wealth	ln-Wealth
Council member × Post × Subperiod 1585-1615	0.555** (0.258)	0.386 (0.321)	3.813* (2.189)	1.496 (2.464)	0.134* (0.081)	0.132* (0.079)		
Council member × Post × Subperiod 1621-1646	1.339*** (0.140)	1.297*** (0.159)	6.904*** (2.119)	6.083*** (2.214)	0.226*** (0.068)	0.229*** (0.065)		
Council member × Post × Subperiod 1651-1700	0.562*** (0.130)	0.477*** (0.166)	5.208*** (1.466)	3.843** (1.714)	0.267*** (0.062)	0.271*** (0.060)		
Council member × Post × Low initial wealth							1.964*** (0.499)	1.863*** (0.520)
Council member × Post × Med.-low initial wealth							0.967*** (0.202)	0.881*** (0.258)
Council member × Post × Med.-high initial wealth							1.009*** (0.182)	0.906*** (0.226)
Council member × Post × High initial wealth							0.385** (0.179)	0.382** (0.193)
Controls	NO	YES	NO	YES	NO	YES	NO	YES
Age & age-sq.	YES							
Locality FE	YES							
Time FE	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

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. I argue that this personal enrichment reflects rent-seeking, or in other words, corrupt conduct. Such corrupt conduct could take a variety of forms: embezzlement, theft from public coffers, bribes in return for patronage, and other manipulations of the regulatory and fiscal system. The autonomy of office-holders and the physical challenges of monitoring immense quantities of diverse coins were only two of the administrative challenges that facilitated personal enrichment (Quarthal 1987). Unfortunately, corrupt conduct was almost impossible to prove for contemporaries, as it is for today's scholars (Boockmann 1998: 367). One therefore has to rely on indicative evidence.

To provide such evidence I investigate 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. If growth of private wealth of council members was the result of rent-seeking, then one would expect that also those individuals that assisted the council in their office benefited. In Table 9 I report results from regressions where the treatment variable is being a city clerk in the individual post period instead of being a council member.¹² The coefficients suggest that also council members' helpers enriched themselves substantially, by 81 to 131 percent, if one reads the

¹²To 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.

Table 5: Mechanisms: Wealth of City Clerks (Diff-in-Diff Estimates)

	(1)	(2)
	ln-Wealth	ln-Wealth
City clerk \times Post	0.811*	1.308**
	(0.463)	(0.543)
Controls	NO	YES
Age & age-sq.	YES	YES
Locality FE	YES	YES
Time FE	YES	YES
Observations	21,579	21,579
Taxpayers	4,490	4,490
Mean of dependent variable	7.523	7.523
R^2	0.088	0.104

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

change in log points as lower bound percentage change.

Do the results of this section support the civic-mindedness narrative of urban political elites? In short, they do not. The city-level results in Section 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, probably through the manipulation of the regulatory and fiscal system.

A natural concern is how externally valid the findings from Nördlingen are. 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 very similar 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.

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 2:

$$W_{i,t} = \alpha_i + \pi_t + \beta_1(Council_i \times Post_{i,t} \times 30YearsWar_t) + \beta_2(Council_i \times Post_{i,t}) + \gamma' \mathbf{X}_{i,t} + \epsilon_{i,t} \quad (4)$$

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 as such. We only observe the “invitation” to engage in rent-seeking, that is, office-holding.

Table 6: Political Office and Wealth During the 30-Years' War (Diff-in-Diff Estimates)

	(1)	(2)	(3)	(4)	(5)	(6)
	ln-Wealth	ln-Wealth	Percentile	Percentile	Top 5%	Top 5%
Council member \times Post \times 30-Years' War	1.048*** (0.270)	1.032*** (0.270)	5.975** (2.997)	6.069** (3.057)	0.204* (0.120)	0.190* (0.114)
Council member \times 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. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 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 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.

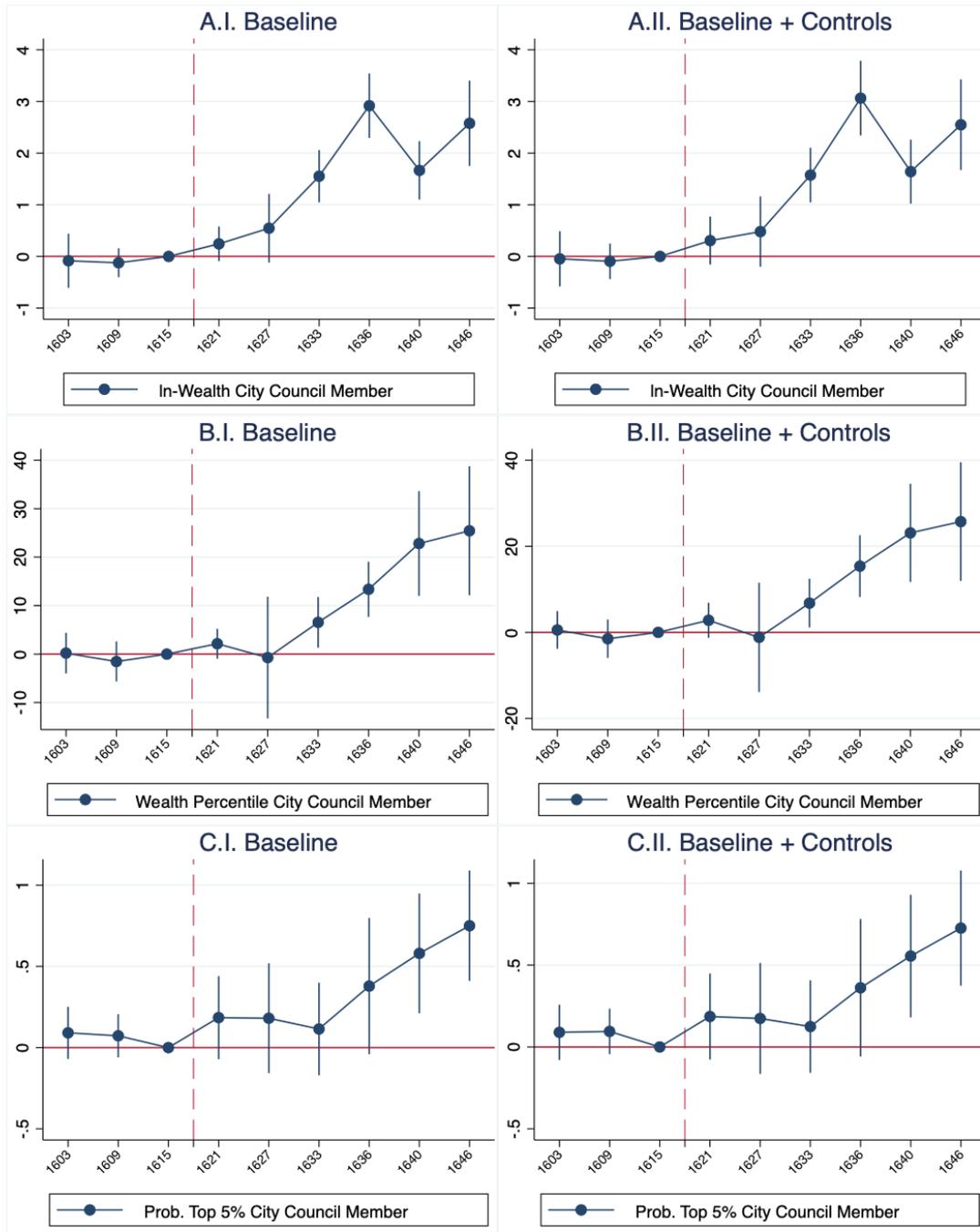
How does the effect of office-holding on personal wealth and economic polarisation evolve over time? To answer that question I estimate the following flexible difference-in-differences model:

$$W_{i,t} = \alpha_i + \pi_t + \sum_{t=-2}^8 \beta_t(Council_i \times Post_{i,t} \times Year_t) + \delta(Council_i \times Post_{i,t}) + \gamma' \mathbf{X}_{i,t} + \epsilon_{i,t} \quad (5)$$

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 3 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 6 were also driven by the epidemic. But the results in Figure 3 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

Figure 3: Political Office and Wealth During the 30-Years' War (Flexible Diff-in-Diff 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.

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.

To further substantiate the claim that the observed effect was actually the result of rent-seeking, I again investigate city clerks' wealth, in Table 7. The logic is the same as in the previous section: if growth of private wealth of council members was the result of better rent-seeking opportunities during the war, then one would expect that also those individuals who assisted the council in their office benefited, such as town secretaries or tax collectors. However, there exists another plausible explanation for the positive relationship between council membership and personal wealth of political elites. City council members were often merchants. In times of war, merchants were among the principal lenders of money to their own cities, like bankers (Stasavage 2011). Since interest rates were typically high at the time, lending money to governments was a very profitable affair (Chilosi et al. 2018). One could argue that what drove wealth concentration was not rent-seeking by political elites, but a rather mechanical "merchant banking effect": merchants met their cities increased demand for war finance by lending increasing amounts of money, and by doing so drove up the personal wealth of city council members. This would amount to an omitted variable bias.

Table 7: Mechanisms: City Clerks and Merchants During the Thirty-Years' War (Diff-in-Diff Estimates)

	(1)	(2)	(3)	(4)
	ln-Wealth	ln-Wealth	ln-Wealth	ln-Wealth
City clerk \times Post \times 30-Years' War	0.591*	0.654*		
	(0.338)	(0.341)		
Council member \times Post \times 30-Years' War \times Merchant			-0.104	-0.145
			(0.464)	(0.479)
City clerk \times Post	YES	YES	NO	NO
Council member \times Post \times 30-Years' War	NO	NO	YES	YES
Council member \times Post	NO	NO	YES	YES
30-Years' War \times Merchant	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	0.110	0.139	0.112	0.141

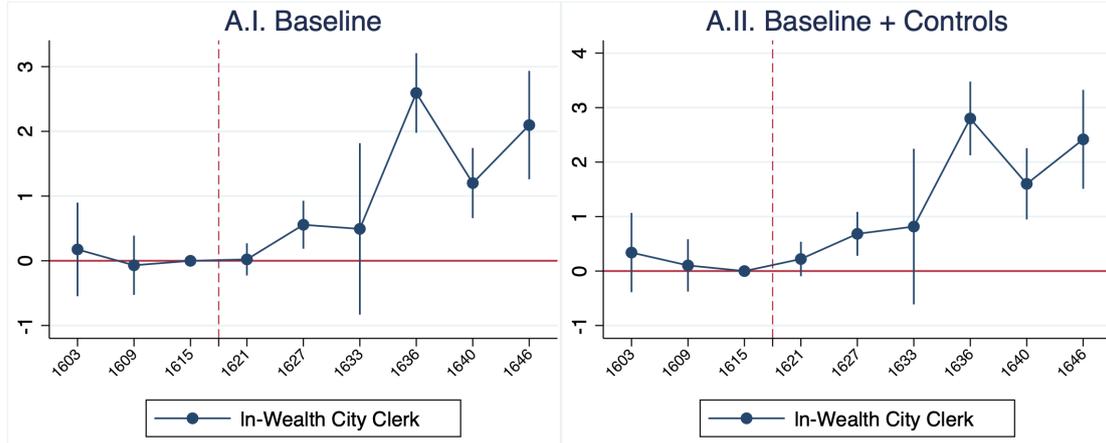
Notes: Estimation method is OLS. The period of analysis is 1603-1646. Standard errors clustered at the household level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

The results in Table 7 support the rent-seeking argument. The coefficients on the interaction term including city clerks in Columns 1 and 2 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 rent-seeking opportunities of the war on their wealth. In Columns 3 and 4, by contrast, I investigate the wealth of merchants, that were also city council members, in their individual post-period, during the Thirty Years' War. I hold the relevant components of the interaction term constant. The estimates are insignificant and slightly

negative, suggesting that these individuals did not drive the effect of the war.

Figure 4 plots flexible estimates (estimated analogously to Equation 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.

Figure 4: City Clerks and Wealth During the 30-Years' War (Flexible Diff-in-Diff 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.

One would like to know precisely which forms of rent-seeking explain the results of this section. Unfortunately, the lack of data makes it impossible to document this systematically. However, considering the historical background of the war, there are at least two plausible candidates. First, the increase in the sheer amount of money that passed through the hands of magistrates and city clerks must have increased the temptation and opportunity to take some of it. That is, one possible explanation is simple theft. Second, magistrates had discretion over how to allocate the material burden of war within the community. They could decide which taxes to raise and on which group to put the burden, in which areas of the city and in which households to quarter soldiers, 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.

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 Conclusion

This paper 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 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 (2021). 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 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,¹³ 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.

¹³For arguments about the importance of the fiscal system for explaining preindustrial inequality, see Alfani 2015, Alfani and Di Tullio 2019, Alfani 2021.

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Appendices

A 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 Stooß 1971, 1974, Baltzarek et al. 1973), a multi-volume encyclopedia of German cities. The encyclopedia was a collaborative project of hundreds of local historians, and provides information about several city characteristics in a very systematic way, for example about political institutions, the population or important events such as epidemics and wars.

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*. (Keyser 1939, 1941, 1952, 1954, 1956, 1957, 1959, 1962, 1964, Keyser and Stooß 1971, 1974, Baltzarek et al. 1973).

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: 11).

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).

B 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).

C Additional Results

C.1 Wealth Changes of Magistrates in Office before the Thirty Years' War

Table 8: Political Office and Wealth During the 30-Years' War (Diff-in-Diff Estimates)

	(1)	(2)
	ln-Wealth	ln-Wealth
Council member \times Post \times 30-Years' War	0.639** (0.264)	0.603** (0.267)
Council member \times 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. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

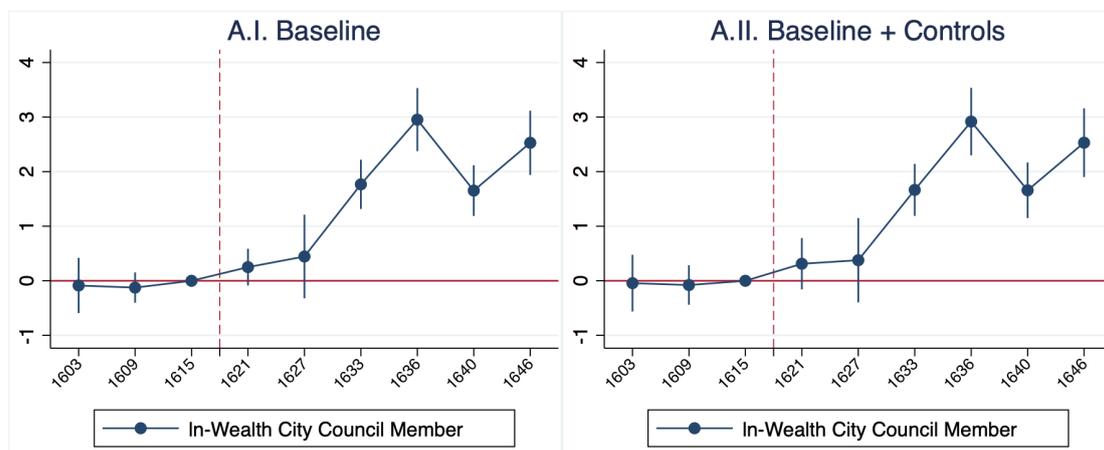
One concern about the results reported in the main text could be that the wealth increase of magistrates during the Thirty Years' War was driven by the selection of richer individuals into the city council. The analysis was already restricted to all individuals for which there is at least one

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 8 show that even those magistrates that were already in office before 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 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.

Figure 5: Political Office and Wealth During the 30-Years' War (Flexible Diff-in-Diff 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.

C.2 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 during the war, with respect to the rest of the population. These effects are highly statistically significant.

Table 9: Actual Wealth of City Clerks During the 30-Years' War (Diff-in-Diff Estimates)

	(1)	(2)
	Wealth (fl.)	Wealth (fl.)
Council member \times Post	33,793.932*** (10,719.840)	35,770.239*** (11,107.365)
Council member \times 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. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

References Appendices

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