

Different Paths to the Modern State in Europe: The Interaction between Warfare, Economic Structure and Political Regime

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ABSTRACT

Theoretical work on taxation and state-building borrows heavily from early modern European experience. While a number of European states increased centralized tax revenues during this period, for others revenues stagnated or even declined and these variations have motivated alternative arguments for the determinants of fiscal and state capacity. This study reviews the arguments concerning the three determinants that have received most attention, namely warfare, economic structure and political regime, and tests them by making use of a new and comprehensive tax revenue dataset. Our main finding is that these three determinants worked in interaction with each other. Specifically, when under pressure of war, it was representative regimes in more urbanized-commercial economies and authoritarian regimes in more rural-agrarian economies that tended to better aggregate domestic interests towards state-building.

1. INTRODUCTION

The process through which early modern European states monopolized tax collection and achieved gains in centralized fiscal capacity has been at the center of the study of state formation. What were the drivers of, and mechanisms for, the consolidation of the fiscal capacity of states? What roles did changes in domestic economic structure and the stimuli of interstate warfare play in state formation? Was it representative or authoritarian regimes that facilitated gains in fiscal and state capacity? These questions have been central not only to the study of politics¹, but also to understanding the economic development process itself.²

The theoretical answers for these questions originate mainly from the experiences of early modern European states, because these states were the first to permanently break cycles of gains and losses in centralized fiscal capacity and build towards the modern state system. There is, however, a void in the literature in terms of quantitative documentation of this process and empirical analysis of its determinants. To fill this void, this study compiles a new and comprehensive tax revenues dataset, reviews the patterns of fiscal capacity across the continent through the early modern era, examines and categorizes various threads in the literature and empirically investigates alternative hypotheses. Our main finding is that warfare tended to have a greater impact on state-building under representative regimes in more urban economies, and under authoritarian regimes in more rural economies.

The first part of the article reviews the long-term trends in fiscal capacity for twelve major European states between 1500 and 1800. Specifically, we present long term series for the central treasury revenues in silver, the common monetary unit of account for the period, and place them in the context of changes in population, prices and per capita incomes. The evidence corroborates a continent-wide trend of increases in centralized fiscal capacity that was significantly higher than the gains in average incomes. However, the patterns also suggest that not all states achieved fiscal gains and, among those that did, there were significant differences in the timing and extent of the gains.

To identify the determinants of these revenue patterns, in the first stage of the analysis we evaluate hypotheses that posit unconditional and independent impacts for war, urbanization and political regime. Specifically, we examine hypotheses that argue that changes in economic

¹ See Bonney (1999), Ertman (1997), Evans, Rueschemeyer, and Skocpol (1985), Finer (1997), Spruyt (2002), Storrs (2009), Tilly (1992), Vu (2010) and other works cited in the text.

² Acemoglu (2005), Besley and Persson (2010), Epstein (2000), North, Wallis and Weingast (2009).

structure (as proxied by urbanization) and interstate wars increased fiscal capacity, as well as the two hypotheses for political regime that alternatively posit that representative and authoritarian regimes were better at raising taxes. The empirical evidence supports positive fiscal impacts for urbanization and wars. The results, however, do not resolve the ambiguity regarding the fiscal impact of political regime, as the estimated difference between the fiscal performances of the regime types is insignificant.

We resolve this ambiguity in the second and third stages of the analysis by developing a hypothesis that combines three insights into early modern Europe. The first is that domestic elites had a higher organizational capacity under a representative regime and a lower organizational capacity under an authoritarian regime. The second is that political regime type, and thus elites' organizational capacity, mattered for state-building primarily during wars. The third is that urban elites were more cooperative and rural elites more antagonistic towards centralized state-building. Taken together, these three insights suggest that, when war pressure hit, the fiscal impact tended to be greater if the cooperative urban elites were well-organized under a representative regime and if the antagonistic rural elites were weak under an authoritarian regime. Conversely, authoritarian regimes in more urban and representative regimes in more rural economies tended to underperform in war. The empirical evidence provides strong support for this hypothesis.

The claim that early modern state-building followed more than one path is central to some of the seminal works in the literature. Most prominently, Tilly (1992) argues that polities with different economic structures followed alternate paths, whereas Ertman (1997) observes that, in some polities authoritarian, and in others representative, regimes were successful in building centralized-bureaucratic states.³ Our findings suggest that both economic structure and political regime mattered for the variation in paths, because it was the matches and mismatches between the two that determined whether a state could successfully respond to war pressure. Our estimation results also allow for a rough classification of each polity's history into episodes where their regime type either improved or hurt their ability to raise taxes for war and to place the findings in historical context.

We have chosen to restrict the scope of this study to the early modern era and exclude the 19th century from our analysis. This choice reflects our concern that the determinants and dynamics of early modern state-building were significantly different in the later period. While politics in the early modern era was an intra-elite affair, by the 19th century industrialization and the

³ For a thorough treatment of these arguments, see section 6.

advent of mass armies increasingly brought states in contact with ordinary citizens and their principal political concern became dissent by the masses.⁴ In a similar vein, while early modern states raised and spent taxes mainly for warfare, the frequency of wars dropped sharply in the 19th century and domestic concerns induced states to spend part of their revenues on public services.⁵ We would also emphasize, however, the broader implications of the findings for state-building - that the demands of interstate system, domestic economic interests and design of the political system worked in conjunction with each other and not as stand-alone influences, generalizes to other periods.

2. REVENUE PATTERNS IN EARLY MODERN EUROPE

In this section we first describe the tax revenue dataset we have compiled for leading European states. We then provide an overview of patterns in total tax revenues and per capita tax revenues during the early modern period based on this data set. Lastly, we discuss the figures for per capita tax revenues divided by per capita incomes, widely employed in the empirical literature as a proxy for state capacity. We establish that the patterns for this last measure are consistent with continent-wide gains in centralized fiscal and state capacity during the early modern era.

For this study, we have compiled annual central treasury tax revenue figures for the leading states of Europe. Specifically, our data set covers Portugal, Spain, England, France, the Dutch Republic, Venice, Prussia, Austrian Habsburgs, Sweden, Polish-Lithuanian Commonwealth, Russia and the Ottoman Empire, from the beginning of the sixteenth century until the end of the eighteenth century.⁶ Each of these twelve states had more than 1% of the continent's population and together they added up to between three to four fifths of the total.⁷ The sample is comprehensive in the sense that the variation in the characteristics of major European states, ranging from territorial empires such as the Ottomans and Austrian Habsburgs to maritime powers with sizable rural hinterlands such as the Dutch Republic, Venice and Portugal, are well represented. Similarly, we are able to base our findings not only on the

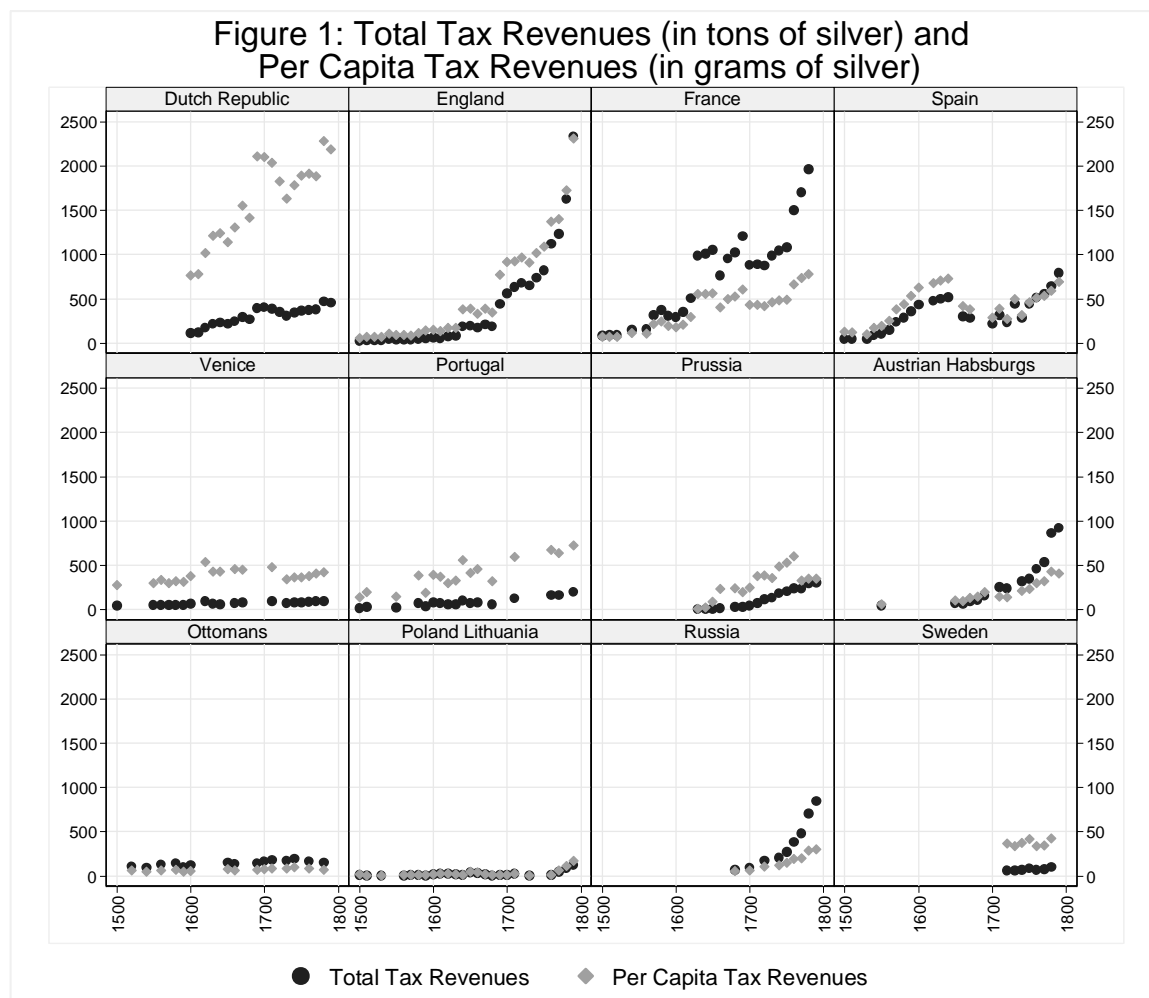
⁴ Acemoglu and Robinson (2005).

⁵ Lindert (2004).

⁶ For England, France, Spain, Venice, Austria, Poland, the Ottomans, and Portugal, the revenue series extends back to 16th century, for Prussia, Dutch Republic and Russia, 17th century, and for Sweden, 18th century.

⁷ Based on McEvedy and Jones (1978). One exception to the 1% threshold may have been early Prussia, for which population figures are not clear. The polities with populations close to the threshold, but missing from the sample are the Swiss Confederacy, Denmark-Norway, and a number of relatively sizable and autonomous political units in Holy Roman Empire and Italy.

experiences of states in western Europe but also on those in central and eastern Europe which have received less attention in the literature.



To make the revenue figures comparable across the polities in the sample, we have converted all monetary magnitudes into tons of silver by multiplying the annual revenues in the monetary units of account in each polity with the silver content of the unit of account for that year. We have taken great care to apply similar definitions of revenue to all the states, however the limitations imposed by the variations in accounting procedures and fiscal structures should be kept in mind.

Figure 1 shows decade averages of annual total central treasury tax revenues of the leading European states during the early modern era. It makes clear that central treasury revenues of most European states increased sharply during the seventeenth and especially the eighteenth centuries, and these revenue patterns are consistent with the historical shifts in the interstate power balance. Most striking in this respect was England, where total central tax revenues, in tons of silver, increased more than 60 fold between the early sixteenth century and late

eighteenth century. In France, total revenues increased more than 15 fold during the same period. In the Dutch Republic, total revenues began at a much higher level and increased four fold during the 17th and 18th centuries. Outside Western Europe, revenues of some but not all states also showed significant increases. In Austria, total revenues increased more than 20 fold from the middle of the 16th century until the end of the 18th century. In Russia further to the east, total central revenues rose more than 10 fold between late 17th and late 18th century. Interstate differences in total revenues reached their peak during the second half of the 18th century when the revenues of not only the more powerful states in western Europe, but also of those in central and eastern Europe such as Austrian Habsburgs and Russia, showed rapid increases. By contrast, those of Venice, the Ottomans, Poland lagged behind.

Not all changes in total tax revenues were necessarily driven by gains in state capacity, however. As Identity 1 below suggests, if we breakdown total tax revenues into its components, it is then possible to distinguish between the changes in total revenues due to changes in socioeconomic variables (i.e. population, price level and real income per capita) and changes in state capacity, as measured here by per capita tax revenues over per capita income ratio.

Identity 1: Total Tax Revenues in Current Prices or Tons of Silver = Population * Price Level * Real Income Per Capita * Real Tax Revenues Per Capita as a percent of Real Income Per Capita

To separate out the impact of population changes, we calculate per capita tax revenues by dividing total revenues by population.⁸ The resulting per capita tax revenue figures are in grams of silver and summarized in Figure 1 based on the scale on its right axis. Not surprisingly, adjusting for population alters the cross-polity rankings. Small, urbanized polities such as England, the Dutch Republic and Venice fare better in per capita terms. In terms of trends, the pattern of gains in taxation over time is still robust for most polities, however. For England, per capita revenues, in grams of silver, increased by more than 20 times from the middle of the sixteenth century to the end of the eighteenth century. In France, per capita revenues increased by 8 times during the same period. In the Dutch Republic, per capita revenues more than doubled during the 17th and 18th centuries. In Austria and Russia, per capita revenues increased by more than 3 and 5 times respectively during the eighteenth century.

⁸ Populations of most but not all European states in our sample increased during the early modern era, mainly for Northwestern Europe, and, with a lag, for Eastern Europe. See McEvedy and Jones (1978).

Lastly, we divide the annual per capita central tax revenues in silver by the daily wages of unskilled workers in silver. This allows us to control for both the effects of changes in average real incomes and the price level in silver.⁹ The wage series are available annually for most polities¹⁰ and due to their high quality are frequently used by economic historians as a proxy for per capita income during this period.¹¹

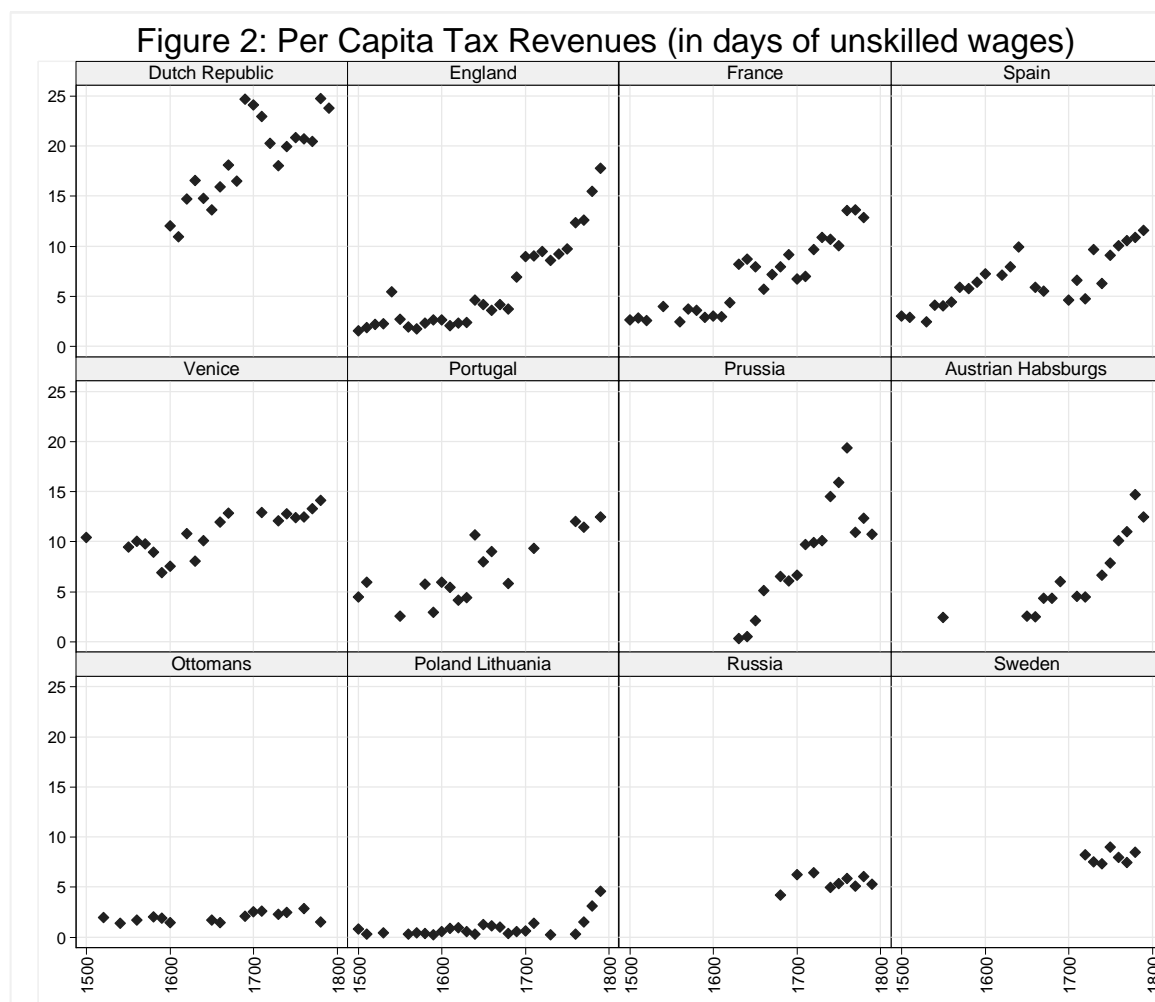


Figure 2 presents per capita tax revenues in days of unskilled workers' wages, our proxy for state capacity. It makes clear that, except for Poland-Lithuania and the Ottomans, there was a strong pattern of gains in state capacity across Europe during the early modern centuries. Figure

⁹ In the first half of the sixteenth century, price levels were higher in southern Europe than the rest of the continent. During the early modern centuries, however, price levels in northwestern and more generally western Europe increased more rapidly and significant differences emerged with the rest of the continent. Per capita real incomes rose in England and the Dutch Republic and to a lesser extent other parts of western Europe, but stagnated or declined in other parts. See Alvarez-Nogal and de la Escosura (2007), Maddison (2007), Van Zanden (2000).

¹⁰ Wage data is mainly based on Allen (2001); also Özmucur and Pamuk (2002).

¹¹ For example, Van Zanden and Prak (2006).

2 also indicates that gains in centralized state capacity proceeded quite unevenly across the continent. During the first half of the sixteenth century, annual tax revenues per capita did not exceed 5 days of unskilled urban wages in most European countries. The only exceptions were small and highly urbanized entities such as Venice and the Dutch Republic. By the end of the eighteenth century, however, differences in fiscal centralization across Europe had increased substantially. While the annual per capita revenues of some central administrations such as Poland-Lithuania and the Ottoman Empire still remained below 5 days of urban wages, many others had reached the 10 to 20 daily wages range and annual per capita revenues of the central administration in the Dutch Republic exceeded 20 days of urban wages. It is worth noting that the middle group where annual per capita revenues reached 10 to 20 daily wages included not only the more urbanized western European countries such as England, France, Spain and Venice but also the more rural and agricultural countries in central and eastern Europe such as Austria and Prussia.¹² These broad trends make clear that the increases in state capacity preceded the Industrial Revolution and the onset of modern economic growth during the 19th century.¹³

These levels of per capita tax collection and per capita tax collection as a multiple of the daily urban wage place Europe in a unique position in the early modern world. We do not have detailed tax revenue and other data for many Asian states. However, such data as we have been able to gather show clearly that the capacities of tax collection central administrations in Asia,

¹² In controlling for changes in per capita incomes, an alternative approach would be to divide per capita tax revenues by per capita GDP figures. The trends we identified are robust for this alternative measure for state capacity. We estimate that tax revenues of the central administrations as a percent of GDP rose in most European countries from less than 5 percent in the sixteenth century to a range between 5 and 10 percent and in a small number of cases that include Britain and the Netherlands to more than 10 percent by the end of the eighteenth century. See, for example, Hunt and O'Brien (1993). We prefer to work with wages rather than per capita GDP figures since per capita GDP estimates are available only for some benchmark years and their quality is lower.

¹³ One concern with using tax revenues as a measure of state capacity is that some sources of revenue were easier to collect than others. For example, taxing the windfall revenues from shipments of precious metals from the colonies, or customs revenue from international trade demanded less effort. If the observed gains in tax revenues were driven mainly by gains in these sources of revenues, it could be argued, they did not necessarily correspond to gains in state capacity. The available evidence suggests, however, these sources of revenues did not play a disproportionate role in revenue gains. For the Spanish Empire, the main beneficiary of the silver and gold inflow from the colonies, the contribution to the central treasury peaked at 25% in the last quarter of the 16th century, and remained under 15% in other periods (Yun-Casalilla, 1998). For the Dutch Republic, despite the central role of maritime trading in its economy, the share of customs revenues in taxes remained around 10% in the 17th and 18th centuries (Fritschy, 2009). For England, the share of custom revenues actually declined, from about 31% in the second half of 17th century to 26% in the second half of 18th (O'Brien, 1988).

during both the early modern era and the nineteenth century, remained well below those of most European states during the eighteenth century. In both China and Iran, for example, per capita tax collections remained well below 5 days of wages during the eighteenth and nineteenth centuries.¹⁴ The only exception was Japan after the Meiji Restoration, where per capita tax collections as a multiple of the daily unskilled wage started at very low levels but increased rapidly to exceed 10 days in the years before World War I equivalent to the levels of Spain at the end of the eighteenth century.¹⁵ Even in the case of Japan, levels of per capita tax revenue as a multiple of the daily urban wage in the early part of the twentieth century remained well below that of Great Britain, France and the Dutch Republic during the second half of the eighteenth century. Because of the rapid economic growth in Europe during the 19th century, the revenue differences, in total and per capita silver terms, became even larger. This disparity in fiscal resources made it very difficult for Asian states to resist European military power.

3. THEORIES OF STATE FORMATION IN EARLY MODERN EUROPE

The long-term trends reviewed in the previous section indicate significant gains in centralized fiscal capacities across Europe before the 19th century. To investigate the determinants of these gains, this section first offers a characterization of the historical environment in which early modern state-building took place. We then review impacts conjectured for three variables, interstate warfare, economic structure and political regime in historical sociology and political science literatures. The arguments for the impacts of these variables in these literatures are often complex and multi-layered, making it difficult to reduce them to testable hypotheses. Therefore, at the risk of oversimplifying these arguments, we focus, in the first round of empirical analysis, on the simplest and most unconditional interpretations and leave more complex interpretations to later sections.

A critical observation for politics before the 19th century is that it was primarily an intra-elite game. In particular, a small segment of the population controlled a disproportionately large share of political and economic resources and the dissent of the masses remained a secondary concern until the French revolution, industrialization and the advent of mass armies. In this environment, state-building mainly concerned the reorganization of intra-elite relations from

¹⁴ See Ma (2011) for China and Floor (1998) for Iran.

¹⁵ Nakabayashi (2008).

fragmented control over violence and taxation towards the build-up of a central apparatus that monopolized them both.¹⁶

In explaining the build-up of the central apparatus, a major thread in the literature argues that it was primarily driven by the external stimuli of interstate warfare.¹⁷ At the core of this argument is the claim that interstate warfare of the early modern era set in motion innovations in military technology, training, and tactics that favored centralized modes of coercive organization and demanded standing armies of ever greater size and cost. In turn, the pressure to provision and manage these armies induced experimentation and advances in fiscal, military and administrative methods and the gains from these formed the backbone of the modern state.¹⁸ These changes were ad hoc responses to exigencies of warfare, and thus improvements were often piecemeal and limited. In Europe, however, the existence of multiple core agricultural areas, large islands and mountain ranges that divide up the continent facilitated a fragmented state system, more or less continuous warfare and ensured that the gains accumulated over time.¹⁹ Hence, in its simplest interpretation, this literature suggests the following hypothesis:

H1: Interstate warfare had a positive effect on centralized fiscal capacity.

A second set of arguments concerns the impact of changes in domestic economic structure from rural-agrarian to urban and commercial economies. In the Marxist tradition, where the state is essentially treated as the instrument of the elites, it is argued that these changes in economic structure realigned elite incentives towards a centralized state apparatus.²⁰ For example, in his discussion of Western Europe, Anderson argues that the rise of centralized monarchies in the early modern period was due to the reorganization of aristocratic power to retain its privileged position in a more centralized fashion.²¹ According to this argument, the dissolution of feudalism, and the rise of towns and markets undermined the aristocracy's coercion based extractive capacity at the village level. Their role was taken on by the rising monarchies, which

¹⁶ North, Wallis and Weingast (2009).

¹⁷ Ames and Rapp (1977), Bean (1973), Besley and Persson (2008), Hintze (1975), Peacock and Wiseman (1961), Rasler and Thomson (1985, 1999), Tilly (1992). For extensions of the argument to the developing countries in the modern period, see Centeno (2002) for Latin America, Lustick (1997) for the Middle East and Herbst (2000) for Sub-Saharan Africa.

¹⁸ Hintze (1975) p. 199.

¹⁹ In contrast, in East Asia, the gains in military technology and organization during episodes of war were followed by stagnation and decay during long episodes of peace. See McNeill (1982) and Morillo (1995).

²⁰ Gourevitch (1978).

²¹ Anderson (1979).

then served to mobilize resources at the scale of newly forming nations. For Wallerstein, the international division of labor between core and peripheral economies led to changes in domestic economic structure, which in turn drove state-building.²² In the advanced and richer economies of the core, the elites favored a strong and effective state to govern economic activity and assert collective interests in the international arena. Alternatively, the monetization of the economy might have facilitated centralized tax collection by making it possible to transfer the revenues to the political center, pay a standing central army or mercenaries in cash and, when necessary, redistribute the funds around the polity.²³ These arguments are all consistent with the following hypothesis:

H2: Urbanization, which was at the nexus of changes in economic structure, had a positive effect on centralized fiscal capacity.

A third set of arguments concerns the impact of political regime types on state-building. In these arguments, the central executive-military apparatus, often personified by the ruler, is modeled as autonomous and distinct from domestic elites. The emphasis is on how domestic elites with coercive and economic capacities, encompassing a wide range of subjects from feudal vassals to merchants, interacted with the ruler and his central apparatus, and how this interaction affected state-building. Political regime types mattered for this interaction, because they corresponded to different levels of the organizational capacity and leverage for domestic elites. Specifically, the distinction between representative regimes, where the elites had access to a formal and inclusive assembly, and authoritarian regimes, where they did not, altered elite capacity. A representative assembly enhanced the organizational capacity of the elites by providing a venue for information sharing, coordination, and sanctioning of deviant members. This, in turn, allowed the elites to formulate policies separate from the ruler, solve collective action problems and credibly act in unison.²⁴ This can be contrasted with authoritarian regimes, where ruler-elite interaction was for the most part bilateral, particularistic and informal, and where the ruler retained the capacity to reward, punish and play elites against each other.²⁵ Consequently, in authoritarian regimes, the elites lacked the capacity to formulate and coordinate their actions independently of the ruler.

²² Wallerstein (1974).

²³ Tilly (1992) p. 88-9.

²⁴ See Greif, Milgrom and Weingast (1994) for a theoretical analysis of the impact of coordination and commitment capacity on the bargaining outcomes.

²⁵ See, amongst others, Hellie (2000) for Russia, Barkey (1994) for the Ottomans, Hoffman and Norberg (1994) for France.

Beyond the common premise that a representative assembly enhanced the organizational capacity of the elites, there are conflicting hypotheses concerning whether this capacity worked against or towards state-building.²⁶ The argument that it worked against state-building is based on an antagonistic characterization of elite-ruler relations. For early modern Europe, this line of argument portrays elites as driven by redistributive concerns, interested in retaining their prerogatives at the local level and opposed to the expansion of the central apparatus. Hence, representative assemblies, where they existed, acted as nodes of resistance against fiscal centralization. In contrast, under an authoritarian regime, rulers were better able to pass laws that broadened the tax net, compelled elites to behave in ways that countered their interests and suppressed their activities to obtain exemptions and favors. The degree to which rulers encroached on elite prerogatives in turn determined the extent of the gains available to be made in terms of centralized fiscal capacity. This line of argument implies the following hypothesis:

H3: Authoritarian regimes had a positive effect on centralized fiscal capacity.

The alternative hypothesis, that representative regimes performed better in raising taxes, builds on a cooperative view of the elite-ruler relations. In this vein, the contract theory of the state argues that there were economies of scale in the provisioning of violence and hence returns to the build-up of a centralized state apparatus.²⁷ The ruler, or more broadly the specialist in violence, provided a group of services, mainly security and justice, and in exchange, wealth holders provided the tax revenues. Representation facilitated this exchange, because it solved collective action and free-rider problems among the taxpayers, permitted better information sharing, reduced costs of negotiation, measurement and monitoring, lent legitimacy to taxation and allowed credible commitments over the terms of exchange. As cases in point, this line of argument invokes the representative regimes and fiscal successes of the Dutch Republic and England after the Glorious Revolution and compares them favorably with the authoritarian regimes and fiscal demise of Spain and France.²⁸ For empirical analysis, the unconditional reading of this argument is that representation increases revenues.²⁹ Formally:

H4: Representative regimes had a positive effect on centralized fiscal capacity.

4. TESTING THE THEORIES

²⁶ Kiser and Linton (1991) and Cheibub (1998) review both sides of the argument.

²⁷ North (1981) p. 23, Levi (1989) p. 18.

²⁸ Hoffman and Norberg (1994).

²⁹ Dincecco (2009).

While theories of state-building borrow heavily from the early modern European experience, they have not been thoroughly tested with actual data from the period. Most empirical studies use 19th and 20th century data³⁰, and the few that use early modern data focus on Western Europe.³¹ Using our new revenue dataset, we try to fill the void for major states across Europe.

In the first stage of the empirical analysis, we test H1-H4 by introducing proxies for each of warfare, urbanization and political regime, and evaluating the significance and magnitude of their coefficients. We delay investigation of hypotheses that allow for interactions between these three determinants until later sections. In the rest of this section, we describe the variables and econometric models employed and present the first stage of our results.

Variables

Our dependent variable is per capita central tax revenues in days of unskilled urban workers' wages (*Taxrev*). In its immediate interpretation, central tax revenues adjusted for population and incomes is a proxy for the fiscal capacity of the state. In this and in other empirical studies, it is also interpreted more broadly as a proxy for state capacity. The justification is, for one, that fiscal capacity captures state's capacity to monitor economic activity and enforce laws to mobilize the polity's resources. Second, a state's fiscal capacity sets the limits of the policies that it can implement.³² This close relationship between fiscal capacity and statehood is explicit in most definitions of the state.³³

The proxy for the impact of war (H1) is the "apportioned" war casualties per thousand of population per year. This variable is calculated over a number of stages. The number of casualties for each war, which captures its size or severity, is apportioned equally between the two contending coalitions of belligerents. We use apportioned rather than actual casualties of the two sides, because the latter is an ex-post measure and puts disproportionate weight on the losing side. The hypothesis we are testing, however, concerns *ex-ante* demand for resources that war placed on the participants. The apportioned casualty is then divided by the duration of the war and the total population of the coalition to reach an estimate of the annual per capita burden

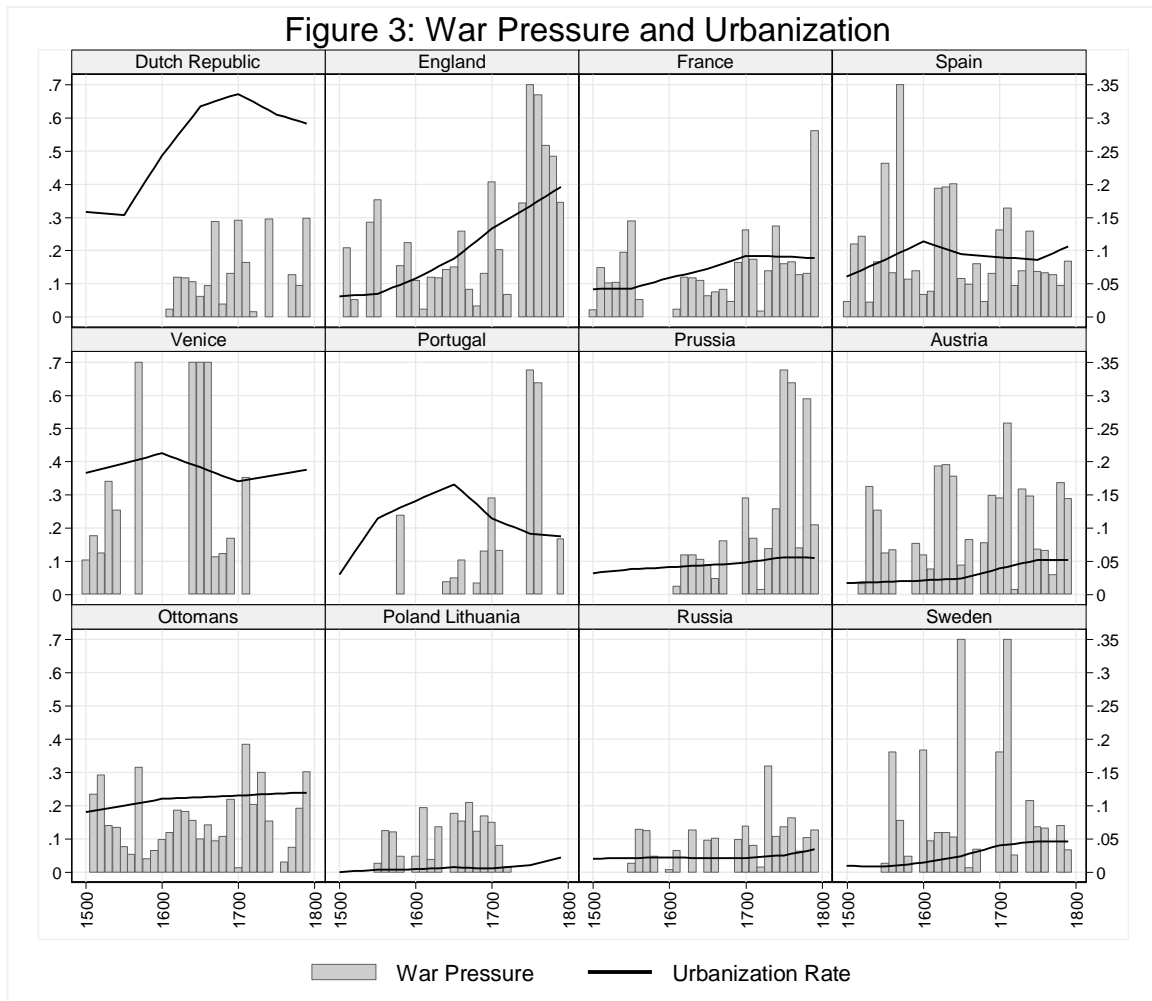
³⁰ For example, Cheibub (1998), Lektzian (2008), Ross (2004), Timmons (2005), Thies (2005) and Thies (2007).

³¹ Dincecco (2009), Kiser and Linton (2001), Rasler and Thompson (1985) and (1999).

³² Hendrix (2010), Lieberman (2002).

³³ See, for example, North (1981) and Ardant and Tilly (1975).

of the war. If a polity engages in more than one war in a year, we add the per capita burdens up across the wars. The resulting proxy for war pressure (*Warp*) is summarized in Figure 3.³⁴



The economic structure argument (H2) posits that, with the shift in economic structure from rural-agricultural to urban-commercial economies, domestic elites became more favorable towards centralized state-building. The natural proxy for this argument is the urbanization rate (*Urb*). Our main data source for this variable is de Vries (1984), who uses a threshold of 10,000 inhabitants to define urban centers and provides the most detailed series for the period with estimates for each half century. We interpolate for the decades in between. Figure 3 summarizes the urbanization series.

A caveat with using urbanization as a proxy is not only that it captures the changes in economic structure, but also, for the early modern period, it is related to changes in average per capita real incomes. In most studies, this distinction is not made, and urbanization is used as a catch-all

³⁴ The values are truncated from above at 0.7 casualties per 1000 population to make the figure more tractable. The details for the calculation of the index are relegated to the Appendix.

proxy for economic development.³⁵ However, because H2 is concerned with the changes in economic structure rather than changes in average incomes, in some specifications, we include a separate proxy, per capita real wages (*Rwage*), to control for the latter.

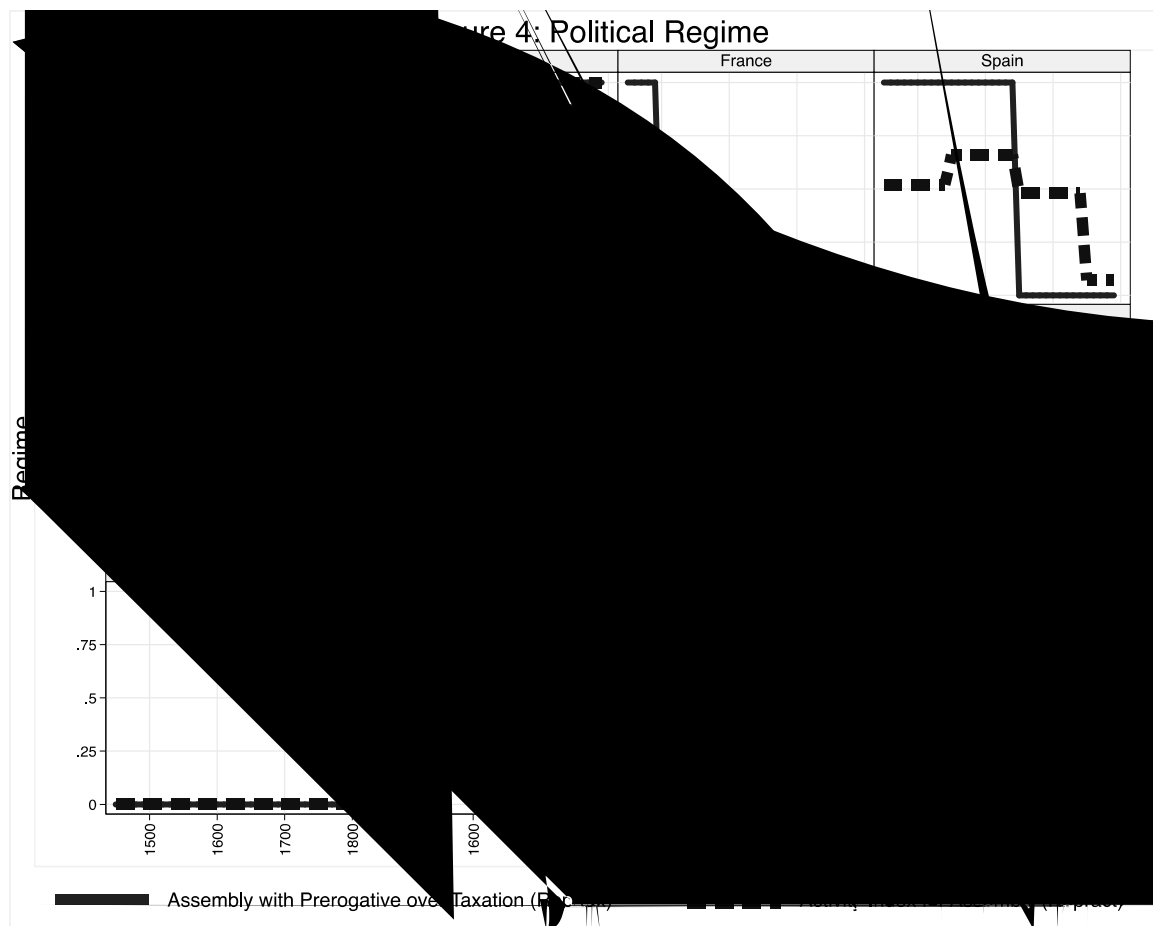
The hypotheses for the impact of political regime (H3 and H4) concern elites' organizational capacity and influence on fiscal outcomes. Our main proxy is a dummy variable for the existence of a representative assembly with prerogative over taxation (*Reprtax*). For each decade, it takes value one if such an assembly exists, and zero otherwise.³⁶ The second proxy, (*Repract*) is an index of activity for the representative assembly covering the whole realm.³⁷ For each century, it counts the number of calendar years in which the assembly was convened. The variable is rescaled to vary between zero, when no assembly existed or the assembly did not convene, and one, when a meeting took place in every year of the century.

These two proxies for regime have their advantages and disadvantages in measuring the organizational capacity of elites. Qualitatively *Reprtax* is the better proxy, in that it explicitly codes for the involvement of the representative assembly in fiscal decision-making. On the other hand, it is a dichotomous variable and does not provide a precise measure of changes in the assembly's influence over time. The activity index, *Repract*, is a finer measure of the actual influence of the assembly, but does not explicitly code for whether the assembly was involved in tax collection and is anyway not available for Poland-Lithuania and Austrian Habsburgs. Figure 4 shows that with the exception of Portugal these two proxies are closely related for the polities in the sample. Nevertheless, for robustness, in the econometric analysis we repeat each empirical specification for both regime proxies.

³⁵ E.g. Acemoglu, Robinson and Johnson (2002) and Van Zanden, Buringh and Bosker (2011).

³⁶ Stasavage (2010).

³⁷ Van Zanden, Buringh and Bosker (2011).



In some specifications, we also include the log of population ($\ln Pop$) to control for the possibility that a change in population may lead to a more or less than proportional change in total tax revenues.

All specifications include polity and decade fixed effects. As such, the regression results are driven by the variation the explanatory variables exhibit around their mean values they take for each polity and each decade. The polity fixed effects allow controlling for any omitted polity specific effects that do not vary over time such as geographical attributes or any time persistent differences in the conventions used for recording tax revenues. The decade fixed effects allow controlling for omitted common shocks and trends such as gains in transportation, communication, information technologies.

The fixed effects also help resolve potential collinearity issues. For example, in our sample, urbanized polities tend to have representative regimes. In the estimation, because fixed effects are included, this correlation is factored out, and it is the changes the two variables exhibit around their respective polity means that drive the results. As can be observed from Figures 3 and 4, the changes in urbanization rates did not necessarily move in step with regime changes, and collinearity is not a major issue.

Table 1: Summary Statistics for Variables

| Variable | Description | Obs. | Mean | Std Dev. | Min. | Max |
|----------------|--|------|------|----------|-------|-------|
| <i>Taxrev</i> | Per cap. tax revenues (in days of wages) | 217 | 7.32 | 5.56 | 0.22 | 24.73 |
| <i>Warp</i> | War pressure (casualties per 1000 pop.) | 217 | 0.15 | 0.17 | 0 | 1.19 |
| <i>Urb</i> | Urbanization rate | 217 | 0.10 | 0.08 | 0 | 0.34 |
| <i>Reprtax</i> | Assembly with prerogative over taxation | 217 | 0.65 | 0.48 | 0 | 1 |
| <i>Repract</i> | Activity index for assembly | 179 | 0.43 | 0.42 | 0 | 1 |
| <i>Rwage</i> | Real wage | 208 | 4.67 | 1.47 | 2.30 | 8.43 |
| <i>lnPop</i> | Log of population in millions | 217 | 1.77 | 0.93 | -0.45 | 3.34 |

Table 1 presents the summary statistics for the variables employed in our econometric analysis. There are 12 polities in the sample, and 30 decades between 1500-1799, implying a maximum number of 360 observations. Prussia and the Dutch Republic can be considered autonomous political entities only by the 17th century, and Portugal drops out of the sample between 1580-1640 due to the unification with Spain, reducing the upper limit for observations to 334. We were able to collect revenue figures for 217 observations, which set the maximum number of observations in our regressions. Real wage data is not available for Russia, and activity index for the assembly is not available for Austria and Poland, reducing the number of observations for econometric specifications that include these variables as regressors. Other econometric requirements and subsample analyses further reduce the number of available observations. All in all, the number of observations in the regressions ranges between 108 and 217.

Baseline Empirical Analysis

Our base econometric specification is OLS with polity and decade fixed effects and panel corrected standard errors. We assume that the disturbances are heteroscedastic across panels, there is a common autocorrelation term for all panels and no contemporaneous correlation across panels. The equation we estimate is:

$$\text{Taxrev}_{it} = \alpha_i + \beta_t + X'_{it}\gamma + \theta_1 \text{Warp}_{it} + \theta_2 \text{Urb}_{it} + \theta_3 \text{Repr}_{it} + \varepsilon_{it}$$

where Taxrev_{it} is per capita tax revenue (in days of wages) for polity i in decade t , α_i are the fixed effects for the polities, β_t are the fixed effect for decades except for the first one, 1500-1509. Urb_{it} denotes the urbanization rate and Warp_{it} the pressure of warfare. For Repr_{it} , political regime, we employ two different proxies, a dummy variable for the existence of a representative assembly with prerogative over taxation (Reprtax_{it}), and activity index for the representative assembly (Repract_{it}). X'_{it} are control variables, namely lagged real wage

(*L.Rwage*) and lagged log population (*L.lnPop*). We lag these variables to alleviate endogeneity concerns. ε_{it} is the disturbance term that exhibits heteroskedasticity and autocorrelation.

TABLE 2 ABOUT HERE

Table 2 reports the first set of regression results. Each regression is repeated twice for the two regime proxies, *Reprtax* in odd-numbered and *Repract* in even-numbered specifications. Specifications 1-2 include regime, urbanization, war pressure, specifications 3-4 also control for population and real wage. In specifications 5-8 we work with subsamples in order to address concerns about sample selection and data quality. The revenue series for the Dutch Republic, Prussia and Russia are available starting in the 17th and for Sweden in the 18th century. If the late availability of these revenue series was due to variables other than those included in our estimations, this would introduce a bias. In order to address this concern, in specifications 5-6, we drop observations for the 16th century and repeat the regressions with a more balanced sample. Finally, in specifications 7 and 8, we drop from the sample Sweden, Portugal and Russia, which have data for less than 10 decades out of a maximum possible of 30, and Poland-Lithuania, for which the quality of revenue data is uneven.

Across the eight specifications, the empirical findings are consistent. We find strong support for a positive impact of wars on centralized fiscal capacity (H1), significant at 5% or lower levels.³⁸

We also find strong support for H2, in that urbanization is significant across the specifications at 1 or 0.1% level.³⁹ This finding is robust when real wage is included as regressor, despite the fact that wage captures similar trends and is less prone to measurement errors than urbanization.

For representation, we do not find evidence of either a negative (H3) or a positive (H4) impact. For both of the regime proxies, the estimated coefficient is insignificant at 10% level in all specifications.

Of the control variables, the coefficient for lagged real wage carries a positive sign in all specifications but is significant at 10% level in only specification 5. The coefficient for lagged

³⁸ The estimated coefficient for *Warp* is in the range 1.704-2.331, meaning each additional casualty per 1000 population is estimated to increase per capita tax revenues by about 2 days of wages. This estimate is arguably a lower limit on wars' impact on state building, since the econometric specification only accounts for an instantaneous and polity specific effect. However, many of the wars in the sample involved a large number of polities, and any innovations driven by the wars were transferred and adopted across the continent. Decade dummies soak up the explanatory power of such continent-wide impacts.

³⁹ One percent higher urbanization rate increases per capita tax revenue by 0.29-0.48 days of wages.

log population, on the other hand, is positive and significant at 10% in all specifications. Note that in this period the territorial changes for polities in the sample are minor (with the exception of Prussia, Austria and England's union with Scotland). Consequently, the estimated positive impact could be interpreted as that of population density rather than population per se.

Endogeneity Concerns

A major concern for the validity of the empirical results is endogeneity bias. Endogeneity might be caused by an omitted variable that affects both an explanatory variable and per capita tax revenues. Alternatively, endogeneity might be due to reverse causation, i.e. the dependent variable, per capita tax revenues, might have an impact on an explanatory variable. In this section, we discuss and attempt to address endogeneity concerns for the estimated impacts of war, regime, and urbanization.

First consider potential endogeneity of the estimated impact of war. In the literature, determinants of war are studied at three levels: international, state/society and political leader level.⁴⁰ At the international level, the realist tradition, based on the premise that distribution of power in a state system determines patterns of war, dominates the literature. For the current study, the concern associated with realist arguments would be reverse causation from a state's tax revenues to its war propensity, since tax revenues of individual states determine balance of power, and realist tradition argues balance of power determines patterns of war. A closer reading of the literature, however, suggests that, neither the impact of taxes on the balance of power, nor the impact of balance of power on wars is clear-cut. Because early modern Europe was a close-knit state system, with wars between large coalitions of states from across the continent, the tax revenue of each individual state was only a small part of the larger calculus of power balance. Moreover, in the realist tradition, there is no theoretical or empirical consensus over what makes war more likely. Conflicting theories in the literature alternatively argue that parity, disparity, changes in or misperceptions of balance of power facilitates war.

The state/society level theories of war emphasize the impact of domestic variables. The two prevalent threads in this category study the impacts of political regime (democratic peace) and economic structure (capitalist peace) on propensity for war. Since the proxies for these two variables are already included in our regressions, omitted variable bias is not a concern.

Likewise, even though each polity's geography is arguably correlated with both its tax revenues and propensity to go to war, the correlation is factored out by the fixed effects. There are also

⁴⁰ The discussion of determinants of war is based on Levy and Thompson (2010), Van Evera (1999) and sources cited therein.

arguments for domestic public opinion and ideologies such as nationalism as determinants of war, but these arguments are less relevant before the 19th century.

Finally, political leader level arguments trace wars to the personalities of key decision makers, their belief systems and emotional states. In the estimation, we do not control for individual rulers, and the endogeneity concern would be that that leaders' personalities might matter both for warfare and taxation. Note, however, to the extent that leaders' personalities were correlated with fixed effects and control variables, they are still factored out. Even if Ottoman sultans were more bellicose than Venetian doges, rulers in the 18th century were less likely to initiate religious wars than those in the 16th century, or the rulers that ascended to the throne in representative regimes were on average of milder temperament than those in authoritarian regimes, these differences in personality do not introduce a bias to the estimated impact of war. There were certainly idiosyncratic leader-to-leader variations above and beyond those that were factored out, but it is not clear that they followed a systematic pattern that would introduce a bias.

For the estimated impact of political regime, on the other hand, there are good reasons to suspect endogeneity. Early modern parliaments mainly convened to discuss matters of taxation, and consequently, unlike war, the relationship between regime and taxation was immediate. If, as historical evidence suggests, rulers convened parliaments only when domestic elites were willing to consent to taxation, this would introduce a positive bias to the estimated fiscal impact of representation. Alternatively, if windfall revenues from external sources led the rulers to suppress the activities of the representative assemblies, this would introduce a negative bias.⁴¹

There is also a clear case for endogeneity of the estimated impact of urbanization due to reverse causation. If higher centralized fiscal capacity encouraged market activity and urbanization through gains in public security, contract enforcement and other means, this would lead to a positive bias in the estimated impact of urbanization.

TABLE 3 ABOUT HERE

Based on the discussion above, we address the endogeneity concerns for representation and urbanization by instrumenting them with their values 50 years or 100 years lagged.⁴² The results

⁴¹ Drelichman and Voth (2008) argue, for example, shipments of precious metals from the New World made the Spanish monarchs less dependent on the Cortes.

⁴² There are two conditions that the instruments must satisfy. Conditional on the controls variables, they must be orthogonal to the error term (exclusion restriction) and they must be correlated with the included endogenous variable (instrument validity). We argue that since the instruments are picked from the distant past, they are unlikely be correlated with the error term.

of 2-Stage Least Squares estimation, reported in Table 3, are consistent with the baseline findings in Table 2. The coefficient for urbanization is positive and significant at the 10% confidence level or lower. The impact for war is significant at the 10% level except for specification 8. Finally, the impact of regime is again indeterminate, with an estimated coefficient negative and significant at the 5% level in specification 1, positive and significant at the 10% level in specification 6, and insignificant in the other six specifications.⁴³

5. INTERACTION BETWEEN REPRESENTATION AND INTERSTATE WARFARE

In the previous section, we found strong evidence that wars and urbanization had on average positive impacts on fiscal capacity. With regard to the alternative hypotheses, that authoritarian (H3) and representative regimes (H4) raised more taxes, however, we did not find conclusive evidence for one or the other. In these two hypotheses, the conjectured fiscal impact of the political regime was unconditional and direct. There are also arguments in the literature that suggests political regime might instead have had an indirect impact, by moderating the impact of war pressure on tax revenues. In this section, we review these hypotheses, arguing respectively that it was authoritarian (H5) and representative regimes (H6) that performed better in turning the stimuli of wars into tax revenues.

The two hypotheses discussed in this section regarding political regime are contingent versions of those discussed in the previous section. The essence of representative assemblies is again identified as their role in improving the organizational capacities of domestic elites. The additional insight is that the organizational capacity of elites mattered most during wars, because historically it was during the wars that bargaining over taxes and reorganization of the state apparatus took place.⁴⁴

This observation, when interpreted together with the two different characterizations of elite attitudes towards the build-up of the central apparatus, implies two conflicting hypotheses for

For instrumental validity, the Angrist-Pischke multivariate F test of excluded instruments reject weak instruments at 10% level or lower in all specifications.

⁴³ A third concern for the empirical results is the persistence of fiscal capacity from one decade to the next. In the baseline model, the persistence is accounted for by a serial correlation in the error term. An alternative approach would be to include the lagged value of the dependent variable as a regressor and estimate a dynamic model. For various hypotheses we test in the article, the results for the dynamic specifications are consistent with the baseline specifications and are relegated to the Appendix.

⁴⁴ Bonney (1999), Van Zanden, Buringh and Bosker (2011), Körner (1995).

the regime-warfare interaction. The view that domestic elites were essentially opposed to state-building implies that authoritarian regimes performed better in transforming war into taxes. This line of argument is most explicit in Hinze (1975) and Downing's (1992) discussion of Europe. In parts of Europe where war pressure was heavy, wars were protracted and states needed to finance themselves through domestic taxation, representative regimes did not fare well and could not compete with authoritarian regimes, whereas in regions where the pressure was light, representative regimes survived. Formally:

H5: Interstate warfare induced greater gains in centralized fiscal capacity under authoritarian regimes.

The alternative hypothesis follows from the cooperative view of elite-ruler relation and the contract theory of state. Contract theory argues that elites paid taxes in exchange for services provided by the ruler, and that representation mattered by facilitating this exchange. Hence, a finer reading of the contract theory suggests that a representative regime is not necessarily associated with higher taxes, but higher taxes relative to the service that the ruler provided.⁴⁵ Since, during the early modern era, the main prerogative and main expenditure item for the ruler and the central apparatus was interstate war-making, this would suggest that a representative regime better matched increases in war pressure with increases in tax revenues. In this vein, Hoffman and Rosenthal (1997) and Rosenthal (1998) argue that in Western Europe representative regimes corresponded to a deal between ruler and domestic elites that entailed financing warfare in exchange for a greater say over its conduct. In formal terms:

H6: Interstate warfare induced greater gains in centralized fiscal capacity under representative regimes.

Empirical Analysis

To investigate H5 and H6, we add the interaction term between war pressure and representation as a regressor to different econometric specifications discussed in the preceding section and investigate its sign. We first do so for OLS with PCSE with two-way fixed effects specification. The regression equation is:

$$\text{Taxrev}_{it} = \alpha_i + \beta_t + X'_{it}\gamma + \theta_1 \text{Warp}_{it} + \theta_2 \text{Urb}_{it} + \theta_3 \text{Repr}_{it} + \theta_4 (\text{Warp}_{it} * \text{Repr}_{it}) + \varepsilon_{it}$$

⁴⁵ Ross (2004) p. 234.

where ε_{it} exhibits heteroskedasticity and autocorrelation. H5 implies that the coefficient of the interaction term, θ_4 , should be negative and significant, and H6 implies it should be positive and significant.

TABLES 4-5 ABOUT HERE

Table 4 reports the regression results. As in Table 2, we report the estimates without (1-2) and with (3-4) the controls for real wage and log population, dropping the 16th century (5-6) and polities with short series and uneven data quality (7-8). In none of the specifications, the interaction term is significant at the 10% level. The specifications in Table 5 are estimated using 2-Stage Least Squares, where urbanization, representation and representation-war pressure interactions are instrumented by their lagged values. The interaction terms are again insignificant at 10%. Hence, the empirical evidence does not lend support to either H5 or H6.

6. INTERACTION BETWEEN URBANIZATION, REPRESENTATION AND INTERSTATE WARFARE

The previous section reviewed alternative hypotheses respectively arguing that authoritarian (H5) and representative (H6) regimes performed better in transforming war stimuli into taxes and the empirical evidence did not support one or the other. This section investigates yet a finer hypothesis (H7) that posits economic structure determined whether authoritarian or representative regimes performed better, because it determined elite incentives with respect to the build-up of the central apparatus.

The hypothesis we investigate in this section is consistent with the characterization of the impact of political regime in the previous section, in that the type of regime mattered as it determined the organizational capacity of domestic elites at times of war. The added insight we offer is that economic structure determined whether a particular regime type moderated war's impact against or towards gains in state capacity, because it determined whether the elites were opposed to or willing to cooperate for state-building. In particular, in rural-agrarian economies, elites tended to oppose state-building, and hence, as conjectured in H5, authoritarian regimes, where elites had low organizational capacity, tended to perform better in turning wars into taxes. In contrast, in urban-commercial economies, elites had more incentive to cooperate with the ruler over taxation, and as conjectured in H6, their greater organizational capacity under representative regimes improved fiscal performance.

As to why rural elites might be more antagonistic and urban elites more cooperative towards state-building, the literature suggests a number of reasons. For the landowning elites in rural-agrarian economies, retaining their coercive capabilities was critical for administering and taxing land and labor, the main economic resources.⁴⁶ Rural elites also depended on their coercive capacity to defend their interests against infringements by the central state.⁴⁷ These factors arguably made them wary of the implications of the monopolization of coercion by the central apparatus. With urbanization, as well as the commercialization and commodification of land and labor, however, the effectiveness of coercion in appropriating economic resources declined. Urban elites were also less averse to the redistributive implications of a centralized coercive apparatus, because commercial wealth was more mobile, harder to monitor and seize by coercion.⁴⁸ In fact, to the extent that it served their interests in interstate war-making over commerce and colonies, elites stood to benefit from a militarily capable central apparatus.⁴⁹ This set of arguments is consistent with the following hypothesis:

H7: Interstate warfare tended to induce greater gains in centralized fiscal capacity under authoritarian regimes at lower urbanization rates and under representative regimes at higher urbanization rates.

Empirical Analysis

In this section we extend the empirical models to investigate H7. To do so, we include the three-way interaction term between the proxies for war pressure, urbanization and representation as a regressor. For unbiased estimation, it is also necessary to include the three two-way interaction terms. Hence, the baseline regression equation for OLS with PCSE estimation is:

$$\text{Taxrev}_{it} = \alpha_i + \beta_t + X'_{it}\gamma + \theta_1 \text{Warp}_{it} + \theta_2 \text{Urb}_{it} + \theta_3 \text{Repr}_{it} + \theta_4 (\text{Warp}_{it} * \text{Repr}_{it}) + \theta_5 (\text{Warp}_{it} * \text{Urb}_{it}) + \theta_6 (\text{Repr}_{it} * \text{Urb}_{it}) + \theta_7 (\text{Warp}_{it} * \text{Repr}_{it} * \text{Urb}_{it}) + \varepsilon_{it}$$

H7 states that the estimated coefficient of the three-way interaction term, θ_7 , should be positive and significant.

TABLES 6-7 ABOUT HERE

⁴⁶ This argument is raised in the Marxist literature, reviewed in section 3, and Tilly (1992), to be discussed below.

⁴⁷ Ardant and Tilly (1975), Karaman (2009).

⁴⁸ For a theoretical model, see Bates and Lien (1985). Also see Odd-Helge and Moore (2008) and the discussion in Hoffman and Norberg (1994) p. 309.

⁴⁹ For example, see O'Brien (1988).

Table 6 reports the OLS with PCSE estimation results. In all specifications, θ_7 is positive and significant at 5% level. Table 7 reports the results for instrumental variables regressions. In specifications 1 and 2, representation proxies *Reprtax* and *Repract* and their two and three way interactions are instrumented by their lagged values and their two and three-way interactions. Specifications 3-4 also control for real wage and log population. Specifications 7-8 drop 16th century and specifications 9-10 drop polities with short series. In specifications 5 and 6 urbanization and its two and three-way interactions are instrumented by its lagged value and its two and three-way interactions. The evidence is again consistent with H7, with a positive coefficient significant at 10% or lower.⁵⁰

7. INTERPRETATION OF THE RESULTS

The empirical evidence we have analyzed strongly supports the hypothesis that the impact of warfare on revenues was conditional on economic structure-political regime combinations. In this section, we interpret this finding and discuss how it relates to the existing literature.

⁵⁰ The estimated coefficients for the period fixed effects, not included in the regression results for sake of brevity, reveal a minor decline followed by a trend of increase over the period. From early 16th century to early 17th century, there is a decline in per capita tax revenues by about 1 day of wages (insignificant at 10% level). Thereafter, there is a trend of increase. Compared to early 16th century, by 1700, per capita taxes are 1-3 days higher (significant at 10%), and by 1800, 4-6 days higher (significant at 0.1%). These patterns are consistent with the commonly accepted timeline for the transformation for European military and state apparatuses, with a continent-wide crisis in early-17th century followed by reorganization and consolidation thereafter.

Table 8: Classification of Polities Based on Urbanization Rates and Actual Regime Type

| | Authoritarian (Reprtax=0) | Representative (Reprtax=1) |
|----------------|---|---|
| Urb. Rate<8.0% | France (1500-1679) ² Prussia (1650-1799) ³ Russia ¹ Sweden (1500-1649) ¹ | England (1500-1639) ⁴ Prussia (1618-1649) ¹ Austrian Habsburgs ¹ Sweden (1650-1799) ¹ Spain (1500-1539) Poland Lithuania ¹ Portugal (1500-1529) ⁵ |
| Urb. Rate>8.0% | France (1680- 1789) Spain (1650-1799) The Ottomans ⁶ | England (1640-1799) ⁷ Spain (1540-1649) ⁸ Portugal (1530-1799) ⁹ Dutch Republic ¹ Venice ¹ |

Decades at which the difference in war pressure's impact on per capita tax revenues for the two regime types is significant at 10% level:

¹All ²1500-1579 ³1650-1729 ⁴1500-1589 ⁵1500-1519

⁶1590-1799 ⁷1680-1799 ⁸1590-1609 ⁹1550-1719

Table 8 puts the empirical results in historical context. The columns in the table correspond to different regime types and the rows correspond to urbanization rates below and above a threshold rate of 8.0%.⁵¹ The estimation results suggest that during the historical episodes in the diagonal cells (i.e. rural-authoritarian and urban-representative combinations) states performed better in wars with their actual regime than they would have under the alternative regime type. These episodes generally overlap with episodes of military ascendancy and successful state-building, such as France and Sweden in the first half and Prussia and England in the second half

⁵¹ The 8.0% urbanization threshold is based on specification 1 in Table 6. For the derivation of this threshold, note that the estimated difference in the fiscal impact of war for the two regime types is captured by the term:

$$\frac{\partial^2 \text{Taxrev}}{\partial \text{War} \partial \text{Reprtax}} = \theta_4 + \theta_7 * \text{urb} = -13.18 + 164.27 * \text{Urb}$$

The first term above suggests that at an urbanization rate of 0%, for each casualty per 1000 population, a representative regime raises 13.18 days less taxes than an authoritarian regime. The second term suggests that each additional percent increase in the urbanization rate, the differential shifts by 1.64 days in favor of a representative regime. Hence, beyond an urbanization rate of 8.0%, representative regimes perform better at funding wars. At the 10% confidence level, the differential between the fiscal performances of two regime types is significant below an urbanization rate of 5.2% and above an urbanization rate of 10.7%.

of the early modern era. For the cases in the off-diagonal cells, it is estimated that, had the states switched their regime type, they would have raised more revenues per unit of war pressure. These are generally cases of states with fiscal troubles, such as the Ottomans, Poland-Lithuania as well as England in the first half and France in the second half of the early modern era.⁵²

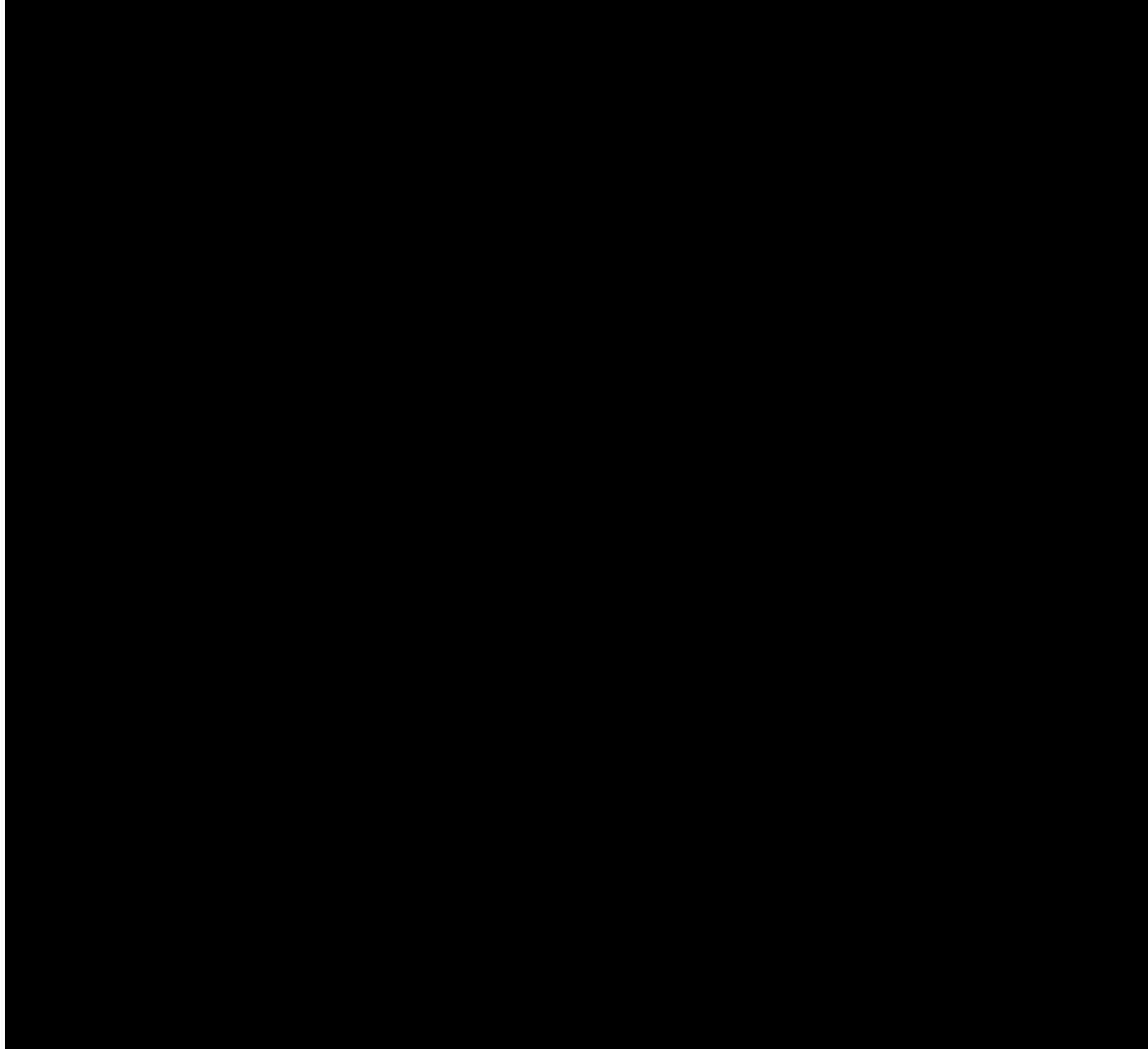


Figure 5 presents the same findings by taking individual countries and making a hypothetical comparison of per capita tax revenues under the two regime types. First, consider two polities that remained rural through the early modern era, Austrian Habsburgs and Prussia. The

⁵² The classification of historical episodes in Table 8 is generally robust across different empirical specifications we estimate. In these specifications, the estimated threshold urbanization rates vary in a narrow band between 7.3%- and 11.5%. In our sample, Prussia, Russia, Sweden, Austria and Poland-Lithuania are consistently below these thresholds, whereas as Dutch Republic and Venice are consistently above them. For Spain, Portugal, France, the Ottomans and England, there were episodes in their histories with urbanization rates close to the threshold levels, and the exact decade at which they switch from one category to another changes with specification.

estimation results suggest that under war pressure both were in better position to raise revenues with authoritarian regimes. In the figure, this is reflected in the pattern that whenever war pressure rises, the wedge between the estimated revenues for an authoritarian regime, the dashed line, and for a representative regime, the solid line, increases. In actuality, Austria had a representative regime which, our results suggest, induced it to underperform. Prussia also initially had a representative regime, but had switched to an authoritarian regime by 1650, and thereafter the revenue performance improved as estimated.⁵³

At the other extreme, the Dutch Republic had urbanization rates of 15% or more. At these rates, the Republic is estimated to have performed better at wars with its actual representative regime than it would have under a hypothetical authoritarian regime. Finally, England offers an interesting case, in that it was rural in the 16th century and urban by the 18th. Hence, in the former era, an authoritarian regime, and in the latter, a representative regime is estimated to better respond to war. England had a representative regime throughout the period, and its actual tax revenues closely follow those estimated for a representative regime.⁵⁴

A pattern that emerges from Table 8 and Figure 5 is that polities moved from cell to cell, improving or worsening their fiscal performance at war. In some cases, this was due to a change in regime type for better (e.g. Prussia) or for worse (e.g. Sweden). In others, regime type stayed the same, but changes in economic structure changed the way regime moderated the impact of war. For example, as discussed above, rapid urbanization in England turned the representative regime from an obstacle into a catalyst of war financing, whereas in France, where the urbanization rate also increased, it undermined the early advantage of the authoritarian regime.

In terms of the theoretical literature, our findings closely relate to Tilly's (1992) seminal work that traces the variations in state-building experiences across Europe back to differences in economic structure. Tilly distinguishes between: "coercion-intensive regions (areas of few cities and agricultural predominance, where direct coercion played a major part in production)"

⁵³ The estimation results also suggest the fiscal wedge between the two regime types could be substantial. For example, during 1620-1649 (Thirty Years War), Austria's urbanization rate was around 2.2 percent and war pressure was about 0.4 casualties per 1000 population per year. Based on estimation results specification 1 in table 6, Austria's per capita tax revenue would be $(-13.18 + 164.27 * 0.022) * 0.4$ or about 4 days of wages higher had it switched to an authoritarian regime.

⁵⁴ Figure 5 also helps visualize and reinterpret the results of the empirical analysis in the earlier sections. For example, in section 4, omitting the interaction terms from the regression, we found that wars improved fiscal performance (H1). Figure 5 suggests that, while this might be true on average, the impact of war varied significantly for different regime-urbanization combinations, and for the wrong combinations, could even be negative.

such as Poland, Hungary, Russia and Prussia; “capital-intensive regions (areas of many cities and commercial predominance, where markets, exchange, and market-oriented production prevailed)” such as Dutch Republic, Portugal Venice; and polities in between the two extremes such as France and England. In the rural coercive-intensive regions, resources remained embedded in agriculture, and the ruler and rival armed landlords jointly exploited the peasantry and bargained over the extracted resources. In the urban-capital intensive regions, rulers necessarily relied on contracts with capital owners over taxation and debt. In Tilly, these variations in economic structure, together with the intense warfare of the era determines the variation in state-structure, with bulky and permanent states in rural regions, lean and small states in capital intensive regions.

Ertman’s (1997) work on state-building, on the other hand, identifies an independent impact for political regime that worked towards different outcomes in different polities. In his classification, representative regimes in England, Sweden and Denmark, and absolutist regimes in Prussia helped build proto-bureaucratic and centralized states, whereas absolutist regimes in France, Spain and representative regimes in Poland-Lithuania and Hungary (before its partition in 1641) worked against it. He argues that the differences in the impacts of regime types was driven by the sequencing of the establishment of the representative assembly, the onset of war pressure and the proliferation of European universities as well as other idiosyncratic shocks such as capable rulers and foreign invasions.

The main difference between our findings and these two works is that we do not trace variations in state-building patterns back to economic structure or regime per se, but to the interaction of the two. Tilly does not specify an independent impact for political regime, and does not propose an explanation for the variation in state-building experience in regions with similar economic structure, i.e. why Prussia and Russia performed well and Poland-Lithuania and Hungary failed. Ertman, on the other hand, does not specify a role for economic structure but rather puts the emphasis on the path-dependency of the state-building process.

It is also worth highlighting that while our results identify two different formulas for successful state-building under war pressure, they come at different political conditions. In particular, the underlying argument suggests, for the central military-executive apparatus, changes in economic structure resulted in different trade-offs between fiscal capacity at war and autonomy from domestic elites. In the rural-agrarian setting, higher fiscal capacity was associated with higher autonomy through an authoritarian regime. In contrast, for urban-commercial economies, higher fiscal capacity occurred at the cost of autonomy, because it required cooperation and consent

seeking through a representative regime. In fact, for the representative regimes in urban economies, such as England in the 18th century and the Dutch Republic, it becomes difficult to speak of a ruler and state apparatus as distinct and autonomous from domestic elites.

On the empirical front, our findings complement and qualify those of Dincecco (2009). Dincecco argues that fiscal gains in Europe were driven by two factors: establishment of parliamentary control over spending; and centralization of tax administration. He argues that, with the exception of England and the Dutch Republic, these changes occurred in the 19th century, as did the gains in fiscal capacity. Our revenue series suggest, in contrast, that the fiscal gains dated as far back as the 16th century and occurred at different periods for different states. Moreover, in terms of explaining the gains, we find that the argument for the positive effect of parliamentary control over spending is underspecified. The reason is that the parliamentary control over spending had a necessary precondition, i.e. elites should be willing to cooperate and engage in a tax-for-control over spending deal. In the urban Dutch Republic, Venice and post-1640 England they were willing to do this, so the deal was struck. In Poland, rural elites were opposed to state-building, and there was no deal. It is, therefore, not the institutional innovation of control over spending per se, but also the underlying elite incentives that drove the fiscal gains.

Finally, our findings also relate to the literature on the impact of political regime on public debt and its role in the rise of modern states.⁵⁵ Our results are most similar to Stasavage's finding that the impact of representative regimes on the capacity to borrow was greater for urban economies, where commercial wealth prevailed over landed interests and dominated the assembly.⁵⁶ This is the same set of polities for which we find evidence for the greater effectiveness of representative regimes in war financing. In other words, the two mechanisms worked towards the same effect. We also want to argue, however, that public debt was ultimately underpinned by expanding revenues. Hence, the basis for the growth in state capacity in this period was taxation, above all.⁵⁷

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the patterns they reveal. In broad terms, the series identify

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TABLE 2: Baseline Regressions

DETERMINANTS OF PER CAPITA TAX REVENUE (IN DAYS OF WAGES)

| OLS with PCSE | | | | | | | | |
|---------------------|----------------------|----------------------|----------------------|-------------------------|----------------------|------------------------------|------------------------|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Warp | 2.331**** (0.68) | 2.202*** (0.75) | 1.998*** (0.69) | 1.704** (0.73) | 2.240*** (0.84) | 1.913** (0.88) | 1.842** (0.75) | 1.779** (0.77) |
| Urb | 47.785**** (7.23) | 40.641**** (8.54) | 37.324**** (8.01) | 29.810*** (9.97) | 39.391**** (9.22) | 28.938*** (10.59) | 38.384**** (10.13) | 36.293*** (12.02) |
| Repr | -1.64 (1.14) | 0.14 (1.64) | -1.01 (1.09) | 0.21 (1.78) | -0.56 (1.29) | 3.29 (2.54) | -0.62 (1.13) | -0.12 (1.89) |
| L.Rwage | | | 0.38 (0.24) | 0.41 (0.32) | 0.522* (0.28) | 0.45 (0.37) | 0.37 (0.31) | 0.37 (0.33) |
| L.InPop | | | 2.314** (1.08) | 2.780** (1.27) | 2.094** (1.06) | 2.458** (1.20) | 2.253* (1.18) | 2.680** (1.26) |
| Observations | 217 | 179 | 201 | 164 | 159 | 129 | 161 | 147 |
| R2 | 0.79 | 0.79 | 0.82 | 0.81 | 0.83 | 0.83 | 0.80 | 0.80 |
| Regime proxy | Reprtax | Repract | Reprtax | Repract | Reprtax | Repract | Reprtax | Repract |
| Dropped from Sample | | Poland, Austria | Russia | Poland, Austria, Russia | Russia, 16th century | Poland, Russia, 16th century | Russia, Poland, Sweden | Poland, Austria, Russia, Portugal, Sweden |

Standard errors in paranthesis

Levels of statistical significance: * 0.1 ** 0.05 *** 0.01 **** 0.001

Estimated using Stata xtpcse procedure, with two-way fixed effects and c(ar1) and hetonly options

TABLE 3: Controlling for Endogeneity

DETERMINANTS OF PER CAPITA TAX REVENUE (IN DAYS OF WAGES)

| Two-Stage IV Estimation | | | | | | | | |
|-------------------------|-----------------------|----------------------|-----------------------|-------------------------|-----------------------|---------------------------------------|----------------------------------|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Warp | 3.587*** (1.09) | 3.176*** (1.12) | 2.766*** (0.99) | 1.981* (1.09) | 2.832*** (1.10) | 2.324** (0.99) | 2.229** (1.07) | 1.30 (1.47) |
| Urb | 73.238**** (10.46) | 58.208*** (22.13) | 68.428**** (12.39) | 45.033** (20.41) | 69.243**** (14.30) | 39.234*** (12.92) | 66.938**** (13.79) | 34.377* (18.21) |
| Repr | -3.246** (1.38) | -0.68 (6.47) | -1.58 (1.20) | 7.16 (10.22) | -1.74 (1.88) | 8.300* (4.70) | -1.21 (1.26) | 15.69 (10.94) |
| Observations | 196 | 161 | 187 | 152 | 159 | 129 | 150 | 136 |
| R2 | 0.66 | 0.70 | 0.71 | 0.71 | 0.70 | 0.73 | 0.74 | 0.58 |
| Regime proxy | Reprtax | Repract | Reprtax | Repract | Reprtax | Repract | Reprtax | Repract |
| Endog. Variables | Reprtax, Urb | Repract, Urb | Reprtax, Urb | Repract, Urb | Reprtax, Urb | Repract, Urb | Reprtax, Urb | Repract, Urb |
| Instruments | L5.Reprtax, L5.Urb | L10.Repract, L5.Urb | L5.Reprtax, L5.Urb | L10.Repract, L5.Urb | L5.Reprtax, L5.Urb | L10.Repract, L5.Urb | L5.Reprtax, L5.Urb | L10.Repract, L5.Urb |
| Control Variables | | | L.Rwage, L.InPop | L.Rwage, L.InPop | L.Rwage, L.InPop | L.Rwage, L.InPop | L.Rwage, L.InPop | L.Rwage, L.InPop |
| Dropped from Sample | | Poland, Austria | Russia | Poland, Austria, Russia | Russia, 16th century | Poland, Austria, Russia, 16th century | Russia, Poland, Portugal, Sweden | Poland, Austria, Russia, Portugal, Sweden |

Standard errors in paranthesis

Levels of statistical significance: * 0.1 ** 0.05 *** 0.01 **** 0.001

Estimated using Stata xtivreg2 procedure with fe first robust bw(1) options

For Repr in (2) and (8) AP F statistics reject weakly identified instruments at 0.05 level

For all other variables in all specifications, AP F statistics reject weakly identified instruments at 0.01 level

TABLE 4: Baseline Regressions with Representation-War Pressure Interaction

| DETERMINANTS OF PER CAPITA TAX REVENUE (IN DAYS OF WAGES) | | | | | | | | |
|---|---------------|-----------------|------------|-------------------------|----------------------|---------------------------------------|----------------------------------|---|
| | OLS with PCSE | | | | | | | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Warp | 3.687* | 3.492** | 2.60 | 2.21 | 3.29 | 2.67 | 2.30 | 2.75 |
| | (2.03) | (1.69) | (2.12) | (1.66) | (2.25) | (1.80) | (2.14) | (2.02) |
| Urb | 47.807**** | 41.089**** | 37.582**** | 30.335*** | 39.914**** | 29.857*** | 38.742**** | 37.006*** |
| | (7.28) | (8.65) | (8.02) | (10.07) | (9.24) | (10.78) | (10.15) | (12.07) |
| Repr | -1.36 | 0.59 | -0.91 | 0.32 | -0.37 | 3.50 | -0.54 | 0.07 |
| | (1.19) | (1.69) | (1.14) | (1.79) | (1.34) | (2.56) | (1.17) | (1.90) |
| Warp*Repr | -1.67 | -1.96 | -0.73 | -0.74 | -1.33 | -1.15 | -0.57 | -1.32 |
| | (2.10) | (1.90) | (2.19) | (1.91) | (2.35) | (2.09) | (2.23) | (2.26) |
| L.Rwage | | | 0.38 | 0.42 | 0.529* | 0.47 | 0.37 | 0.38 |
| | | | (0.24) | (0.32) | (0.28) | (0.37) | (0.31) | (0.33) |
| L.InPop | | | 2.227** | 2.685** | 1.928* | 2.300* | 2.176* | 2.494* |
| | | | (1.09) | (1.29) | (1.08) | (1.23) | (1.20) | (1.28) |
| Observations | 217 | 179 | 201 | 164 | 159 | 129 | 161 | 147 |
| R2 | 0.79 | 0.79 | 0.82 | 0.81 | 0.83 | 0.82 | 0.80 | 0.80 |
| Regime proxy | Reprtax | Repract | Reprtax | Repract | Reprtax | Repract | Reprtax | Repract |
| Dropped from Sample | | Poland, Austria | Russia | Poland, Austria, Russia | Russia, 16th century | Poland, Austria, Russia, 16th century | Russia, Poland, Portugal, Sweden | Poland, Austria, Russia, Portugal, Sweden |

Standard errors in paranthesis

Levels of statistical significance: * 0.1 ** 0.05 *** 0.01 **** 0.001

Estimated using Stata xtpcse procedure, with two-way fixed effects and c(ar1) and hetonly options

TABLE 5: Controlling for Endogeneity with War Pressure-Representation Interaction

| DETERMINANTS OF PER CAPITA TAX REVENUE (IN DAYS OF WAGES) | | | | | | | | |
|---|---|---|---|--|---|--|---|--|
| | Two-Stage IV Estimation | | | | | | | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Warp | 6.372** | 5.705** | 5.186* | 4.287* | 5,19 | 3,64 | 4,60 | 0,54 |
| | (2,93) | (2,42) | (3,09) | (2,45) | (3,28) | (2,59) | (3,12) | (4,09) |
| Urb | 74.674**** | 57.492*** | 70.092**** | 48.005** | 70.727**** | 41.429*** | 68.816**** | 34.031* |
| | (10,48) | (20,74) | (12,67) | (19,12) | (14,71) | (13,50) | (14,22) | (18,30) |
| Repr | -2,198 | 1,542 | -0,971 | -5,703 | -0,989 | 8.607* | -0,668 | 15,322 |
| | (1,38) | (5,94) | (1,30) | (6,79) | (1,97) | (4,71) | (1,33) | (10,38) |
| Warp*Repr | -3,531 | -3,943 | -2,905 | -1,924 | -2,924 | -1,869 | -2,916 | 1,017 |
| | (3,12) | (2,67) | (3,23) | (2,74) | (3,47) | (2,86) | (3,30) | (4,09) |
| Observations | 196 | 161 | 187 | 152 | 159 | 129 | 150 | 136 |
| R2 | 0,67 | 0,71 | 0,71 | 0,74 | 0,70 | 0,73 | 0,74 | 0,59 |
| Regime proxy | Reprtax | Repract | Reprtax | Repract | Reprtax | Repract | Reprtax | Repract |
| Endog. Variables | Reprtax, Urb and their | Repract, Urb and their | Reprtax, Urb and their | Repract, Urb and their | Reprtax, Urb and their | Repract, Urb and their | Reprtax, Urb and their | Repract, Urb and their |
| Instruments | L5.Reprtax, L5.Urb and their interactions | L10.Repract, L5Urb and their interactions | L5.Reprtax, L5.Urb and their interactions | L10.Repract, L5.Urb and their interactions | L5.Reprtax, L5.Urb and their interactions | L10.Repract, L5.Urb and their interactions | L5.Reprtax, L5.Urb and their interactions | L10.Repract, L5.Urb and their interactions |
| Control Variables | | | L.Rwage, L.InPop | L.Rwage, L.InPop | L.Rwage, L.InPop | L.Rwage, L.InPop | L.Rwage, L.InPop | L.Rwage, L.InPop |
| Dropped from Sample | | Poland, Austria | Russia | Poland, Austria, Russia | Russia, 16th century | Poland, Austria, Russia, 16th century | Russia, Poland, Portugal, Sweden | Poland, Austria, Russia, Portugal, Sweden |

Standard errors in paranthesis

Levels of statistical significance: * 0.1 ** 0.05 *** 0.01 **** 0.001

Estimated using Stata xtivreg2 procedure with fe first robust bw(1) options

For Repr in (4) and (8) AP F statistics reject weakly identified instruments at 0.05 level

for all other variables in all specifications, AP F statistics reject weakly identified instruments at 0.01 level

TABLE 6: Baseline Regressions with 2 and 3-way Interactions

DETERMINANTS OF PER CAPITA TAX REVENUE (IN DAYS OF WAGES)

| | OLS with PCSE | | | | | | | |
|---------------------|------------------------|-----------------------|-----------------------|-------------------------|-----------------------|---------------------------------------|----------------------------------|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Warp | 15.281*** (5.46) | 14.767** (5.74) | 15.212** (6.23) | 12.536** (6.22) | 16.871*** (6.54) | 14.683** (7.03) | 15.976** (6.55) | 14.774** (6.27) |
| Urb | 50.705* (27.32) | 33.313** (16.14) | 59.688** (27.19) | 21.89 (17.75) | (6.01) (37.42) | 29.82 (18.21) | 39.43 (28.78) | 60.939* (32.12) |
| Repr | -1.24 (2.37) | 0.32 (2.76) | 0.43 (2.36) | -0.29 (2.86) | -3.68 (3.15) | 5.900* (3.48) | -0.18 (2.30) | 2.86 (3.55) |
| Warp*Repr | -13.184** (5.59) | -17.038** (7.12) | -13.303** (6.35) | -14.524* (7.62) | -15.911** (6.68) | -23.079*** (8.60) | -15.299** (6.80) | -16.434** (7.46) |
| Warp*Urb | -164.851*** (63.68) | -142.060** (64.35) | -174.706** (70.37) | -127.276* (68.61) | -182.216** (75.75) | -149.799* (77.28) | -187.154** (76.64) | -158.057** (71.04) |
| Repr*Urb | (3.16) (26.23) | 8.78 (19.52) | (20.89) (25.94) | 11.15 (20.68) | 47.80 (37.29) | (6.21) (22.12) | (2.39) (26.27) | (27.16) (32.87) |
| Warp * Repr*Urb | 164.266** (64.21) | 161.496** (68.23) | 174.829** (70.86) | 144.992** (72.51) | 188.429** (76.25) | 200.497** (81.19) | 193.365** (77.58) | 174.648** (72.99) |
| L.Rwage | | | 0.35 (0.24) | 0.38 (0.32) | 0.32 (0.27) | 0.45 (0.37) | 0.28 (0.30) | 0.35 (0.32) |
| L.InPop | | | 1.66 (1.09) | 2.168* (1.26) | 1.26 (1.07) | 1.74 (1.24) | 1.51 (1.18) | 1.87 (1.28) |
| Observations | 217 | 179 | 201 | 164 | 159 | 129 | 161 | 147 |
| R2 | 0.79 | 0.80 | 0.83 | 0.83 | 0.84 | 0.85 | 0.82 | 0.83 |
| Regime proxy | Reprtax | Repract | Reprtax | Repract | Reprtax | Repract | Reprtax | Repract |
| Dropped from Sample | | Poland, Austria | Russia | Poland, Austria, Russia | Russia, 16th century | Poland, Austria, Russia, 16th century | Russia, Poland, Portugal, Sweden | Poland, Austria, Russia, Portugal, Sweden |

Standard errors in paranthesis

Levels of statistical significance: * 0.1 ** 0.05 *** 0.01 **** 0.001

Estimated using Stata xtpcse procedure, with two-way fixed effects and c(ar1) and hetonly options

TABLE 7: Controlling for Endogeneity with 2 and 3-way Interactions

DETERMINANTS OF PER CAPITA TAX REVENUE (IN DAYS OF WAGES)

| Two-Stage IV Estimation | | | | | | | | | | |
|---|---------------------------------|----------------------------------|---------------------------------|----------------------------------|------------------------------|------------------------------|---------------------------------|---------------------------------------|----------------------------------|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
| Warp | 24.547*** (7.25) | 27.343*** (9.92) | 25.113*** (8.60) | 25.792*** (9.93) | 31.061*** (7.99) | 24.364*** (8.33) | 23.141*** (8.88) | 29.911*** (11.16) | 27.661*** (8.73) | 28.983*** (9.89) |
| Urb | 23.91 (32.01) | 15.33 (23.57) | 72.301*** (27.83) | 1.30 (32.15) | 70.445** (30.94) | 77.232*** (36.94) | 24.29 (44.30) | -12.35 (35.72) | 36.71 (28.62) | -29.39 (51.02) |
| Repr | -6.57 (4.07) | -5.19 (25.92) | 1.47 (3.65) | 2.09 (6.26) | -1.44 (2.51) | 1.03 (3.27) | -0.16 (5.04) | 2.77 (3.41) | -0.17 (3.93) | -2.66 (6.88) |
| Warp*Repr | -19.219** (7.87) | -30.367* (16.31) | -22.746** (8.84) | -30.198** (12.58) | -28.439*** (8.31) | -30.969*** (10.49) | -22.262** (9.47) | -40.568*** (13.26) | -27.914*** (9.54) | -32.772*** (11.88) |
| Warp*Urb | -260.732*** (87.00) | -269.314** (129.18) | -295.056*** (97.30) | -288.042** (118.28) | -354.108*** (97.86) | -237.097** (96.49) | -252.827** (99.08) | -337.854** (137.26) | -328.247*** (103.95) | -321.081*** (118.64) |
| Repr*Urb | 25.36 (33.11) | 39.78 (87.53) | -38.26 (29.74) | 21.32 (31.35) | 2.24 (30.22) | -18.90 (37.73) | 13.17 (48.01) | 42.85 (39.56) | -7.08 (31.58) | 57.18 (56.50) |
| Warp*Repr*Urb | 249.942*** (88.48) | 297.009* (158.16) | 297.471*** (97.20) | 322.300** (127.55) | 351.512*** (97.71) | 279.771*** (102.71) | 262.429*** (100.02) | 402.707*** (142.44) | 343.873*** (106.17) | 352.636*** (122.35) |
| Observations | 217 | 179 | 201 | 164 | 187 | 152 | 159 | 129 | 161 | 147 |
| R2 | 0.69 | 0.73 | 0.76 | 0.76 | 0.73 | 0.76 | 0.74 | 0.74 | 0.79 | 0.76 |
| Regime proxy | Reprtax | Repract | Reprtax | Repract | Reprtax | Repract | Reprtax | Repract | Reprtax | Repract |
| Endog. Variables | Reprtax and its interactions | Repract and its interactions | Reprtax and its interactions | Repract and its interactions | Reprtax and its interactions | Repract and its interactions | Reprtax and its interactions | Repract and its interactions | Reprtax and its interactions | Repract and its interactions |
| Instruments | L5.Reprtax and its interactions | L10.Repract and its interactions | L5.Reprtax and its interactions | L10.Repract and its interactions | L5.Urb and its interactions | L5.Urb and its interactions | L5.Reprtax and its interactions | L10.Repract and its interactions | L5.Reprtax and its interactions | L10.Repract and its interactions |
| Control Variables | | | L.Rwage, L.InPop | L.Rwage, L.InPop | L.Rwage, L.InPop | L.Rwage, L.InPop | L.Rwage, L.InPop | L.Rwage, L.InPop | L.Rwage, L.InPop | L.Rwage, L.InPop |
| Dropped from Sample | | Poland, Austria | Russia | Poland, Austria, Russia | Russia | Poland, Austria, Russia | Russia, 16th century | Poland, Austria, Russia, 16th century | Russia, Poland, Portugal, Sweden | Poland, Austria, Russia, Portugal, Sweden |
| Standard errors in paranthesis | | | | | | | | | | |
| Levels of statistical significance: * 0.1 ** 0.05 *** 0.01 **** 0.001 | | | | | | | | | | |
| Estimated using Stata xtivreg2 procedure with fe first robust bw(1) options | | | | | | | | | | |
| For Repr in (2), AP F statistics does not reject weakly identified instruments at 0.1 level | | | | | | | | | | |
| For Repr in (1), Repr*Urb in (2), urb in (6) AP F statistics reject weakly identified instruments at 0.05 level | | | | | | | | | | |
| For all other variables in all specifications, AP F statistics reject weakly identified instruments at 0.01 level | | | | | | | | | | |

APPENDIX

Note to the Editor: We intend to place this appendix on the web rather than include it in the published article.

Data Sources for Tax Revenues, Population, Real and Nominal Wages

The tax revenue figures are annual net monetary revenues to central treasuries excluding loans. In order to be able to compare the figures across polities, they are converted from domestic currencies to silver. For econometric analysis, the figures are averaged over decades. In calculating the averages, we did not interpolate the revenue figures for missing years.

Forming a comprehensive historical dataset of tax revenues poses a number of challenges. There were differences in fiscal systems and budgeting procedures across Europe, and each fiscal system underwent significant changes over the centuries. Consequently, the figures we provide should be considered estimates rather than exact figures. Below we list the source we used for each date. We hope to benefit from feedback from experts on individual polities regarding any misinterpretation of the sources, necessary adjustments and suggestions for new sources to improve and expand the dataset. As a precaution against the uneven quality of data across the polities, in the econometric analyses, we report separately the results for the subsamples with more reliable series.

Austrian Habsburgs: Sources for revenue series are Rauscher (2008) for 1550, Berenger (1995) for 1650-1699, Dickson (1995) for 1717-1778 and Mitchell (2003) for 1781-1800. To account for different domestic currency units we used the conversion rates 1 Florin=1 Gulden=60 Kreuzer and 1 Gulden=2 Kronen. To convert the revenues from domestic currency to silver we used silver value of Kreuzer between 1550-1753 from Allen (2001) and 1 Gulden =11.5 grams of silver between 1754-1800. The urban wages are for Vienna from Allen (2001). Population figures are for Austria-Hungary from McEvedy and Jones (1978)

Dutch Republic: For 1609-1794 we used revenue series from Fritschy (2009) subtracting the estimated loan receipts. For these years we converted the figures in Guilders to silver according to Allen (2001). Wages are for Amsterdam, from the same source. Population is from de Vries and van der Woude (1997).

England/ Great Britain: The revenue figures are total revenues of the English Crown for 1500-1749 from O'Brien (1995), central government revenue of Great Britain for 1750-1800 from Mitchell (2003). Population figures are for England and Wales from 1500 to 1707. The

population of Scotland is added after 1707. Population estimates are from McEvedy and Jones (1978). Wages for London and the silver content of money is from Allen (2001).

France: The revenue figures are from Bean (1973) for 1500-1546, Hoffman (1986) for 1560-1779 and White (1989) for 1783 and 1788. The silver value of livre tournois and Franc and wages for Paris are from Allen (2001). Population series are from McEvedy and Jones (1978).

Ottoman Empire: Revenue series are taken from Genç and Özvar (2006). Silver content of the currency and daily wages for Istanbul are taken from Pamuk (2001) and Özmucur and Pamuk (2002).

Poland Lithuania: Polish-Lithuanian fiscal apparatus remained decentralized through the early modern era. The permanent revenues of *kwarta* purse were insufficient to cover military expenses, and the kings remained dependent on temporary and irregular grants by the nobility dominated *Sejm* (Filipczak-Kocur 1995). For these reasons the revenue figures are subject to greater variance and are less precise compared to other polities in our sample. The figures for 1500, 1510, 1530, 1563, 1569, 1730, 1765, 1775 and 1791 are state revenues from Stone (2001). The figures for 1788, 1789 and 1790 are state revenues from Lewitter (1976). The figures for 1576-1717 are the total of *kwarta* and *sejm* revenues from Filipczak-Kocur (1995). For 1649-1717 and 1576-1587 the figures are for Kingdom of Poland only. Silver content of Zloty is from Wójtowicz (2006). Wages are for Krakow in the 16th and for Warshaw in 17th and 18th centuries from Allen (2001). Population is from McEvedy and Jones (1978).

Portugal: The revenue figures are based on Valerio (2002) for 1506, 1518, 1588, 1607, 1619, 1716, 1763, Macedo (1998) for 1557, 1588, 1593, 1607, 1619, 1660, 1681, 1716, Costa (2009) for 1593, 1607, 1612, 1619, 1625, 1632, 1641, 1656, 1660 and 1680 and Cardoso and Lains (2010) for 1762-76 and 1797-99. For years with more than one source on revenues, we take the average. We drop the observations for 1580-1640 from the econometric analysis because in this period Portugal formed a union of kingdoms with Spain. Wage data and silver content for Real were provided by Jaime Reis based on "Prices, Wages and Rents in Portugal, 1500-1900" project financed by the Portuguese Foundation for Science and Technology. Population is based on McEvedy and Jones (1978)

Prussia: Revenue series are based on Kellenbenz (1980) for 1600, 1638, 1643, 1652, 1661 and Körner (1995) for 1688-1800. Population figures are based on McEvedy and Jones (1978). The estimates on the decline in population during the Thirty Years War (1618-1648) vary, and we use an average rate of 40% decline (Rabb 1962, Pfister and Fertig 2010). Because the tax revenue figures for this period are very low, higher or lower estimates for population decline do

not have a major impact on the econometric results. Since wage series is not available for a Prussian town we use Leipzig series from Allen (2001) as a substitute. Silver content of Thaler until 1800 is from Shaw (1895).

Russia: Sources for revenues are Anisimov (1982) for 1680, 1701 and 1724, Kahan (1985) for 1720-3 and 1763-96 and Troitskii (1966) for 1749, 1751 and 1758. Population is for Russian Empire from McEvedy and Jones (1978). The nominal wages and silver content of Ruble are based on data provided by Global Price and Income History Group. We use Moscow wages for 1650-1670 and 1700-1709 and Saint Petersburg wages for 1713-17, 1720-23, 1805 and 1807 respectively available at http://gpih.ucdavis.edu/files/Wages_Moscow_1613-1871.xls and http://gpih.ucdavis.edu/files/Russia_p_w_1590s-1871.xls. For the decades in-between, we interpolate. We use silver content of silver Ruble based on figures provided by Peter Lindert at http://gpih.ucdavis.edu/files/Russia_Ag_content_ruble_1535-1913.xls. Real wage series for Russia is not available.

Spain: The Spanish revenue figures are for Castilian Crown until 1716 and Spanish Crown thereafter. Sources are Thompson (1994) for 1504-1546, 1607-1645 and 1674, Mauro and Parker (1980) for 1515 and 1623, Drelichman and Voth (2007) for 1555-1596, Kamen (1980) for 1666, Kamen (1974) for 1703-1718, Ozanam (1978) for 1722-1750, Galebert (1995) for 1753-1788, Cuenca Esteban (1981) for 1789-1800. To account for different domestic currency units we used the rates 1 Ducat=11 Reales de Vellón =374 Mavaredis and 1 Peseta= 4 Reales de Vellón. Silver value of Maravedis for 1504-1800. The population figures are from Alvarez-Nogal and De La Escosura (2007). Wage series are for Valencia until 1729 and Madrid thereafter from Allen (2001).

Sweden: The revenue figures for 1722-1777 are the sum of proper and special revenues from Fregert and Gustafsson (2008). The figures are converted using the rates 1 Krona= 6 Daler Silvermynt and 1 Rigsdaler=25.6973gr.of silver. The Daler Silvermynt-Rigsdaler conversion rate is based on Edvinsson (2010), wage series on Söderberg (2010) and population on McEvedy and Jones (1978).

Republic of Venice: Revenue, population and wage data were provided by Luciano Pezzolo.

Formulas and Data Sources for the War Pressure Variable

The data on wars is based on Clodfelter (2002). We include all interstate wars where there is at least one European polity on each side, and exclude colonial conquests. The exception to the rule is Ottoman wars with Iran and Egypt that were relatively strong states in this period. We

work with a definition of casualty that includes number of deaths in combat including those due to disease as well as the numbers of captured and missing soldiers, but exclude civilian deaths. Clodfelter provides the most detailed historical source, but does not always distinguish between the subcategories of casualties, and we make adjustments to the extent the data permits. Population figures are mainly based on McEvedy and Jones (1978).

The notation for the calculation of the war pressure variable (*Warp*) is as follows. Polities are indexed by $i, j, k \in C$, wars are indexed by $w \in W$, years are indexed by $t \in \{1500, 1501, \dots, 1799\}$. $Pop_{k,t}$ denotes population of polity k at year t (in millions). $Cas_{w,t}$ denotes the annual casualty for war w in year t (in thousands). $I_{k,w,t}$ is an indicator variable that takes value 1 if polity k participates in war w in year t and 0 otherwise. For each war w polity k participates in year t , we denote the set of k 's allies with $A_{k,w,t} \subset C$ and the set of k 's adversaries with $B_{k,w,t} \subset C$. $Warp_{k,w,t}$ denotes per capita war pressure for polity k in year t due to war w and $Warp_{k,t}$, our proxy, is the total per capita war pressure for polity k in year t .

The war pressure for polity k engaged in war w as part of coalition A in year t is calculated by dividing the annual casualty figures first by two and then by A 's population:

$$Warp_{k \in A_{k,w,t}, w, t} = \frac{Cas_{w,t}}{2 \sum_{i \in A_{k,w,t}} Pop_{i,t}}$$

Finally, per capita war pressure for polity k at year t , $Warp_{k,t}$, is calculated by adding up the war pressures due to different wars k engaged in year t :

$$Warp_{k,t} = \sum_{w \in W} Warp_{k,w,t} * I_{k,w,t}$$

Estimation Results for Dynamic Regressions with Lagged Dependent Variable

In the OLS with panel corrected standard errors models discussed in the text, the persistence of fiscal capacity from one decade to the next is accounted by a serial correlation in the error term. An alternative approach to modeling persistence would be to include the lagged value of the dependent variable in the regression. There are two main econometric models for doing so. The first of these is Ordinary Least Squares (OLS) with lagged dependent variable, two-way fixed effects and panel corrected standard errors. Without the interaction terms, the regression equation is:

$$Taxrev_{it} = Taxrev_{i(t-1)} + \alpha_i + \beta_t + X'_{it}\gamma + \theta_1 Warp_{it} + \theta_2 Urb_{it} + \theta_3 Repr_{it} + \varepsilon_{it}$$

where $\text{Taxrev}_{i(t-1)}$ is the per capita tax revenue in $t-1$ and ε_{it} is the disturbance term that exhibits heteroskedasticity. The second model is Arellano Bond difference GMM with orthogonal deviations and lagged dependent variable. Since most countries in our sample have a large number of periods, Arellano Bond estimation is considered less appropriate here, but we also report the results for it.⁵⁸

Table 9 presents findings for the first stage of the analysis, without any interaction terms. The results are generally consistent with the results in the main text. Specifications 1-8 are dynamic OLS estimation results for different proxies for regime type, with or without the control variables, and for different subsamples. Specifications 9-10 are Arellano Bond estimation results for the full sample. In all 10 specifications war pressure is significant at 5% or lower significance levels. The coefficient for urbanization is positive in all specifications, but is insignificant at 10% confidence level in specifications 2, 4, 6-8. It should be noted, however, the inclusion of a lagged dependent variable generally leads to lower levels of significance for variables that are persistent over time. Finally, the two proxies for representation are insignificant at the 10% level across the specifications.

Table 10 reports the results for the second stage of the analysis, which includes the interaction term between war and regime. Specifications 1-8 are OLS with PCSE, 9-10 by Arellano Bond dynamic estimation. The results are consistent across the specifications in that the interaction term is insignificant at 10% level.

Finally, Table 11 is the results for the dynamic specifications for the full set of two-way and three-way interactions. Consistent with the main text, in all specifications, θ_7 is positive and significant at 5% level.

TABLES 9-11 ABOUT HERE

⁵⁸ When fixed effects and lagged dependent variable are both included in the OLS regression, lagged dependent variable is correlated with the error term, and this creates a downward bias in the coefficient (Nickell 1981). However, as number of periods increase, dynamic panel bias gets smaller and OLS is more efficient than IV estimation. Arellano Bond is unbiased, and also permits IV estimation for the lagged dependent variable and other regressors employing their lagged values as instruments. As the number of periods increases, however, the proliferation of the number of instruments undermines the statistical power of Sargan test for their validity. To limit the extent of this problem, unless otherwise stated, we restrict the choice of instruments to a single lag. See Beck and Katz (2009), Roodman (2009), and Wawro (2002).

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| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
|----------|---------------------|---------------------|---------------------|---------------------|---------------------|---------------------|---------------------|---------------------|---------------------|---------------------|
| L.Taxrev | 0.564**** (0.07) | 0.535**** (0.08) | 0.555**** (0.08) | 0.533**** (0.07) | 0.531**** (0.07) | 0.508**** (0.07) | 0.545**** (0.07) | 0.546**** (0.07) | 0.435**** (0.07) | 0.443**** (0.07) |

| TABLE 10: Dynamic Regressions with Lagged Dependent Variable and Representation-War Pressure Interaction | | | | | | | | | | |
|--|---------------------|--------------------|---------------------|-------------------------------|-------------------------|--|---|---|---------------------|---------------------------------|
| DETERMINANTS OF PER CAPITA TAX REVENUE (IN DAYS OF WAGES) | | | | | | | | | | |
| | OLS with PCSE | | | | | | | | | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | Arellano Bond Dynamic Estimator |
| L.Taxrev | 0.560*** (0.07) | 0.523*** (0.08) | 0.554*** (0.08) | 0.527*** (0.09) | 0.531*** (0.08) | 0.501*** (0.10) | 0.544*** (0.09) | 0.539*** (0.09) | 0.449*** (0.08) | 0.463*** (0.09) |
| Warp | 3.45 (2.21) | 4.154* (2.13) | 3.22 (2.30) | 4.239* (2.25) | 3.55 (2.42) | 4.745* (2.48) | 3.43 (2.41) | 4.425* (2.27) | 4.222** (1.78) | 4.572*** (1.69) |
| Urb | 15.935*** (5.62) | 12.30 (8.02) | 13.724** (6.22) | 11.31 (8.37) | 16.521* (8.55) | 11.69 (10.35) | 12.701* (7.47) | 13.84 (9.27) | 20.094*** (6.11) | 14.443* (7.38) |
| Repr | 0.03 (0.80) | 0.60 (1.25) | -0.06 (0.82) | 0.08 (1.32) | 0.19 (1.16) | 1.15 (2.47) | 0.11 (0.87) | -0.22 (1.36) | 0.06 (0.70) | 0.65 (0.68) |
| Warp*Repr | -0.79 (2.30) | -1.42 (2.36) | -0.81 (2.39) | -1.89 (2.54) | -0.84 (2.56) | -2.23 (2.79) | -0.99 (2.59) | -2.27 (2.60) | -1.37 (1.77) | -1.85 (1.87) |
| Observations | 169 | 140 | 164 | 135 | 133 | 108 | 135 | 125 | 157 | 130 |
| R2 | 0.95 | 0.94 | 0.95 | 0.94 | 0.95 | 0.93 | 0.94 | 0.95 | | |
| ar1 | | | | | | | | | -1.91 | -1.86 |
| ar1p | | | | | | | | | 0.06 | 0.06 |
| ar2 | | | | | | | | | 0.36 | 0.54 |
| ar2p | | | | | | | | | 0.72 | 0.59 |
| Regime proxy | Reprtax | Repract | Reprtax | Repract | Reprtax | Repract | Reprtax | Repract | Reprtax | Repract |
| Control Variables | | | L.Rwage, L.InPop | L.Rwage, L.InPop | L.Rwage, L.InPop | L.Rwage, L.InPop | L.Rwage, L.InPop | L.Rwage, L.InPop | L.Rwage, L.InPop | L.Rwage, L.InPop |
| Dropped from Sample | | Poland, Austria | Russia | Poland, Austria, Russia | Russia, 16th century | Poland, Austria, Russia, 16th century | Russia, Poland, Portugal, Sweden | Poland, Austria, Russia, Portugal, Sweden | | Poland, Austria |
| Standard errors in paranthesis | | | | | | | | | | |
| Levels of statistical significance: * 0.1 ** 0.05 *** 0.01 **** 0.001 | | | | | | | | | | |
| (1)- (8) estimated using Stata xtpcse procedure, with two-way fixed effects and hetonly options | | | | | | | | | | |
| (9)-(10) estimated using stata xtbond2 procedure with decade fixed effects and noleveleg, orthogonal, robust options | | | | | | | | | | |

| TABLE 11: Dynamic Regressions with Lagged Dependent Variable and 2 and 3-way Interactions | | | | | | | | | | |
|---|------------------------|-----------------------|------------------------|-------------------------------|-------------------------|--|---|---|-------------------------|---------------------------------|
| DETERMINANTS OF PER CAPITA TAX REVENUE (IN DAYS OF WAGES) | | | | | | | | | | |
| OLS with PCSE | | | | | | | | | | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | Arellano Bond Dynamic Estimator |
| L.Taxrev | 0.548**** (0.07) | 0.501**** (0.08) | 0.556**** (0.07) | 0.517**** (0.08) | 0.525**** (0.08) | 0.474**** (0.09) | 0.528**** (0.08) | 0.528**** (0.08) | 0.483**** (0.09) | 0.432**** (0.14) |
| Warp | 18.007*** (5.76) | 15.450**** (5.73) | 19.234*** (6.24) | 17.130*** (6.40) | 20.411*** (6.52) | 19.434*** (6.97) | 21.982*** (6.68) | 20.665*** (6.75) | 19.110**** (3.58) | 16.389**** (3.59) |
| Urb | 31.09 (20.29) | 6.23 (14.44) | 35.060* (20.39) | 7.91 (14.91) | 6.56 (28.26) | 17.60 (18.92) | 22.62 (21.37) | 28.75 (22.63) | 36.383* (19.42) | 10.00 (9.63) |
| Repr | 1.23 (1.69) | 1.11 (2.10) | 1.44 (1.68) | 0.82 (2.12) | -0.54 (2.45) | 4.60 (3.24) | 0.89 (1.70) | 2.15 (2.40) | 1.38 (1.54) | 1.41 (1.23) |
| Warp*Repr | -17.168*** (6.01) | -18.047** (7.22) | -18.483*** (6.45) | -19.999** (7.80) | -19.979*** (6.78) | -27.026*** (8.53) | -21.081*** (7.03) | -22.512*** (7.93) | -17.894**** (4.40) | -19.385*** (6.11) |
| Warp*Urb | -222.912*** (69.07) | -152.887** (66.76) | -239.041*** (73.87) | -169.915** (72.80) | -243.044*** (77.35) | -205.861** (79.98) | -270.092**** (79.88) | -224.109*** (78.21) | -233.535**** (58.18) | -159.710*** (53.16) |
| Repr*Urb | -17.80 (18.49) | 4.64 (15.64) | -22.85 (18.62) | 2.60 (16.05) | 10.85 (27.87) | -11.70 (20.71) | -11.00 (19.18) | -18.44 (22.50) | -20.28 (18.29) | 2.71 (10.72) |
| Warp*Repr*Urb | 234.104**** (69.93) | 180.377** (70.10) | 249.382**** (74.52) | 197.144*** (75.85) | 257.060*** (78.17) | 257.170*** (83.30) | 279.761**** (80.85) | 245.808*** (79.87) | 243.059**** (62.48) | 189.315*** (62.80) |
| Observations | 169 | 140 | 164 | 135 | 133 | 108 | 135 | 125 | 157 | 130 |
| R2 | 0.95 | 0.95 | 0.95 | 0.95 | 0.95 | 0.94 | 0.95 | 0.95 | | |
| ar1 | | | | | | | | | -1.99 | -1.78 |
| ar1p | | | | | | | | | 0.05 | 0.08 |
| ar2 | | | | | | | | | 0.65 | 0.77 |
| ar2p | | | | | | | | | 0.52 | 0.44 |
| Regime proxy | Reprtax | Repract | Reprtax | Repract | Reprtax | Repract | Reprtax | Repract | Reprtax | Repract |
| Control Variables | | | L.Rwage, L.InPop | L.Rwage, L.InPop | L.Rwage, L.InPop | L.Rwage, L.InPop | L.Rwage, L.InPop | L.Rwage, L.InPop | L.Rwage, L.InPop | L.Rwage, L.InPop |
| Dropped from Sample | | Poland, Austria | Russia | Poland, Austria, Russia | Russia, 16th century | Poland, Austria, Russia, 16th century | Russia, Poland, Portugal, Sweden | Poland, Austria, Russia, Portugal, Sweden | | Poland, Austria |
| Standard errors in paranthesis | | | | | | | | | | |
| Levels of statistical significance: * 0.1 ** 0.05 *** 0.01 **** 0.001 | | | | | | | | | | |
| (1)-(8) estimated using Stata xtpcse procedure, with two-way fixed effects and hetonly options | | | | | | | | | | |
| (9)-(10) estimated using stata xtabond2 procedure with decade fixed effects and noleveleq, orthogonal, robust options | | | | | | | | | | |