# State Capacity and Long-Run Performance*

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**Abstract**

We examine the long-term links between state capacity and economic performance. Our database is novel and spans 11 countries and 4 centuries in Europe, the birthplace of modern economic growth. A dynamic simultaneous equation panel model indicates that the performance effects of states with modern extractive and productive capabilities are significant, large, and robust to a broad range of methods, specifications, controls, and sub-samples. We find that the establishment of an effective state increased per capita GDP by 10 percent over a half-century and by 14 percent over a full century.

*Keywords*: political regimes, state capacity, public services, economic performance, European history.
*JEL codes*: C33, H11, H41, N43, O23, P48.

PRELIMINARY

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# 1 Introduction

Modern economic theory typically assumes that states can tax at will and commmit public funds to a broad set of military and civilian goods. However, effective states are only a recent development in global history.[1] Furthermore, they represent just a fraction of modern nations. Like their historical predecessors, today's developing states often confront problems of low revenues and unproductive expenditures.[2] A clear understanding of state capacity is thus critical for explaining why some countries achieve long-run economic growth but others do not.

Indeed, state capacity is now a key concern of economists.[3] A major recent work by Besley and Persson (2011) defines state capacity in terms of two complementary capabilities that enable the state to act. The first concerns the state's extractive role as a tax collector and the second its productive role as a provider of public services (e.g., transportation networks, courts). Besley and Persson argue that state capacity – the combined extractive and productive capabilities of the state – forms a critical part of development clusters that vary closely with income levels.

However, the current economics literature is largely theoretical in nature. There are still few rigorous empirical works about the long-term links between state capacity and economic performance. To address this important gap, this paper examines the economic impacts of fundamental political transformations that resolved long-standing state capacity problems in Europe, the birthplace of modern economic growth.

We argue that sovereign governments in European history typically faced two key political problems: fiscal fragmentation and absolutism. Although rulers had weak authority over taxation, they had strong control over expenditures. Under this equilibrium, revenues were low and executives typically spent available funds on military adventures rather than on public services with broad economic benefits.

What is striking is just how squarely the political transformations that we study fit within Besley and Persson's (2011) conceptual framework. We argue that the im-

[1] See Mathias and O'Brien (1976), Levi (1988), Brewer (1989), Tilly (1990), Hoffman and Rosenthal (1997), Epstein (2000), O'Brien (2001, 2011a), Dincecco (2011), Karaman and Pamuk (2011), and Rosenthal and Wong (2011).

[2] For state capacity problems in sub-saharan Africa, see Migdal (1988), Herbst (2000), and Bates (2001). By contrast, states have played key roles in the successful development experiences of Asian Tiger nations. See Wade (1990) and Kang (2002).

[3] See Acemoglu et al. (2004), Acemoglu (2005), Acemoglu et al. (2011a), and Gennaioli and Voth (2011).

plementation of uniform tax systems at the national level – which we call "fiscal centralization" – enabled European states to effectively fulfill their extractive role. This transformation typically occurred swiftly and permanently from 1789 onward. Similarly, we argue that the establishment of parliaments that could monitor public expenditures at regular intervals – called "limited government" – enabled them to effectively fulfill their productive role. This transformation typically occurred decades after fiscal centralization over the nineteenth century. By the mid-1800s, most European states had achieved modern levels of extractive and productive capabilities, implying that they could gather large tax revenues and effectively channel funds toward non-military public services.

We argue that these critical improvements in state capacity had strongly positive performance effects. To rigorously develop our claim, we perform a dynamic simultaneous equation panel analysis on a novel database that spans eleven countries from the height of the Old Regime to the eve of World War I (1650-1913). The key strength of this modeling approach is the ability to explicitly test the set of linkages from political regime type to fiscal policies, and then from fiscal policies to economic outcomes. This approach also allows us to account for country- and time-specific unobservables and other historical features that affected performance, including external and internal conflicts, early technology, state antiquity, agricultural transitions, and human capital.

The results of our analysis indicate that the performance effects of states with modern extractive and productive capabilities are large, statistically significant, and robust to a broad range of methods, specifications, controls, and sub-samples. We find that the establishment of an effective state increased per capita GDP by 10 percent over a half-century and by 14 percent over a full century. For instance, if France had had a fiscally centralized and parliamentary limited regime from 1650 onward, then per capita GDP would have risen by 17 percent by 1913, making it one of the richest economies in Europe. Furthermore, for reasons that we discuss in Section 3, the magnitudes of these results should be interpreted as lower-bound estimates of the economic effects of political change.

The historical evidence that we describe in the next section suggests that it is plausible to treat political transformations as exogenous to fiscal and economic outcomes. Fiscal centralization and limited government were typically the result of radical exogenous shocks or the confluence of idiosyncratic geographical, political, and social

factors. The precise timing of institutional change, moreover, was typically unpredictable and subject to chance. Standard tests for exogeneity support this claim. Our modeling approach also accommodates feedback effects from economic and fiscal developments to subsequent political reforms. To check the exogeneity assumption even further, we perform two additional exercises: tests for reverse causation from Angrist and Pischke (2009) and sovereign deaths as a natural experiment. While neither exercise can totally rule out endogeneity concerns, the key results are robust. These exercises thus reinforce our argument that political changes had significant positive economic effects through improved public finances.

Our paper is related to the literature that examines the links between historical institutional factors and current economic performance, including Engerman and Sokoloff (1997), La Porta et al. (1998), Hall and Jones (1999), Acemoglu et al. (2001, 2002, 2005a), Banerjee and Iyer (2005), and Nunn (2008). None of these works, however, focus on state capacity.[4] Furthermore, this literature does not typically identify just how history matters (Nunn, 2009).[5] Our paper tests a specific mechanism – namely, the impacts of effective revenue extraction and spending – through which state capacity improvements have long-term economic effects. Overall, our macro-oriented approach complements microeconomic research that uses randomized controlled experiments to test whether specific policy interventions are effective (Duflo et al., 2007).

Our paper is also related to the literature that argues that the state was an active participant in the development of modern capitalist systems, including Gerschenkron (1966), Magnusson (2009), and O'Brien (2011a). We provide a data-intensive, rigorous counterpart to these works. Our paper thus contributes to the debate regarding the institutional origins of the Industrial Revolution (Acemoglu et al., 2005b, Mokyr, 2008).

The rest of the paper proceeds as follows. Section 2 describes the historical background and Section 3 develops our theoretical implications. Section 4 presents the data, Section 5 the descriptive statistics, and Section 6 a case study of France. Section 7 describes the econometric methodology and Section 8 the results. Section 9 performs robustness checks. Section 10 concludes.

[4]One important exception is Bockstette et al. (2002), which investigates the economic impacts of early statehood. They find a strong positive link between state antiquity and current development. Similarly, Dincecco and Prado (2011) find a strong positive relationship between current fiscal capacity and performance. They use historical war casualties to instrument for current fiscal institutions.

[5]A recent exception is Dell (2010), which argues that public goods provision by large landowners in Peruvian history has persistent development effects.

# 2 Historical Background

This section characterizes the two fundamental political transformations that resolved key state capacity problems in European history. We argue that fiscal centralization allowed states to effectively fulfill their extractive role and limited government their productive role.[6]

## Fiscal Centralization

Most polities in Europe were fiscally fragmented before the nineteenth century. Contrary to the conventional wisdom, monarchs confronted a host of incumbent local institutions that reduced their fiscal powers. Epstein (2000, pp. 13-14) writes that

> decades of research on pre-modern political practices...has shown how "absolutism" was a largely propagandistic device devoid of much practical substance...The strength of a monarch's theoretical claims to absolute rule was frequently inversely proportional to his de facto powers.

One general feature of fragmented states was the close relationship between local tax control and political autonomy. Provincial elites had strong incentives to oppose fiscal reforms that threatened traditional tax rights. The result was a classic public goods problem. Since each local authority attempted to free-ride on the tax contributions of others, the revenues that national governments could extract per capita were low.

To resolve the problem of local tax free-riding, executives had to gain the fiscal authority to impose standard tax menus rather than bargain place by place over individual rates. So long as states equalized rates across provinces at relatively high levels, government revenues per head rose.

A clear and simple definition of fiscal centralization facilitates comparison across states. We define that the centralization process was completed the year that the national government first secured its revenues through a standard tax system with uniform rates throughout the country.[7]

[6] Our account follows Dincecco (2011, chs. 2-3), who also provides sources.

[7] This definition does not imply that central governments became tax monopolists. The history of the United States just after the Revolution of 1776 illustrates this point. Under the Articles of Confederation, the first U.S. constitution, Congress could only request tax funds from states. Fiscal centralization took place in 1788, when the new constitution granted Congress the legal power to ensure that states complied with national tax standards. However, U.S. states could still levy local taxes.

All pre-centralized regimes were classified as entirely fragmented, even for states where fiscal divisions were relatively small. This choice implies that some regimes counted as fully fragmented will encompass data associated with higher per capita revenues. Average improvements after fiscal centralization will therefore be smaller than otherwise. Systematic underestimation of the fiscal effects of centralization biases the data against the hypothesis that fiscal centralization increased revenues. The results of the econometric analysis will thus be stronger than otherwise if it still indicates that fiscally centralized regimes had significant positive fiscal effects.

Table 1 displays the dates of fiscal centralization across sample countries. The Norman Conquest of 1066 undercut provincial authority in England and established a uniformity of laws and customs that other states did not achieve until much later.[8] Structural changes took place swiftly and permanently in many parts of continental Europe after the tumultuous fall of the Old Regime. The National Assembly transformed the tax system in France by eliminating traditional privileges at the start of the Revolution (1789-99). Napoleon completed this process after taking power in 1799. The First French Republic conquered the Low Countries in 1795, and the Southern Netherlands including Belgium became French departments. The Batavian Republic, the successor to the Dutch Republic, established a national system of taxation under French rule in 1806. Although French conquest at the start of the 1800s was also the major catalyst for fiscal change on the Italian peninsula, the unification of tax systems among pre-unitary Italian states did not occur until after the defeat of Austria and the founding of the Kingdom of Italy in 1861. Finally, Prussia undertook major administrative reforms including fiscal centralization after its loss to France in the Battle of Jena-Auerstedt in 1806.

Although Napoleon defeated Austria in 1805 and invaded Portugal in 1807 and Spain in 1808, he failed to implement lasting administrative changes in those territories. Fiscal centralization did not take place in the Austrian Empire until after the 1848 revolutions, which had important implications for bureaucratic structures. Most notably, the central government in Vienna began to implement an effective Cisleitha-

[8]England conjoined with Wales in 1536. The Act of Union of 1707 conjoined Scotland. A similar Act conjoined Ireland in 1800 (the Irish Free State was established in 1922). For consistency, the term "England" rather than "Great Britain" or the "United Kingdom" is used throughout the text. However, we must distinguish between medieval English fiscal and political institutions and British ones. Brewer (1989, pp. 5-6) writes: "There was certainly an English medieval state, made from a Norman template, but not a British one…Nevertheless the English core of what was eventually to become the British state was both geographically larger and better administrated than its French equivalent."

nian tax system in Hungary.[9] Fiscal centralization also occurred in the 1840s in Spain during a period of major reforms. Significant changes in public finances in Portugal took place in the 1850s after the end of a long revolutionary era. The 1859 reform led to the centralization and regulation of government accounts.

Pre-modern fiscal structures remained in Scandinavia through much of the 1800s. Major changes did not occur until the second half of the nineteenth century or later. The 1861 reform in Sweden abolished the traditional system of dividing tax subjects into different classes with many sub-groups and rules for fixed contributions. Similarly, the 1903 reform in Denmark eliminated traditional tax structures and introduced a modern income tax with standard, country-wide rates.

## Limited Government

By eliminating local tax free-riding, fiscal centralization increased the ability of national governments to extract greater revenues. Since rulers retained control over expenditures, however, the consolidation of fiscal powers may have exacerbated problems of executive discretion.

Although rulers spent government funds as they pleased, elites in parliament exercised tax authority. Hoffman and Rosenthal (1997) argue that the one true goal of absolutist monarchs was to wage war for personal glory and for homeland defense. One key reason was the problem of royal moral hazard in warfare (Cox, 2011). In Hoffman's (2009, p. 24) words, monarchs

> overspent on the military and provided more defense than their citizens likely desired. But they had little reason not to. Victory... won them glory, enhanced reputations, and resources... Losses never cost them their throne.

Since elites feared that executives would spend additional revenues in wasteful ways like foreign military adventures, they demanded the power of budgetary oversight before raising new taxes. To evade parliament, rulers resorted to fiscal predation, which reinforced the fear that they could not be trusted. Elites in parliament thus resisted tax requests and revenues were low.

Regular control over state budgets firmly established the fiscal supremacy of parliament. In turn, the likelihood of poor spending choices by executives fell. Although

[9] Austria and Hungary were the largest territories of the Austrian Empire (1804-67). The Compromise of 1867 led to the establishment of the Austro-Hungarian Empire (1867-1918). For consistency, the term "Austria" is used throughout the text.

structural reforms implied that rulers would receive greater revenues, the surrender of budgetary control was the only credible way for executives to guarantee that a portion of the new funds would be used on non-military public services that elites in parliament desired. Here it is important to note that, even if rulers and elites each had incentives to set new rules over government expenditures, the historical record indicates that limited government was typically established at critical junctures that came at the confluence of idiosyncratic geographical, political, and social shocks. The next subsection examines this point at length.

A valid depiction of parliamentary authority must capture its real power to act on the budget. It must also be clear and simple enough to apply across states. The substance of our definition derives from the spirit of constitutional reform from the classic work of North and Weingast (1989). We define that limited government was established the year that parliament gained the stable constitutional right to control the national budget on an annual basis. The requirement that parliament's power of the purse held for at least two consecutive decades ensures stability. To make the coding as objective as possible, years and regimes for which there are widespread academic consensus were chosen.

Selecting early dates to define political regimes as limited implies that average outcomes under parliamentary regimes will be worse than otherwise. For instance, one can argue that a stable form of limited government did not truly emerge in Germany until after World War II (the Weimar Republic endured for only 14 years, from 1918 to 1933) or in Spain until after the death of Franco in 1975. If that were the case, then the correct coding would be to categorize pre-twentieth-century Prussian and Spanish regimes as absolutist. Similar arguments can be made for Italy and Portugal. Since public finances in Europe typically improved over time, the choice of early dates means that some regimes classified as limited will encompass data associated with lower non-military spending per capita. Average improvements after parliamentary reforms will therefore be smaller than otherwise. Systematic underestimation of the fiscal impacts of limited government biases the data against the hypothesis that parliamentary reforms increased non-military spending. Any results of the econometric analysis that still indicate that limited government had significant positive effects on spending habits will thus be stronger than otherwise.

Furthermore, the establishment of limited government was not necessarily irreversible. There were some instances of switching back and forth with absolutism

over the 1800s. As described, our definition sets a stability threshold by requiring that parliamentary budgetary authority held for at least two straight decades.

Nineteenth-century France illustrates the coding methodology. The Bourbon monarchy was restored after the final defeat of Napoleon in 1815. This regime was constitutional in name alone. In 1830, King Charles X dissolved parliament, manipulated the electorate in favor of his supporters, placed the press under government control, and called for new elections. These measures incited the July Revolution the next day. King Louis Philip, the replacement for the deposed monarch, agreed to follow constitutional principles, but his tenure was beset by the economic crisis of the mid-1840s and ended with the Revolution of 1848. Since the reign of Louis Philip endured for less than two decades, our benchmark scheme does not code the July regime as limited. However, the case study in Section 6 explicitly accounts for its fiscal effects. Napoleon III, who was elected president of the Second Republic in 1848, staged a successful coup in 1851 and established an authoritarian regime (called the Second Empire) that lasted nearly 20 years. The emperor was captured during the Franco-Prussian War (1870-1), and the provisional government of the Third Republic was quickly formed. This regime was consolidated in the aftermath of the conflict, which France lost, and endured for 70 years until the German invasion of 1940. Since the Third Republic best satisfies the triple criteria of parliamentary regularity, stability, and scholarly consensus, our coding methodology dated the emergence of limited government in France to 1870.

Table 2 displays the dates of limited government across sample countries. Parliamentary reforms typically occurred decades after fiscal centralization. Belgium was established as a constitutional monarchy after revolting and declaring independence from the Netherlands in 1830. In the Netherlands itself, a new constitution that required the executive to submit annual budgets for parliamentary approval was promulgated during the Year of Revolutions in 1848. King Frederick William IV of Prussia also granted a liberal constitution that year.[10] In Italy, the constitution first endorsed by King Charles Albert of Piedmont in 1848 was extended to the entire kingdom after the defeat of Austria in 1861. The Compromise of 1867, which established Austria and Hungary as distinct political entities, marked the start of the constitutional era in Austria following the Austro-Prussian War (1866). Spain fought several

[10]Tilly (1966) argues that there were binding fiscal constraints from 1848 onward, although the government operated without legislative approval of its military budgets during the 1860s. Also see Ziblatt (2006, pp. 113-16).

civil wars over the 1800s. After decades of failed attempts, a stable parliamentary regime was established in 1876 following the Third Carlist War (1872-6).

By contrast, limited government and fiscal centralization took place within a decade of each other in Sweden and Portugal. Although Sweden enacted a constitution in 1809, the executive retained absolute veto authority, and parliament met only once every five years. The parliamentary reform of 1866, which replaced the traditional Diet of Estates with a modern bicameral legislature, established limited government. This institutional change occurred five years after fiscal centralization in 1861. Like Spain, Portugal fought a series of civil wars over the nineteenth century. A stable constitutional regime was established at the end of the revolutionary era in 1851, eight years before fiscal centralization in 1859.

There are two cases in which limited government was implemented well in advance of fiscal centralization. In Denmark, King Frederick VII renounced his absolutist powers and established a two-chamber parliament after the political revolutions of 1848.[11] Fiscal centralization did not take place in Denmark until 1903. The Dutch Republic (1572-1795) is typically classified as constitutional.[12] Recall, however, that the Republic was fiscally fragmented at the national level.

## Reverse Causation?

The historical evidence suggests that we can plausibly treat political transformations as exogenous to fiscal and economic outcomes. As described, the establishment of uniform tax systems was often the result of radical, externally imposed reform. In the German territories, in the Low Countries, and on the Italian (and to a lesser extent, the Iberian) peninsula, fiscal centralization was the result of French conquest from 1792 onward.[13] As O'Brien (2011a, p. 436) writes:

> It seems that only the exogenous shocks of the kind delivered by... the outcomes that flowed from the French Revolution (1789-1815) led to serious reforms to the fiscal constitutions of other ancien régimes on the mainland.

[11] However, the constitutional revision of 1866 restricted the suffrage in ways that favored the conservative and the wealthy.

[12] See De Long and Shleifer (1993), Acemoglu et al. (2005a), and Stasavage (2005).

[13] A similar logic holds for the establishment of centralized institutions in England following the Norman Conquest in the eleventh century.

Indeed, Acemoglu et al. (2011b) study this case as a quasi-natural experiment to test for the long-term economic effects of the French Revolution.

Elsewhere, fiscal centralization often took place in the midst of large-scale administrative reforms that established new state bureaucracies. Major institutional changes typically occurred during times of economic, political, and social upheaval. The establishment of a uniform tax system in France itself during the Revolution (1789-99) illustrates the conflux of these factors, as does the case of Prussia during the Napoleonic Wars (1803-15), Austria during the Year of Revolutions (1848), and Portugal and Spain near times of civil wars.

A similar claim can be made for the establishment of limited government. Acemoglu et al. (2008, 2009) find that economic development does not cause transitions to democracy. Rather, important historical junctures, such as the French Revolution or the Revolutions of 1848, set countries on divergent politico-economic paths.[14]

This point relates to the exact timing of institutional change. There is little evidence that supports the claim that European states undertook political transformations in direct response to economic or fiscal conditions. Rather, these reforms were typically the result of major shocks or the one-off convergence of a host of idiosyncratic factors.

Even if political transformations did occur due to public finances or the economy, however, the exact timing of institutional change was typically unpredictable. The Glorious Revolution of 1688 in England illustrates this argument. Upon the death of Charles II in 1685, James II became king. Protestant elites were troubled by the fact that James II was a devout Catholic with strong ties to France. The year 1688 was also the start of the War of the Grand Alliance, fought between France and a European-wide coalition including William III of Orange, who was crowned King of England alongside Queen Mary in 1689 after James II was deposed.

One can argue that the coming together of particular events at a certain point in time – or, in a nutshell, chance – brought about limited government in England in 1688 but not before. Several previous attempts failed, including the 1685 rebellion led by the Duke of Monmouth. By this logic, one can also make the case that constitutional reform in England could have occurred on any number of occasions from 1640 to 1700, or not at all. In the words of Pincus (2009, pp. 480-1):

> At various points in the later seventeenth century both later Stuart kings

[14]Glaeser et al. (2004) argue that economic growth and human capital accumulation lead to subsequent improvements in political institutions. Our econometric analysis controls for these factors.

> even enjoyed widespread popular support…They were not pursuing ill-advised strategies whose failure was preordained…It was not inevitable that James's Catholic modernization strategy would fail.

Similar arguments for historical contingency can be made for France in 1789, the Year of Revolutions in 1848, and other critical junctures.

The historical record supports our claim that it is plausible to treat political transformations as exogenous to fiscal and economic outcomes. Furthermore, difference-in-Sargan, Durbin-Wu-Hausman, and Spencer-Berk statistical tests indicate that we can treat political regimes as exogenous at standard confidence levels. To address any remaining concerns about reverse causation, we employ a wide range of econometric strategies in Sections 8 and 9. Beyond controls for country- and time-specific unobservables and other historical features that affected performance, our benchmark model accounts for feedback effects from economic and fiscal developments to subsequent political reforms. We also perform two additional exercises for robustness: tests for reverse causation from Angrist and Pischke (2009) and sovereign deaths as a natural experiment.

# 3 Theoretical Implications

We argue that fiscally centralized and politically limited regimes enabled states to extract large tax funds and then productively use them. Fiscal centralization increased the amount of revenues that governments extracted per head by eliminating local tax free-riding. Since executives could make credible commitments to spend new funds on non-military public services rather than on ill-advised wars, limited government made parliaments more willing to submit to greater tax burdens. Hence, it also increased revenues per capita.

Although higher tax revenues per head made it easier for executives to provide public services under fiscally centralized regimes, the consolidation of fiscal powers may have had an adverse impact on public finances through greater wasted spending. It is thus unclear whether expenditures on non-military public services actually rose under centralized versus fragmented regimes. However, by regularly monitoring the government's budget, and thereby reducing the likelihood of bad spending choices by executives, parliamentary power of the purse should have increased non-military expenditures.

Schultz and Weingast (1998) claim that, in the context of the long-term international rivalries that characterized pre-modern Europe, the ability of limited regimes to make credible spending commitments was a critical military advantage over absolutist ones. For instance, average total expenditures for parliamentary England in the war-intensive century following the Glorious Revolution of 1688 were 9.23 gold grams per capita, more than double the average for absolutist France (Dincecco, 2011). We may think that parliamentary power of the purse gave citizens confidence that military decisions and investments were relatively sound.[15]

Once military supremacy – and thus international peace – was truly established (i.e., the Pax Brittanica after 1815), then our theoretical implication should hold firm, as parliamentary fiscal power facilitated the switch toward the provision of non-military public services.[16] Indeed, average per capita non-military spending for England during the Pax Brittanica was 12.29 gold grams per head, roughly three times its average for the war-intensive eighteenth century (Mitchell, 1988).[17] We examine these points further in the case study of France in Section 6.

Figure 1 summarizes the basic linkages between political regimes, fiscal characteristics, and performance outcomes.[18] Regime type affected both the state's ability to extract tax revenues per capita and the way that it then spent them, which in turn influenced performance.

Likewise, Table 3 describes the fiscal and economic features of different political regimes. Under fragmented and absolutist regimes, tax revenues per head should have been low and any available funds should have been unproductively used on foreign military adventures. Economic performance should have been poor as a result. Under centralized and limited regimes, by contrast, per capita revenues should have been high and funds should have been productively used on non-military public services. By solving both political problems, performance should have been relatively

[15]Given England's unique status as the first industrialized nation, state economic intervention may have been of less overall importance (Gerschenkron, 1966). However, Magnusson (2009, ch. 4) and O'Brien (2011a,b) argue that the British government played a notable industrial role.

[16]Although O'Brien (2011a,b) makes a similar point, he argues for stronger linkages between the state's military and fiscal might and its ability to play a productive economic role. The first part of our econometric analysis, which focuses exclusively on the state's capacity to extract tax revenues, accounts for this possibility.

[17]Similarly, the average share of non-military spending in total expenditures was nearly 40 percent higher.

[18]This figure broadly resembles the flow chart in Figure 1.7 of Besley and Persson (2011), which depicts the political determinants of state-building and thus income levels.

better. Under fragmented and limited regimes, any available funds should have been productively used, but revenues per head should have been low. Hence, performance should have been higher than under fragmented and absolutist regimes but lower than under centralized and limited ones. Finally, under centralized and absolutist regimes, per capita revenues should have been high, but funds would not necessarily have been productively used. Although it is thus likely that performance under centralized and absolutist regimes would have been lower than under centralized and limited ones, depending on ruler spending decisions it could have been higher than under other regime types.

Limited governments with broad voting franchises may pursue redistributive welfare programs that favor short-run consumption over public investments that are conducive to long-run growth (Przeworski et al., 2000, ch. 3). Democracy may thus discourage development. However, this concern is less worrisome for the nascent, elite parliamentary regimes of nineteenth-century Europe. The franchise was typically highly restricted, reducing the potential for large-scale redistributive schemes (Carstairs, 1980, Flora, 1983). Furthermore, welfare spending by central governments was generally low (Lindert, 2004, ch. 2). A key non-military expense for many nineteenth-century states was transportation infrastructure, and in particular railway networks (O'Brien, 1983, Cardoso and Lains, 2010a). This type of public investment should have reduced trade costs and increased income (e.g., Eaton and Kortum, 2002, Bogart, 2008).

Overall, our theoretical implications indicate that we should observe significant impacts of political transformations on fiscal and economic outcomes. However, there are at least three reasons why our econometric results should be interpreted as lower-bound estimates. First, Section 2 shows how the ways in which we define fiscal centralization and limited government generate a downward bias to the magnitudes of our fiscal estimates.

Furthermore, our focus on a single, specific channel through which political and fiscal improvements affected economic performance – namely, the impacts of greater revenues and non-military expenditures – creates a downward bias to our economic estimates. Although overall taxes were typically low in fragmented and absolutist regimes, the problem of divided fiscal authority implied that they could still be very harmful at the margin (Hoffman and Rosenthal, 1997). Since rates were high in sectors over which rulers traditionally presided and taxes were politically feasible, resources

would be diverted into those sectors that were tax exempt. To prevent the erosion of the tax base, rulers had to enact rigid, inefficient laws that decreased investment mobility. Similarly, divided political authority over rights of eminent domain held up infrastructure projects that were otherwise profitable (e.g., Rosenthal, 1992). Since many groups could block new projects, transaction costs were high, reducing the number and scope of growth-enhancing investments. The virtue of our approach, which funnels the performance effects of political regimes through a specific fiscal mechanism, is the ability to pinpoint a precise set of causal linkages. However, the cost is that we must overlook other key ways in which fragmented and absolutist regimes distorted incentives and impeded performance, strongly reducing the magnitudes of our economic estimates.

Interstate political competition generates even further downward bias. Over the nineteenth century, states came to view industrial strength as an important basis for military prowess (Magnusson, 2009, Rosenthal and Wong, 2011). Major public investments like railway infrastructure served military as well as economic purposes, and were thought to have important consequences for the European balance of power. Even authoritarian rulers had strong incentives to invest in non-military public services (Rosenthal and Wong, 2011, ch. 6). Furthermore, the onset of industrialization gave rise to new sources of social unrest, increasing the minimum amount of non-military public services that authoritarians (e.g., Napoleon III) had to provide to sustain power (Acemoglu and Robinson, 2000). These two factors imply that actual differences in non-military spending between absolutist and limited regimes were smaller than our theoretical prediction would indicate, reducing our economic estimates to an even greater extent. Combined, these three reasons – the strictness of the regime definitions, the focus on a single causal channel, and the role of political competition – imply that the magnitudes of our econometric results should be considered lower-bound estimates of the fiscal and economic effects of political transformations. In practice, it is likely that these magnitudes were greater.[19]

[19] Although a recent literature argues that higher taxation in today's Europe helps account for the shortfall in worker productivity relative to the U.S. (e.g., Prescott, 2004), our econometric analysis does not reveal any negative performance effects of greater state capacity. When state capacity is already high (e.g., at today's OECD-country levels), however, there is reason to think that different tax compositions may influence performance. For instance, Hines and Summers (2009) argue that globalization is likely to lead to an increase in consumption-style taxes in rich countries like the United States.

# 4 Data

The data on government revenues from 1650 to 1913 are from Dincecco (2011). Systematic data for non-military expenditures are not available before the nineteenth century. These data, which we take from a variety of secondary sources, run from 1816 to 1913. The Appendix describes the sources and construction methods for the spending data.

For reasons of data availability, comparability, and reliability, we focus on taxing and spending by national governments, rather than general taxing and spending that included local and regional governments.[20] All of our sample countries (save Prussia after the establishment of the federal German Empire in 1871) had centralized political structures during the sample period. Furthermore, national governments were typically better able than local or regional ones to provide the types of non-military public services (e.g., major infrastructure projects like railways) that interest us. Thus, the use of national government data should not bias our analysis.

Bonney (1995, pp. 423-506) and O'Brien (2011a, pp. 408-20) discuss the limitations of historical budgetary data. European states did not maintain detailed fiscal records during the seventeenth and eighteenth centuries. National governments may have calculated yearly budgets in a variety of ways. Some states computed budgets with revenues that they intended to extract, even if funds did not enter government coffers until years later. Insofar as possible, the revenues used here were tax receipts for national governments in a given year. Ordinary and extraordinary figures were summed, and loan incomes were subtracted.[21] Since the different ways in which Old Regime governments tabulated yearly revenues suggest that they typically overestimated the amounts of resources available to them, average revenues under fragmented and absolutist regimes should have appeared larger than otherwise. Furthermore, government accounting practices typically improved over time, reducing the number and magnitude of misestimates. These features bias the data against the hypothesis that political transformations led to greater tax incomes.[22] By the nine-

[20] There was also the possibility of the private provision of "public" services like infrastructure (e.g., Bogart, 2005, 2008).

[21] According to O'Brien (2011a, pp. 415-16), the role of colonial revenues in the development of modern fiscal systems was negligible. He argues that, after accounting for conquest costs and annual outlays for defense and governance, net flows of colonial tributes into state coffers were typically small or negative. However, colonial goods generally faced customs taxes at home ports. Our data include tax amounts from these sources.

[22] If Old Regime governments made payments in kind to fund public services like infrastructure, then it is

teenth century, national governments had typically developed modern fiscal administrations, or were in the process of doing so. The 1816-1913 data on non-military expenditures should thus be reliable overall.

To make revenue and expenditure calculations comparable across countries, all currency units were transformed into gold grams. This conversion greatly reduces potential inflationary effects. The years between missing revenue observations were linearly interpolated. Population figures were also linearly interpolated between census years. Since there were few major one-off fiscal changes or population shocks (e.g., plague) from 1650 to 1913 besides the two political transformations that we focus on, the interpolated data should provide reasonable estimates. However, as the linkages between tax bases and government spending were weaker than those for revenues, particularly during wars, we did not interpolate the years between missing expenditure observations. Finally, the data were averaged over five-year periods to smooth short-run fluctuations and mitigate measurement errors (e.g., Beck and Levine, 2004).[23]

# 5 Descriptive Statistics

Table 4 summarizes the relationships between political regimes and economic performance from 1650 to 1913. Our main performance measure is GDP per capita in 1990 international Geary-Khamis dollars from Maddison (2010). Average per capita GDP levels for centralized and absolutist (1,446), fragmented and limited (2,043), and centralized and limited (2,176) regimes were high relative to those for fragmented and absolutist ones (1,016). The GDP estimates are relatively reliable for the 1816-1913 period, but less so before the nineteenth century. As an alternative performance measure, we calculate urbanization rates as urban populations as fractions of total populations from De Vries (1984). Urbanization trends across political regimes resemble those for per capita GDP.[24]

We argue that a key reason why economic outcomes were better under centralized

possible that no official record of these expenditures would exist. However, the economic impacts of these public services would still be had. This feature would thus reduce the difference in average performance between fragmented and absolutist regimes and other regime types.

[23] Our results remain similar if 10- or 25-year averages are used.

[24] As a second alternative, we used the Barro-Ursúa GDP data for the 1816-1913 period. The drawback to these data is that they are less widely available for the first half of the nineteenth century. However, the results were similar.

and limited regimes was because those states were able to both extract large tax funds and productively use them. Table 5 summarizes the relationships between political regimes and revenues from 1650 to 1913. Average per capita revenues in gold grams for centralized and absolutist (7.05), fragmented and limited (10.49), and centralized and limited (12.97) regimes were high relative to those for fragmented and absolutist ones (2.42).

Similarly, Table 6 summarizes the relationships between political regimes and non-military spending from 1816 to 1913. Average per capita non-military expenditures in gold grams for centralized and absolutist (6.95), fragmented and limited (7.27), and centralized and limited (11.57) regimes were high relative to those for fragmented and absolutist ones (1.81). As described in Section 3, railway networks were a major non-military expense for many nineteenth-century governments. Transport systems are also a key example of productive state investments in physical services in Besley and Persson (2011, ch. 1). As an alternative to non-military spending, we calculate the amount of railway kilometers built by the state per square kilometer of domestic territory from Bogart (2008).[25] Railway construction trends across political regimes are similar to those for per capita non-military spending (not shown).

Finally, Table 7 summarizes the 1816-1913 spending data that is disaggregated beyond non-military expenditures. These data are only available for six sample countries. As described in Section 3, state spending on poor relief, unemployment compensation, health, and housing was typically low. We focus on expenditures on two non-military public services that nineteenth-century governments typically provided: infrastructure and education. Average per capita infrastructure and education spending for centralized and limited regimes (1.12 and 0.78 gold grams, respectively) was high relative to other regime types.[26]

# 6 Case Study of France

The overall trends in Tables 3 to 6 are consistent with our argument that political transformations improved economic performance through better public finances. These

[25]Besides the state, private companies also constructed railway networks. Even if the state did not have a direct role in financing, building, or operating railways, however, its indirect role as facilitator was typically important.

[26]There were no data observations for the infrastructure or education variables for fragmented and absolutist regimes. This lack of data is perhaps not surprising given the typical backwardness of this regime type.

patterns also hold for individual countries. To further illustrate the linkages between political regimes and fiscal and economic outcomes, we now examine France, a core sample country for which long data series over several regime types are available.[27]

Figure 2 plots French national government revenues from 1650 to 1913. Revenues were low, averaging slightly more than 3 gold grams per capita, under the fragmented and absolutist regime that lasted through 1789. There was a sharp increase in French revenues, which roughly doubled to 10 gold grams per head, in the two decades after fiscal centralization in 1790. French revenues leveled out, but never fell back to pre-1789 levels, in the decades just after the Napoleonic era. In the 1840s, they began to increase once more, reaching more than 16 gold grams per capita by the end of the 1860s. The establishment of a stable centralized and limited regime took place in the aftermath of the Franco-Prussian War (1870-1). This set of events was associated with a sharp jump in revenues, which more than doubled to nearly 40 gold grams per head by 1913.

How about expenditures? Figure 3 plots spending on infrastructure and education by the French national government from 1816 to 1913.[28] Infrastructure and education expenditures under the centralized and absolutist regime were low, averaging less than 0.50 gold grams per capita, through the late 1820s. However, this spending nearly doubled to 0.81 gold grams per head under the short-lived centralized and limited July regime (1830-48).[29] Napoleon III established authoritarian rule in 1851. During his reign, he fought five wars.[30] Although there was a small uptick in infrastructure and education spending at the start of the 1860s, it was relatively flat, averaging just 1 gold gram per capita. With the establishment of a stable centralized and limited regime in 1870-1, there was a rapid jump in infrastructure and education expenditures, which doubled to more than 2 gold grams per head by the start of the 1880s. Infrastructure and education spending continued to increase through 1913, reaching 3.54 gold grams per capita.

To complete this picture, Figure 4 plots the share of infrastructure and education expenditures in total expenditures for the French national government over the same

[27] Our account follows Dincecco (2011, chs. 3-5 and 8), who also provides sources.

[28] Due to data limitations, we focus this part of our case analysis on the nineteenth century.

[29] The vertical lines demarcating this regime in Figures 3 and 4 are dashed to indicate that it was not counted as limited under our benchmark coding scheme. Also see Section 2.

[30] These were the Crimean War (1853-6), Franco-Austrian War (1859), Second Italian War of Independence (1859-61), Battle of Mentana (1867), and Franco-Prussian War (1870-1).

period. This share doubled from 5 to 10 percent under the centralized and limited July regime from 1830 to 1848. Under the authoritarianism of Napoleon III, however, it fell, first to 6 percent during the late 1850s, and next to 4 percent with the Franco-Prussian War. Under the centralized and limited regime established in 1870-1, the share of infrastructure and education spending reversed course, reaching 9 percent by the start of the 1880s. This share continued to rise, although at a slower rate, through 1913.

Like the descriptive statistics, the case-study evidence for France supports our argument regarding the fiscal and economic benefits of political transformations. Both tax centralization and limited government were associated with greater revenues, and limited government with greater non-military expenditures. French average per capita GDP rose from 959 international dollars under the fragmented and absolutist regime to 1,357 under the centralized and absolutist one, and to 2,428 under the centralized and limited one (Table 4). However, neither the descriptive or case-study evidence fully characterizes the linkages between political regimes and fiscal and economic outcomes. The fiscal and economic variables may be endogenous or autocorrelated across time. Furthermore, country-specific unobservables may simultaneously influence political, fiscal, and economic outcomes. Historical features beyond political regimes (e.g., conflict) also affected performance. To provide a rigorous test of our argument, we now perform an econometric analysis that explicitly accounts for these factors.

# 7 Econometric Methodology

The structural form of our econometric model is based on the theoretical framework as summarized in Figure 1, where (1) political regime type affects the state's ability to extract tax revenues and (2) the way that it then spends them, which (3) in turn influences performance. We thus write the benchmark structural model as the following system of three equations:

$$\begin{aligned}\log Rev_{i,t} = \alpha_0 + \alpha_1 \log Rev_{i,t-1} + \alpha_2 CA_{i,t} + \alpha_3 FL_{i,t} + \alpha_4 CL_{i,t} + \alpha_5 \log GDP_{i,t} + \\ \alpha_6 \mathbf{X}_{i,t} + \alpha_7 \mathbf{\Lambda}_t + \mu_{i,1} + \epsilon_{i,t,1},\end{aligned} \tag{1}$$

$$\log Exp_{i,t} = \beta_0 + \beta_1 \log Exp_{i,t-1} + \beta_2 \log Rev_{i,t} + \beta_3 CA_{i,t} + \beta_4 FL_{i,t} + \beta_5 CL_{i,t} + \beta_6 \mathbf{X}_{i,t} + \beta_7 \mathbf{\Lambda}_t + \mu_{i,2} + \epsilon_{i,t,2}, \quad (2)$$

$$\log GDP_{i,t} = \gamma_0 + \gamma_1 \log GDP_{i,t-1} + \gamma_2 \log Exp_{i,t} + \gamma_3 \mathbf{X}_{i,t} + \gamma_4 \mathbf{\Lambda}_t + \mu_{i,3} + \epsilon_{i,t,3}, \quad (3)$$

where $i = 1, \ldots, N$ denotes countries, $t = 1, \ldots, T_i$ denotes time, $Rev_{i,t}$ denotes per capita revenues, $Exp_{i,t}$ denotes per capita non-military expenditures, $GDP_{i,t}$ denotes per capita GDP, $CA_{i,t}$, $FL_{i,t}$ and $CL_{i,t}$ are political regime indicators, $\mathbf{X}_{i,t}$ and $\mathbf{\Lambda}_t$ are vectors of additional regressors, $\mu_{i,j}$, $j = 1, 2, 3$, are country-specific fixed effects that reflect the influence of time-invariant features (e.g., culture, geography, size), and $\epsilon_{i,t,j}$ capture transitory shocks and other omitted factors. The country effects are potentially correlated with all of the regressors in $j$, while the random disturbance terms are assumed to be uncorrelated over time and across countries.[31]

The dependent variables for per capita revenues in Equation 1, per capita non-military expenditures in Equation 2, and per capita GDP in Equation 3 are expressed in natural logarithms to attenuate the variability in per capita values. Logs also facilitate the interpretation of our results. The persistence of fiscal and economic processes is captured by the inclusion of lagged values of the dependent variables on the right-hand sides of the system's equations, with lag orders selected based on the Bayesian Information Criterion (BIC).[32]

The key independent variables are the political regime indicators $CA_{i,t}$, $FL_{i,t}$, and $CL_{i,t}$ in Equation 1, which take the value 1 for each sample year that a country had a centralized and absolutist (fragmented and limited, centralized and limited) regime and 0 otherwise, with the fragmented and absolutist regime $FA_{i,t}$ as the benchmark. These dummies represent a clear, concise, and intuitive way to measure the fiscal impacts of political arrangements.[33] Recall from Section 2 that our coding of political regimes biases the data against the hypothesis that political transformations improved public finances.

[31] Our dynamic simultaneous equation panel methodology, which emphasizes the possibility of feedback effects, broadly resembles the conception of economic development in Besley and Persson (2011, chs. 1, 8, figure 1.7). They draw upon the notion of development as a process of cumulative effects in Myrdal (1968).

[32] The consistency of the BIC model selection criterion holds irrespective of whether the underlying variables are stationary or integrated (Pesaran, 1997).

[33] As an alternative, we used an ordered specification from least to most advanced political regime type according to Table 3. The results remained similar.

The vector $\mathbf{X}_{i,t}$ comprises a set of controls. Military spending was by far the largest component of national budgets through the 1800s (Hoffman and Rosenthal, 1997). To account the effects of warfare, we include a dummy variable for each year of external conflicts in Western or Eastern Europe according to Dincecco (2011, table 7.1). We also control for the fiscal impact of internal conflicts, which disrupted tax and spending flows. To account for this factor, we include a dummy variable for each year of civil war, coup, or revolution, also from Dincecco (2011, table 7.2). Acemoglu et al. (2005a) argue that Atlantic trade was key to Europe's early economic success. They use time-invariant characteristics to measure trade potential, which our country fixed effects capture.[34] Comin et al. (2010) argue that early technology differences affect long-run development. To control for "old" technology, we include their measure of technology adoption in 1500.[35] Bockstette et al. (2002), Chanda and Putterman (2007), and Putterman and Weil (2010) argue that a long history of statehood and an early transition from foraging to agriculture influence long-term growth. To control for state antiquity, we include the measure of initial state capacity for 1651-1700 or 1801-50 from Putterman (2007). To control for agricultural transitions, we include the measure from Putterman (2006). Glaeser et al. (2004) argue that human capital accumulation rather than political regime type is the key source of long-run development. The measures for early technology, state antiquity, and agricultural transitions partly capture the impact of human capital. To further control for this factor, we include literacy rate data from van Zanden (2009, table 8) and Foreman-Peck (2011, table 5).[36] Alesina and Wacziarg (1998) argue that country size influences the scale and composition of government spending. To account for size, we include population data from Dincecco (2011, appendix 1). The vector $\mathbf{\Lambda}_t$ captures cross-sectional dependence, including a set of observable factors common to all sample countries (Pesaran et al., 1999).[37]

Equations 1 to 3 can only be estimated for the 1816-1913 period because, as described in Section 4, systematic data on non-military expenditures do not become available until the nineteenth century. We thus estimate a restricted system of two

[34]These are whether the country was an Atlantic trader (England, France, the Netherlands, Portugal, and Spain), or the Atlantic coastline-to-area ratio. They also control for the aggregate volume of Atlantic trade, which our time fixed effects capture.

[35]This variable (called *tr3mig*) takes an average of sectoral technology adoption indexes in 1500 and is adjusted for post-1500 migration.

[36]Since early literacy data are not available for Austria, Denmark, Portugal, we do not include this control in all specifications.

[37]Our results remain similar if country-specific time trends are used.

equations for the 1650-1913 panel:

$$\log Rev_{i,t} = \alpha_0 + \alpha_1 \log Rev_{i,t-1} + \alpha_2 CA_{i,t} + \alpha_3 FL_{i,t} + \alpha_4 CL_{i,t} + \alpha_5 \log GDP_{i,t} + \alpha_6 \mathbf{X}_{i,t} + \mu_{i,1} + \epsilon_{i,t,1}, \tag{4}$$

$$\log GDP_{i,t} = \gamma_0 + \gamma_1 \log GDP_{i,t-1} + \gamma_2 \log Rev_{i,t} + \gamma_3 \mathbf{X}_{i,t} + \mu_{i,3} + \epsilon_{i,t,3}. \tag{5}$$

Endogenous right-hand side regressors makes OLS estimates inconsistent (Baltagi, 2005). Lagged values of the dependent variables among the regressors introduces another source of endogeneity due to their correlation with the state-specific effects $u_{i,j}$. GMM methods enable us to handle both problems, by differencing the equations to eliminate unobserved individual effects and using lagged values as instruments for the endogenous right-hand side regressors (Blundell and Bond, 1998). Unlike fixed-effect estimators, GMM methods can also account for possible weak endogeneity among the right-hand side variables, because some regressors may be correlated with past errors and unobserved individual effects. This possibility is particularly important here, since economic and fiscal developments could affect subsequent political reforms.

Despite these advantages, GMM methods are not particularly well-suited for our investigation. Blundell and Bond (1998) show that GMM techniques are only appropriate for settings with small $T$ and large $N$. The efficiency gains from GMM methods become negligible as $T$ increases, while the "many instruments" problem and computational difficulties associated with larger instrument sets become more acute (Judson and Owen, 1996, MaCurdy, 2007). Furthermore, lagged differenced values are typically "weak" instruments for time-persistent series like ours (Blundell and Bond, 1998).

Akashi and Kunitomo (2010) have recently proposed a "doubly-filtered" GMM estimator that is more robust than standard GMM techniques and avoids the exponential increase in the number of instruments as $T$ grows. This method orthogonalizes the dependent and independent variables with respect to the fixed effects by using a "backward filter" that subtracts the average of the lagged values of each variable from its current value. The instruments are concomitantly "forward filtered," subtracting an average of the future values of each variable from its current value. The one-period-lagged, backward-filtered regressors are then used as instruments for the

forward-filtered variables to account for simultaneity bias. The doubly-filtered estimator has been shown to outperform standard GMM estimators in large $T$, small $N$ settings.

Our econometric analysis is divided into two parts. The first tests the linkages between political regimes, tax revenues, and performance for the 1650-1913 panel. The second incorporates the available expenditure data and tests the linkages between regimes, revenues, non-military spending, and performance from 1816 to 1913. In both cases, we estimate our model using GMM and doubly-filtered GMM methods.

# 8 Estimation Results

## 1650-1913 Panel

Table 8 presents the results of our estimations for the 1650-1913 period in two panels. Panel A displays the estimates of the effects of public revenues on economic performance from Equation 5, and Panel B the estimates of the effects of political regimes on public revenues (Equation 4).

Column (1) reports the results for the standard GMM estimator. Political transformations had significant positive fiscal effects in Panel B. The move from the fragmented and absolutist regime to the centralized and absolutist one increased per capita revenues by 18 percent per five-year interval, and the move to the centralized and limited one by 21 percent. The effect of the move to the fragmented and limited regime is positive but not significant. However, it becomes significant in the specifications shown in Columns (2) to (6). Public revenues (both contemporaneously and lagged) in turn had significant positive effects on GDP per capita in Panel A. Although the five-year fiscal impacts are small, performance effects accumulated over time. We illustrate the cumulative economic impacts of political transformations in the next subsection. Finally, the coefficients on the lag of per capita GDP in Panel A and the lag of per capita revenues in Panel B are positive and significant, indicating strong persistence over five-year intervals. GDP also had a positive concurrent effect on per capita revenues in Panel B.

How about the controls? Initial state capacity in 1650 had significant positive impacts on public revenues and economic performance alike. This finding supports our claim from Section 3 that institutional features of states (e.g., administrative compe-

tence) beyond the channel that we propose here played important fiscal and economic roles. Country size as measured by population also had significant positive effects on both dependent variables, although the significance of these impacts on public revenues falls in some later specifications. Both external and internal conflicts had negligible effects on revenues and per capita GDP.

Column (2) repeats the specification in Column (1) for the doubly-filtered GMM estimator. The fiscal impacts of political transformations in Panel B increase. Furthermore, the impact of public revenues on economic performance in Panel A doubles.

Recall from Section 7 that data for literacy rates, our proxy for human capital, are only available for eight sample countries. Columns (3) and (4) include this control for the standard GMM and doubly-filtered GMM estimators, respectively. The effects of political transformations on public revenues and in turn public revenues on economic performance remain similar in magnitude and significance to those shown in Columns (1) and (2). Indeed, the impacts of political transformations on per capita revenues are even more pronounced than before. Human capital as measured by literacy rates had positive impacts on both public revenues and economic performance, although these effects are only significant for the doubly-filtered GMM estimator.

Columns (5) and (6) repeat the specifications in Columns (1) and (2) for our alternative performance measure, urbanization rates. Since urbanization rates persisted for notably longer periods than per capita GDP, these specifications use 25-year intervals. The results resemble those displayed in previous columns. Public revenues in turn had significant positive impacts on urbanization in Panel A. These results also hold if we include literacy rates (not shown). Unlike the specifications that use per capita GDP as our performance measure, we do not find a concurrent effect of urbanization rates on public revenues in Panel B.

In sum, the results described in this subsection indicate that political transformations had significant positive impacts on economic performance through public revenue increases. These effects are strongly significant and robust across alternative specifications.

Data limitations prevent us from explicitly testing the links from revenues to non-military expenditures, and in turn from non-military expenditures to performance, in this setting. However, systematic non-military spending data are available over the nineteenth century, allowing us to gain a full picture of the linkages from political regimes to economic performance. The next subsection discusses these findings.

## 1816-1913 Panel

Table 9 presents the results of our estimations for the 1816-1913 period in three panels. To save space, we focus on the doubly-filtered GMM estimations. However, the results are similar in magnitude and significance for the standard GMM estimator. Panel A displays the estimates for Equation 3, which tests the effects of non-military expenditures on economic performance, Panel B the estimates of the effects of public revenues on non-military expenditures (Equation 2), and Panel C the effects of political regimes on public revenues (Equation 1).

Columns (1) and (2) report the results for the benchmark specification, including the control for literacy rates in the second column.[38] The impacts of political transformations on public finances and thus economic performance are all highly significant. Political transformations had significant positive effects on public revenues and non-military spending alike (Panels C and B, respectively). Greater revenues themselves also had significant positive impacts on non-military spending in Panel B. Lastly, non-military expenditures (both contemporaneously and lagged) had significant positive effects on economic performance in Panel A. We illustrate the dynamic and cumulative fiscal and economic impacts just ahead.

The results for the controls and lagged dependent variables generally resemble those for the 1650-1913 panel. Now external and internal conflicts have significant positive effects on public revenues in Panel C and significant negative effects on economic performance in Panel A. Furthermore, internal conflicts have significant positive effects on non-military spending in Panel B. This result supports the claim that social unrest increased the provision of public services (Acemoglu and Robinson, 2000).

Column (3) repeats the specification in Column (1) for state-built railway kilometers per square kilometer, our alternative measure to non-military spending. Public revenues had significant positive impacts on state railway investments in Panel B, which in turn had significant positive effects on economic performance in Panel A. A 1,000 kilometer increase in state-built railway kilometers led to a 10 percent increase in per capita GDP per five-year interval. These results are similar in magnitude and significance if we include literacy rates (not shown).

Columns (4) and (5) disaggregate the available non-military expenditure data into

[38] Recall from Table 6 that there are few data observations for non-military expenditures for the fragmented and limited regime (10 in total, 8 for Denmark and 2 for Portugal). We thus exclude this regime from this part of our econometric analysis.

spending on infrastructure or education. Due to the small number of observations available, we restrict our attention to the analysis of the impact of state expenditures on economic performance in Panel A.[39] Spending on infrastructure and education alike had a significant positive performance effects. A 1 percent increase in per capita infrastructure expenditures led to a 12 percent increase in per capita GDP per five-year interval, while a 1 percent increase in per capita education expenditures led to a 7 percent GDP increase.

Overall, these results provide further evidence that political transformations had significant positive impacts on economic performance through public finance improvements. By explicitly testing the linkages from government revenues to non-military expenditures, and in turn from non-military expenditures to performance, the findings for the 1816-1913 period complement those for the 1650-1913 one.

## Dynamic and Cumulative Effects

The estimates obtained from the structural model indicate that political transformations had strong direct impacts on public revenues and non-military expenditures, and through this channel, strong indirect impacts on economic performance. To illustrate the marginal effects of political transformations on fiscal and economic outcomes, we now compute the dynamic and cumulative multipliers based on the reduced-form parameters derived from the specification in Column (1) of Table 9.[40]

Figure 5 plots the dynamic multipliers for the average fiscal impacts of political transformations at five-year intervals over a century-long period, holding all other exogenous variables constant. While the revenue effects (left panel) peaked immediately, non-military spending effects (right panel) peaked after 10 years. The peak impact of the move from the fragmented and absolutist regime to the centralized and absolutist one increased per capita revenues by 27 percent, and the move to the centralized and limited regime by 35 percent. Similarly, the peak impact of the move from the fragmented and absolutist regime to the centralized and absolutist one increased

[39] Recall from Section 5 that there were no data observations for the infrastructure or education variables for fragmented and absolutist regimes.

[40] Starting from the structural representation of the model, $\Gamma Y_t + \Lambda Y_{t-1} + \Phi Z_t + _t = U_t$, the reduced form is given by $Y_t = \Pi Y_{t-1} + \Theta Z_t + V_t$, with $\Pi = -\Gamma^{-1}\Lambda$, $\Theta = -\Gamma^{-1}\Phi$, and $V_t = \Gamma^{-1}U_t$. The impact on $Y$ of changes in the exogenous variables $Z$ can then be quantified through a multiplier analysis based on the reduced-form parameters $\Pi$ and $\Theta$ (Greene, 2003). Note that this exercise cannot account for feedback effects from economic and fiscal outcomes to subsequent political reforms.

per capita non-military spending by 26 percent, and the move to the centralized and limited regime by 34 percent.

Although the direct effects of political transformations on public finances ultimately eroded, the positive indirect impacts on economic performance endured. Figure 6, which plots the average cumulative economic impacts of political transformations, indicates that performance effects accrued with time. Over a half-century, the move from the fragmented and absolutist regime to the centralized and limited regime increased per capita GDP by 10 percent, due to a per capita revenue increase of 190 percent and a non-military expenditure increase of 129 percent. Over a full century, the move to the centralized and limited regime increased per capita GDP by 14 percent. Similarly, the move to the centralized and absolutist one increased per capita GDP by 8 percent over a half-century and by 11 percent over a full century. Figure 7 indicates that both the dynamic and cumulative trends also hold for the 1650-1913 panel.

To further illustrate the performance effects of political transformations, we perform a counterfactual exercise that compares average actual per capita GDP in 1913 against two alternative scenarios: per capita GDP in 1913 if the average sample country had always had a centralized and limited regime, or if it had always remained a fragmented and absolutist regime. If the average country was centralized and limited from 1650, then per capita GDP in 1913 would have been 3,875 international dollars rather than 3,333, a 16 percent increase. If the average country remained fragmented and absolutist over the entire period, however, then per capita GDP in 1913 would have been 2,876 international dollars, a 14 percent decrease. The former result indicates that if for instance France had always had a centralized and limited regime, then per capita GDP would have been 17 percent higher by the eve of World War I. Political improvements would have thus established France as one of Europe's richest economies alongside Belgium and England, surpassing competitors Denmark, Prussia, and the Netherlands. Finally, recall from Section 3 that, due to the strictness of our regime definitions, our focus on a single causal channel, and the role of interstate political competition, the magnitudes of these results should be interpreted as lower-bound estimates of the fiscal and economic effects of political transformations.

# 9 Robustness

The historical evidence described in Section 2 suggests that it is plausible to treat political transformations as exogenous to fiscal and economic outcomes. A variety of standard statistical exogeneity tests support this claim. Moreover, our econometric methodology accommodates feedback effects from economic and fiscal developments to subsequent political reforms. To test the exogeneity assumption even further, we now perform two additional exercises: tests for reverse causation from Angrist and Pischke (2009) and sovereign deaths as a natural experiment.

## Angrist-Pischke Tests for Reverse Causation

To write.

## Sovereign Deaths as a Natural Experiment

As an alternative robustness check, we use sovereign deaths as a natural experiment following Besley and Kudamatsu (2008). If a sovereign with an eligible male heir dies due to natural causes just prior to fiscal centralization or limited government, then we can treat the subsequent political transformation as exogenous, since it is plausible to assume that pre- and post-death fiscal and economic conditions were similar.

Although there were no cases where the timing of sovereign deaths came just before (e.g., within one year) of political transformations, there were two close cases: Portugal in 1853 and Sweden in 1859. The first close case was the death of regnant queen Maria II of Portugal during childbirth. Her oldest son Pedro V immediately became king, reigning until his death in 1861. Although fiscal centralization did not officially occur until 1859, the 1850s under Pedro V were a period of great institutional change (Cardoso and Lains, 2010b, pp. 261-70).[41]

Before the 1850s, Portugal was isolated and poor. There was only one major road, linking Lisbon and Porto, and one major port, in Lisbon. Illiteracy was nearly 90 percent. During Pedro V's reign, average per capita revenues rose by nearly 20 percent relative to the previous decade and non-military expenditures by over 30 percent. The state made major investments in roads, railways, ports, and schools. Finally, the Portuguese economy expanded over the second half of the nineteenth century (Cardoso

[41] Recall from Table 2 that limited government was established in 1851 in Portugal.

and Lains, 2010b, pp. 251-3).

The second close case, the 1859 accession of King Charles XV of Sweden following the natural death of King Oscar I, has similar conclusions. Under Charles XV, who died in 1872, key institutional changes took place (Schön, 2010, pp. 176-8). Fiscal centralization was established in 1861 and limited government in 1866.

Sweden was a relatively poor agricultural economy during the first half of the nineteenth century. Under Charles XV, average per capita revenues rose by 20 percent relative to the previous decade and non-military expenditures by over 60 percent. The state played a major role in economic modernization from the late 1850s onward, with large-scale investments in railways, electrification, and telecommunications. Finally, the second half of the 1800s was an era of industrial dynamism in Sweden (Magnusson, 2009, pp. 114-21, Schön, 2010, pp. 162-6, 175-6, 182-4).

Overall, although neither robustness check can completely exclude the possibility of reverse causation from fiscal and economic conditions to political transformations, the key results remain strong. The robustness checks thus reinforce our argument that political transformations had significant positive economic effects through improved state capacity improvements.

# 10 Conclusion

This paper presents robust new evidence about the long-term links between state capacity and economic development. We focus on Europe, the birthplace of modern economic growth. Sovereign governments in European history were typically fiscally fragmented and absolutist. We argue that the establishment of states with modern extractive and productive capabilities had strongly positive performance effects.

To rigorously develop our claim, we perform a dynamic simultaneous equation panel analysis on a novel database that spans eleven countries and four centuries. This modeling approach enables us to explicitly test the set of linkages from political regime type to fiscal policies, and then from fiscal policies to economic outcomes. The results indicate that the performance effects of state capacity improvements are significant, large, and robust to a broad range of methods, specifications, controls, and sub-samples. We find that the establishment of an effective state increased per capita GDP by 10 percent over a half-century, and by 14 percent over a full century.

To the best of our knowledge, our results are among the first to provide rigor-

ous proof that state capacity plays a key role in long-run economic development. While we believe that this paper takes an important step, there is still ample room for future empirical work on state capacity. Our macro-oriented approach focuses on big-picture concerns. One valuable extension would be to use quasi-natural or randomized controlled micro-level experiments that pinpoint the finer causal effects of specific capacity-oriented policy interventions.

# Data Appendix

Data for per capita tax revenues and the control variables from 1650 to 1913 are from Dincecco (2011, appendices A.1, A.2, A.3). These data are downloadable from the website http://sites.google.com/site/mdincecco/. See Section 4 for further details.

Data sources for military, infrastructure, and education expenditures per capita are listed as follows. Disaggregated expenditure data in home currencies were converted into gold grams following the methodology in Dincecco (2011, appendix A.2). Data for total expenditures and population are from Dincecco (2011, appendices A.1, A.2) unless otherwise stated. These data concern total spending by national governments including debt service and incorporate loan amounts when given. Non-military expenditures per head were computed as per capita total expenditures minus per capita military expenditures.

*Austria.* Military spending data are from Pammer (2010, Figure 5.1). Infrastructure and education expenditure data are not available.

*Belgium.* Military spending data are from Singer (1987). They were downloaded from the Correlates of War website as the National Military Capabilities Dataset, Version 4.0. Infrastructure and education expenditure data are not available.

*Denmark.* Military spending data are from Singer (1987). They were downloaded from the Correlates of War website as the National Military Capabilities Dataset, Version 4.0. Infrastructure and education expenditure data are not available.

*England.* Military, infrastructure, and education spending data are from Mitchell (1988, public finance table 4). To compute military expenditures, spending for the Army and Ordnance and for the Navy were summed. Infrastructure expenditures uses the spending category for Works and Buildings, and education expenditures the category for Education, Art, and Science.

*France.* Military, infrastructure, and education spending data are from Fontvieille

(1976, Tables CVXI-XXXV). Infrastructure expenditures uses the spending category for Public Works.

*Italy.* Military spending data are from the Ufficio Storico (1980, pp. 508-9) for 1861-9 and Hobson (1993) for 1870-1913. Infrastructure and education expenditures are from Brosio and Marchese (1986, table 4a). Infrastructure spending uses the expenditure category for Public Works.

*Netherlands.* Military spending data are from van Zanden (1996, table 4) for 1816-41. Van Zanden provides data averages for 1816-20, 1821-4, 1825-9, 1831-4, 1835-9, and 1841-50. The average for 1816-20 was used for 1816, the average for 1821-4 for 1821, and so on. The military spending shares closely match those from van Zanden and van Riel (2010, table 2.1). Total expenditure data from this source were used in combination with the information on shares to back out military expenditures. For 1816-30 we divided these figures by the expenditure share for the Southern Netherlands (i.e., Belgium, Luxembourg, and their hinterlands) according to van Zanden (1996, table 5) to derive military expenditures for the (Northern) Netherlands, as data for total expenditures from Dincecco (2011) exclude the Southern Netherlands. The source for the 1816-41 data does not distinguish between infrastructure and education spending. Rather, both are included under the expenditure category for Home Affairs. Military, infrastructure, and education spending data are from van Zanden and van Riel (2010, table 2.3) for 1850-1913. They provide data shares at 10-year intervals for 1850, 1860, 1870, 1880, 1890, 1900, and 1913. Total expenditure data from this source were used in combination with the information on shares to back out military expenditures.

*Portugal.* Military, infrastructure, and education spending data are from Silveira (1987, table 8) for 1816-27, Mata and Valério (2001, table 1) for 1832-45, and Mata (1993, table 1) for 1851-1913. To compute military expenditures, spending by the Ministerio da Guerra (after 1827; Exercito beforehand) and the Ministerio da Marihna were summed. Infrastructure expenditures uses spending by the Ministério das Obras Públicas. There was no education ministry over this period. Education expenditures thus uses the category for the education burden (i.e., Encargos cum Instruções). Since the total military spending calculation matches well with the Encargos cum Difesa category (and perfectly from 1884 onward), we are confident that the same holds for education.

*Prussia.* The German Reich (1871-1945) was a federal system and a great deal of taxing and spending was done at the state (e.g., Prussian) level. The federal government

was responsible for military expenditures and welfare (Ziblatt, 2006). Spoerer (2010, table 4.1) provides Prussian military and welfare expenditures for 1847 and 1867. After unification there are only Reich data available for these categories. These data were not used because there was no clear way of integrating the pre-1871 Prussian series with the post-1870 Reich one. Spoerer's data for Prussia were supplemented with 1820 data for military defense from Ziblatt (2006, table 3.1). Here total Prussian expenditures from 1821 were used due to data availability.

*Spain.* Military spending data are from Carreras and Tafunell (2006), table 12.8 for 1816-42 and table 12.13 for 1845-1913. To compute military expenditures, spending by the Ministerio de Guerra (through 1842; the Minsterio de Defensa from 1845 onward) and the Ministerio de Marina were summed. The sources for the 1816-99 data do not distinguish between infrastructure and education spending. Rather, both are included under the expenditure category for the Ministerio de Estado through 1842 and the Ministerio de Fomento from 1845 onward.[42]

*Sweden.* Military spending data are from Krantz and Schön (2010, table XI). At the central government level, there are no separate expenditure categories for infrastructure or education. Rather, it is probable that both are included under the spending category for civil services. We thus use this category as a proxy for these two types of expenditures.

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[42]Disaggregated infrastructure and expenditure data for the Ministerio de Fomento are displayed for 1900-13. These data indicate that infrastructure (Ministerio de Obras Públicas) and education (Ministerio de Educación y Cienca) comprised all of the Minsterio de Fomento's expenditures for these years.

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Figure 1: Flow Chart

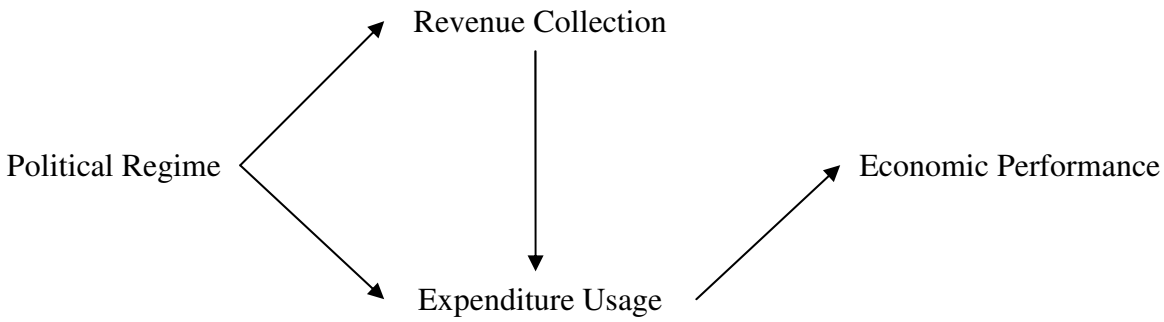
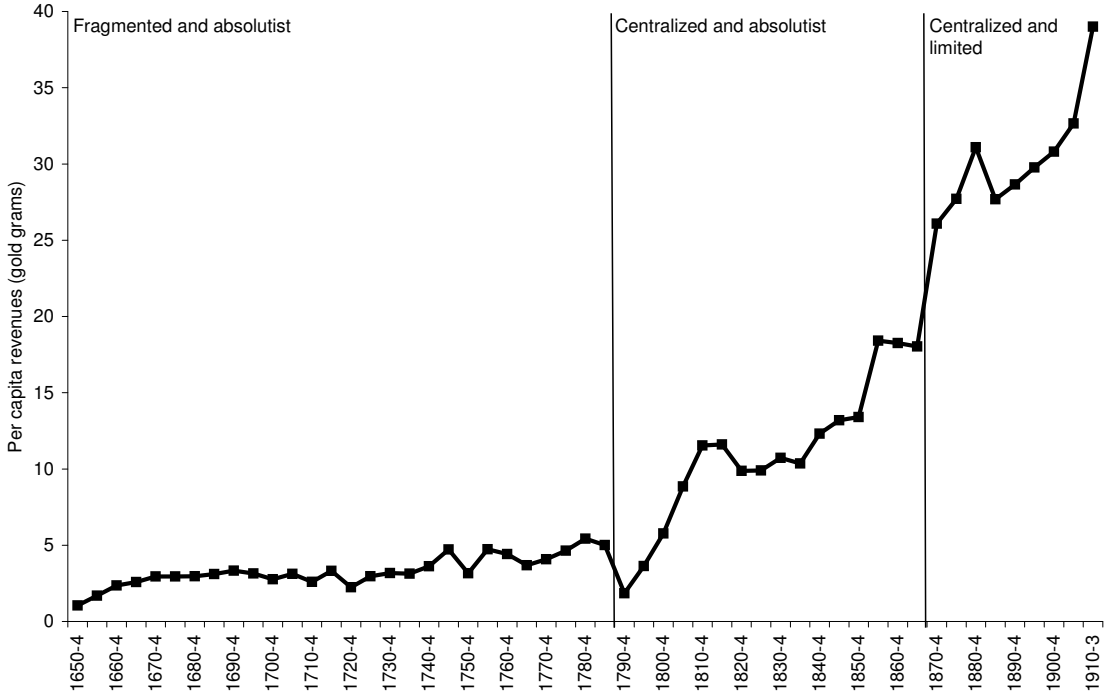
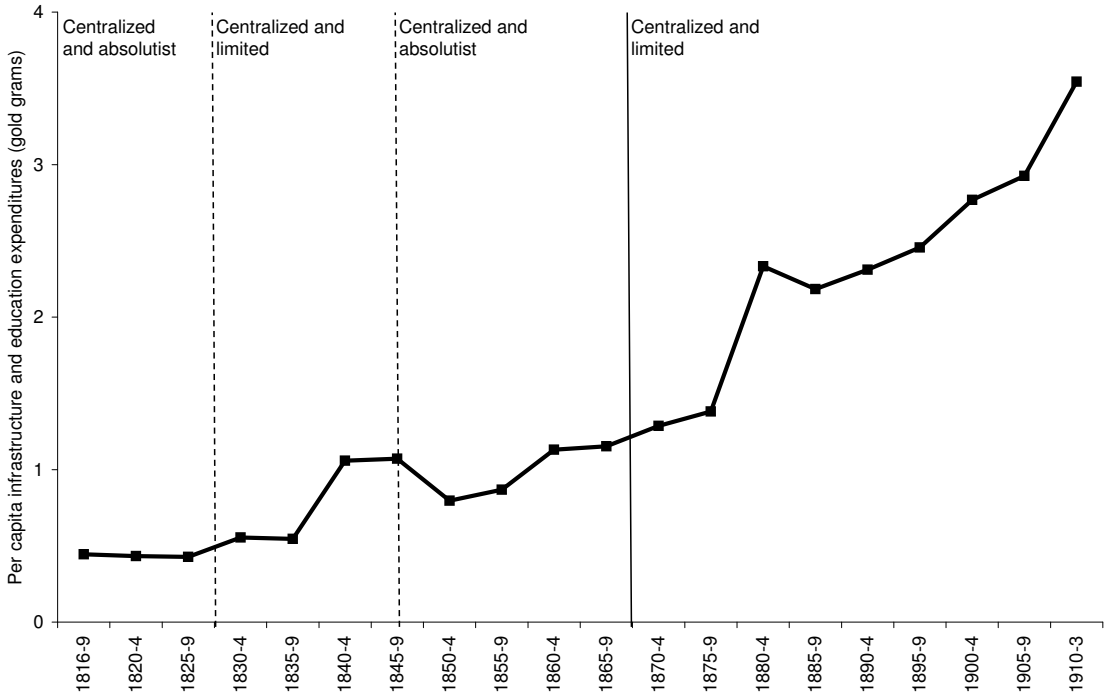


Figure 2: Per Capita Revenues, France, 1650-1913



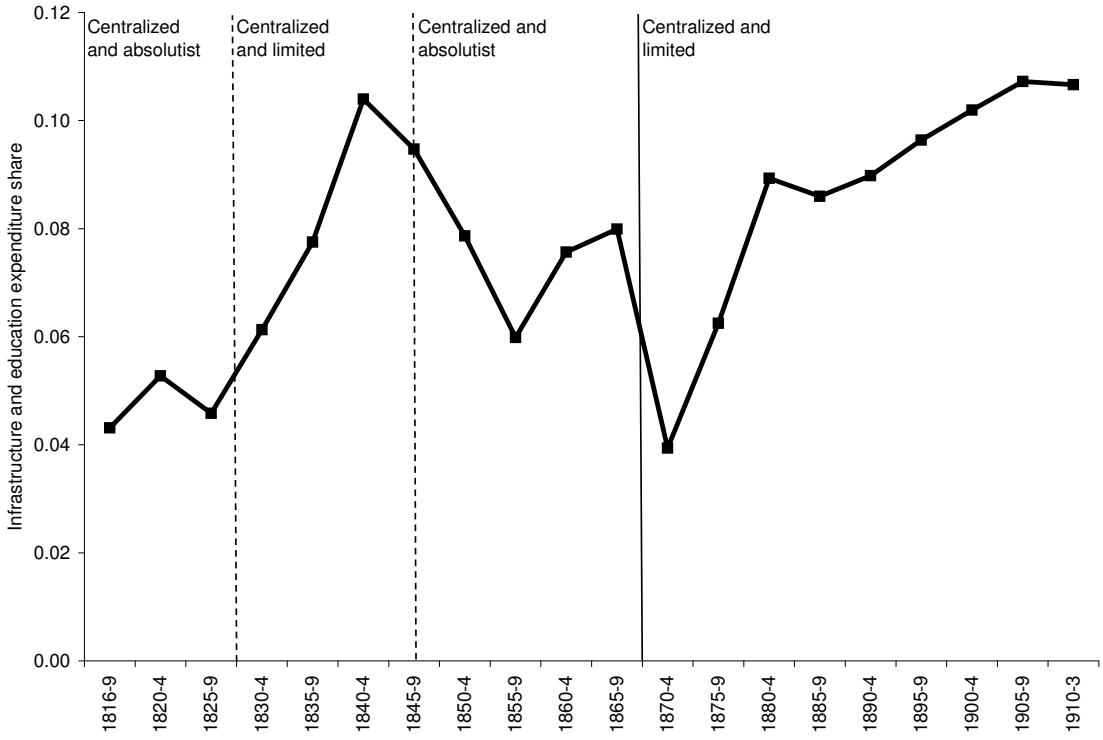

Source: Dincecco (2011).

Figure 3: Per Capita Infrastructure and Education Spending, France, 1816-1913




Source: See text.

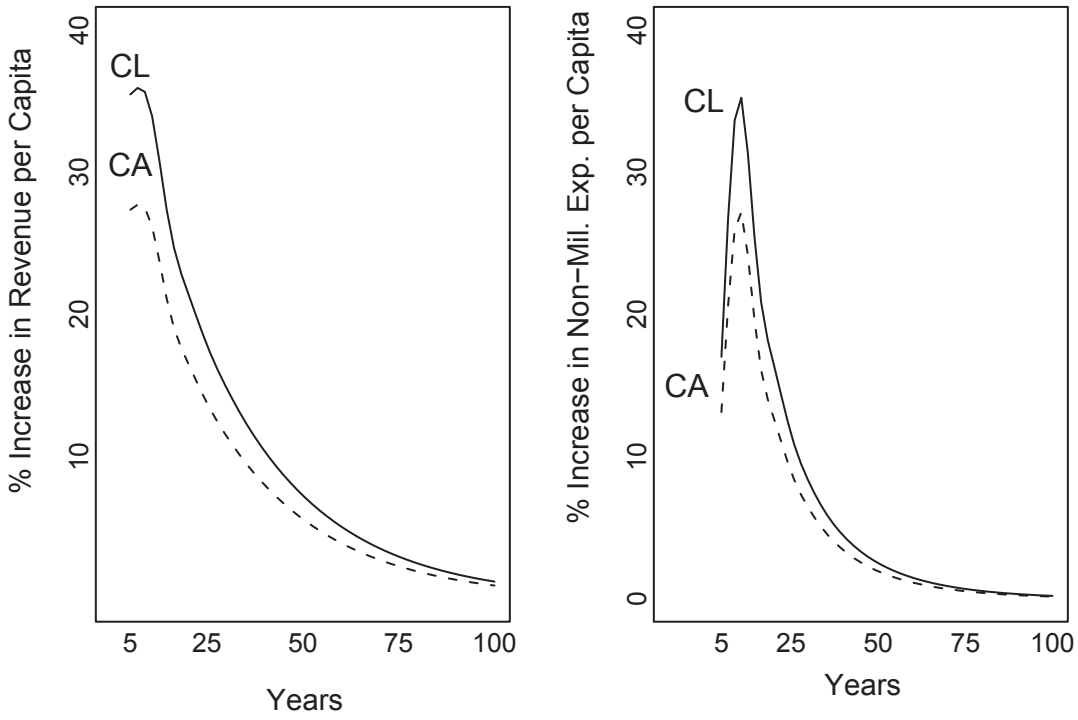
Figure 4: Infrastructure and Education Spending Share, France, 1816-1913




Source: See text.

Note: Figure displays share of infrastructure and education expenditures in total expenditures.

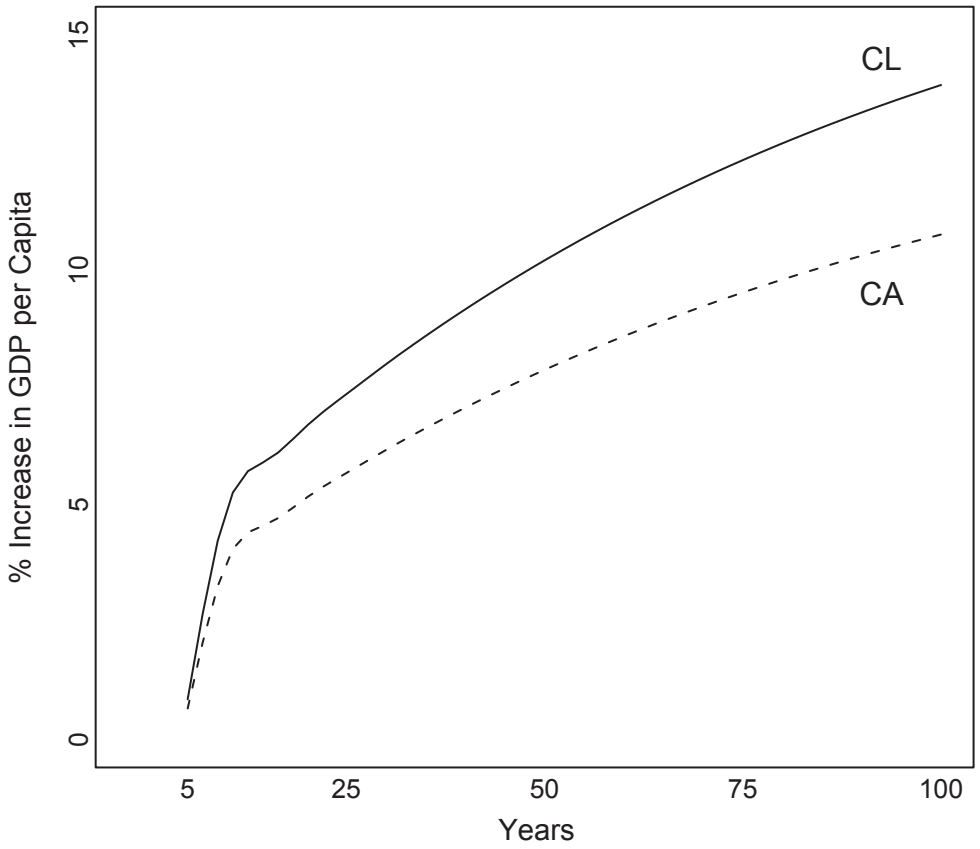
Figure 5: Dynamic Fiscal Effects of Regime Type, 1816-1913




Source: See text.

Note: CA=Centralized and absolutist, CL=Centralized and limited. Left (right) panel displays average effect of regime type on per capita revenues (per capita non-military expenditures) relative to fragmented and absolutist regime at 5-year intervals. Fragmented and limited regime excluded due to lack of observations.

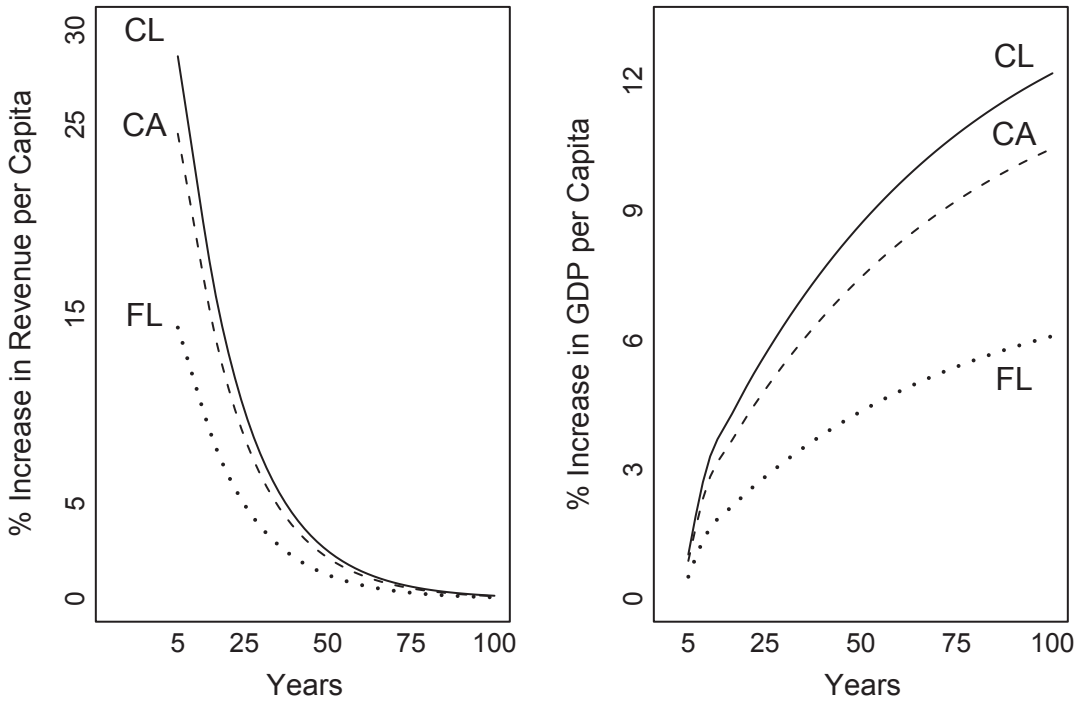
Figure 6: Cumulative Performance Effects of Regime Type, 1816-1913




Source: See text.

Note: CA=Centralized and absolutist, CL=Centralized and limited. Figure displays average cumulative effect of regime type on economic performance relative to fragmented and absolutist regime. Fragmented and limited regime excluded due to lack of observations.

Figure 7: Dynamic and Cumulative Effects of Regime Type, 1650-1913




Source: See text.

Note: CA=Centralized and absolutist, FL=Fragmented and limited, CL=Centralized and limited. Left panel displays average effect of regime type on per capita revenues relative to fragmented and absolutist regime at 5-year intervals. Right panel displays average cumulative effect of regime type on economic performance relative to fragmented and absolutist regime.

Table 1: Dates of Fiscal Centralization in Europe

| | Year | Event |
|---|---|---|
| England | 1066 | Establishment of uniform rule after Norman Conquest |
| France | 1790 | Major administrative reforms after Revolution of 1789 |
| Belgium | 1795 | Major administrative reforms after French annexation |
| Netherlands | 1806 | Major administrative reforms under French control |
| Prussia | 1806 | Major administrative reforms after French defeat in battle |
| Spain | 1845 | Major administrative reforms after Moderate Coup of 1843 |
| Austria | 1848 | Major administrative reforms during Year of Revolutions |
| Portugal | 1859 | Major administrative reforms after Revolutionary Era |
| Italy | 1861 | Tax unification after defeat of Austria and founding of kingdom |
| Sweden | 1861 | Abolition of pre-modern tax system |
| Denmark | 1903 | Abolition of pre-modern tax system |

Source: Dincecco (2011).

Note: See text for definition of fiscal centralization.

Table 2: Dates of Limited Government in Europe

| | Year | Event |
|---|---|---|
| Netherlands | 1572 | Establishment of Dutch Republic (1572-1795) after revolt from Spain |
| | 1848 | Implementation of new constitution during Year of Revolutions |
| England | 1688 | Establishment of constitutional monarchy during Glorious Revolution |
| Belgium | 1831 | Founded as constitutional monarchy after Revolution of 1830 |
| Prussia | 1848 | Establishment of constitutional monarchy during Year of Revolutions |
| Denmark | 1848 | Establishment of constitutional monarchy during Year of Revolutions |
| Portugal | 1851 | Establishment of stable constitutional monarchy after Revolutionary Era |
| Italy | 1861 | Founded as constitutional monarchy after defeat of Austria |
| Sweden | 1866 | Introduction of bicameral legislature |
| Austria | 1867 | Establishment of constitutional monarchy after defeat by Prussia |
| France | 1870 | Formation of constitutional regime during war with Prussia |
| Spain | 1876 | Establishment of stable constitutional monarchy after civil war |

Source: Dincecco (2011).

Note: See text for definition of limited government.

Table 3: Fiscal and Economic Characteristics of Political Regimes

| Regime Type | Revenues | Non-Military Spending | Performance |
|---|---|---|---|
| Fragmented and Absolutist | Low | Low | Low |
| Centralized and Absolutist | High | Low or High | Low or Increases |
| Fragmented and Limited | Low | High | Increases |
| Centralized and Limited | High | High | High |

Table 4: Per Capita GDP Levels, 1650-1913

| | | All Regimes | FA | CA | FL | CL |
|---|---|---|---|---|---|---|
| Totals | Obs | 356 | 128 | 57 | 27 | 144 |
| | Mean | 1,670 | 1,016 | 1,446 | 2,043 | 2,176 |
| Austria | Obs | 21 | 7 | 5 | | 9 |
| | Mean | 1,872 | 1,422 | 1,743 | | 2,451 |
| Belgium | Obs | 16 | | | | 16 |
| | Mean | 2,662 | | | | 2,662 |
| Denmark | Obs | 11 | | | 9 | 2 |
| | Mean | 2,908 | | | 2,378 | 3,439 |
| England | Obs | 53 | | 8 | | 45 |
| | Mean | 1,625 | | 1,169 | | 2,081 |
| France | Obs | 54 | 28 | 17 | | 9 |
| | Mean | 1,581 | 959 | 1,357 | | 2,428 |
| Italy | Obs | 10 | | | | 10 |
| | Mean | 1,668 | | | | 1,668 |
| Netherlands | Obs | 39 | | 10 | 16 | 13 |
| | Mean | 2,331 | | 2,027 | 1,995 | 2,970 |
| Portugal | Obs | 30 | 17 | | 2 | 11 |
| | Mean | 963 | 907 | | 928 | 1,053 |
| Prussia | Obs | 46 | 24 | 9 | | 13 |
| | Mean | 1,459 | 973 | 1,239 | | 2,164 |
| Spain | Obs | 43 | 29 | 7 | | 7 |
| | Mean | 1,291 | 950 | 1,200 | | 1,725 |
| Sweden | Obs | 33 | 23 | 1 | | 9 |
| | Mean | 1,591 | 1,172 | 1,462 | | 2,140 |

Source: Maddison (2010).

Note: FA=Fragmented and Absolutist, CA=Centralized and Absolutist, FL=Fragmented and Limited, CL=Centralized and Limited. Data are expressed in 1990 international Geary-Khamis dollars and use 5-year averages.

Table 5: Per Capita Revenues, 1650-1913

| | | All Regimes | FA | CA | FL | CL |
|---|---|---|---|---|---|---|
| Totals | Obs | 356 | 128 | 57 | 27 | 144 |
| | Mean | 8.23 | 2.42 | 7.05 | 10.49 | 12.97 |
| Austria | Obs | 21 | 7 | 5 | | 9 |
| | Mean | 7.89 | 3.11 | 6.02 | | 14.54 |
| Belgium | Obs | 16 | | | | 16 |
| | Mean | 14.78 | | | | 14.78 |
| Denmark | Obs | 11 | | | 9 | 2 |
| | Mean | 11.90 | | | 9.80 | 13.99 |
| England | Obs | 53 | | 8 | | 45 |
| | Mean | 8.09 | | 2.69 | | 13.49 |
| France | Obs | 54 | 28 | 17 | | 9 |
| | Mean | 14.31 | 3.32 | 11.01 | | 28.60 |
| Italy | Obs | 10 | | | | 10 |
| | Mean | 14.41 | | | | 14.41 |
| Netherlands | Obs | 39 | | 10 | 16 | 13 |
| | Mean | 12.20 | | 10.86 | 12.07 | 13.66 |
| Portugal | Obs | 30 | 17 | | 2 | 11 |
| | Mean | 1.38 | 0.76 | | 0.96 | 2.42 |
| Prussia | Obs | 46 | 24 | 9 | | 13 |
| | Mean | 6.32 | 3.62 | 3.84 | | 11.51 |
| Spain | Obs | 43 | 29 | 7 | | 7 |
| | Mean | 2.35 | 1.00 | 2.34 | | 3.72 |
| Sweden | Obs | 33 | 23 | 1 | | 9 |
| | Mean | 5.29 | 2.89 | 3.63 | | 9.36 |

Source: Dincecco (2011).

Note: FA=Fragmented and Absolutist, CA=Centralized and Absolutist, FL=Fragmented and Limited, CL=Centralized and Limited. Data are expressed in gold grams and use 5-year averages.

Table 6: Per Capita Non-Military Spending, 1816-1913

| | | All Regimes | FA | CA | FL | CL |
|---|---|---|---|---|---|---|
| Totals | Obs | 165 | 27 | 29 | 10 | 99 |
| | Mean | 6.90 | 1.81 | 6.95 | 7.27 | 11.57 |
| Austria | Obs | 20 | 6 | 5 | | 9 |
| | Mean | 8.26 | 2.77 | 6.01 | | 16.00 |
| Belgium | Obs | 16 | | | | 16 |
| | Mean | 14.74 | | | | 14.74 |
| Denmark | Obs | 11 | | | 8 | 3 |
| | Mean | 10.19 | | | 8.06 | 12.32 |
| England | Obs | 19 | | | | 19 |
| | Mean | 12.15 | | | | 12.15 |
| France | Obs | 20 | | 11 | | 9 |
| | Mean | 15.03 | | 9.92 | | 20.15 |
| Italy | Obs | 9 | | | | 9 |
| | Mean | 12.85 | | | | 12.85 |
| Netherlands | Obs | 10 | | 4 | | 6 |
| | Mean | 12.95 | | 13.00 | | 12.89 |
| Portugal | Obs | 19 | 6 | | 2 | 11 |
| | Mean | 1.18 | 0.46 | | 0.83 | 2.24 |
| Prussia | Obs | 3 | | 2 | | 1 |
| | Mean | 3.86 | | 2.72 | | 5.00 |
| Spain | Obs | 19 | 6 | 6 | | 7 |
| | Mean | 1.80 | 0.71 | 1.88 | | 2.81 |
| Sweden | Obs | 19 | 9 | 1 | | 9 |
| | Mean | 3.98 | 1.61 | 3.34 | | 6.98 |

Source: See Appendix.

Note: FA=Fragmented and Absolutist, CA=Centralized and Absolutist, FL=Fragmented and Limited, CL=Centralized and Limited. Data are expressed in gold grams and use 5-year averages.

Table 7: Infrastructure and Education Spending, 1816-1913

| | | All Regimes | FA | CA | FL | CL |
|---|---|---|---|---|---|---|
| Panel A: Per Capita Infrastructure Expenditures | | | | | | |
| Totals | Obs | 70 | 0 | 13 | 2 | 55 |
| | Mean | 0.64 | | 0.68 | 0.21 | 1.02 |
| England | Obs | 19 | | | | 19 |
| | Mean | 0.23 | | | | 0.23 |
| France | Obs | 20 | | 11 | | 9 |
| | Mean | 0.84 | | 0.68 | | 1.00 |
| Italy | Obs | 9 | | | | 9 |
| | Mean | 2.92 | | | | 2.92 |
| Netherlands | Obs | 6 | | | | 6 |
| | Mean | 1.57 | | | | 1.57 |
| Portugal | Obs | 13 | | | 2 | 11 |
| | Mean | 0.35 | | | 0.21 | 0.48 |
| Prussia | Obs | 3 | | 2 | | 1 |
| | Mean | 1.34 | | 0.67 | | 2.02 |
| Panel B: Per Capita Education Expenditures | | | | | | |
| Totals | Obs | 70 | 0 | 13 | 2 | 55 |
| | Mean | 0.37 | | 0.29 | 0.04 | 0.78 |
| England | Obs | 19 | | | | 19 |
| | Mean | 0.72 | | | | 0.72 |
| France | Obs | 20 | | 11 | | 9 |
| | Mean | 0.64 | | 0.11 | | 1.18 |
| Italy | Obs | 9 | | | | 9 |
| | Mean | 1.29 | | | | 1.29 |
| Netherlands | Obs | 6 | | | | 6 |
| | Mean | 0.51 | | | | 0.51 |
| Portugal | Obs | 13 | | | 2 | 11 |
| | Mean | 0.06 | | | 0.04 | 0.08 |
| Prussia | Obs | 3 | | 2 | | 1 |
| | Mean | 2.17 | | 1.31 | | 3.03 |

Source: See Appendix.

Note: FA=Fragmented and Absolutist, CA=Centralized and Absolutist, FL=Fragmented and Limited, CL=Centralized and Limited. Data are expressed in gold grams and use 5-year averages.

Table 8: Estimation Results, 1650-1913

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Standard GMM | Filtered GMM | Standard GMM | Filtered GMM | Standard GMM | Filtered GMM |
| Panel A: Dependent Variable is Economic Performance Measure | | | | | | |
| | $GDP_t$ | $GDP_t$ | $GDP_t$ | $GDP_t$ | $Urban_t$ | $Urban_t$ |
| Per capita $revenues_t$ | $0.03^{***}$ | $0.06^{***}$ | $0.02^{***}$ | $0.05^{***}$ | $0.04^{***}$ | $0.05^{***}$ |
| | (0.01) | (0.01) | (0.01) | (0.01) | (0.01) | (0.01) |
| Per capita $GDP_{t-1}$ | $0.95^{***}$ | $0.89^{***}$ | $0.91^{***}$ | $0.88^{***}$ | | |
| | (0.03) | (0.05) | (0.04) | (0.06) | | |
| Urbanization $rate_{t-1}$ | | | | | $0.93^{***}$ | $0.91^{***}$ |
| | | | | | (0.06) | (0.08) |
| Per capita $revenues_{t-1}$ | $0.01^{***}$ | $0.02^{***}$ | $0.01^{***}$ | $0.02^{***}$ | $0.01^{**}$ | $0.04^{***}$ |
| | (0.00) | (0.00) | (0.00) | (0.00) | (0.01) | (0.01) |
| External war dummy | $-0.02$ | $-0.04$ | $-0.01$ | $-0.02$ | $-0.01$ | $-0.02$ |
| | (0.03) | (0.05) | (0.01) | (0.03) | (0.02) | (0.05) |
| Internal war dummy | $-0.01$ | $-0.01$ | $-0.01$ | $-0.01$ | $-0.01$ | $-0.01$ |
| | (0.01) | (0.01) | (0.01) | (0.01) | (0.01) | (0.01) |
| Initial state capacity | $0.14^{***}$ | $0.17^{***}$ | $0.11^{***}$ | $0.15^{***}$ | $0.08^{**}$ | $0.14^{***}$ |
| | (0.03) | (0.05) | (0.02) | (0.04) | (0.03) | (0.05) |
| Population | $0.08^{***}$ | $0.13^{***}$ | $0.08^{**}$ | $0.11^{**}$ | 0.05 | $0.058^{**}$ |
| | (0.03) | (0.04) | (0.04) | (0.04) | (0.03) | (0.05) |
| Literacy rate | | | 0.06 | $0.08^{**}$ | | |
| | | | (0.04) | (0.04) | | |
| P-value of Wald $\chi^2$ | 0.00 | 0.00 | 0.00 | 0.00 | 0.03 | 0.00 |

Table 8, Continued: Regression Results, 1650-1913

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Panel B: Dependent Variable is Per Capita Revenues$_t$ | | | | | | |
| Centralized and absolutist regimes | 0.18*** | 0.24*** | 0.19*** | 0.26*** | 0.27*** | 0.35*** |
| | (0.04) | (0.03) | (0.04) | (0.03) | (0.09) | (0.07) |
| Fragmented and limited regimes | 0.09 | 0.14* | 0.01* | 0.15** | 0.15** | 0.22** |
| | (0.05) | (0.08) | (0.06) | (0.08) | (0.07) | (0.09) |
| Centralized and limited regimes | 0.21*** | 0.28*** | 0.22*** | 0.29*** | 0.32*** | 0.45*** |
| | (0.07) | (0.09) | (0.06) | (0.08) | (0.14) | (0.14) |
| Per capita revenues$_{t-1}$ | 0.69*** | 0.74*** | 0.65*** | 0.69*** | 0.63*** | 0.71*** |
| | (0.07) | (0.12) | (0.08) | (0.13) | (0.08) | (0.13) |
| Per capita GDP$_t$ | 0.27* | 0.35** | 0.25** | 0.37** | | |
| | (0.15) | (0.15) | (0.13) | (0.14) | | |
| Urbanization rate$_t$ | | | | | 0.10 | 0.12 |
| | | | | | (0.12) | (0.15) |
| Per capita GDP$_{t-1}$ | 0.21 | 0.29* | 0.22 | 0.29** | 0.19 | 0.26 |
| | (0.16) | (0.15) | (0.16) | (0.14) | (0.17) | (0.17) |
| External war dummy | 0.06 | 0.10 | 0.05 | 0.10 | 0.05 | 0.08 |
| | (0.04) | (0.06) | (0.04) | (0.06) | (0.04) | (0.07) |
| Internal war dummy | 0.02 | 0.04 | 0.03 | 0.04 | 0.01 | 0.02 |
| | (0.06) | (0.07) | (0.06) | (0.07) | (0.04) | (0.05) |
| Initial state capacity | 0.12*** | 0.14*** | 0.14*** | 0.16*** | 0.06** | 0.11*** |
| | (0.03) | (0.05) | (0.03) | (0.06) | (0.03) | (0.05) |
| Population | 0.06** | 0.11** | 0.05 | 0.10** | 0.4 | 0.07 |
| | (0.03) | (0.04) | (0.03) | (0.04) | (0.03) | (0.05) |
| Literacy rate | | | 0.05 | 0.07** | | |
| | | | (0.04) | (0.03) | | |
| P-value of Wald $\chi^2$ | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 |
| Country fixed effects | Yes | Yes | Yes | Yes | Yes | Yes |
| Linear time trend | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 345 | 345 | 287 | 287 | 68 | 68 |
| Number of countries | 11 | 11 | 8 | 8 | 11 | 11 |

***Significant at 1%; **Significant at 5%; *Significant at 10%

Note: Revenue, GDP, and population data are in natural logarithms. Data in Columns (1) to (4) use 5-year averages, and data in Columns (5) and (6) use 25-year averages. Robust standard errors are in parentheses.

Table 9: Estimation Results, 1816-1913

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Filtered GMM | | | | | |
| | Panel A: Dependent Variable is Economic Performance Measure | | | | | |
| | $GDP_t$ | $GDP_t$ | $Urban_t$ | $GDP_t$ | $GDP_t$ | $GDP_t$ |
| Per capita non-military $exps_t$ | $0.05^{***}$ | $0.06^{**}$ | $0.03^{***}$ | | | |
| | (0.01) | (0.02) | (0.01) | | | |
| State-built railway $km_t$ | | | | $0.10^{**}$ | | |
| | | | | (0.04) | | |
| Per capita infrastructure $exps_t$ | | | | | $0.12^{***}$ | |
| | | | | | (0.02) | |
| Per capita education $exps_t$ | | | | | | $0.07^{***}$ |
| | | | | | | (0.01) |
| Per capita $GDP_{t-1}$ | $0.91^{***}$ | $0.90^{***}$ | | $0.87^{***}$ | $0.88^{***}$ | $0.90^{***}$ |
| | (0.03) | (0.03) | | (0.04) | (0.02) | (0.03) |
| Per capita non-military $exps_{t-1}$ | $0.03^{***}$ | $0.03^{***}$ | $0.03^{***}$ | | | |
| | (0.01) | (0.01) | (0.01) | | | |
| Urbanization $rate_{t-1}$ | | | $0.94^{***}$ | | | |
| | | | (0.04) | | | |
| State-built railway $km_{t-1}$ | | | | $0.06^{*}$ | | |
| | | | | (0.04) | | |
| Per capita infrastructure $exps_{t-1}$ | | | | | $0.05^{***}$ | |
| | | | | | (0.01) | |
| Per capita education $exps_{-1}$ | | | | | | $0.03^{***}$ |
| | | | | | | (0.01) |
| Per capita $revenues_{t-1}$ | $0.02^{**}$ | $0.02^{**}$ | 0.01 | $0.03^{***}$ | $0.04^{***}$ | $0.02^{**}$ |
| | (0.01) | (0.01) | (0.01) | (0.01) | (0.01) | (0.01) |
| External war dummy | $-0.03^{***}$ | $-0.02^{*}$ | $-0.01$ | $-0.03^{***}$ | $-0.03^{***}$ | $-0.02^{***}$ |
| | (0.01) | (0.01) | (0.03) | (0.01) | (0.01) | (0.01) |
| Internal war dummy | $-0.02^{**}$ | $-0.02^{*}$ | $-0.02^{**}$ | $-0.04^{***}$ | $-0.03^{***}$ | $-0.03^{***}$ |
| | (0.01) | (0.01) | (0.01) | (0.01) | (0.01) | (0.01) |
| Initial state capacity | $0.28^{***}$ | $0.29^{***}$ | $0.28^{***}$ | $0.11^{**}$ | $0.27^{***}$ | $0.29^{***}$ |
| | (0.07) | (0.07) | (0.07) | (0.07) | (0.05) | (0.06) |
| Population | $0.06^{**}$ | $0.08^{**}$ | $0.06^{**}$ | $0.07^{*}$ | $0.10^{**}$ | $0.12^{***}$ |
| | (0.03) | (0.04) | (0.03) | (0.04) | (0.04) | (0.04) |
| Literacy rate | | $0.12^{***}$ | | | | |
| | | (0.02) | | | | |
| P-value of Wald $\chi^2$ | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 |

Table 9, Continued: Regression Results, 1816-1913

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Panel B: Dependent Variable is Non-Military Exps or State-Built Railway Km | | | | | | |
| | $\text{Nonmil}_t$ | $\text{Nonmil}_t$ | $\text{Nonmil}_t$ | RR $\text{km}_t$ | | |
| Centralized and absolutist regimes | | | | | | |
| Centralized and limited regimes | | | | | | |
| Per capita $\text{revenues}_t$ | $0.48^{***}$ | $0.44^{***}$ | $0.48^{***}$ | $0.45^{***}$ | | |
| | (0.09) | (0.08) | (0.09) | (0.10) | | |
| Per capita non-military $\text{exps}_{t-1}$ | $0.67^{***}$ | $0.61^{***}$ | $0.67^{***}$ | | | |
| | (0.08) | (0.07) | (0.08) | | | |
| Urbanization $\text{rate}_{t-1}$ | | | $0.05^{***}$ | | | |
| | | | (0.01) | | | |
| State-built railway $\text{km}_{t-1}$ | | | | $0.47^{***}$ | | |
| | | | | (0.11) | | |
| Per capita $\text{revenues}_{t-1}$ | $0.20^{**}$ | $0.22^{***}$ | $0.20^{**}$ | $0.22^{**}$ | | |
| | (0.08) | (0.07) | (0.08) | (0.09) | | |
| Per capita $\text{GDP}_{t-1}$ | $0.28^{**}$ | $0.29^{**}$ | $0.28^{**}$ | $0.30^{***}$ | | |
| | (0.12) | (0.11) | (0.12) | (0.10) | | |
| External war dummy | 0.03 | 0.04 | 0.03 | 0.04 | | |
| | (0.05) | (0.05) | (0.05) | (0.05) | | |
| Internal war dummy | $0.11^{**}$ | $0.12^{***}$ | $0.11^{**}$ | $0.08^{**}$ | | |
| | (0.04) | (0.03) | (0.04) | (0.04) | | |
| Initial state capacity | $0.22^{***}$ | $0.19^{***}$ | $0.22^{***}$ | $0.17^{**}$ | | |
| | (0.07) | (0.06) | (0.07) | (0.07) | | |
| Population | $0.11^{**}$ | $0.11^{***}$ | $0.11^{**}$ | $0.07^{**}$ | | |
| | (0.04) | (0.04) | (0.04) | (0.04) | | |
| Literacy rate | | $0.11^{***}$ | | | | |
| | | (0.03) | | | | |
| P-value of Wald $\chi^2$ | 0.06 | 0.03 | 0.06 | 0.04 | | |

Table 9, Continued: Regression Results, 1816-1913

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Panel C: Dependent Variable is Per Capita Revenues$_t$ | | | | | | |
| Centralized and absolutist regimes | 0.27*** | 0.24*** | 0.27*** | 0.29*** | | |
| | (0.07) | (0.06) | (0.07) | (0.08) | | |
| Centralized and limited regimes | 0.35*** | 0.37*** | 0.35*** | 0.36*** | | |
| | (0.05) | (0.06) | (0.05) | (0.05) | | |
| Per capita revenues$_{t-1}$ | 0.81*** | 0.80*** | 0.81*** | 0.77*** | | |
| | (0.08) | (0.07) | (0.08) | (0.10) | | |
| Per capita GDP$_t$ | 0.53*** | 0.51*** | 0.53*** | 0.51*** | | |
| | (0.17) | (0.14) | (0.17) | (0.13) | | |
| Urbanization rate$_t$ | | | 0.05*** | | | |
| | | | (0.01) | | | |
| Per capita GDP$_{t-1}$ | 0.33*** | 0.34** | 0.33*** | 0.37** | | |
| | (0.16) | (0.15) | (0.16) | (0.18) | | |
| Per capita non-military exps$_{t-1}$ | 0.21** | 0.22** | 0.21** | | | |
| | (0.09) | (0.07) | (0.09) | | | |
| State-built railway km$_{t-1}$ | | | | 0.18** | | |
| | | | | (0.01) | | |
| External war dummy | 0.06** | 0.06** | 0.06** | 0.08 | | |
| | (0.03) | (0.02) | (0.03) | (0.05) | | |
| Internal war dummy | 0.11** | 0.12** | 0.11** | 0.13** | | |
| | (0.05) | (0.05) | (0.05) | (0.07) | | |
| Initial state capacity | 0.27** | 0.26** | 0.27** | 0.21** | | |
| | (0.09) | (0.08) | (0.09) | (0.01) | | |
| Population | 0.12** | 0.11** | 0.12** | 0.09* | | |
| | (0.09) | (0.05) | (0.09) | (0.05) | | |
| Literacy rate | | 0.10*** | | | | |
| | | (0.03) | | | | |
| P-value of Wald $\chi^2$ | 0.00 | 0.00 | 0.00 | 0.00 | | |
| Country fixed effects | Yes | Yes | Yes | Yes | Yes | Yes |
| Time fixed effects | Yes | Yes | Yes | Yes | Yes | Yes |
| Linear time trend | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 169 | 126 | 169 | 169 | 65 | 65 |
| Number of countries | 11 | 11 | 11 | 11 | 6 | 6 |

***Significant at 1%; **Significant at 5%; *Significant at 10%

Note: Revenue, expenditure, railway, GDP, and population data are in natural logarithms. Data use 5-year averages. Robust standard errors are in parentheses.