

Intra-Elite Competition and Long-Run Fiscal Development

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This paper exploits an original database that spans 30-plus developed and developing nations between 1870 and 2010 to perform the first empirical analysis of the relationship between historical levels of intra-elite competition and fiscal development over the long run. We argue that the timing of industrialization affects the extent of historical competition between agricultural and capitalist elites, which in turn helps shape key initial decisions over fiscal size and structure. Under “early” industrialization, intra-elite competition levels tended to be greater, promoting fiscal development characterized by high overall taxation and tax progressivity. Under “late” industrialization, by contrast, agricultural elites were more likely to retain political dominance, promoting fiscal states characterized by low overall taxation and tax regressivity. We show evidence for a positive, statistically significant, and robust relationship between historical intra-elite competition levels and long-run fiscal development. This focus on intra-elite competition improves our understanding of the fundamental determinants of cross-national fiscal differences today.

There are striking differences in the size and structure of modern fiscal states. To illustrate, figure 1 plots the overall tax take (as measured by the tax-to-GDP ratio) and tax progressivity (as measured by the direct tax share) across 30-plus developed and developing nations. Over the 2000s, the overall tax take in this sample ranged from roughly 10% to 40% of gross domestic product (GDP) across nations, while revenue from progressive taxation ranged from roughly 25% to 80%.

To help explain cross-national fiscal differences today, this paper puts forth an argument that links historical levels of intra-elite competition to long-run fiscal development. This approach builds on previous works that relate infighting among elites to economic and political change (e.g., Albertus 2015; Ansell and Samuels 2014; Congleton 2011; Garfias 2018; Lizzeri and Persico 2004; Mares and Queralt 2015). We argue that industrialization may prompt “new” capitalist elites to challenge the traditional political dominance of “old” agricultural elites. The

historical extent of this intra-elite conflict helps shape key initial decisions over fiscal size and structure, which influences how fiscal states subsequently evolve.

Our argument analyzes the basic fiscal decision-making process that historical elites may have undertaken. First, elites had to decide whether to invest in greater fiscal capacity and fund new public goods (e.g., transportation infrastructure, urban sanitation) with the potential to improve productivity in an industrializing economy. Second, if elites did in fact make such an investment, then they had to decide how to allocate the new tax costs associated with it.

We argue that the timing of industrialization influenced this decision-making process by elites. Specifically, we distinguish between early and late industrializing nations. For early industrializers, the industrialization process took place during the first (1760–1830) or second (1870–1913) waves. For late industrializers, however, large-scale industrialization did not typically take place until after World War II.

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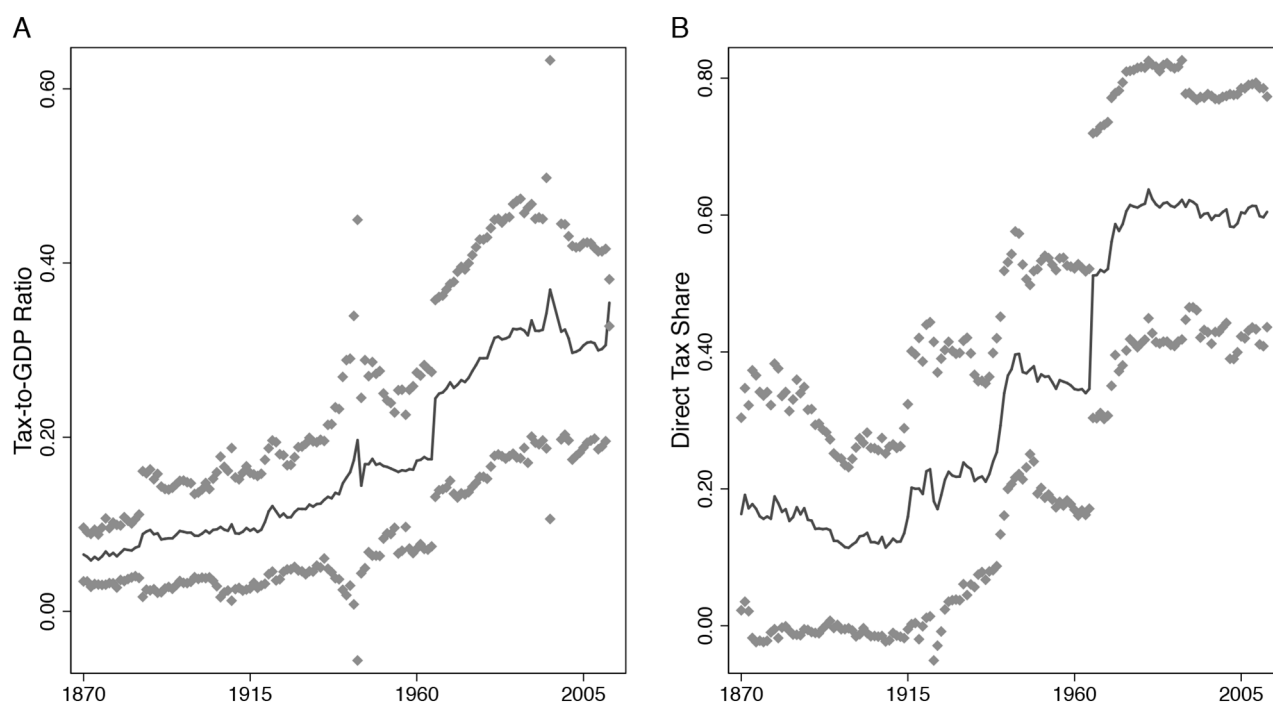


Figure 1. Fiscal development, 1870–2010. Solid line is mean value for full sample. Diamonds are standard deviations above and below the mean. See tables in sections 14 and 15 of the appendix for data sources and construction methods.

In the early industrializing context, the industrial sector typically threatened to “crowd out” the agricultural one. Thus, agricultural and capitalist elites were pitted against each other in a sort of zero-sum economic game. Agricultural elites were likely to lose from new public goods investments, which could increase the pace at which the economy shifted from agriculture to industry. Capitalist elites, by contrast, were likely to economically benefit from higher public goods provision.

To pay for new public goods, capitalist elites in this historical context would have most preferred to shift additional tax costs onto others. They were politically unable, however, to implement higher property taxes on agricultural elites. Similarly, higher consumption taxes (e.g., value-added taxation, or VAT) were still not economically or technologically viable at this time. Higher trade taxation, meanwhile, would (eventually) harm the industrial sector by curtailing access to international markets. Thus, capitalist elites in early industrializers were willing to shoulder a higher tax burden through progressive direct taxation on themselves, so long as the increase in industrial output due to higher public goods provision exceeded their new tax costs.

In the late industrializing context, by contrast, industrialization was often meant to support, rather than crowd out, rural development. Here, agricultural elites (along with nascent capitalist elites) hoped to mechanize agriculture to maintain their comparative advantage in international trade. Given that

their economic interests were rather narrow, however, the scope for new public good investments was likely to have been quite low. Furthermore, unlike most early industrializers, late industrializers could at times rely on foreign direct investments in public infrastructure. And, due to late timing, higher consumption taxes (e.g., VAT) were now viable, enabling agricultural elites to (partially) avoid shouldering a higher tax burden themselves through progressive direct taxation.

We argue that such initial decisions influenced fiscal development over the long run. Figure 2 shows descriptive evidence in support of this claim. This figure breaks down fiscal development by sample country from 1870 onward. Consistent with our argument, high tax progressivity tends to undergird high fiscal capacity in early industrializers such as the United Kingdom, France, and Germany. Similarly, as our argument would predict, high tax regressivity and low fiscal capacity appear to go hand in hand in late industrializers such as Brazil, India, and Turkey.

To test the predictions of our argument, we exploit an original fiscal database that spans 31 nations between 1870 and 2010. This database provides us with a novel perspective on long-run fiscal development across a broad swath of developed and developing nations. To construct it, we have integrated individual fiscal time series data from more than 30 secondary sources, including historical compilations, national statistical offices, and statistics from the International Monetary Fund,



Figure 2. Fiscal development by country, 1870–2010. See tables in sections 14 and 15 of the appendix for data sources and construction methods.

Organization for Economic Cooperation and Development, World Bank, and other such organizations.¹

Our empirical analysis proceeds in two parts. We first show descriptive evidence for a strong relationship between the timing of industrialization and historical levels of intra-elite competition. We find that competition between agricultural and capitalist elites tended to be high under early industrialization but low under late industrialization. We next turn to our main analysis about the relationship between historical intra-elite competition levels and long-run fiscal development. We show that this relationship is positive and statistically significant. For example, we find that greater intra-elite competition is associated with a 1%–3.3% increase in the overall tax take and a 1.5%–7.3% increase in the direct tax share. To put such magnitudes into perspective, average overall taxation for our sample was 20% of GDP over 1870–2010, while average tax progressivity was 39%. Thus, our estimates suggest that the increase in fiscal capacity associated with greater intra-elite competition was equivalent to up to

17% of actual overall taxation over this period, and up to 19% of actual tax progressivity.

We proceed as follows. The next section develops our argument. “Alternative Arguments” relates our argument to alternative arguments put forth in the literature, including interstate warfare, partisan control of government, economic modernization, and several others. “Empirical Analysis” presents the empirical strategy and main results, while the following section tests for robustness. The final section concludes.

CONCEPTUAL FRAMEWORK

We develop our argument in two parts. The first part characterizes, in basic terms, the fiscal decision-making process that historical elites in newly industrializing nations may have undertaken. The second part analyzes this decision-making process across two different historical contexts: early versus late industrializers. Our argument produces three predictions that will guide our empirical analysis.

Decision-making process

To help characterize the basic fiscal decision-making process by historical elites, we put forth a very simple formal model. Say that there are two types of elites: agricultural elites *A* and

1. The tables in sections 14 and 15 of the appendix describe the sources and construction methods for this database. We greatly thank Mauricio Prado for his help with data construction.

capitalist elites C . What distinguishes each type of elite is their sector-specific production skill. Agricultural elites specialize in agricultural production, while capitalist elites specialize in industrial production. Let the (initial) output of agricultural elites be y_A and that of capitalist elites be y_C .

Prior to industrialization, agricultural elites were typically the incumbent power holders in society (Ansell and Samuels 2014; Kuznets 1955). With industrialization, however, capitalist elites may have begun to challenge the political dominance of agricultural elites, implying the potential for greater intra-elite conflict over public policy (Boix 2011; Justman and Gradstein 1999; Moore 1966).

In the context of industrialization, historical elites must make two basic sequential decisions over fiscal development. First, they must decide whether to invest in greater fiscal capacity in order to fund a higher amount of public goods that may improve economic productivity. Second, if elites do in fact make such an investment, then they must decide how to allocate the new tax costs associated with it. In this context, therefore, it makes sense to conceptualize “intra-elite competition” as the extent to which the policy preferences of agricultural and capitalist elites over such investment and taxation decisions are at odds.²

With respect to the first decision, historical elites must choose whether to fund new public goods with the potential to translate into productivity gains in an industrializing economy (Congleton 2011; Lindert 2004; Lizzeri and Persico 2004; Pincus and Robinson 2011).³ For example, such public goods may include enhanced transportation infrastructure (e.g., railway networks) and/or urban sanitation (e.g., sewerage systems). Accordingly, let the output of capitalist elites—who as described above have a sector-specific skill in industrial production—increase to $\tilde{y}_C \geq y_C$ under this higher provision of public goods.

The implications of new public goods for the output of agricultural elites, by contrast, depends on how they affect the productivity of the agricultural sector relative to the industrial one. A traditional view holds that greater industrial production may “crowd out” agricultural production (Rostow 1959). This scenario may have been more common under early (vs. late) industrialization. In Britain, for example, new opportunities for industrial work reduced the labor supply available for agriculture (Allen 2009). To retain workers, agricultural elites had to increase wages, reducing profitability. Here, agricultural elites stand to lose (or at least benefit less) from new

public good investments, which will increase the pace at which the economy shifts from agriculture to industry (Congleton 2011; Kaldor 1963). In this case, let γ reflect the “production cost” of crowding out to agricultural elites, where $0 < \gamma \leq 1$. Alternatively, new public good investments may actually enhance the overall productivity of the agricultural sector (rather than crowd it out). In this scenario, let the output of agricultural elites increase to $\tilde{y}_A \geq y_A$ in response to new public goods. This case may have been more common for late industrializers (Kohli 2004). For example, railway investments in Argentina, Brazil, and Mexico spurred economic growth in their respective agricultural sectors (Haber 2005).

With respect to the second decision, if historical elites do in fact invest in new public goods, then they must choose how to allocate the new tax costs. Intuitively, the new tax amount that elites must pay should exceed the status quo amount, τ_L , which we may think of as a traditional property tax. While τ_L can cover minimal public goods such as national defense and basic infrastructure, it is not enough to cover the new sorts of public goods as described above. To cover the new tax costs, elites may rely on the following main options: trade taxation τ_R , indirect taxation τ_I , and/or progressive direct taxation τ_D .⁴ Note that both agricultural and capitalist elites alike have an incentive to shift new tax costs onto the other elite group if and when possible (Beramendi and Queralt 2014; Mares and Queralt 2015, 2017).

Optimal decisions under early industrialization

We now analyze the fiscal decision-making process by elites across two different historical contexts, starting with early industrializing nations. Figures A1 and A2 (figs. A1–A12 are available online in the appendix) illustrate this decision-making process and the payoffs for the agricultural and capitalist elites, respectively, for this historical context.

A traditional view claims that, at least for early industrializers, the industrial sector threatened to crowd out the agricultural one (Congleton 2011; Kaldor 1963; Rostow 1959). Thus, in this historical context, we may think of agricultural and capitalist elites as pitted against each other in a sort of zero-sum economic game. If new public goods exacerbated the crowding-out problem (e.g., by making industrial work more attractive relative to agriculture), then the agricultural sector may have been worse off in relative (and even absolute) terms. In this case, therefore, agricultural elites were less likely to favor new fiscal investments, because their payoff under the status quo exceeded that under any alternative scenario in which

2. Garfias (2018) makes use of the term “intra-elite competition” to analyze nascent state development in the context of an agricultural society. We discuss our paper relative to his in detail in “Alternative Arguments.”

3. For a theoretical account of this process, see Barro (1990).

4. We discuss two other potential options, higher property taxation and foreign direct investment, ahead.

taxation increased, regardless of the allocation of new tax costs. Formally,

$$y_A - \tau_L > \gamma \cdot y_A - \tau_{R,I,D}. \quad (1)$$

Capitalist elites, by contrast, were more likely to favor new investments in fiscal capacity, so long as the increase in industrial output due to higher public goods provision exceeded the new tax costs:

$$\begin{aligned} \tilde{y}_C - \tau_{R,I,D} > y_C - \tau_L \\ \Rightarrow \tilde{y}_C - y_C > \tau_{R,I,D} - \tau_L. \end{aligned} \quad (2)$$

How, then, to secure the additional tax revenue necessary to support the new public goods? Capitalist elites may have most preferred to implement higher taxes on immobile assets (e.g., land). However, they faced strong opposition from agricultural elites, the incumbent power holders in society. Mares and Queralt (2015, 2017), for example, show evidence that the introduction of the income tax was often made by traditional agricultural elites as a strategic move to shift tax costs onto new capitalist elites.

Indirect taxation on consumption τ_I was another potential option. Higher consumption taxation such as VAT, however, was not a viable way for early industrializers to cover new fiscal investments, since (1) for implementation, large-scale consumption taxes called for relatively modern technology, which was not yet available, and (2) for VAT to yield enough revenue, relatively high preexisting development levels were needed (Aidt and Jensen 2009). Historically, early industrializers only shifted toward VAT in the last quarter of the twentieth century, once progressive direct taxation had reached its limits as a plausible revenue source (Beramendi and Rueda 2007; Kato 2003).

Thus, even though capitalist elites would have most preferred to shift the new tax burden onto others, the main feasible options likely came down to higher trade taxation τ_R or progressive direct taxation τ_D . During nascent industrialization, domestic firms may in fact benefit from trade protection in terms of high tariffs, which allow them to grow (Krugman 1991; Reinert 2007). Once such firms begin to dominate national markets, and/or improve productivity enough to gain a comparative advantage internationally, however, support for trade liberalization may increase (Brambor and Lindvall 2014; Dixit 1985). Congleton (2011, 239–43), for example, shows that average tariff rates in Europe fell over the nineteenth century, as capitalist elites sought greater access to international markets for their products.⁵ Similarly, figure A4 plots the trade tax share

by sample country from 1870 onward. The trade tax share generally fell during early industrialization, which suggests that trade taxation cannot fully account for higher public goods provision in such cases.

Given the negative potential impact of higher trade taxation on the industrial sector, therefore, capitalist elites may have been willing to shoulder a higher tax burden themselves through progressive direct taxation. The specific political context of early industrialization may have reinforced this choice. Progressive direct taxation was originally adopted under restricted suffrage, in part under the expectation that tax rates would not increase beyond those favored by capitalist elites (Aidt and Jensen 2014; Beramendi and Queralt 2014). Though progressive direct taxation had important redistributive consequences over the twentieth century (Besley and Persson 2013), pre-World War I income tax rates were relatively low (Aidt and Jensen 2009; Seligman 1914).⁶

Overall, our argument suggests that we should observe positive relationships between early industrialization, the level of competition between agricultural and capitalist elites, and fiscal development, both in terms of overall taxation and the relative importance of tax progressivity. Furthermore, we may expect initial fiscal decisions to have influenced the ways in which policy makers dealt with subsequent fiscal demands in response to franchise extensions, the two world wars, and other major events. In this way, the legacy of fiscal decisions under early industrialization could endure over the long run.

Optimal decisions under late industrialization

The agricultural sector in developing nations traditionally held a comparative advantage in international trade (Baer 1972; Edwards 1993). Thus, there was typically less impetus for industrialization. Furthermore, labor costs stayed low, reducing the demand for labor-saving technological innovations (Allen 2009).

Eventually, however, industrialization may have begun to make economic sense. Agricultural elites (along with nascent capitalist elites) may have hoped to mechanize agriculture in order to maintain their comparative advantage (Haber 2005). This process was meant to support, rather than upend, rural development (Collier and Collier 2002; Hora 2002). Put differently, the goal of agricultural elites in this historical context was to organize the new industrial sector such that it served their core interests (Kohli 2004). To achieve this goal, agricultural elites were able to draw on their large political influence.

5. He cautions, however, that this downward trend was marked by an “ebb and flow of tariffs” and did not take place in one fell swoop (Congleton 2011, 241).

6. Scheve and Stasavage (2010, 2012), e.g., show evidence that class conflict over progressive direct taxation did not typically emerge until World War I and World War II.

In the late industrializing context, therefore, it makes sense to view new public good investments as a way to exploit economic complementarities between the agricultural and industrial sectors (see fig. A3), rather than as a sort of zero-sum game (as was the case for early industrializers). Thus, agricultural elites may have favored higher public goods provision, because they would increase agricultural output (Haber 2005; Hora 2002). Formally,

$$\tilde{y}_A - \tau_{R,I,D} > y_A - \tau_L. \quad (3)$$

Given that the economic interests of agricultural elites were quite narrow, however, the scope for investments in new public goods was likely to have been lower in the late-industrializing context than in the early industrializing one. Railway improvements in late-industrializing Argentina, for example, were made in a stark hub-and-spoke design, meant mainly to transport primary goods to Buenos Aires for export (Keeling 1993). Early industrializers Britain and Germany, by contrast, developed complex railway networks in order to transport workers, raw materials (e.g., coal), and intermediate goods throughout the country (Fremdling 1977). Similarly, late industrializers may have found it difficult to match the price and quality of core industrial producers (Baer 1972). In this way, the late timing of industrialization may have further reduced the incentive (at least at the margin) to invest in new public goods.

The financing options for new public good investments, moreover, were different for late (vs. early) industrializers. Our simple model emphasizes how historical elites may have financed new public goods through higher taxation. In several cases, however, late industrializers received foreign direct investments in public infrastructure by core industrialized nations. The British, for example, made extensive investments in docks and ports, electrical power, and railways in Latin America (Stone 1977). Importantly, the provision of such public goods did not entail higher taxation by the governments in late-industrializing nations themselves.

The political logic of late industrialization influenced not only the state's decision over the amount of new public goods to invest in but also how to structure any new taxation to fund them. As for early industrializers, higher trade taxation τ_R may have been attractive early on to protect the nascent industrial sector. The ability to make new sectors competitive took longer for late industrializers, because they had to make up for efficiency deficits against core industrialized nations. Most late industrializers, however, did not have large enough domestic markets to support a thriving industrial sector. For this reason, they often shifted to export-oriented production, eventually reducing trade taxes (Haggard 1990). Given the late timing,

higher indirect taxation on consumption τ_b , including VAT, became a viable way for agricultural elites to help recover lost revenue from trade taxes (Ha and Rogers 2017; Wibbels and Arce 2003) and to help pay for new public goods. The VAT, moreover, enabled agricultural elites in late industrializers to avoid—at least in part—shouldering a higher tax burden themselves through progressive direct taxation.

Relative to the early industrializing context, therefore, our argument suggests that optimal fiscal decision making should have looked quite different under late industrialization. There should have been less competition between agricultural and capitalist elites. Though fiscal development may have taken place, overall taxation should have stayed relatively low and should have been relatively regressive. Low initial investments in fiscal capacity, moreover, may have made subsequent fiscal investments more difficult, thereby helping cement the legacy of fiscal decisions undertaken during late industrialization (Queralt 2015).

Predictions

Our argument produces one ancillary and two main empirical predictions.

- A. Early industrialization should have promoted a higher level of competition between agricultural and capitalist elites. Intra-elite competition should have remained relatively low, however, under late industrialization. We view this as an ancillary prediction that helps us set up the following two main predictions.
 1. Greater intra-elite competition between agricultural and capitalist elites should lead to an increase in the overall level of fiscal capacity (size).
 2. Greater intra-elite competition between agricultural and capitalist elites should lead to an increase in tax progressivity (structure).

ALTERNATIVE ARGUMENTS

Before proceeding to our empirical analysis, we now relate our argument to several alternative arguments that are present in the literature. This discussion also helps motivate the different controls that our empirical analysis will employ.

Interstate warfare

Interstate military competition and warfare is one prominent explanation for fiscal development (e.g., Besley and Persson 2009; Dincecco and Prado 2012; Downing 1992; Gennaioli and Voth 2015; Mann 1986; Tilly 1975). To finance military efforts, a state may undertake administrative reforms that

strengthen the overall tax system (Tilly 1975). Similarly, to promote equal burden-sharing in wartime, a state may enact progressive direct taxation on elites that are unlikely to be conscripted for battle (Scheve and Stasavage 2010, 2012).

We view our argument as complementary to those that highlight warfare. While this literature emphasizes international factors that may influence fiscal development, we focus on a wholly domestic factor: inter-elite competition. This focus helps us explain differences in fiscal development between states that did not (frequently) mobilize for major wars. For example, both Spain and Sweden were neutral in World Wars I and II, yet fiscal development today differs between them. While high overall taxation and tax progressivity characterizes Sweden, fiscal development in Spain still lags behind much of Europe (see fig. 2). Similarly, non-European nations such as Argentina and Chile did not mobilize for either world war. Yet there is a significant divergence in long-run fiscal development among them (Bergman 2003). Nonetheless, our empirical analysis will control for war participation.

A related type of argument is known as the fiscal contract view of fiscal development (e.g., Bates and Lien 1985; Besley and Persson 2013; Levi 1988). To raise new funds (and thus finance military efforts), an autocratic ruler may surrender (partial) political control. In turn, it may become more likely that some of the new funds will be spent on items that will directly benefit elites, making them more willing to agree to higher taxation in the first place. Our empirical analysis will account for broad political development trends in a variety of ways (e.g., year fixed effects, region-specific time trends) and will explicitly control for democracy levels.

Leftist control of government

The partisan orientation of incumbent politicians is another well-known explanation for fiscal policy outcomes (e.g., Hibbs 1977; Huber and Stephens 2001). Left-wing parties tailor public policy toward the working class. Thus, they are more likely than right-wing parties to increase both the overall level and progressivity of taxation when in office, in order to fund redistributive public goods that benefit their working-class base. In our view, this argument is quite plausible. Still, there may be political constraints that limit the ability of left-wing parties in developed nations to enact progressive tax reforms (Beramendi and Rueda 2007; Przeworski and Wallerstein 1988). Furthermore, this argument may have been more relevant for early industrializers than for late industrializers, as partisan competition itself may be thought of as a “luxury good” that is only typically found in established democracies. At any rate, we will control for the partisan orientation of government in our empirical analysis.

A related argument highlights the interactive effect of democratization and urbanization on fiscal development (Andersson 2017). According to this view, fiscal policy depends on whether the voting franchise is extended to the urban or rural poor. While the urban poor strictly prefer to shift the tax burden from consumption onto property and income, the preferences of the rural poor are less clear-cut. To account for this argument, our empirical analysis will control for urbanization, democracy, and the interaction effect between them.

Economic modernization

A third prominent argument links the overall level and progressivity of taxation to economic development. If most citizens are poor, then high taxation may not be feasible (Becker and Mulligan 2003). Similarly, the state may lack the bureaucratic capacity to administer sophisticated forms of taxation (e.g., a progressive direct tax). According to this logic, economic development will make fiscal change more likely, regardless of other international or domestic factors. Our argument, by contrast, suggests that fiscal outcomes may still differ across nations at similar levels of economic development, depending on the expected benefits (and costs) of new public goods, and the specific tax revenue environment (e.g., whether VAT was technologically viable). Still, our empirical analysis will account for past economic development levels in several ways (e.g., year fixed effects, region-specific time trends, lagged dependent variable) and will explicitly control for per capita GDP.

Other alternatives

Finally, the political economy literature highlights several other factors that may influence fiscal policy. First, landholding inequality may affect whether capitalist elites play a role in government policy making (Albertus 2015; Albertus and Menaldo 2014; Ansell and Samuels 2014). Namely, high landholding inequality may imply a well-organized agricultural sector that can effectively fend off political demands by capitalist elites. Second, trade openness may influence fiscal development. For example, the government may expand in size in order to provide social insurance and reduce the risks of negative trade shocks (Rodrick 1998). Similarly, abundant natural resources may generate nontax revenue that enables governments to provide public goods without increasing extractive capacity via higher taxation (Ross 1999). Third, fractionalization along ethnic, linguistic, or religious lines may influence society’s preferences over public goods provision (Alesina et al. 2003). In our view, each of the above factors is a plausible determinant of fiscal development. For the most part, however, they do not explicitly speak to the

fiscal role of intra-elite competition between agricultural and capitalist elites. Still, we will control for each factor above in our empirical analysis.

Two recent contributions that do in fact analyze the fiscal consequences of intra-elite competition are Mares and Queralt (2015, 2017). Mares and Queralt argue that traditional agricultural (i.e., landed) elites favored the introduction of the income tax in order to shift the direct tax burden away from themselves and onto new capitalist elites in early industrializing nations. In this way, traditional agricultural elites attempted to delay economic and political change that would be detrimental to their interests.

We view our argument as complementary to that of Mares and Queralt. Both arguments emphasize the fiscal implications of intra-elite competition. Still, there are importance differences across the two approaches. Mares and Queralt focus on the political calculus that drove the historical introduction of the income tax in the developed world. Our paper, by contrast, analyzes the long-run development of the size and structure of fiscal states between 1870 and today across both developed and developing nations alike. To this end, we have constructed a large, original fiscal database. Furthermore, our paper links the timing of industrialization to historical levels of intra-elite competition, which in turn helps shape key initial decisions over fiscal size and structure. Unlike Mares and Queralt, this approach enables us to generate diverse empirical predictions for early versus late industrializers. Mares and Queralt focus on early industrializers only. In the late-industrializing context, traditional agricultural elites were often very powerful. We show, however, that high tax regressivity and low fiscal capacity, rather than progressive direct taxation, tend to characterize late industrializers (see fig. 2). Our argument, which highlights the relatively low level of intra-elite competition under late industrialization, helps explain this fiscal outcome. By making the timing of industrialization central to our argument, therefore, our approach offers new insights into the relationship between intra-elite competition levels and long-run fiscal development.

Garfias (2018) is another recent contribution about the fiscal consequences of intra-elite conflict. Garfias analyzes the effects of Great Depression-era commodity price shocks on state development in Mexico. He argues that negative price shocks reduced the economic power of traditional elites, providing political actors with a unique window of opportunity to increase the state's local presence.

Though both our paper and that of Garfias make use of the term “intra-elite competition,” we each apply it to very different phases of the state development process. Garfias highlights the basic conflict between state actors and economic elites in an agricultural society. Here, greater fiscal capacity

results from the state's ability to consolidate political power away from traditional elites in response to negative economic shocks. In our paper, by contrast, the underlying property rights environment is already secure, and the conflict of interest is between agricultural and capitalist elites (i.e., rather than between state actors and agricultural elites as in Garfias). For us, intra-elite competition concerns the extent to which agricultural-capitalist policy preferences over public investment and taxation decisions diverge. This sort of conflict is most likely to manifest itself in newly industrializing nations—a conceptual point in time that only takes place after the state was able to resolve the basic conflict present in Garfias. By analyzing intra-elite competition in the context of industrialization, we provide new insights into a different—yet pivotal—stage of the state development process.

EMPIRICAL ANALYSIS

Our empirical analysis proceeds in two parts. Recall from the section “Conceptual Framework” that our argument produces one ancillary and two main empirical predictions. We first turn to the ancillary prediction. Given space constraints, we focus on descriptive evidence to provide support for this prediction. We then turn our attention to a rigorous econometric analysis of the two main predictions.

Ancillary prediction

Our argument suggests that, to an important extent, historical levels of political competition between agricultural and capitalist elites reflect the timing of industrialization. Under early industrialization, it was more likely that capitalist elites would be pitted against agricultural elites in a sort of zero-sum economic game, promoting greater intra-elite competition. Under late industrialization, by contrast, agricultural elites were more likely to retain their traditional dominance, reducing inter-elite competition.

We now show descriptive evidence that is consistent with this ancillary prediction. Ideally, we would like a standardized measure of competition between agricultural and capitalist elites across our sample of developed and developing nations from the late nineteenth century to the present. In practice, however, such a measure is not available. Fortunately, we do have two different types of proxy data that, when combined, will help us evaluate whether our ancillary prediction holds water.

The first type of data concerns the timing of industrialization, which we measure in several ways. First, we take the historical shares of employment in agriculture and industry, respectively, from Banks and Wilson (2015). Similarly, we take the share of agricultural activity in GDP (Banks and Wilson 2015). Finally, we take a historical measure of occu-

pational diversity in society from Vanhanen (2005). The rationale for each of the above variables is that political competition between agricultural and capitalist elites should reveal itself in terms of a growing nonagricultural sector.

The second type of data concerns intra-elite political competition, which we measure in two ways: executive recruitment and political contestation. We focus on these variables for both conceptual and practical reasons. As described in the conceptual framework, intra-elite conflict in the context of industrialization centers on the extent to which agricultural-capitalist policy preferences over public investment and taxation decisions are at odds. Policy disagreements between agricultural and capitalist elites should therefore manifest themselves in terms of the amount of jockeying over the choice of political leaders. In this respect, both variables are intuitively linked with the basic logic of our argument. The second reason is practical, given the general paucity of quantitative historical data. Namely, both variables are systematically available across our sample of developed and developing nations from 1870 onward. To construct the executive recruitment variable, we rely on Marshall, Gurr, and Jagers (2013), who provide data for three components related to the regulation, competitiveness, and openness of the recruitment process.⁷ Scholars have shown that executive recruitment is an accurate reflection of political competition levels (Coppedge, Alvarez, and Maldonado 2008; Gates et al. 2006). We sum the scores over each component by country and year. Next, we compute the running total for each year over 1870–2010. Finally, we scale this total by the total number of observations over this period, which may differ by country.⁸ We take the political contestation variable from Miller (2015). This variable employs a principal-components analysis over several features of political contestation, including whether there is an independent political opposition, the extent of electoral competition, the presence of intragovernmental constraints, and the closeness of electoral outcomes.⁹

If our ancillary prediction holds water, then we should observe close relationships between the two types of data described above. Capitalist elites were more likely to be pitted against agricultural elites in a sort of zero-sum economic

game under early (vs. late) industrialization. This type of relationship should manifest itself in terms of a positive correlation between (1) the industrial employment share or occupational diversity and (2) intra-elite competition levels. Reciprocally, we should observe a negative relationship between (1) the agricultural employment share or agricultural share of GDP and (2) intra-elite competition levels.

Figure 3 plots the average values of the above variables over 1870–2010 for each sample country against the average value of executive recruitment, our first measure of intra-elite competition. Consistent with our ancillary prediction, there is a strongly positive correlation between the sectoral importance of industry and the level of intra-elite competition. As our argument would predict, moreover, the relationship between the sectoral importance of agriculture and the level of intra-elite competition is strongly negative. Figure 4 depicts similar relationships for political contestation, our second measure of intra-elite competition.

Overall, this descriptive evidence provides support for our ancillary prediction that there is a strong relationship between the timing of industrialization and historical levels of intra-elite competition.¹⁰ Under early industrialization, capitalist elites were more likely to be pitted against agricultural elites in a sort of zero-sum economic game. In this historical context, intra-elite competition tended to be relatively high. Agricultural elites, by contrast, were more likely to retain their traditional dominance under late industrialization. Intra-elite competition tended to be low in this historical context.

Finally, to provide another form of evidence in support of our ancillary prediction, we draw on Beramendi and Queralt (2014), who analyze the relationships between party organizations, the extent of the franchise, and fiscal development across 10 historical democratizing regimes in Europe. They show corollary evidence that, when the Liberals (who represented new capitalist interests) gained power in parliament relative to the Conservatives (who represented traditional agricultural interests), the size of the fiscal state increased (as measured by the tax-to-GDP ratio). This evidence is consistent with the basic logic of our argument, which claims that the historical emergence of capitalist elites as competitors to agricultural elites would manifest itself in terms of diverse policy outcomes.

7. The regulation variable is scored on a 1–3 scale, the competitiveness variable on a 0–3 scale, and the openness variable on a 0–4 scale. We exclude –66 (“interruption”) and –88 values (“transition”).

8. For example, there are four missing observations for Argentina over 1870–2010. Thus, we scale Argentina’s running total for each year by 140 – 4 (where 140 is the maximum number of observations if none are missing).

9. As for our main intra-elite competition measure, we compute the running total of the political contestation scores for each available year over 1870–2010 for each country, which we then scale by the total number of observations.

10. The panel regression analysis in table A2 provides additional support for this prediction. Namely, there are correctly signed and statistically significant relationships between sectoral importance and intra-elite competition levels for stringent regressions that include country and period fixed effects, region-specific time trends, and the lagged dependent variable.

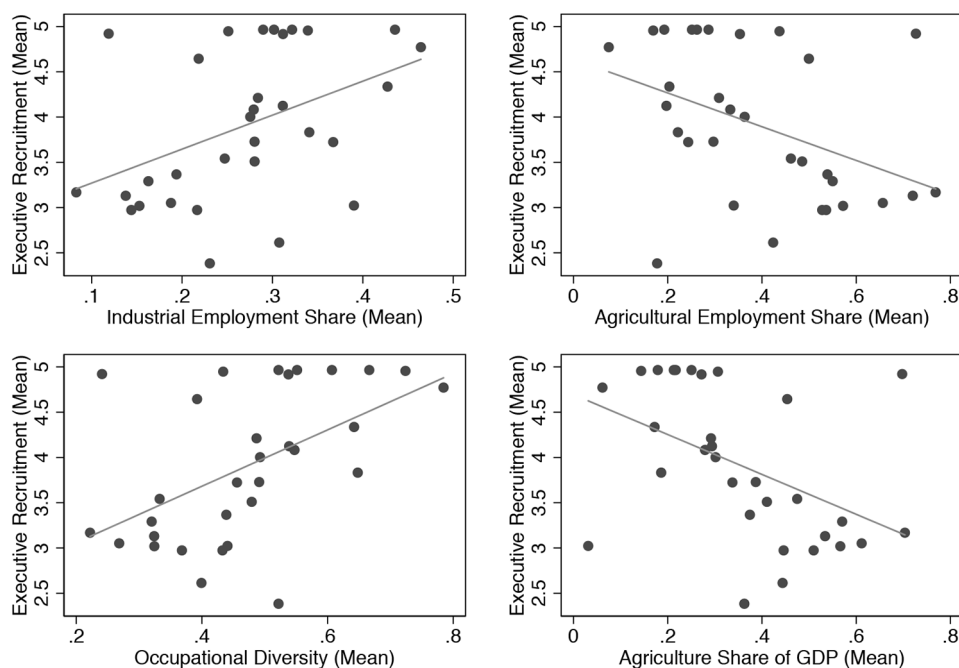


Figure 3. Sectoral importance and executive recruitment, 1870–2010. Data are averaged over 1870–2010. See text for data sources and construction methods.

Main predictions

The previous subsection shows descriptive evidence in support of our ancillary prediction, which helps us set up the two main predictions of our argument. To test them, we now turn to a rigorous econometric analysis.

Specifically, we use ordinary least squares (OLS) to estimate:

$$F_{i,t} = \alpha + \beta E_{i,t-1} + \mu_i + \lambda_t + \gamma \mathbf{X}_{i,t-1} + \epsilon_{i,t}, \quad (4)$$

where i indexes each country and t indexes each period. The term $F_{i,t}$ is one of two fiscal development outcomes to be described ahead. The term $E_{i,t-1}$ is one of the two measures of intra-elite competition as described in the previous subsection. The terms μ_i and λ_t are country and period fixed effects, respectively. The term $\mathbf{X}_{i,t-1}$ is a vector of controls for time-varying observable characteristics to be described ahead. The term $\epsilon_{i,t}$ is a random error term. All standard errors are robust, clustered at the country level to account for any within-country serial correlation in the error term. Table A1 (tables A1–A26 are available online in the appendix) presents the descriptive statistics for the regression variables.

To measure fiscal development $F_{i,t}$, we rely on our original historical panel database. Recall from the conceptual framework that our argument has implications for both the overall level of fiscal capacity and tax progressivity. To measure overall fiscal capacity, we compute the ratio of total

tax revenues to GDP.¹¹ To measure tax progressivity, we compute the share of direct taxation in total tax revenues (where direct taxation includes income taxation, payroll taxation, property taxation, and social security).

The vector $\mathbf{X}_{i,t-1}$ includes time-varying controls for interstate warfare, partisan control of government, and per capita income. Such controls help proxy for the main alternative arguments as described in the section “Alternative Arguments.” To account for the potential role of warfare, we follow Scheve and Stasavage (2012) and include a binary variable that equals one for each year that a country participated in an interstate war and at least 2% of the population was serving in the military.¹² To account for the potential role of partisanship, we include a binary variable that equals one for each year that a country has a leftist head of government according to Brambor, Lindvall, and Stjernquist (2013).¹³ Fi-

11. We exclude four observations from our analysis for which the tax-to-GDP ratio is greater than one: 1944 for Japan and 1996–98 for Turkey. The main regression results remain robust, however, if these observations are included.

12. A main virtue of this Scheve-Stasavage-style variable is that it helps distinguish between the magnitudes of different wars, as large-scale conflicts (e.g., World Wars I and II) are more likely to be coded 1 than small-scale ones (i.e., due to the mobilization condition). Still, our results remain robust if we code warfare in other ways (e.g., a binary variable that equals 1 for each year that a country participated in an interstate war).

13. Specifically, this variable equals 1 if the variable *hogideo* takes the value “L.”

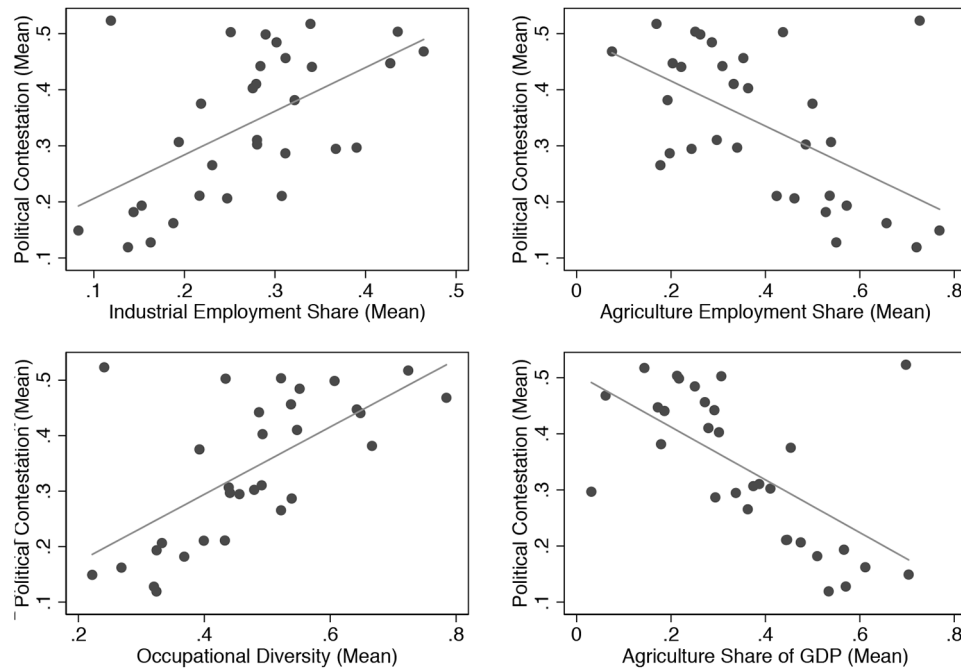


Figure 4. Sectoral importance and political contestation, 1870–2010. Data are averaged over 1870–2010. See text for data sources and construction methods.

nally, to account for the possibility that the overall level and progressivity of taxation may depend on a country’s level of economic development, we include real capita GDP (in 1990 Geary-Khamis dollars) from Maddison (2013). Note that the time-varying controls for interstate warfare, partisan control of government, and per capita income are “bad controls” (Angrist and Pischke 2009) in the sense that they themselves could (at least in part) be outcomes of intra-elite competition. For this reason, we will typically show the results both without and with them.

Our empirical strategy accounts for unobservable characteristics that may affect both fiscal development and intra-elite competition alike. Country fixed effects help control for initial conditions (i.e., economic, demographic, political, social) and country-level features that are fixed over time such as geography. Period fixed effects help control for global shocks. Still, methodological concerns may remain.

Omitted variable bias is one potential concern. As described, fixed effects help account for time-invariant country characteristics and global shocks. However, unobserved time-varying factors may still affect our results. We address this concern in several ways. First, we modify our fixed effects model to include region-specific time trends, which help control for unobservable regional factors that vary over time, including demographic, economic, political, fiscal, and urbanization trends.¹⁴ Second, we include the lagged dependent

14. We include region-specific time trends for six regions: Asia, Europe, the Middle East, North America, Oceania, and South America.

variable $F_{i,t-1}$,¹⁵ which helps control for a country’s most recent level of fiscal development.¹⁶ Third, we account for a wide range of additional time-varying observables beyond the benchmark controls in $\mathbf{X}_{i,t-1}$.

Reverse causation is another potential concern, because fiscal development levels may affect intra-elite competition itself. We address this concern as follows. First, region-specific time trends control for fiscal trends at the regional level. Second, the lagged dependent variable controls for the most recent level of fiscal development for each nation. Third, we perform Granger-style causality tests.

Finally, our argument suggests that the influence of intra-elite competition on fiscal development may not be immediate. We thus focus our main analysis on five-year averaged data. Still, as we will show, the main results are robust to yearly and 10-year averaged data.

Main results

Table 1 presents the estimation results for the relationship between intra-elite competition and overall taxation, our

15. Including the lagged dependent variable creates Nickell bias (Nickell 1981). However, this bias decreases with the panel’s time dimension T . For our unbalanced panel with yearly observations, T ranges from 30 to 122, with an average value of 87. Thus, Nickell bias should be relatively small.

16. Furthermore, to account for scale effects (Kenny and Winer 2006), we always include the lagged tax-to-GDP ratio in the stringent specification when our outcome variable is tax progressivity.

Table 1. Elite Competition and Overall Taxation, 1870–2010: Main Results

	(1)	(2)	(3)	(4)
Executive recruitment _{<i>t</i>-1}	.033** (.014) [.023]	.009*** (.003) [.006]		
Political contestation _{<i>t</i>-1}			.283*** (.078) [.001]	.079*** (.024) [.003]
War mobilization _{<i>t</i>-1}		.019 (.024) [.436]		.021 (.025) [.401]
Left government _{<i>t</i>-1}		.008 (.006) [.182]		.007 (.006) [.253]
ln(per capita GDP) _{<i>t</i>-1}		.015 (.010) [.150]		.016 (.011) [.157]
Tax-to-GDP ratio _{<i>t</i>-1}		.710*** (.046) [.000]		.695*** (.050) [.000]
Country fixed effects	Yes	Yes	Yes	Yes
Period fixed effects	Yes	Yes	Yes	Yes
Region trends	No	Yes	No	Yes
Controls	No	Yes	No	Yes
R-squared	.732	.910	.748	.911
Observations	682	658	682	658
Number of countries	31	31	31	31

Note. Dependent variable: tax-to-GDP ratio. Estimation method is OLS with five-year averaged data. All regressions include country and period fixed effects. Robust standard errors clustered at country level in parentheses, followed by corresponding *p*-values in brackets.

* *p* < .10.

** *p* < .05.

*** *p* < .01.

first measure of fiscal development. Columns 1 and 3 show the results for the parsimonious specification that includes country and period fixed effects, respectively, for each of our two measures of intra-elite competition. There is a highly significant relationship between intra-elite competition and overall taxation. The coefficient estimate for $E_{i,t-1}$ is 0.033 for the executive recruitment variable and 0.283 for the political contestation one.

The stringent specifications in columns 2 and 4, respectively, include region-specific time trends, the lagged dependent variable, and the time-varying controls. Relative to the parsimonious specifications, the coefficient estimates for $E_{i,t-1}$ are smaller in magnitude but remain highly significant. Con-

sistent with the main arguments in the literature, the coefficient estimates for warfare, leftist government, and per capita income are all positively signed.¹⁷

Table 2 presents the estimation results for tax progressivity, our second measure of fiscal development. Columns 1–4 repeat the parsimonious and stringent specifications from the previous table. There is a highly significant relationship between intra-elite competition and tax progressivity across all four specifications. The coefficient estimates for $E_{i,t-1}$ range between 0.015 and 0.073 for the executive recruitment variable, and between 0.082 and 0.419 for the political contestation one.

Overall, the results in tables 1 and 2 support the argument that greater intra-elite competition leads to long-run fiscal development. There is a robust relationship between intra-elite competition and both overall taxation and tax progressivity. For example, the estimates in table 1 indicate that a one-point increase in executive recruitment was associated with a 1%–3.3% increase in the overall tax take (relative to GDP). Such magnitudes are relatively large. Average taxation for our sample was 20% of GDP over 1870–2010. Thus, our estimates indicate that the increase in taxation associated with greater intra-elite competition was equivalent to 5%–17% of actual overall taxation over this period. Similarly, the estimates in table 1 indicate that a one-point increase in executive recruitment was associated with a 1.5%–7.3% increase in the share of direct taxation, which translates into 3.8%–19% of actual tax progressivity over this period.

ROBUSTNESS

The main results support our argument that intra-elite competition has positive consequences for long-run fiscal development, both in terms of overall capacity (size) and tax progressivity (structure). In this section, we test the robustness of these results in a wide variety of ways. Given space constraints, we restrict our discussion of the robustness analysis to our first measure of intra-elite competition $E_{i,t-1}$ (namely, executive recruitment).¹⁸

Subsample analysis

Our main analysis accounts for time-invariant and time-varying heterogeneity through fixed effects by country and time, region-specific time trends, and a standard battery of

17. Warfare becomes significant for the yearly data (table A4), while leftist government becomes significant for the 10-year averaged data (table A6).

18. The results of this robustness analysis for our second measure of intra-elite competition, political contestation, are very similar overall in terms of sign and statistical significance (as shown in section 13 of the appendix).

Table 2. Elite Competition and Tax Progressivity, 1870–2010: Main Results

	(1)	(2)	(3)	(4)
Executive				
recruitment _{<i>t</i>-1}	.073*** (.018) [.000]	.015*** (.005) [.002]		
Political				
contestation _{<i>t</i>-1}			.419*** (.102) [.000]	.082* (.042) [.060]
War mobilization _{<i>t</i>-1}		-.004 (.036) [.911]		-.004 (.036) [.909]
Left government _{<i>t</i>-1}		.004 (.009) [.680]		.002 (.009) [.794]
ln(per capita GDP) _{<i>t</i>-1}		.071*** (.023) [.004]		.070*** (.022) [.004]
Tax-to-GDP ratio _{<i>t</i>-1}		-.066 (.042) [.126]		-.071 (.045) [.123]
Direct tax share _{<i>t</i>-1}		.709*** (.037) [.000]		.714*** (.036) [.000]
Country fixed effects	Yes	Yes	Yes	Yes
Period fixed effects	Yes	Yes	Yes	Yes
Region trends	No	Yes	No	Yes
Controls	No	Yes	No	Yes
R-squared	.790	.933	.786	.933
Observations	682	658	682	658
Number of countries	31	31	31	31

Note. Dependent variable: direct tax share. Estimation method is OLS with five-year averaged data. All regressions include country and period fixed effects. Robust standard errors clustered at country level in parentheses, followed by corresponding *p*-values in brackets.

* *p* < .10.

** *p* < .05.

*** *p* < .01.

country-level controls. Still, we can perform additional tests for heterogeneity across place and time.

To determine whether any specific nation drives our results, we exclude each of them one by one. Figure A5 shows the results of this test for overall taxation, while figure A6 shows them for tax progressivity. Both figures rely on the stringent specification. For overall taxation, the coefficient estimates for $E_{i,t-1}$ range from 0.009 to 0.012, with *p*-values that range from .002 to .054 (of which 29 of 31 *p*-values are less than .010). For tax progressivity, the coefficient estimates

for $E_{i,t-1}$ range from 0.013 to 0.018, with *p*-values that range from .000 to .025 (of which 29 of 31 *p*-values are less than .010). Thus, excluding nations one by one does not alter the main results by much.

Similarly, figure A7 presents the results when we exclude world regions one by one. The coefficient estimates for $E_{i,t-1}$ are relatively stable, and are always significant.

To further test for heterogeneity across time, figure A8 shows the results for the stringent specification when we exclude 30-year periods (i.e., “generations”) one by one. The coefficient estimates for $E_{i,t-1}$ are very stable and, again, are always significant. Thus, no single generation appears to drive our results.

Finally, table A3 presents the results for the stringent specification when we exclude “severe” outlier observations, defined as those with residuals more than three times greater than the standard deviation. The coefficient estimates for $E_{i,t-1}$ are generally similar in magnitude and significant to the main results.

Overall, these tests provide additional evidence that our results are quite robust across place and time.

Alternative data averages

Given that the influence of intra-elite competition on fiscal development may not be immediate, we focus our main analysis on five-year averaged data. To show that our results do not depend on this particular averaging strategy, Table A4 repeats the main analysis for yearly data, while table A5 repeats it for 10-year averaged data. The coefficient estimates for $E_{i,t-1}$ remain significant across all specifications (all *p*-values are less than or equal to .050). The magnitudes for $E_{i,t-1}$ are relatively similar between the five- and 10-year averaged data, and are somewhat similar between the yearly and five-year averaged data. In the latter case, the inclusion of the lagged dependent variable $F_{i,t-1}$ reduces the size of the coefficient estimates for $E_{i,t-1}$ for the yearly data.

Error correction models

The error correction model is an alternative modeling technique to our main empirical strategy. Table A6 presents the results for both the parsimonious and the stringent specification for this technique, which takes $\Delta F_{i,t}$ as the outcome variable and includes $\Delta E_{i,t-1}$, along with the changes in the benchmark time-varying covariates, $\Delta \mathbf{X}_{i,t-1}$, as additional controls. The coefficient estimates for our variable of interest $E_{i,t-1}$ remain positive and significant.

Matching

As another empirical technique, we make use of matching methods. Namely, we weight each sample observation by its match with the following treated variables (as described

previously): interstate warfare, partisan control of government, per capita income, and the urbanization rate.¹⁹

Tables A7 and A8 show the results for the stringent specification under matching. The coefficient estimates for $E_{i,t-1}$ are positive and significant across all generated samples of the treated variables. Furthermore, alternative propensity score matching techniques (i.e., kernel, nearest neighbor, radius) deliver similar results.

Additional controls

The main results are robust to the inclusion of three standard controls for time-varying observable characteristics (i.e., interstate warfare, partisan control of government, per capita income). We now show that our results are robust to a variety of other time-varying controls that the political economy literature highlights (as described in the section “Alternative Arguments”). They are landholding inequality, trade openness, natural resource dependence, the urbanization rate, democracy levels, and social identity.²⁰ To measure landholding inequality, we take the number of family-owned farms from Vanhanen (2005). To measure trade openness, we take log per capita exports from Banks and Wilson (2015). To measure natural resource dependence, we take revenues from oil, gas, coal, and metals as a share of GDP from Haber and Menaldo (2011). To measure urbanization, we take the urbanization rate from Miller (2015). To measure democracy, we take democracy levels from Boix et al. (2013; as reported by Miller 2015). To measure social identity, we take the variables for ethnic, language, and religious fractionalization from Alesina et al. (2003).

Tables A9 and A10 show the results of this analysis for the stringent specification. For each fiscal development outcome, columns 1–5 include each of the following additional controls—landholding inequality, trade openness, natural resource dependence, the urbanization rate, and democracy levels—one by one. The coefficient estimates for $E_{i,t-1}$ are always positive and significant. With respect to the new controls, the coefficient estimates for trade openness are also

19. We estimate weights according to the `psmatch2` command in Stata (full Mahalanobis matching). To use this command, we first transformed the continuous treated variables into binary measures equal to 1 for values greater than or equal to the median sample values. Furthermore, for the matching exercise, we used the war mobilization variable in Scheve and Stasavage (2010) rather than the (slight) variant described in the subsection “Main Predictions.” Otherwise, there were too few observations to exploit.

20. As for the benchmark controls in $\mathbf{X}_{i,t-1}$, the additional controls are “bad controls” (Angrist and Pischke 2009) in the sense that they themselves may be outcomes of intra-elite competition. In fact, trade openness (i.e., tariff policy) is a decision variable in our model in “Conceptual Framework.” For this reason, we interpret the results in this subsection with caution. Nonetheless, we believe that it is useful to show that our main results are robust to the inclusion of such controls.

positive and significant for both overall taxation and tax progressivity, while landholding inequality and the urbanization rate are significant for the former outcome. In column 6, we explicitly account for Andersson (2017), who argues that long-run fiscal development depends on whether the voting franchise is extended to the urban (i.e., vs. rural) poor. We mimic his empirical strategy by interacting the urbanization rate with the level of democracy. The coefficient estimates for $E_{i,t-1}$ remain positive and significant. Furthermore, the coefficient on the urbanization-democracy interaction effect (i.e., Andersson’s variable of interest) is also significant for both overall taxation and tax progressivity.

Finally, table A11 reports the results for the stringent specification when we control for ethnic, linguistic, and religious fractionalization, respectively. To make each fractionalization variable time-variant, we interact them with period fixed effects (otherwise, country fixed effects will subsume them). The coefficient estimates for $E_{i,t-1}$ remain very similar in magnitude and significance to the main results.

Granger-style causality tests

Fiscal development levels may affect intra-elite competition itself. To address this concern, our main analysis controls for (1) initial fiscal development levels through country fixed effects, (2) fiscal trends through region-specific time trends, and (3) previous fiscal development levels through the lagged dependent variable. To further test for reverse causation, we now perform Granger-style causality tests (Angrist and Pischke 2009).

Our main results indicate that there is a significant relationship that runs from intra-elite competition to fiscal development. If $E_{i,t-1}$ affects $F_{i,t}$ but not vice versa, then lags of $E_{i,t-\tau}$, $\tau = 1, \dots, q$ should significantly predict $F_{i,t}$ when lags of $F_{i,t-\tau}$, $\tau = 1, \dots, q$ and $F_{i,t-\tau}$, $\tau = 1, \dots, q$ are simultaneously included in equation (5).

$$F_{i,t} = \alpha + \sum_{\tau=1}^q \beta_{1,\tau} E_{i,t-\tau} + \sum_{\tau=1}^q \beta_{1,\tau} F_{i,t-\tau} + \mu_i + \lambda_t + \gamma' \mathbf{X}_{i,t-1} + \epsilon_{i,t}. \quad (5)$$

Reciprocally, when lags of $E_{i,t-\tau}$, $\tau = 1, \dots, q$ and $F_{i,t-\tau}$, $\tau = 1, \dots, q$ are included in equation (6), $F_{i,t-\tau}$, $\tau = 1, \dots, q$ should not significantly predict intra-elite competition.

$$E_{i,t} = \alpha + \sum_{\tau=1}^q \beta_{1,\tau} E_{i,t-\tau} + \sum_{\tau=1}^q \beta_{1,\tau} F_{i,t-\tau} + \mu_i + \lambda_t + \gamma' \mathbf{X}_{i,t-1} + \epsilon_{i,t}. \quad (6)$$

Table A12 presents the results for the Granger-style causality tests. F -tests indicate that $E_{i,t-\tau}$, $\tau = 1, \dots, q$ are signifi-

cant predictors for both overall taxation and tax progressivity across several lag values: 3, 10, and 15. By contrast, F -tests indicate that $F_{i,t-\tau}$, $\tau = 1, \dots, q$ are not significant predictors of intra-elite competition across the same range of lag values. This analysis suggests that intra-elite competition “Granger causes” fiscal development, providing further evidence that reverse causation does not drive our results.

Additional fiscal capacity outcomes

To show that our results do not depend on our main measures of fiscal development (i.e., tax-to-GDP ratio, direct tax share), we construct two additional fiscal capacity outcomes. The first such variable is the indirect tax share. According to our argument, greater intra-elite competition should lead to an increase in tax progressivity. This prediction suggests that the relationship between intra-elite competition and the indirect tax share should be negative. The second additional variable is direct tax bias, computed in the spirit of Besley and Persson (2011) as the ratio of direct taxes to indirect taxes. The predicted relationship between intra-elite competition and this fiscal capacity outcome should be positive.

Table A13 repeats the main analysis for the two additional fiscal capacity outcomes. Consistent with our argument, the coefficient estimates for $E_{i,t-1}$ are always negative and highly significant when the indirect tax share is the outcome variable. And, as predicted, the coefficient estimates for $E_{i,t-1}$ are always positive and significant when direct tax bias is the outcome variable.

Public expenditure outcomes

A final implication of our argument is that intra-elite competition should promote higher public goods provision. Ideally, we would like systematic data on public expenditure outcomes across our sample of developed and developing nations from 1870 to today. Such data, however, are not readily available. Thus, as an alternative, table A14 shows the results for the stringent specification for total spending, nondefense spending, and spending on transportation and housing (all as shares of GDP) for 10-plus national governments in Europe over 1870–1975 for which systematic data from Flora, Kraus, and Pfenning (1983) are in fact available. There is a positive and significant relationship between intra-elite competition and total spending and spending on transportation and housing. While this relationship remains positive when the outcome variable is nondefense spending, the coefficient estimate for $E_{i,t-1}$ just misses significance (the p -value is .118). Overall, these results are consistent with the implication of our argument that greater intra-elite competition should promote higher public goods provision.

CONCLUSION

In this paper, we have argued that the timing of industrialization affects historical levels of intra-elite competition, which in turn helps shape key initial decisions over fiscal size and structure. Under early industrialization, it was more likely that capitalist elites would be pitted against agricultural elites in a sort of zero-sum economic game. In this historical context, intra-elite competition tended to be greater, promoting the development of large fiscal states characterized by tax progressivity. Under late industrialization, by contrast, agricultural elites were more likely to retain their traditional dominance. In this context, therefore, intra-elite competition tended to be low, yielding relatively small fiscal states characterized by tax regressivity.

To test the predictions of our argument, we have exploited an original database that spans 30-plus developed and developing nations between 1870 and 2010. Our main empirical analysis provides evidence for a positive, statistically significant, and robust relationship between intra-elite competition among agricultural and capitalist elites and the size and structure of fiscal states. The magnitudes of our estimates are sizable.

Our paper has implications for the literature on the historical origins of fiscal capacity. As described in the section “Alternative Arguments,” the main arguments present in this literature focus on interstate warfare, leftist control of government, and economic modernization. What such arguments tend to overlook, however, is the extent of intra-elite conflict in society. Interstate warfare, for example, cannot fully explain differences in long-run fiscal development between nations such as Spain and Sweden or Argentina and Chile, none of which were major participants during the world wars. Our main results indicate that—at least for our sample database—the fiscal consequences of intra-elite competition typically outweigh (in terms of statistical significance) those of the main alternative arguments in the literature. By highlighting the role of intra-elite competition, therefore, our paper improves our understanding of the long-run fiscal development process.

Beyond the contribution above, our paper has implications for the literature on the role of the state in long-run economic development (e.g., Acemoglu, García-Jimeno, and Robinson 2015; Besley and Persson 2013; Dincecco and Katz 2016; Evans 1995; Migdal 1988; Wade 1990). Governments can play productive economic roles through the provision of new public goods (e.g., urban sewerage systems). Our paper sheds light on the ways in which historical competition—or lack thereof—between agricultural and capitalist elites influenced public goods provision and, thus, economic outcomes. Similarly, our paper helps explain enduring fiscal weakness in today’s developing world, which we relate to lower historical levels of intra-elite competition. Fiscal weakness, in turn, can reduce

the provision of growth-enhancing public goods. In such ways, our paper offers new insights into the intertwined relationships between political, fiscal, and economic development.

We conclude with three potential directions for future research. Our paper examines the persistence of fiscal differences between early and late industrializers over time. Future research should analyze the conditions under which fiscal development may take place even under governments previously stuck in low tax–low capacity traps. This inquiry calls for study of political variation within early or late-industrializer groups themselves (vs. between-group variation only). There may be differences in distributive outcomes, for example, among state-led late industrializers that were autocratic rather than democratic. Second, future research should explore how innovations in tax technology (e.g., VAT) have influenced fiscal differences between early and late industrializers. Historical inquiry into the political coalitions that helped sway initial fiscal decisions one way or the other—given the tax technology available at the time—should be valuable. Finally, future research should study the links between the timing of industrialization, the state’s ability to broadcast power throughout its territory, and spatial patterns of economic inequality. In this way, we will gain a more complete understanding of distributional politics within the world’s largest democratic regimes.

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