

# When do Elites Support Increasing Taxation?

## Evidence from the American South

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### Abstract

When do elites support the expansion of the state's ability to tax? Despite the disproportionate influence that elites are theorized to exert on politics, answers to this question remain elusive. We argue that elites will support increased taxation, including on themselves, when they believe greater fiscal capacity will yield collective goods that further their interests, they have political control, and they expect this control to persist into the future. We test our argument by measuring changes in state taxation using an annual panel of Southern slave states between 1840 and 1860. Our strategy exploits both institutional differences across these states in the *de jure* political control of the slaveowning elite and rising international demand for Southern cash crops. We find that states with greater slaveowner control responded to increased demand for their crops by raising taxes on slaves and land, their primary assets, to fund railroad construction.

**Keywords:** Political Economy, Taxation, Elites, Political Institutions, Public Goods

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The political determinants of tax incidence are a key area of study in comparative political economy. Much of the literature focuses on the conditions in which higher taxes are likely to be successfully imposed on the wealthy (e.g., Meltzer and Richard 1981; Boix 2003; Acemoglu and Robinson 2001; Albertus and Menaldo 2014; Beramendi et al. 2018).<sup>1</sup> However, less attention has been dedicated to the conditions under which economic elites will not only prefer higher levels of fiscal extraction but support greater taxation on themselves, possibly because these instances are thought to be rare.

In this paper, we investigate the factors that might lead economic elites to shoulder heavier tax burdens and, by extension, enable the expansion of the state's ability to tax. Building on the fiscal exchange literature (e.g., Bates and Lien 1985; Levi 1988; North and Weingast 1989; Hoffman and Norberg 1994; Timmons 2005), we argue that the rich will be substantially more likely to support increasing taxation on themselves if three conditions hold simultaneously: 1) they hold political power in the present period, 2) are likely to hold political power in the future, and 3) can benefit from the production of collective goods. We consider collective goods, such as large-scale investments in infrastructure that can facilitate bringing products to export markets, to be those which are more efficient and less risky to produce if publicly financed. Political control is crucial as it provides elites the ability to allocate public spending disproportionately towards goods that further their own interests. Yet key to our argument is future political control. Even if the net benefits of funding collective goods is high in the present, increasing the taxing capacity of the state poses risks to the long-run interests of elites if political power may shift in the future.

We test our argument using an original dataset of state tax rates and revenues from the American South during the pre-Civil War period. Our research design leverages a lasting international commodity price shock, which increased the value of production and therefore the demand for capital-intensive infrastructure by slaveowning elites. At the same time, we exploit the variation in state legislative apportionment rules – which gave disproportionate

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<sup>1</sup>Acemoglu et al. (2015) provides a recent survey of this literature.

influence to large slave owners in the legislatures of some states but not others – to study the conditional nature of elite support for increasing taxation. We then show that: 1) tax rates and revenues rose in states where the plantation class had disproportionate political power due to malapportionment; 2) the increases in revenue and rates in the malapportioned states came from taxes with greater incidence on assets owned by elites and from regions within states where plantation owners predominated; 3) taxes levied more broadly across the white population (e.g., the poll tax) did not increase faster in the malapportioned states; and 4) the revenue expansion was matched by a rise in public expenditures that would bolster the slave economy (railroads), but not by an increase in spending on goods (public education) that would benefit the white population more generally. Consistent with our argument, these rural elites withdrew their consent once power became contested in the decades following the Civil War, leading to a decline in fiscal capacity (Suryanarayan and White 2020).

Our paper helps fill an important gap in the existing literature on the determinants of taxation and fiscal capacity. It identifies precise and simple conditions that facilitate taxation of the rich, by the rich, and for the rich. As such, we depart from the recent literature’s emphasis on intra-elite conflicts and of passing the incidence of taxation onto other elites or segments of the economy (e.g., Ansell and Samuels 2014; Beramendi et al. 2018; Mares and Queralt 2015, 2020; Scheve and Stasavage 2010). Furthermore, our case – which shows the increase in state-level taxation on the slaveowning elite, a group frequently portrayed as being averse to taxation – offers a particularly tough test of our argument. We show that biased electoral institutions gave these wealthy actors the ability to ignore redistributive demands from the non-slaveowning white majority, freeing them to impose taxes on themselves, knowing they could use the revenue on collective goods that enhanced their economic interests.

To be clear, our story should not be interpreted as one of benevolent behavior by elites, nor a case in which venal behavior nevertheless resulted in widely shared long-run development. Simply put, we find that elites increased taxation on themselves solely to fund

public spending that would increase their profits and the value of the enslaved people whom they brutally exploited. Furthermore, we find no evidence that greater fiscal capacity translated into more spending on public goods that would improve broader welfare; nor do we suggest that these crimes against humanity generated long-term development. Rather, we describe elite-driven fiscal expansions for what they are: strategic behavior under specific conditions to further their own interests. The implications for this previously under-studied mechanism should influence not only our understanding of the determinants of greater investments in state capacity but also the range of possible consequences of such investments on development.

## **When do governments tax the rich?**

The received wisdom says that elites tend to be taxed more heavily in democratic societies and particularly so in those where inequality is high (Meltzer and Richard 1981; Acemoglu and Robinson 2001; Acemoglu et al. 2015; Boix 2003). An emerging literature has challenged the view of democracy as the primary mechanism by which the incidence of taxation on the rich rises (e.g., Ansell and Samuels 2014; Scheve and Stasavage 2010). More recent scholarship has instead stressed various forms of intra-elite conflict as a potential trigger for progressive direct taxation, generally with one elite group successfully imposing taxes on another elite group (e.g., Mares and Queralt 2015, 2020; Beramendi et al. 2018).

In a separate literature, taxation on the rich is seen as the product of a negotiated exchange based on mutual interests of elites and rulers, tied together by some form of credible commitment (such as an assembly) and by the provision of valuable collective goods, notably security (see, for example, North and Weingast 1989; Hoffman and Norberg 1994; Luong and Weinthal 2004). In return for supporting fiscal development, elites receive some control over how resources are spent, thereby creating an explicit link between taxation and representation. Scheve and Stasavage (2016) further unpack this classical argument emphasizing the concept of “shared sacrifice,” whereby the wealthy disproportionately fund warfare while

the masses bear the brunt of the fighting. In this case, taxes on the rich represent a form of compensatory redistribution. Overall, these so-called fiscal exchange theories rest on the idea that taxing others without incorporating their input into public spending or introducing other mechanisms of accountability is relatively costly and difficult to maintain (e.g., Bates and Lien 1985; Levi 1988; Scheve and Stasavage 2016).<sup>2</sup>

Regardless of whether elites are taxing themselves via some sort of exchange or being taxed by another group, agricultural elites are generally viewed as being especially resistant to taxation and to the construction of effective and centralized fiscal systems, particularly in the context of labor repression and high inequality (e.g., Sokoloff and Engerman 2000; Sokoloff and Zolt 2006; Suryanarayan and White 2020). Their resistance is thought to stem from several sources. First, their assets are visible, easily appropriable, and not especially mobile, making them a very tempting and exploitable tax handle. Second, they are not thought to be as reliant on collective goods that states might provide (e.g., Galor et al. 2009). Their success at blocking taxes is argued to come from both *de jure* political institutions that overweight their preferences as well as *de facto* forms of power (e.g., Alston and Ferrie 1999; Acemoglu and Robinson 2008; Ziblatt 2009; Albertus and Menaldo 2014).

Building on the fiscal exchange literature and the insights of Saylor (2014), who emphasized how commodity booms create demands for infrastructure, we argue that the following conditions, if present simultaneously, will result in support by economic elites, including rural elites, for increasing taxation on themselves. First, elites must control the political system in the present period. As others have argued, economic actors are more likely to make fiscal contributions in the presence of a credible commitment guaranteeing that their preferred tax policies and spending priorities will be implemented (e.g., Bates and Lien 1985; Levi 1988; Dincecco 2011; Cox 2016; Garfias 2019). Second, elites must expect their political dominance to persist into the future. This not only ensures that the enhanced extractive

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<sup>2</sup>These may include the costs of repression or the loss of economic efficiency. See Moore (2008) for a discussion of fiscal contractualism versus coercive extraction.

tools of the state will not be used against their interests (e.g., Besley and Persson 2011), but also protects elites against the increased expectations that higher public spending can engender among citizens (e.g., Gottlieb 2021). Third, elites must obtain economic benefits from the production of some collective good(s) that are difficult to provide privately – i.e., those that involve large amounts of capital and long payoff times, impose high coordination costs, require significant monitoring to limit free-riding, or depend upon coercion to be effectively provided (Acemoglu and Robinson 2001).<sup>3</sup>

## The Willingness of Slaveowners in the American South to Increase Taxes on Themselves

We now evaluate our claims using an original panel of state-level taxation from the American South between 1840 to 1860. Our sample consists of the fourteen states where more than 99 percent of all US slaves resided in 1850 (1850 Census). Our empirical strategy leverages three key features of this period and these states: i) the fiscal and political autonomy of states within the US federal system, ii) an international commodity boom that increased demand for Southern cash crops, and iii) variation across Southern states in slaveowner *de jure* political control.

### Autonomy of Antebellum State Governments

We first exploit the fact that US states during this period retained near complete autonomy over the areas relevant to testing our argument. For one, they were allowed to design their

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<sup>3</sup>Similar to Besley and Persson (2011), our argument emphasizes the role of political stability in lengthening the horizon of rulers. Their model predicts that in a “redistributive state,” investments in state capacity are driven by a rent extraction motive and accompanied by increased taxation on non-ruling groups. Our argument builds on this prediction and posits that when political elites are asset owners with business interests, stable political control may not only increase investments in capacity but also *self*-taxation.

political institutions without federal interference.<sup>4</sup> As detailed below, this allows us to exploit institutional variation across states which influenced the degree of elite *de jure* control of these state governments. Second, the U.S. Constitution and Congress left large swaths of legal, economic, and social life for each state to regulate within its territory. With a few important exceptions, such as trade policy (between the states and internationally),<sup>5</sup> the states were largely on their own to promote economic development, including in key areas such as infrastructure (Wallis and Weingast 2018).<sup>6</sup> Each state was also responsible for creating its own legal code, as well as the administrative capacity to enforce it.<sup>7</sup> Finally, the resources to enforce order, promote development, and provide any public services, such as education, would need to be raised internally (e.g., taxes, loans). With the important exception of custom duties on trade with other states and countries, states were empowered to devise any schemes of taxation they pleased (e.g., income taxes, *ad valorem* property taxes). Additionally, they neither faced competition on taxes with the federal government, nor did they have tax obligations to it; that is, funds raised locally were spent locally.<sup>8</sup> In short, in the

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<sup>4</sup>While the federal Congress was given the right to regulate state-level politics through the Guarantee Clause of the Constitution (Article IV, Section 4.1), this power was never exercised during the antebellum period (1789-1860).

<sup>5</sup>The Contract Clause (Article I, Section 10) also restricted states from abrogating contracts with legislation, issuing money, and entering into treaties with other countries.

<sup>6</sup>Federal public goods, which were funded almost entirely by import tariffs, largely consisted of national defense, the postal service, and land grants.

<sup>7</sup>Most notably, Southern states had to construct institutions to enforce the enslavement of substantial proportions of each state's population.

<sup>8</sup>Substitution effects on taxation between the federal and sub-national units are not a concern, as there was no federal-level direct taxation during the period of our study. With rare exceptions (e.g., warfare), the federal government's tax revenue consisted almost entirely of custom duties prior to the Civil War (1861-65).

highly decentralized antebellum United States, states were almost exclusively responsible for regulating behavior and promoting general welfare, and for virtually all taxation and public spending (Wallis 2000).

### **International Commodity Boom for Slave State Crops**

The second feature we exploit regards the timing of a large international commodity boom. Specifically, the period from approximately 1844 to 1860 witnessed a sustained positive price shock to cash crops that relied on slave labor, notably cotton. In Liverpool, the primary international market for cotton, the price of “Middling American cotton” rose from a post-1800 low of 4.1 pence per pound in 1848 to 7.8 in 1857 (Mitchell 1963, p. 492); in New Orleans, the main domestic market, cotton rose from a post-1800 low of 5.5 cents per pound in 1844 to 11.5 cents in 1858 (Gray and Thompson 1933, p. 1026). The prices of other Southern cash crops (e.g., tobacco, sugar) also more than doubled over this period (Gray and Thompson 1933, p. 1033-1038). Unsurprisingly, production and the value of these crops increased substantially during these years. Sugar production in Louisiana, for example, almost tripled, going from 87 million pounds in 1840 to 255 million in 1860 (Gray and Thompson 1933, p. 1033), with sugar prices rising more than 156 percent between 1840 and 1857. The production of cotton in the South rose from approximately 834 million pounds in 1840 to 2.28 billion pounds in 1860. As a result, the value of cotton exports exploded from approximately \$50,000,000 in 1846 to nearly \$200,000,000 by 1860 (North 1960, p. 233).<sup>9</sup> Tobacco exports similarly increased from 3.4 million pounds in 1843 to 17.7 million in 1860 (Gray and Thompson 1933, p. 1036), while prices were more than 3.2 times higher in 1857 compared to 1843.

Figure B1 in Appendix B shows that this rising demand for and output of Southern export crops corresponded with a substantial increase in commodity prices between the mid-

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<sup>9</sup>Approximately 75% of cotton produced was exported; and, cotton exports alone comprised more than half of the total value of American exports in this period.

1840s and 1859, and especially after 1849. Figure B1 (a) shows the 5-year moving average of cotton prices in New Orleans from 1840 and 1860; Figure B1 (b) shows the 5-year moving average of a commodity index reflecting variation in cotton, sugar, and tobacco prices over the same period.

Despite rising demand, Southern planters, however, faced a critical constraint when it came to getting even more products to market: the existing infrastructure network of the South prevented substantial amounts of fertile land that was too far from navigable natural water sources from being profitably cultivated (Wright 2020). For slaveowners, and the U.S. itself, massive investments in infrastructure were necessary to connect millions of acres of potential farm land to markets. Despite this need, economic historians have argued that economic development of the South lagged the North in part due to chronic underinvestment in infrastructure (Ransom and Sutch 1988; Wright 2020). Given that the benefits of increased infrastructure would heavily accrue to a narrow slaveowner class,<sup>10</sup> funding railroad construction privately, as was largely the case in Europe and the UK, might have been preferable to increasing a state’s overall fiscal extraction. Yet this option was largely infeasible in the spacious, capital poor, and sparsely populated antebellum-era South where the private returns were highly unlikely to compensate for the costs and risk involved (Fishlow 1965; Goodrich 1974; Heath 1950; Larson 2002). According to Reed (1962, p. 184), for example, “seventy-five per cent of the railroads chartered in the 1830’s [in Louisiana] failed to materialize [due to constraints on capital].” More broadly, Larson (2002, p. 239) claimed that the South was too “underdeveloped and incapable of supporting large-scale internal improvements on the strength of private fortunes alone.” Given the lack of federal spending on large-scale infrastructure, public financing of infrastructure would need to come from Southern state and local governments (Wallis and Weingast 2018; Callen 2016). The South’s

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<sup>10</sup>Enslaved labor comprised more than half of the Southern agricultural labor force and nearly 50% of all Southern wealth in 1860 (Wright 2006, p. 60); furthermore, only 10% of Southern adult white males owned 86% of the region’s slaves (1860 Census).

lack of large urban areas also meant that Southern state governments would need to be the primary source for public funding (Fishlow 1965, p. 397). As Marrs (2009, p. 24) argues: “States proved to be a critical solution to the problem of railroad financing in the South.”

In sum, the boom in international demand for Southern cash crops meant that slaveowners across the South would have benefited from substantially increasing public investments in infrastructure.<sup>11</sup> Not only would such investment open up more land for cultivation, but would also increase the demand for, and the value of, slaves, the primary asset of slaveowners (Ransom and Sutch 1988).

### **Malapportionment and Slaveowner Power in the State Legislatures**

The final feature of this case we leverage is the appreciable differences in the formal allocation in political power across Southern states. In seven of the fourteen states, representation in both chambers of the state legislature was systematically malapportioned in favor of higher slave-share districts (e.g., counties); in the remaining seven states, by contrast, representation in both chambers followed a “one (adult white) man, one vote” principle. The bias in representation took three primary forms. In some states, the number of lower house (hereafter, LH) representatives and upper house (hereafter, UH) senators apportioned to each county was based on the total population of the county which included slaves (e.g., Florida LH and UH, Georgia LH, Louisiana LL and UH, Maryland LH, North Carolina LH).<sup>12</sup> In the chambers of a few other states, apportionment was based on the amount of state taxation paid by each administrative unit (e.g., North Carolina UH, South Carolina LH). Lastly, a number of states used a system in which each administrative unit received a fixed or equal number

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<sup>11</sup>Figure A1 in the Appendix shows the spatial distributions of the enslaved population across counties and the extension of railroads in 1850 and 1860.

<sup>12</sup>Louisiana and Maryland also capped representation for New Orleans and Baltimore, the South’s two largest cities. In Georgia, the 37 most populous counties received two representatives each and the rest received one.

of representatives regardless of population (Georgia UH, Maryland UH, South Carolina UH, Virginia LH and UH). By comparison, in the remaining seven states, representation in the state legislature was based solely on each county’s white population or eligible voter population.<sup>13</sup> Furthermore, these states’ constitutions required frequent reapportionment based on white population (often as frequently as every four years). The states that comprise the malapportioned states (hereafter, MS) and the non-malapportioned states (hereafter, NMS) are reported in Appendix Table A1. For each state, the table also shows the basis of representation in the legislature.

Systematic malapportionment provided a unique source of political power to slaveowners. For one, slaveowners, especially planters, were a minority of the voting population in each of these fourteen states.<sup>14</sup> Yet, the economic geography of slavery meant that slaveowners were typically spatially concentrated within each state.<sup>15</sup> Thus, systems of apportionment that over-represented high slave-dependent areas – whether by including slaves in the population count, basing representation on taxes paid, or using a fixed basis that over-represented less populated rural areas<sup>16</sup> – could manufacture majorities in the state legislatures.

Key to our argument, this bias to legislative representation also provided a powerful source of enduring political power. Namely, this distortion was “self-enforcing.” Not only did this source of power alleviate the need of the slaveowning elite to use resources to main-

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<sup>13</sup>Suffrage restrictions were not a source of bias, as no Southern state had a property requirement for voting and only North Carolina retained a tax-paying requirement by 1860 (Engerman and Sokoloff 2005).

<sup>14</sup>Using the ratio of slaveowners to adult white males from the 1860 Census as a rough estimate of the share of the electorate who owned slaves, shows that South Carolina had the highest share with just under 40%. This same ratio with 20 or more slaves, which is the traditional definition of a planter, was less than 8% in every state.

<sup>15</sup>Appendix Figure A1 shows the geographic distribution of slavery across the South.

<sup>16</sup>Less than 4% of the slaves resided in the South’s urban counties (1860 Census).

tain this advantage, as the biases were enshrined in state constitutions, but the legislative majorities conferred by this bias meant that any equalizing reforms would require the consent of slaveowners.<sup>17</sup> With elites from high slave-share districts retaining the capacity to block any proposal that would have diluted their control, no malapportioned state reformed to a white-population basis during the antebellum period. Furthermore, because this was an era in which governors and the judiciary were institutionally weak, maintaining majorities in the legislature was the key to controlling Southern state governments.<sup>18</sup> In other words, malapportionment gave slaveowners in those states a firm hold on power, one which they could reasonably believe would persist into the future.

There are a number of important concerns regarding the use of malapportionment to test our argument, including whether malapportionment can be taken as exogenous and the extent to which malapportionment actually conferred an advantage to slaveowners. We address these concerns briefly here and provide extensive supporting details and evidence in Appendix D. For one, while states were clearly not randomly selected into being malapportioned versus non-malapportioned, we argue this concern is mitigated by two factors. First, the *de jure* legislative bias in favor of high slave share areas predated statehood. Specifically, colonial-era legislatures overrepresented high slave-share areas (Beramendi and Jensen 2019); these colonial-era biases were then transplanted into the initial post-colonial constitutions, locking in the power of plantation owners (Green 1966, p. 97-98). Of relevance to our study of taxation on slaveowners and state support for railroads between 1840 and 1860,

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<sup>17</sup>An analogous example is that low-population American states do not need to expend resources to maintain the enormous advantages in the US Senate conferred by the two-senators-per-state basis in the US Constitution.

<sup>18</sup>Governors typically lacked veto power, served shorter terms, and in a few cases were actually selected by the legislature. Legislatures also often possessed appointment power of judges and other key bureaucrats (Tarr 2000). Thornton (2014, p. 59), for example, declared that “the [Alabama] legislature was nearly the whole state government.”

this means that this bias also long pre-dated the invention of the cotton gin and commercialization of cotton, and the end of slave imports into the US in all but two states (Florida and Louisiana). By comparison, in the seven NMS, a population-basis of apportionment was adopted from the outset and persisted throughout the antebellum period. We argue therefore that the most important factor determining the rules of legislative apportionment implemented in each state’s first constitution was whether this bias was present in the period prior to statehood. If a slaveowning elite was well established before statehood, they were able to structure the colonial institutions in their favor. By comparison, more egalitarian initial constitutions emerged in frontier states in which a colonial-era slaveowner class was less established. That is, the pre-statehood power of slaveowners, and with it malapportionment, was largely determined by colonial settlement patterns.

Second, and despite many constitutional changes across most of these states (including to representative institutions), none of the MS reformed to a “one white man, one vote” basis during the antebellum period. Similarly, none of the NMS deviated from “one white man, one vote” during the period. Once implemented, slaveowners in the NMS were unable to alter their state’s fidelity to the “one white man, one vote” principle. In Appendix D, we use original data on representation to demonstrate the persistence and magnitude of this bias in favor of slaveowners in the MS, and the lack bias in favor of slaveowners in the NMS.<sup>19</sup> As a result, and although slaveowners in the NMS, especially the more slave dependent ones, also possessed political power that far exceeded their numbers (e.g., Wooster 1969; Thornton 2014), the fact that the system of representation was not biased in their favor meant that their power was always more contestable and never guaranteed. Elites in the NMS had to weigh the benefits of more collective goods in the current period against the fear that their tax revenues and the state’s increased fiscal capacity more generally would be in the hands

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<sup>19</sup>See Appendix Figure D1. We complement this evidence with a brief review of the constitutional reforms to each state’s system of apportionment and of the rich historical literature describing the consequences of legislative malapportionment on these states’ politics.

of the non-slaveowning white majority, whose interests might diverge from theirs.

To further mitigate concerns about malapportionment status being correlated with other factors that could influence the association between commodity prices and taxation outcomes, in Appendix Figure A3 we conduct a balance test over a number of state-level characteristics that could potentially predict the divergent fiscal trajectories we observe. The variables included in the balance test are: total population, enslaved population share, urban population share, state income, dispersion of cotton suitability within states, cotton production in 1840, state capacity in 1850, density of navigable rivers, as well as measures of agricultural suitability for cotton, sugar, and tobacco.<sup>20</sup> A statistically significant correlation between apportionment status and one of these covariates would suggest the presence of a potential alternative explanation for the decision of states to increase taxation. Different levels of cotton suitability between the MS and NMS, for example, would indicate that one group had more to gain from increasing taxation and investing in railroads than the other. Similarly, divergent cotton production levels in 1840 would suggest that some states benefited from greater productive capabilities when commodity prices began to rise. The dispersion of cotton suitability, in turn, captures the cohesiveness of cotton-growing interests within each state, which can facilitate (or impede) collective action. Differences in the initial level of state capacity may have allowed some states to raise taxes more rapidly than others.<sup>21</sup> Finally, the density of navigable rivers also matters in that it may have made the need for railroads more pressing in some areas than in others. We find no statistically significant differences in any of these covariates across malapportioned and non-malapportioned states.

In short, circa 1840, our comparison states had roughly similar endowments. They dif-

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<sup>20</sup>See Appendix Table C1 for sources and additional details on each variable.

<sup>21</sup>We measure state capacity as the share of government officials per white capita in 1850 (e.g., Suryanarayan and White 2020). This information is unavailable in the 1840 census. In Appendix F we present a number of additional tests assessing initial state capacity across states, each finding little evidence of a meaningful difference.

ferred primarily on the supply side: in half of the states, the slaveowning elite's power to control taxes and public spending was substantially less contested in both the present period and for the foreseeable future; in the other half, their control was less certain. Whereas the commodity boom increased the value of land and slaves and infrastructure bottlenecks constrained those assets from reaching their full potential across both the MS and NMS, only the elite in the MS had the power and incentive to use their secure hold on power to tax themselves to finance collective goods that would leverage the boom for their benefit. We thus expect tax and spending differentials to emerge to across the two types of states, with the MS increasing the incidence of taxation on their elites and public spending on railroads at a faster clip than their non-malapportioned counterparts.

## Data

To test our main hypothesis, we collect three types of information: 1) state government tax revenues, 2) state tax rates on slaves and land, and 3) state poll tax rates (i.e., capitation taxes levied equally on each white adult male). The combination of measures helps ensure that we are capturing both the *choice* to increase the incidence of taxation on assets of elites (but not the broader electorate) and the concomitant effect on revenues actually collected.

For state tax revenues, we created an annual panel of the amount of state tax revenues collected in each Southern slave state between 1840 and 1860, relying primarily on contemporaneous reports from state treasurers, auditors and the like. We focused specifically on tax revenues and excluded other types of revenues, such as loans and income earned from assets (e.g., land sales, dividends from banks, railroads). In addition to direct taxes (e.g., property taxes, capitation taxes), we included indirect taxes, such as licensing fees on occupations and sales taxes. Of the 294 possible observations in our panel, we located total state tax revenues for 241 state years. Critically, for each state we have at least 13 observations (out of a possible 21) and at least two observations for each 5-year period from 1840 to 1860.<sup>22</sup>

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<sup>22</sup>Florida and Texas were admitted in 1845, and thus have only 15 possible state years.

With these data, we create two aggregate measures of taxation: tax revenue per white capita and tax revenue as a share of state income (i.e., state tax/GSP). Annual measures of each state’s white population are based on linear interpolations from three decennial censuses between 1840 and 1860. Similarly, we use measures of state income created by economic historians, and linear interpolation for the intervening years. Details about data construction and sources for these variables is in Appendix C.

We also use each state’s legislative session laws to create an annual panel of statutory tax rates on the primary assets of slaveowners, slaves and land, and the poll tax levied on adult white males. Tax rates on land are straightforward, as each state used an ad valorem property tax system for land. Creating a measure of tax rates on slaves that is comparable across states, however, is complicated because some states used a capitation tax (tax per slave) and others used an ad valorem property tax. Making measures comparable, therefore, required us to use the actual value of slaves to impute tax rates from the levels of capitation taxes. While necessary, this approach makes the created measure of ‘tax rates on slaves’ in the states that used capitation taxes more susceptible to fluctuations in slave prices. This can affect the comparability between capitation tax and ad valorem tax states when there is high volatility in slave prices, which was the case during most of the 1840s. By the late 1840s, however, most states had shifted to an ad valorem system *and* slave prices began to exhibit a secular increase with reduced volatility.<sup>23</sup> Thus, to minimize the amount of noise in the data and for the sake of increased comparability across states, we only use the tax rate on slaves as a dependent variable between 1848 and 1860. Finally, we use the state statutes to construct an annual panel of the amount of poll tax levied on each eligible adult white male. This measure should capture the intent of the state legislatures to tax lower income whites. Again, extensive details about our sources and methods for constructing

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<sup>23</sup>In 1840, six states taxed slave property using a uniform ad valorem tax rate. By 1860, this increased to nine. See Figure B2 for the trend in slave prices.

each variable, including a discussion of their limitations, can be found in Appendix C.<sup>24</sup>

## Changes in State Taxation from 1844 to 1860

We begin with visual inspections of the conjecture that the rising commodity prices translated into increased taxation in the MS. Figure 1 plots the 5-year rolling averages for each year from 1844 to 1860 for each of these five measures for both groups of states (with the exception of the tax rate on slaves which only covers 1848 to 1860).

In each figure, the average across the MS and NMS is either similar or converging in the late 1840s. By the early 1850s, however, there is an appreciable divergence in taxes per white capita (Figure 1a), state taxes as a share of state income (1b), ad valorem tax rates on land (1c), and tax rates on slaves (1d). As predicted, we see no differential on the regressive poll tax over this period (1e).

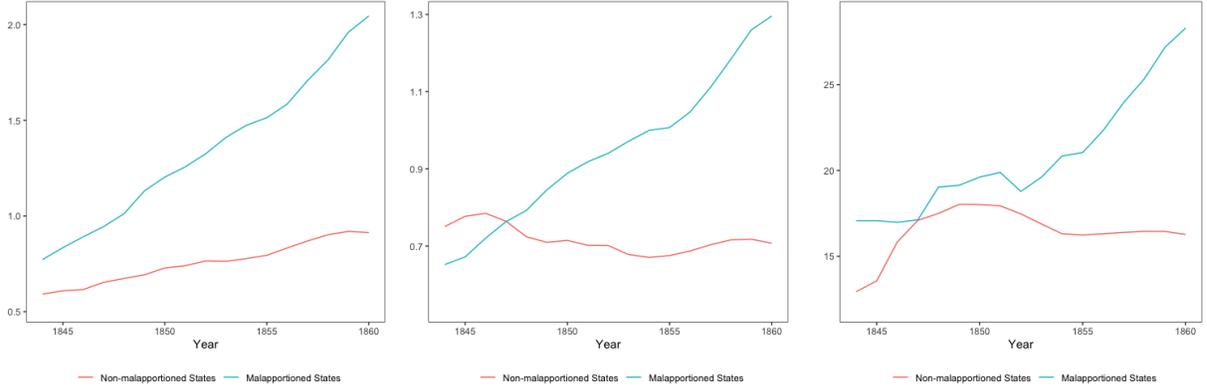
This differential response to rising commodity prices is perhaps most sharply evident with the tax rate on slaves. As slave values rose in response to the commodity shock, state legislatures needed to increase capitation taxes, the ad valorem rates, or the assessed values of slaves, lest the real incidence on this asset fall. While we lack the data to test changes to state-level assessments, we can see whether states raised either the levels of capitation taxes or the ad valorem rates.<sup>25</sup> All of the NMS either maintained their existing rate or decreased it over the 1850s. By contrast, taxes on slaves increased in five of the MS, led by North Carolina and Virginia, which approximately quadrupled their capitation tax on slaves over the course of the decade. Only two of the MS either maintained or decreased their rate of taxation on slaves (Georgia and Maryland). We discuss those cases in Appendix E.

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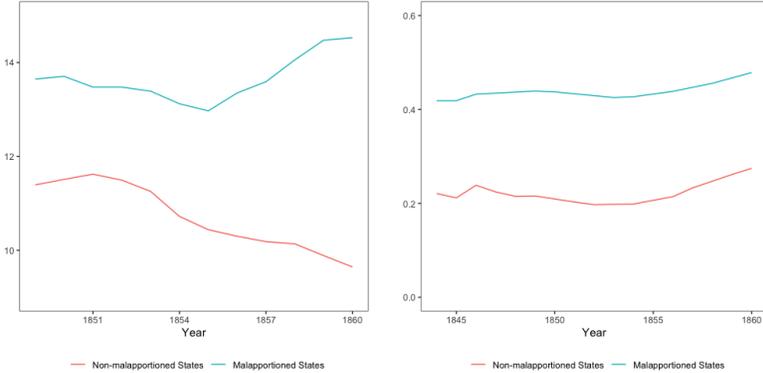
<sup>24</sup>Appendix C also includes a validation test of the *tax rate on slaves* measure.

<sup>25</sup>See Figure C1 for evidence that raising rates on slaves increased tax revenues collected.

Figure 1: Taxation by Malapportionment Status, 1844-1860



(a) State Taxes pwc (\$)    (b) State Taxes/Income (%)    (c) Ad Valorem Land Tax



(d) Taxes Rate on Slaves    (e) Poll Tax Rates

5-year rolling averages across malapportioned (MS) and non-malapportioned (NMS) states of: (a) taxes per white capita, (b) state taxes as a share of state income, (c) ad valorem tax rates on land, (d) tax rates on slaves from 1848 to 1860, and (e) poll tax rates, 1844 to 1860.

## Benchmark Results

Our empirical strategy follows a difference-in-differences approach that assesses whether changes in commodity prices affect taxation outcomes disproportionately in malapportioned versus non-malapportioned states (e.g., Dube and Vargas 2013):

$$y_{it} = \beta_0 \ln P_{it} + \beta_1 \ln P_{it} \times M_{it} + \beta_2 \mathbf{X}_{it} + \lambda_i + \gamma_t + \epsilon_{it} \quad (1)$$

$y_{it}$  is the dependent variable, a state-level measure of tax revenues or rates, for state  $i$  at time  $t$ .  $M_{it}$  is an indicator variable that takes the value one if the state legislature of state  $i$

is malapportioned in year  $t$ , and 0 otherwise. Our main variable of interest, the commodity price index, reflects the exposure of states to the fluctuations in the prices of cotton, sugar, and tobacco weighted by the average suitability of each state to the cultivation of these crops relative to the Southern average.<sup>26</sup>  $P_{it}$  thus captures the relative exposure of elites in state  $i$  to the commodity shock in year  $t$ . The parameter  $\beta_1$  captures the differential effect of commodity prices on taxation outcomes in malapportioned states. Both dependent and independent variables are measured as 5-year moving averages to reduce small fluctuations and filter data noise.  $\mathbf{X}_{it}$  represents a vector of time-varying covariates, namely, total population (log), urban population (log), and income (log);  $\lambda_i$  and  $\gamma_t$  are state and year fixed effects, respectively, and  $\epsilon_{it}$  is an error term.

The results for our benchmark models are reported in Table 1. The first two columns investigate the effect of commodity prices on total state taxation as a proportion of income and on taxes per white capita. In the third and fourth columns, the dependent variables are the tax rate on slaves and the ad valorem tax rate on land, respectively. Finally, column 5 examines whether apportionment status and commodity prices had a similar effect on regressive forms of taxation by using the poll tax rate as an outcome.<sup>27</sup> The estimates are substantively similar across specifications and dependent variables. There is no meaningful estimated change in tax outcomes in the NMS following commodity price increases. The positive and significant interaction term between apportionment status and the price index captures the expected moderation effect. Per our expectation, international commodity prices differentially affect tax revenues and rates in the MS. Specifically, a one standard

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<sup>26</sup>This measure is preferred to an index based on actual production values because the latter are more susceptible to endogeneity concerns. See Appendix section B.1 for more details on the construction of the index and Table C1 for data sources.

<sup>27</sup>We cluster standard errors at the state level in all specifications. Additionally, we report wild cluster bootstrap p-values due to the limited number of cross-sectional observations (Cameron and Miller 2015).

deviation increase in the commodity index leads to half a standard deviation increase in taxes/income, and to almost one standard deviation raise in revenues per capita in the MS when compared to NMS. Notably, the negative sign in column 5 indicates that despite the observed increase in overall revenues, regressive poll taxes did not rise in the MS (and if anything declined). Combined, these results show that elites in the MS financed state-level fiscal expansion by taxing themselves, eschewing taxes that fell more heavily on the non-slaveowning white majority.

Table 1: Two-way Fixed Effects: Commodity Price Index and Taxation Outcomes, 1844-1860

	Taxes/Income	Taxes per White Capita	Tax Rate on Slaves (log)	Tax Rate on Land	Poll Tax Rate
	(1)	(2)	(3)	(4)	(5)
Commodity Index (log)	-0.023 (0.305)	-0.415 (0.418)	-0.731 (0.942)	0.036 (0.154)	0.149 (0.135)
Commodity Index (log) $\times$ Malapportionment	0.631*** (0.215) [0.020]	0.981*** (0.323) [0.028]	0.732** (0.294) [0.051]	0.165** (0.078) [0.128]	-0.153 (0.102) [0.224]
Additional covariates	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
R <sup>2</sup>	0.232	0.203	0.362	0.290	0.376
Observations	277	277	140	242	282
Number of States	14	14	14	14	14

*Note:* Main variables measured as 5-year moving averages. All regressions include state population (log), urban population (log), and income (log) as controls, state-level clustered standard errors in parentheses, and wild-cluster bootstrap p-values in brackets. \*p<0.1; \*\*p<0.05; \*\*\*p<0.01.

## Robustness

Appendix A also provides a more complete set of results, including a variety of robustness checks, in which we vary our sample, estimation strategy, and measures. In Table A2, we replicate our baseline analysis using international cotton prices as an alternative measure of the commodity shock. Our substantive findings remain unchanged. In Table A3, we show the robustness of our baseline results to the introduction of time-invariant covariates

interacted with year indicators to capture potential differences in taxation trajectories. These covariates include state-specific conditions – such as surface area, total length of navigable rivers, cotton suitability, and an indicator of whether the state is coastal – which can plausibly influence fiscal trends. The results are not only consistent with those obtained in our baseline specifications, but are even stronger.

A key assumption required to interpret these results causally is that, in the absence of an increase in commodity prices, taxation outcomes in the MS and NMS would have followed a similar trajectory. Although untestable, this assumption implies that fiscal trends in these two groups of states should be parallel prior to the price shock. Figures A4 and A5 show that this is the case for both state tax revenues as a share of income, and taxes per white capita. In both figures, the trajectories of tax revenues in the MS and NMS are almost identical prior to the rise in commodity prices (from 1835-1844). By contrast, after 1844, these two groups diverge noticeably, with the MS experiencing larger increases in taxation. In Table A4, we directly investigate whether there are differential taxation trends across groups prior to the commodity price shock. We find that the interaction of a trend variable with malapportionment status has a coefficient that is not statistically distinguishable from zero, indicating that taxes in the MS and NMS have the same slope during this period, which provides additional evidence in support of the plausibility of the parallel-trends assumption.

In Tables A5 and A6, we adopt an alternative estimation strategy, using a first-difference approach.<sup>28</sup> Given that our theoretical expectations refer to changes rather than levels, first-difference models provide a useful alternative to our benchmark specifications. While higher commodity prices may not always be associated with greater tax rates and revenues, marginal increases in the price index – irrespective of the initial level of taxation within each state – should provide greater incentives for elites in the MS to tax themselves in order to finance valued collective goods. The results also provide strong support for the primary

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<sup>28</sup>Note that various forms of clustering are not appropriate in these models (see Moody 2016). Nonetheless, our results remain unchanged when errors are clustered at the state level.

hypothesis that elites taxed themselves more intensely when they had a secure hold on power and the revenues could be used opportunistically to increase their wealth.

Tables A7 through A11 provide results without potentially idiosyncratic states – such as low slave-dependent border states that did not secede, states with a vote-tax link where there might be an incentive to use taxes to restrict the franchise (Mares and Queralt 2015), and states that had sizable cities or an urban population share that exceeded 10%, where agrarian elites could have increased fiscal extraction solely by imposing new taxes on urban centers. The results hold across the various samples, with the coefficients remaining significant and, in some cases, also increasing in magnitude.<sup>29</sup>

Tables A13 through A16 substitute our main right-hand side political variable – malapportionment – with a number of different factors capturing either history or endowments (rather than the allocation of political power). First, older states tend to be more malapportioned (for reasons discussed above). Thus, we test whether year of statehood explains our findings. Similarly, we use an indicator that measures whether a given area became a state before 1800 (proxying both for age and the timing of the invention of the cotton gin). Four of the six *cotton states* were malapportioned, and thus we also test our model using a *cotton state* indicator. Given that the collective good of interest was infrastructure, we consider whether states located on the Mississippi River had significantly lower demand for this good. We test this using *Mississippi river* indicator. Finally, one might wonder whether “coastal-ness” is a more direct correlate of need for spending on railroads in this initial period than apportionment status. Figure A6 summarizes the results of these alternative specifications by showing the coefficient of the interaction term for each model alongside that of our baseline specification (i.e.,  $Price\ Index \times Malapportionment$ ). Overall, the weaker and inconsistent effects of these alternative moderators support our theoretical emphasis on the

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<sup>29</sup>In particular, Table A9 includes an additional specification (column 7) that accounts for whether states raised taxes on slaves through capitation or ad valorem rates.

centrality of malapportionment status for the divergent patterns of taxation we observe.<sup>30</sup>

In Table A12, in both the fixed-effect and first-difference specifications, we add the size of the enslaved population (log) as a covariate to account for the potential confounding role of this variable and investigate the possibility that changes in the intensity of slavery (rather than malapportionment) accounts for the differential taxation trends. The results remain consistent with our main findings.

Next, we drill down on the question of whether other time-varying factors may account for the tax patterns we find: increases in tax revenues (columns 1 and 2) and rates on land and slaves (columns 3 and 4) in the MS relative to the NMS, and no differential trend in poll tax rates (column 5). While some state-specific characteristics are largely differenced out or captured by the state fixed-effects, one could argue that differential rates of economic growth, for instance, may be driving our results. Although it is possible that our income and population variables imperfectly capture variation in growth rates over time, for this alternative explanation to be valid, we would expect to see an increase in rates and revenues across all tax categories, not just those levied on the property of elites (i.e., land and slaves).

We investigate this possibility further by using the revenues collected by the poll taxes on whites as a possible falsification test that some sort of mechanical drift lies behind our revenue results. In the various state fiscal reports described above, many reported the tax revenues specifically derived from poll taxes. Given that we know that poll tax rates did not differentially rise during this period, this measure of poll tax *revenues* should capture any mechanical growth in tax collection driven by a general increase in income levels – as rising incomes would have enabled more whites to pay the tax. Hence, the interaction term using these same specifications would be positive if differential growth rates were mechanically driving up tax revenues in the MS. We report these results in Table A18. As with poll tax rates, the interaction terms are negative (and now significant), suggesting that the real

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<sup>30</sup>We also test whether previous debt defaults explain the differential tax patterns we document. We find no evidence in support of this idea (see Appendix C).

incidence on ordinary whites might have fallen in the MS in this period. This evidence also supports our claim that the growth in overall state tax revenues in the MS was driven by the increase in tax rates on land and slaves, rather than a mechanical drift from rising incomes.

We provide an additional simple test to assuage concerns that changes in tax rates would have required much more time to manifest in greater revenues. Namely, we test whether changes in tax rates on slaves and land predict changes in tax revenues across states using both contemporaneous values as well as various lags. As shown in Tables A19 and A20, changes in rates strongly predict changes in revenues and the strongest effects are contemporaneous, with lagged coefficients progressively decreasing in size.

Lastly, in Appendix G, we provide a county-level test of state tax incidence. We perform a cross-sectional analysis using county-level data of the within-state distribution of state taxes. Specifically, we test whether the incidence of state taxation is rising in county slave population and is greater in the slave-dependent counties of the MS compared similar counties in the NMS. Using the aforementioned fiscal reports, we created a county-level measure of state taxes levied in each county per white capita circa 1860. With these data, we run a series of linear models where we investigate the association between the size of the enslaved population at the county level and state tax revenues. Due to space concerns, we describe the details of these tests and report the results in Appendix G. Briefly, as Table G1 shows, state taxes per white capita are significantly higher in the high slave share counties *and* high slave share counties in the MS collect more taxes than similar counties in the NMS.

In sum, the time-series evidence unambiguously indicates that tax rates and taxes paid by slaveowners in the MS increased significantly more than those on ordinary whites in their states, and vis-à-vis the taxes shouldered by plantation owners in the NMS. Furthermore, the county-level, cross-sectional evidence shows that as of 1860 the levels of taxes on plantation owners were much higher in the MS than in the NMS.

## Collective Goods: Railroads versus Education

A critical aspect of our argument is that economic elites will support increasing taxation on themselves if this revenue funds collective goods that enhance their interests. We now turn to railroad construction and trends in public education; the former disproportionately favored slaveowners, while the latter, presumably, disproportionately benefited the average white citizen.<sup>31</sup>

To measure public support for railroads, we rely on data by Heath (1950), who collected all public (federal, state, and local) spending on railroads in the South prior to 1861. In total, at least \$144 million of public funds were spent constructing railroads in these states prior to 1861 (out of \$252 million total (public and private) spending on railroads in the South (Fishlow 1965, p. 397)).<sup>32</sup> Of the public total, 57% of this came from state governments.<sup>33</sup>

From Heath's data on state government spending, we create three measures, which are presented in Panel A of Table 2. For each measure, the data is presented as an average across the MS (Column 1) and NMS (Column 2), respectively. The p-value of this difference is reported in column 3. The individual state-level data is presented in Appendix Table A21. The first variable in Panel A shows total spending on railroads by state governments in this period as a proportion of each state's white population (*pw*) in 1860. On average, govern-

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<sup>31</sup>Public education was the most significant early redistributionist program in the U.S. and a critical pillar of future American prosperity (Sokoloff and Engerman 2000).

<sup>32</sup>According to Heath (1950, p. 43), the public total is a lower bound, as it excludes many forms of public sector support (e.g., tax exemptions). At the same time, the private sector fraction is an upper bound, as companies often significantly overstated actual paid-in capital.

<sup>33</sup>The rest came from local governments (38%) and the federal government (5%). While sub-state spending was important in a few state (notably Kentucky), all states with above average railroad mileage in 1860 had a majority of public spending on railroads come from the state governments; as shown in Table A21, the three states most reliant on county and municipal expenditures (KY, LA and AL) all had below average railroad mileage by 1860.

ments in the MS spent approximately six times more *pwc* than in the NMS (p-value=0.02). The second measure in Panel A normalizes state spending on railroads by state income in 1860. When accounting for income, the MS still spent three times more (p=0.06). The final measure in Panel A reports the share of public spending on railroads by the state government. In the states in which slaveowners had greater *de jure* control of government we see a much higher proportion of public spending occurring at the state level (p=0.03).

In Panel B, we use railroad mileage data from Atack (2015) to assess the possibility that public spending on railroads merely cloaks rent-seeking corruption by elites in the MS. Whether normalizing total railroad mileage by white population (p=0.11) or state income (p=0.01), the MS constructed more railway mileage on average, indicating that the funds translated into output and did not solely line the pockets of governing elites.

Greater public spending on railways in the MS would not be dispositive evidence of our argument that the supply side mattered if the geography of the NMS meant that they needed fewer railroads. While we cannot completely dismiss this possibility, two pieces of evidence suggest that differences in demand were not pivotal. First, historians have demonstrated intense demand for state support for railroads across all of the Southern states and anecdotal evidence points to problems of political supply (e.g., Heath 1950; Goodrich 1974; Larson 2002). Thornton (2014, p. 107), for example, notes the problem of obtaining public support for railroads in highly enslaved, but not malapportioned, Alabama: “Time and again, when a small loan or expenditure could have added millions of dollars to the commerce of the state by facilitating trade, the legislature refused to act.” Second, when we compare railway construction by 1880, we see almost no difference on average across the seven MS in railroad mileage as a share of income (0.0029 vs. 0.0025, p=0.58) and mileage per white capita (0.23 vs 0.19, p=0.36). That is, following the radical change in state governments implemented by Congress during Reconstruction (1867-1877), the two sets of states converged.<sup>34</sup>

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<sup>34</sup>We perform two additional tests to rule out differences in demand. Namely, Callen (2016) finds that in 1860, manufacturing capital and enslaved population size predict whether states

Table 2: State Government Support for Railroads & Public Education, 1860

	Malapportioned States (MS) (average) (1)	Non-malapportioned States (NMS) (average) (2)	Difference- in-Means (p-value) (3)
<b>Panel A: State Government Railroad Spending</b>			
State railroad spending pwc (\$)	30.6	5.6	0.02
State railroad spending / Income (\$)	16.7	4.8	0.06
State spending / Total public railroad spending (%)	72.0	34.3	0.03
<b>Panel B: Total Railway Mileage</b>			
Railway mileage pwc	0.22	0.11	0.11
Railway mileage / Income	0.003	0.001	0.01
<b>Panel C: State Government Education Spending</b>			
State PS spending / Whites, 5-14 (\$)	0.72	0.89	0.58
State PS spending / PS pupils (\$)	2.12	2.06	0.93
<b>Panel D: School Attendance</b>			
Whites attending school / Whites, 5-14 (%)	56.4	63.4	0.21
White PS pupils / Whites, 5-14 (%)	32.4	43.6	0.23

Note: Each value is the average across the malapportioned states (column 1) and non-malapportioned states (column 2). The p-value of the difference in means is shown in column 3. Railroad spending comes from Heath (1950). Railway mileage comes from Atack (2015). *Pwc* indicates per white capita. Education variables were constructed from the 1860 U.S. Census. PS refers to public schools.

We now turn to education, using the 1860 Census, which provides state-level data for white school attendance, pupils in public and private schools, and sources of educational financing. We combine this information with the white school-aged population (ages 5 to 14) and state income to create measures state government education expenditures (Panel C of Table 2) and of attendance rates (Panel D of Table 2). Each measure in both panels demonstrate that significantly greater state government support for railroads did not result in higher public support for education.

In Panel C, we first compare state government spending on public education as a share across the US actively promoted railroad development. Table A22 shows that within the South, our supply-side variable, malapportionment, is a stronger and more consistent predictor of state-level variation in railroad quantity. Table A23, in turn, shows that county slave share strongly predicts where railroads were built within both the NMS and MS.

of the state's white population, ages 5 to 14. On average, it is actually greater in the NMS ( $p=0.58$ ). Next, we calculate state government support as a proportion of public school (PS) pupils. Again, we do not see a significant advantage for the MS, as state government spending per PS pupil was almost identical ( $p=0.93$ ). Similarly, the first measure in Panel D shows that a greater share of white school-aged children attended school in the NMS (63%) than in the MS (56%). The census also asked state superintendents to report the number of pupils in public schools. We use this to measure the share of each state's white school-aged population who were pupils in public schools. Again, the average is higher in the NMS.

## Conclusion

In this paper, we investigate the conditions that lead economic elites to support increasing taxation on themselves. Given the disproportionate influence that elites are widely theorized to exert on politics, this is an important and surprisingly understudied question. We contribute to this literature by identifying simple conditions predicting elite-driven fiscal expansions. Namely, we argue that elites should support increases in the state's taxing power if the following three conditions hold: 1) they have political power in the present period, 2) this power is likely to extend into the future, and 3) their interests can substantially benefit from the production of collective goods. If any of these three conditions does not hold, then we argue we are less likely to see elite support for greater taxation on themselves.

We assess our argument using a panel of state government taxation across fourteen slave states from 1840 to 1860. Exploiting institutional variation across states in legislative representation that provided slaveowning elites in some states a greater and more enduring lock on power, as well as variation in the timing of elite demand for collective goods caused by an international commodity boom, we find strong support for our argument. An important feature of our evidence is its completeness. Not only do we show that both tax rates *and* revenues rose in malapportioned states, but we pinpoint the incidence: the increased burden fell on assets of slaveowners, rather than ordinary citizens. Furthermore, as shown in the text

and Appendix A, the results are robust to competing hypotheses and with alternative econometric models, measures, samples, and types of standard errors. Finally, while we see no differences in public education spending across groups of states, we find that the malappor-tioned state governments spent substantially more on goods preferred by elites (railroads). The expansion of railroads, in turn, bound the elites even tighter to an institution that ex-ploited millions, something first pointed out by the controversial historian Ulrich Phillips.<sup>35</sup> It is certainly plausible that the knock-on effects included an increased willingness of elites to fight for slavery, greater capacity to finance that fight, and an enhanced desire to ensure continued access to coerced labor by any available means, including after the war. In other words, railroads, combined with global demand for southern commodities, meant that the Black population would be a target for continued economic exploitation as long as they remained a source of labor for large landowners.

Our argument and evidence may raise a number of questions that help shed light on other arguments and mechanisms in the comparative political economy literature. When explaining the willingness of elites to expand the fiscal state, recent work has emphasized the importance of passing the incidence of taxation onto other groups in society. We showed that the ability to do so is not necessary for the elite to favor fiscal expansion, though it might be their preferred route. The obvious question is why did Southern elites not allocate the new tax costs associated with such investments to other groups, given their political control? One potential explanation is that relying on other sources of revenue, such as consumption taxes, was not feasible: tax collection in agricultural economies that operate primarily through informal structures – i.e., without broad reliance on banking systems or written records of economic transactions – is costly, as enforcement consumes a large proportion of potential revenue (Moore 2008). Large-scale consumption taxes, for example, might have required better technologies or higher levels of development (Aidt and Jensen 2009).

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<sup>35</sup>Phillips (1908, p. 20) claimed that, “The building of railroads led to little else but the extension and intensification of the plantation system and the increase of the staple output.”

Second, given their lack of influence over spending, non-slaveholding whites might have migrated to other states if the tax burden became too high. Indeed, the threat of migration was not just hypothetical. According to the 1860 Census, approximately 25% of the whites born in the original Southern states, as well as those born in earlier admitted slave states, such as Alabama, Kentucky, and Tennessee, had migrated out of these states. Furthermore, outside of large cities, such as Baltimore, New Orleans and St. Louis, Southern states received few European immigrants.

Finally, taxing other groups would likely have required high levels of coercion and potentially triggered demands for representation among the newly taxed. Extraction without some measure of exchange between the state and societal groups generates resistance (Levi 1988; Bernstein and Lü 2003; Timmons 2005). In any event, the fact that poll taxes, a regressive tax whose incidence fell heavily on non-slaveowning whites, did not rise suggests that Southern elites were unwilling or unable to shift taxes to groups with mobility options and without a significant voice in government.

Another question concerns the institutional settings that are consistent with our argument. That is, what typically fits the combination of unchallenged political control by the rich, no foreseeable threats to their rule, and the existence of cost-effective collective goods that directly benefit the wealthy? We speculate that a surprising answer may be that our arguments may fit well with neither autocracies nor democracies, but rather highly distorted democracies, such as those that restrict suffrage or incorporate other institutions that limit the political power of the masses or the centralization of power in an unelected ruler. We can imagine some places with similar conditions, such as Apartheid-era South Africa, where progressive taxation accompanied and undergirded mass repression. Our paper shows that taxation of the rich, for the rich, by the rich is feasible with a specific set of institutions and economic conditions, and may even suggest a small twist on the fiscal contract adage of “no taxation without representation.” For the rich, “no taxation if others have (or might obtain) representation” may be more appropriate.

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## Online Appendices

# **When do Economic Elites Support Increasing Taxation? Evidence from the American South**

- Appendix A: Supplementary Analysis – additional figures and tables as referenced in the text (pages 1-26)
- Appendix B: Commodity Prices (pages 27-28)
- Appendix C: Data Sources and Construction Details (pages 29-39)
- Appendix D: The Origins and Advantages to Slaveowners Conferred by Malapportionment (pages 40-48)
- Appendix E: Taxation and Public Spending in Georgia and Maryland (pages 49-50)
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## Appendix A: Supplementary Analysis

Table A1: Slave States and Representation

	Basis of Representation	
	Upper house (Senate) (1)	Lower house (H. of Rep.) (2)
<b>Malapportioned States (MS)</b>		
Florida	federal pop.	federal pop.
Georgia	fixed (1)	federal pop.*
Louisiana	total pop.*	total pop.*
Maryland	fixed (1)	total pop.*
North Carolina	taxation	federal pop.
South Carolina	fixed (1)	taxation
Virginia	fixed	fixed
<b>Non-malapportioned States (NMS)</b>		
Alabama	white pop.	white pop.
Arkansas	white males	white males
Kentucky	qualified voters	qualified voters
Mississippi	white pop.	white pop.
Missouri	white pop.	white pop.
Tennessee	qualified voters	qualified voters
Texas <sup>†</sup>	free pop.	free pop.

Note: Federal population refers to the formula in which non-whites (slaves, free blacks) are counted as three-fifths of a person for the purposes of apportionment (as was the case with the US Constitution until the 14<sup>th</sup> Amendment (1868)). An asterisk indicates states in which they had a maximum number of representatives/senators any individual district could be apportioned. The number in parenthesis denotes states in which each administrative district received an equal number of representatives/senators.

Figure A1: Railroads and County Slave Share (% Pop), 1860

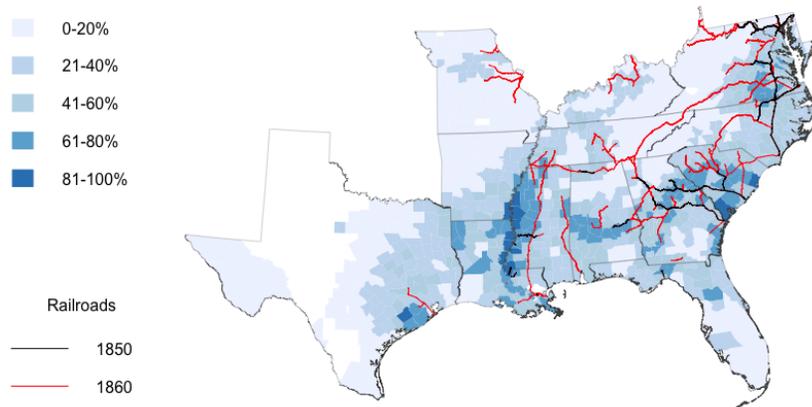


Figure A2: Apportionment Status and Enslaved Share (% of Pop.), 1860

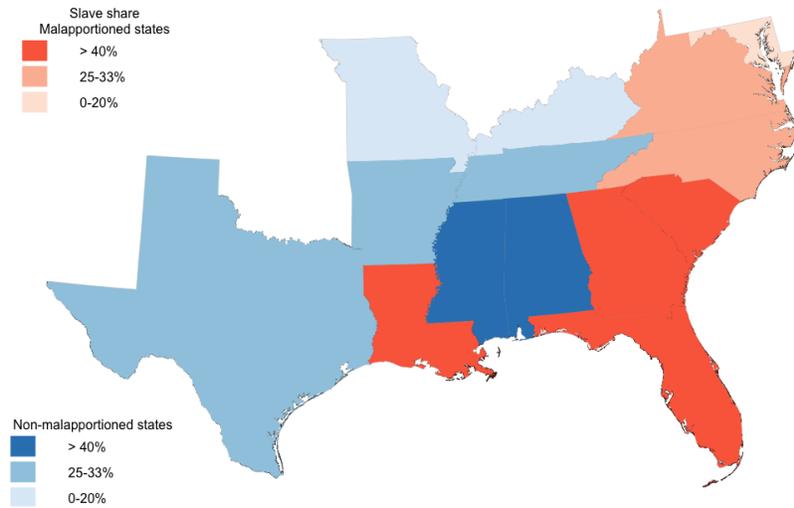
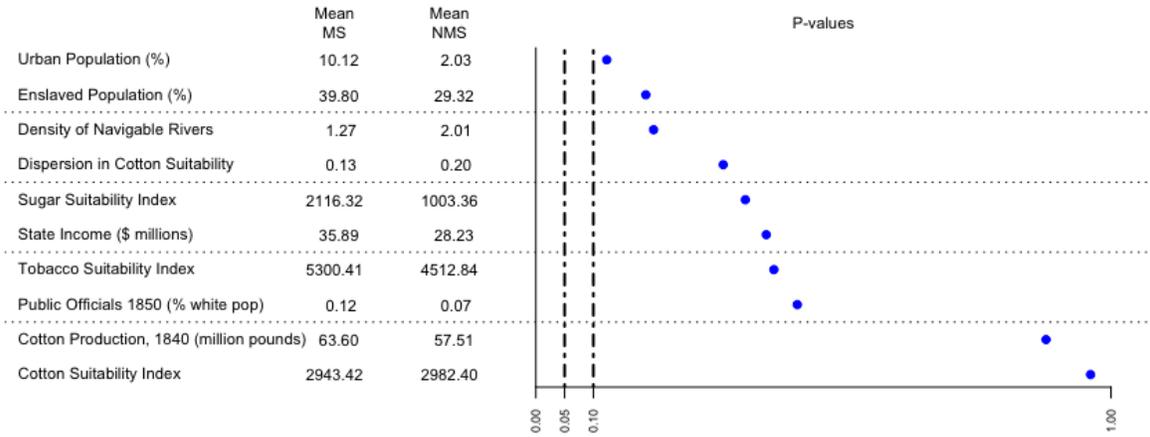


Figure A3: Balance Test for Pre-Treatment Covariates, 1840



*Note:* Cotton, sugar, and tobacco suitability indices represent the maximum potential cotton yield based on soil, climate, and growing conditions as evaluated by the U.N. Food and Agriculture Organization (FAO). Shares of urban and enslaved population, cotton production, and state income are measured in 1840; density of navigable rivers (representing the total length of rivers over the surface area of each state) is obtained from [Atack \(2015a\)](#); and the share of public officials per white capita is measured in 1850 (the previous Census wave does not provide this information). To measure the dispersion of cotton suitability indices across counties within each state, we use the quartile coefficient of dispersion. See [Table C1](#) for more details in sources.

Table A2: Cotton Prices and Taxation Outcomes

	Taxes/Income	Taxes per White Capita	Tax Rate on Slaves (log)	Tax Rate on Land	Poll Tax Rate
	(1)	(2)	(3)	(4)	(5)
Cotton Prices $\times$ Malapportionment	0.836*** (0.263) [0.010]	1.280*** (0.462) [0.047]	0.807** (0.392) [0.104]	0.201** (0.087) [0.128]	-0.165 (0.125) [0.282]
Additional covariates	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
R <sup>2</sup>	0.238	0.217	0.341	0.258	0.370
Observations	277	277	140	242	282
Number of States	14	14	14	14	14

*Note:* Main variables measured as 5-year moving averages. All regressions include state population (log), urban population (log), and income (log) as controls, state-level clustered standard errors in parentheses, and wild-cluster bootstrap p-values in brackets. \*p<0.1; \*\*p<0.05; \*\*\*p<0.01.

Table A3: Time-Invariant Controls: Commodity Price Index and Taxation Outcomes

	<i>Malapportionment</i>				
	Taxes/ Income (1)	Taxes per White Capita (2)	Tax Rate on Slaves (log) (3)	Tax Rate on Land (4)	Poll tax Rate (5)
Commodity Index (log)	-3.200*** (0.826)	-4.262*** (1.150)	0.082 (1.474)	-0.459*** (0.121)	0.186 (0.279)
Commodity Index × Malapportionment	2.717*** (0.562) [0.006]	3.071*** (0.779) [0.030]	1.479*** (0.353) [0.015]	0.342** (0.152) [0.272]	-0.271 (0.200) [0.439]
States	14	14	14	14	14
State FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Additional covariates	Yes	Yes	Yes	Yes	Yes
Time-invariant controls × Year FE	Yes	Yes	Yes	Yes	Yes
Observations	277	277	154	242	282
R <sup>2</sup>	0.536	0.502	0.625	0.497	0.611

Figure A4: Parallel Trends in State Taxation Prior to Increase in Commodity Prices: State Taxes (% Income)

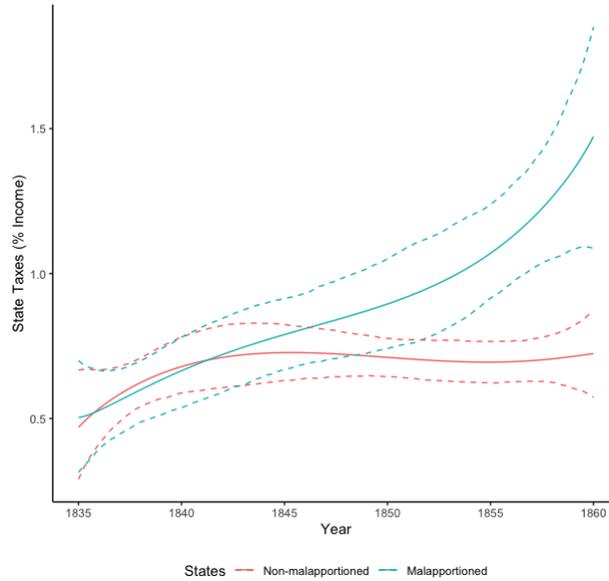
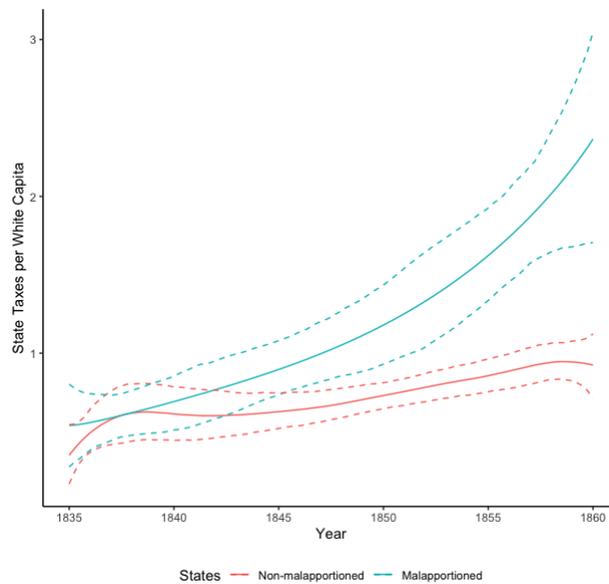


Figure A5: Parallel Trends in State Taxation Prior to Increase in Commodity Prices: Taxes Per White Capita



*Note:* The figures display Nadaraya-Watson non-parametric regressions with Epanechnikov kernel (bandwidths chosen by rule-of-thumb estimator) of state taxes as a share of income (A4) and taxes per white capita (A5) with 95% confidence intervals.

Table A4: Taxation Outcomes: Pre-Treatment Parallel Trends, 1835-1844

	Taxes/Income	Taxes per white capita
	(1)	(2)
Malapportionment $\times$ Trend	-0.006 (0.038)	0.006 (0.018)
Additional covariate	No	No
State FE	Yes	Yes
Year FE	Yes	Yes
Observations	65	65
R <sup>2</sup>	0.002	0.008
States	12	12

*Note:* Main variables measured as 5-year moving averages. State-level clustered standard errors in parentheses. \*p<0.1; \*\*p<0.05; \*\*\*p<0.01.

Table A5: First-Difference Models: Commodity Price Index and Taxation Outcomes

	Taxes/ Income	Taxes per White Capita	Tax rate on Slaves (log)	Tax Rate on Land	Poll Tax Rate
	(1)	(2)	(3)	(4)	(5)
Commodity Index (log)	-0.173* (0.094)	0.032 (0.109)	0.053 (0.148)	-0.014 (0.017)	0.086*** (0.025)
Commodity Index $\times$ Malapportionment	0.291** (0.128)	0.316** (0.149)	0.337* (0.184)	0.058** (0.024)	-0.069** (0.034)
Additional covariates	Yes	Yes	Yes	Yes	Yes
Observations	263	263	126	228	268
R <sup>2</sup>	0.118	0.075	0.200	0.097	0.153
States	14	14	14	14	14

Table A6: First-Difference Models: Cotton Prices and Taxation Outcomes

	Taxes/ Income	Taxes per White Capita	Tax rate on Slaves (log)	Tax Rate on Land	Poll Tax Rate
	(1)	(2)	(3)	(4)	(5)
Cotton Prices (log)	-0.076 (0.085)	0.052 (0.098)	0.069 (0.138)	-0.009 (0.016)	0.040* (0.023)
Cotton Prices $\times$ Malapportionment	0.233** (0.117)	0.299** (0.136)	0.246 (0.183)	0.044* (0.023)	-0.010 (0.032)
Additional covariates	Yes	Yes	Yes	Yes	Yes
Observations	263	263	126	228	268
R <sup>2</sup>	0.115	0.081	0.186	0.084	0.129
States	14	14	14	14	14

*Note:* Main variables measured as 5-year moving averages. All regressions include state population (log), urban population (log), and income (log). \*p<0.1; \*\*p<0.05; \*\*\*p<0.01.

Table A7: State Taxes as a Share of Income

<b>Panel A</b>	<i>Two-Way Fixed Effects</i>					
	Tax Revenue/Income					
	(1)	(2)	(3)	(4)	(5)	(6)
Commodity Index (log)	0.409 (0.342)	0.047 (0.320)	-0.023 (0.305)	-0.051 (0.351)	-0.053 (0.305)	0.176 (0.369)
Commodity Index × Malapportionment		0.847*** (0.285)	0.631*** (0.215)	0.787*** (0.267)	0.753*** (0.217)	0.740*** (0.250)
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Additional covariates	Yes	No	Yes	Yes	Yes	Yes
Observations	277	277	277	214	235	215
R <sup>2</sup>	0.179	0.123	0.232	0.290	0.354	0.316
<b>Panel B</b>	<i>First Difference</i>					
	Tax Revenue/Income					
	(1)	(2)	(3)	(4)	(5)	(6)
Commodity Index (log)	-0.018 (0.065)	-0.194** (0.098)	-0.173* (0.094)	-0.230** (0.113)	-0.178* (0.100)	-0.248** (0.097)
Commodity Index × Malapportionment		0.321** (0.133)	0.291** (0.128)	0.453*** (0.146)	0.268* (0.150)	0.485*** (0.136)
Only seceding states	No	No	No	Yes	No	No
States w/o vote-tax link	No	No	No	No	Yes	No
Without urbanized states	No	No	No	No	No	Yes
Additional covariates	Yes	No	Yes	Yes	Yes	Yes
Observations	263	263	263	203	223	204
R <sup>2</sup>	0.100	0.022	0.118	0.133	0.126	0.156
States	14	14	14	11	12	11

Table A8: State Taxes per White Capita

<b>Panel A</b>	<i>Two-Way Fixed Effects</i>					
	Tax Revenue/White Population					
	(1)	(2)	(3)	(4)	(5)	(6)
Commodity Index (log)	0.256 (0.377)	-0.279 (0.405)	-0.415 (0.418)	-0.653 (0.458)	-0.456 (0.456)	-0.040 (0.422)
Commodity Index × Malapportionment		1.196*** (0.371)	0.981*** (0.323)	1.022*** (0.374)	1.285*** (0.353)	0.692** (0.279)
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Additional covariates	Yes	No	Yes	Yes	Yes	Yes
Observations	277	277	277	214	235	215
R <sup>2</sup>	0.130	0.126	0.203	0.206	0.334	0.268
<b>Panel B</b>	<i>First Difference</i>					
	Tax Revenue/White Population					
	(1)	(2)	(3)	(4)	(5)	(6)
Commodity Index (log)	0.201*** (0.076)	0.008 (0.110)	0.032 (0.109)	-0.034 (0.148)	0.027 (0.116)	0.032 (0.099)
Commodity Index × Malapportionment		0.348** (0.150)	0.316** (0.149)	0.405** (0.191)	0.350** (0.174)	0.337** (0.139)
Only seceding states	No	No	No	Yes	No	No
States w/o vote-tax link	No	No	No	No	Yes	No
Without urbanized states	No	No	No	No	No	Yes
Additional covariates	Yes	No	Yes	Yes	Yes	Yes
Observations	263	263	263	203	223	204
R <sup>2</sup>	0.059	0.043	0.075	0.073	0.075	0.114
States	14	14	14	11	12	11

Table A9: Tax Rate on Slaves (log)

<b>Panel A</b>	<i>Two-Way Fixed Effects</i>						
	Tax Rate on Slaves (log)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Commodity Index (log)	0.133 (1.142)	-1.460 (1.027)	-0.731 (0.942)	-0.634 (1.177)	-0.855 (0.901)	-0.731 (1.143)	-0.881 (0.888)
Commodity Index × Malapportionment		1.062*** (0.407)	0.732** (0.294)	0.963*** (0.284)	0.742** (0.326)	0.973*** (0.323)	0.593** (0.287)
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Additional covariates	Yes	No	Yes	Yes	Yes	Yes	Yes
Observations	140	140	140	110	120	110	140
R <sup>2</sup>	0.282	0.224	0.362	0.467	0.428	0.405	0.390
<b>Panel B</b>	<i>First Difference</i>						
	Taxes on Slaves/Slave Value (log)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Commodity Index (log)	0.226* (0.115)	0.019 (0.154)	0.053 (0.148)	0.034 (0.185)	0.045 (0.148)	0.057 (0.178)	0.072 (0.147)
Commodity Index × Malapportionment		0.490*** (0.187)	0.337* (0.184)	0.463** (0.225)	0.344* (0.192)	0.479** (0.230)	0.327* (0.182)
Only seceding states	No	No	No	Yes	No	No	No
States w/o vote-tax link	No	No	No	No	Yes	No	No
Without urbanized states	No	No	No	No	No	Yes	No
Additional covariates	Yes	No	Yes	Yes	Yes	Yes	Yes
Ad valorem dummy	No	No	No	No	No	No	Yes
Observations	126	126	126	99	108	99	126
R <sup>2</sup>	0.178	0.091	0.200	0.253	0.205	0.222	0.222
States	14	14	14	11	12	11	

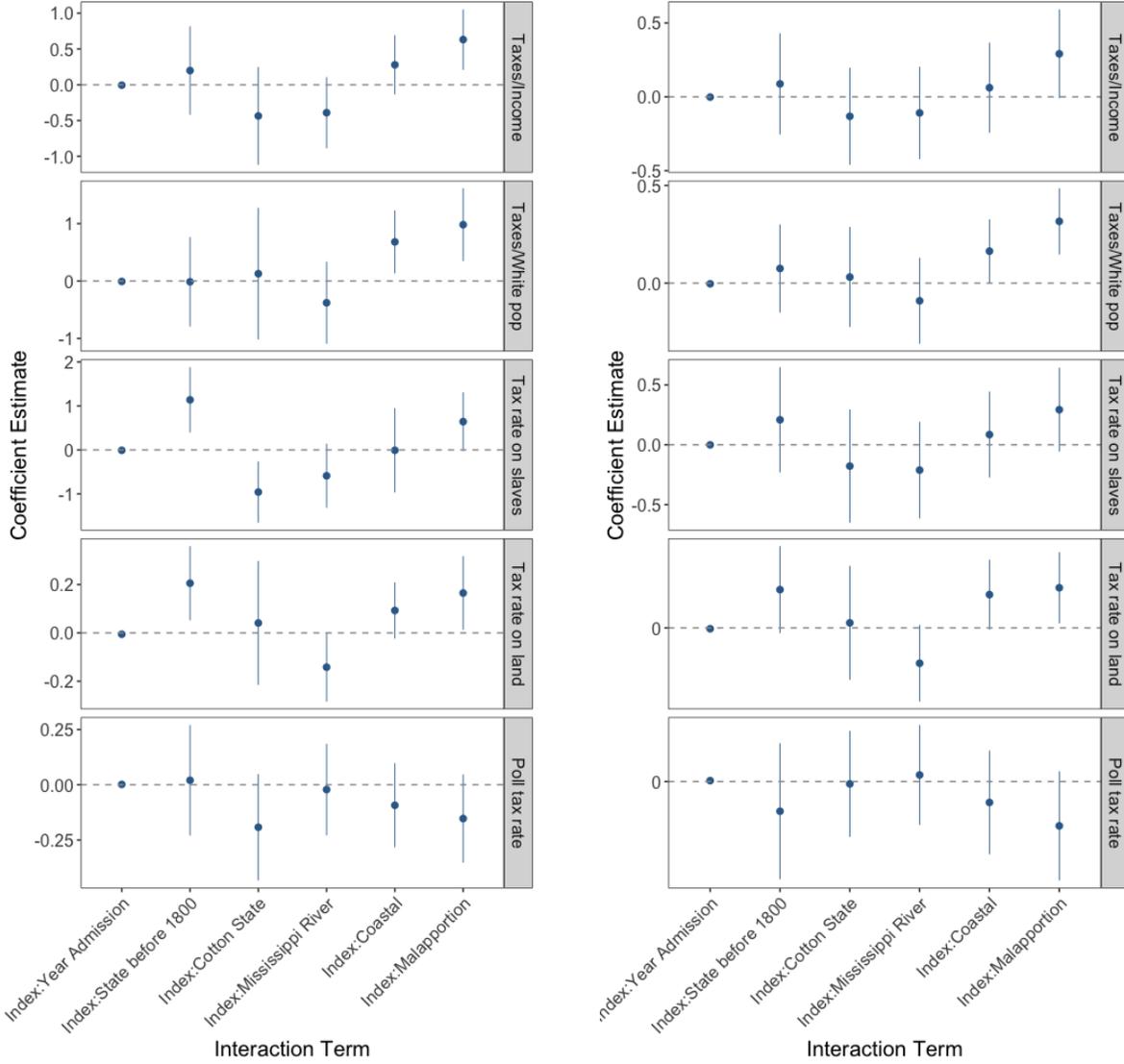
Table A10: Average Ad Valorem Tax Rate on Land

<b>Panel A</b>	<i>Two-Way Fixed Effects</i>					
	Ad Valorem Tax Rate on Land					
	(1)	(2)	(3)	(4)	(5)	(6)
Commodity Index (log)	0.201 (0.129)	0.099 (0.148)	0.036 (0.154)	-0.059 (0.187)	0.204* (0.110)	-0.055 (0.202)
Commodity Index × Malapportionment		0.181* (0.095)	0.165** (0.078)	0.241*** (0.093)	0.097 (0.082)	0.249*** (0.091)
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Additional covariates	Yes	No	Yes	Yes	Yes	Yes
Observations	242	242	242	180	200	188
R <sup>2</sup>	0.194	0.196	0.290	0.370	0.295	0.398
<b>Panel B</b>	<i>First Difference</i>					
	Ad Valorem Tax Rate on Land					
	(1)	(2)	(3)	(4)	(5)	(6)
Commodity Index (log)	0.016 (0.012)	-0.016 (0.018)	-0.014 (0.017)	0.004 (0.025)	-0.010 (0.015)	-0.004 (0.021)
Commodity Index × Malapportionment		0.062** (0.024)	0.058** (0.024)	0.050 (0.032)	0.043* (0.023)	0.061** (0.030)
Only seceding states	No	No	No	Yes	No	No
States w/o vote-tax link	No	No	No	No	Yes	No
Without urbanized states	No	No	No	No	No	Yes
Additional covariates	Yes	No	Yes	Yes	Yes	Yes
Observations	228	228	228	169	188	177
R <sup>2</sup>	0.073	0.035	0.097	0.120	0.068	0.125
States	14	14	14	11	12	11

Table A11: Poll Tax per White Male

<b>Panel A</b>	<i>Two-Way Fixed Effects</i>					
	Poll tax					
	(1)	(2)	(3)	(4)	(5)	(6)
Commodity Index (log)	0.045 (0.130)	0.207* (0.117)	0.149 (0.135)	0.234* (0.133)	0.165 (0.133)	0.164 (0.191)
Commodity Index × Malapportionment		−0.034 (0.116)	−0.153 (0.102)	−0.269** (0.105)	−0.200** (0.088)	−0.217** (0.104)
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Additional covariates	Yes	No	Yes	Yes	Yes	Yes
Observations	282	282	282	219	240	219
R <sup>2</sup>	0.344	0.016	0.376	0.472	0.419	0.459
<b>Panel B</b>	<i>First Difference</i>					
	Poll tax					
	(1)	(2)	(3)	(4)	(5)	(6)
Commodity Index (log)	0.050*** (0.017)	0.080*** (0.027)	0.086*** (0.025)	0.146*** (0.035)	0.089*** (0.024)	0.114*** (0.031)
Commodity Index × Malapportionment		−0.063* (0.036)	−0.069** (0.034)	−0.125*** (0.044)	−0.086** (0.036)	−0.088** (0.044)
Only seceding states	No	No	No	Yes	No	No
States w/o vote-tax link	No	No	No	No	Yes	No
Without urbanized states	No	No	No	No	No	Yes
Additional covariates	Yes	No	Yes	Yes	Yes	Yes
Observations	268	268	268	208	228	208
R <sup>2</sup>	0.140	0.034	0.153	0.223	0.176	0.190
States	14	14	14	11	12	11

Figure A6: Coefficient Estimates of Interaction Terms



(a) Two-Way Fixed Effects

(b) First Difference

*Note:* Coefficient estimates and confidence intervals of interaction between Index (commodity price index) and year of admission to the union, statehood before 1800 indicator, cotton state indicator, Mississippi River indicator, coastal state dummy, and our main moderating variable, malapportionment. All models include income, population size, and urban population (logged) as covariates, robust standard errors in first-difference models, and state-level clustered standard errors for two-way fixed effects models.

Table A12: Commodity Price Index and Taxation Outcomes with Size of Enslaved Population (log) as Control

	<i>Dependent variable:</i>				
	Taxes/Income	Taxes per White Capita	Tax Rate on Slaves (log)	Tax Rate on Land	Poll Tax Rate
	(1)	(2)	(3)	(4)	(5)
Commodity Index (log)	-0.204 (0.307)	-0.587 (0.364)	0.321 (0.813)	0.042 (0.150)	0.140 (0.143)
Enslaved Population (log)	-0.625* (0.378)	-0.593 (0.430)	-0.374 (0.295)	0.014 (0.049)	-0.030 (0.105)
Commodity Index × Malapportionment	0.736*** (0.253)	1.080*** (0.345)	0.647** (0.319)	0.163** (0.078)	-0.148 (0.110)
State FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Additional covariates	Yes	Yes	Yes	Yes	Yes
Observations	277	277	154	242	282
R <sup>2</sup>	0.291	0.233	0.365	0.291	0.377
	<i>First Difference</i>				
	Taxes/Income	Taxes per White Capita	Tax Rate on Slaves (log)	Tax Rate on Land	Poll Tax Rate
	(1)	(2)	(3)	(4)	(5)
Commodity Index (log)	-0.195** (0.095)	0.007 (0.110)	0.072 (0.146)	-0.011 (0.018)	0.085*** (0.025)
Enslaved Population (log)	-0.246 (0.161)	-0.272 (0.188)	-0.220 (0.198)	0.030 (0.030)	-0.017 (0.043)
Commodity Index × Malapportionment	0.318** (0.128)	0.346** (0.150)	0.302* (0.179)	0.054** (0.024)	-0.067* (0.035)
Additional covariates	Yes	Yes	Yes	Yes	Yes
Observations	263	263	140	228	268
R <sup>2</sup>	0.126	0.083	0.177	0.101	0.153

*Note:* Estimation methods are two-way fixed effects and first difference. Main variables measured as 5-year moving averages. All regressions include population size (log), urban population (log), income (log), and size of enslaved population (log) as covariates, and robust standard errors clustered at state level in parentheses. \*p<0.1; \*\*p<0.05; \*\*\*p<0.01

Table A13: Alternative Moderator: Year of Statehood

	<i>Two-Way Fixed Effects</i>				
	Taxes/ Income	Taxes per White Capita	Tax Rate on Slaves (log)	Tax Rate on Land	Poll tax Rate
	(1)	(2)	(3)	(4)	(5)
Commodity Index (log)	11.433 (14.478)	14.259 (14.028)	17.647 (25.533)	10.050*** (3.775)	-2.921 (5.356)
Commodity Index × Year of statehood	-0.006 (0.008) [0.536]	-0.008 (0.008) [0.392]	-0.009 (0.014) [0.634]	-0.006*** (0.002) [0.068]	0.002 (0.003) [0.633]
State FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Additional covariates	Yes	Yes	Yes	Yes	Yes
Observations	277	277	140	242	282
R <sup>2</sup>	0.184	0.134	0.292	0.304	0.349
States	14	14	14	14	14
	<i>First Difference</i>				
	Taxes/ Income	Taxes per White Capita	Tax Rate on Slaves (log)	Tax Rate on Land	Poll tax Rate
	(1)	(2)	(3)	(4)	(5)
Commodity Index (log)	3.017 (6.110)	5.826 (7.103)	1.032 (8.546)	2.181* (1.136)	-2.682* (1.615)
Commodity Index × Year of statehood	-0.002 (0.003)	-0.003 (0.004)	-0.0004 (0.005)	-0.001* (0.001)	0.002* (0.001)
Additional covariates	Yes	Yes	Yes	Yes	Yes
Observations	263	263	126	228	268
R <sup>2</sup>	0.101	0.061	0.178	0.088	0.149
States	14	14	14	14	14

Table A14: Alternative Moderator: Cotton State Indicator

<i>Two-Way Fixed Effects</i>					
	Taxes/ Income	Taxes per White Capita	Tax Rate on Slaves (log)	Tax Rate on Land	Poll tax Rate
	(1)	(2)	(3)	(4)	(5)
Commodity Index (log)	0.254 (0.236)	0.301 (0.234)	2.455** (1.089)	0.217** (0.099)	-0.025 (0.128)
Commodity Index × Cotton State	-0.436 (0.349) [0.299]	0.128 (0.585) [0.861]	-0.959*** (0.355) [0.167]	0.041 (0.131) [0.829]	-0.192 (0.123) [0.163]
State FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Additional covariates	Yes	Yes	Yes	Yes	Yes
Observations	277	277	140	242	282
R <sup>2</sup>	0.198	0.131	0.393	0.198	0.382
States	14	14	14	14	14
<i>First Difference</i>					
	Taxes/ Income	Taxes per White Capita	Tax Rate on Slaves (log)	Tax Rate on Land	Poll tax Rate
	(1)	(2)	(3)	(4)	(5)
Commodity Index (log)	0.021 (0.077)	0.191** (0.090)	0.306** (0.141)	0.014 (0.014)	0.051** (0.021)
Commodity Index × Cotton State	-0.132 (0.140)	0.031 (0.164)	-0.178 (0.181)	0.007 (0.028)	-0.004 (0.038)
Additional covariates	Yes	Yes	Yes	Yes	Yes
Observations	263	263	126	228	268
R <sup>2</sup>	0.103	0.059	0.184	0.073	0.140
States	14	14	14	14	14

Table A15: Alternative Moderator: Statehood before 1800 Indicator

<i>Two-Way Fixed Effects</i>					
	Taxes/ Income	Taxes per White Capita	Tax Rate on Slaves (log)	Tax Rate on Land	Poll tax Rate
	(1)	(2)	(3)	(4)	(5)
Commodity Index (log)	0.203 (0.254)	0.270 (0.299)	1.300 (1.027)	-0.027 (0.157)	0.024 (0.153)
Commodity Index × State before 1800	0.198 (0.316) [0.621]	-0.013 (0.398) [0.976]	1.137*** (0.379) [0.041]	0.205*** (0.078) [0.048]	0.020 (0.128) [0.874]
State FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Additional covariates	Yes	Yes	Yes	Yes	Yes
Observations	277	277	140	242	282
R <sup>2</sup>	0.182	0.130	0.393	0.307	0.345
States	14	14	14	14	14
<i>First Difference</i>					
	Taxes/ Income	Taxes per White Capita	Tax Rate on Slaves (log)	Tax Rate on Land	Poll tax Rate
	(1)	(2)	(3)	(4)	(5)
Commodity Index (log)	-0.074 (0.107)	0.153 (0.125)	0.125 (0.148)	-0.021 (0.021)	0.079*** (0.028)
Commodity Index × State before 1800	0.088 (0.134)	0.075 (0.156)	0.209 (0.194)	0.055** (0.025)	-0.046 (0.036)
Additional covariates	Yes	Yes	Yes	Yes	Yes
Observations	263	263	126	228	268
R <sup>2</sup>	0.102	0.060	0.186	0.092	0.145
States	14	14	14	14	14

Table A16: Alternative Moderator: Mississippi River Indicator

	<i>Two-Way Fixed Effects</i>				
	Taxes/ Income	Taxes per White Capita	Tax Rate on Slaves (log)	Tax Rate on Land	Poll tax Rate
	(1)	(2)	(3)	(4)	(5)
Commodity Index (log)	0.550 (0.375)	0.393 (0.444)	0.079 (1.113)	0.218* (0.114)	0.053 (0.140)
Commodity Index × MS River	-0.390 (0.254) [0.211]	-0.379 (0.365) [0.386]	-0.588 (0.372) [0.342]	-0.142* (0.073) [0.154]	-0.022 (0.106) [0.859]
State FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Additional covariates	Yes	Yes	Yes	Yes	Yes
Observations	277	277	140	242	282
R <sup>2</sup>	0.200	0.141	0.345	0.270	0.345
States	14	14	14	14	14
	<i>First Difference</i>				
	Taxes/ Income	Taxes per White Capita	Tax Rate on Slaves (log)	Tax Rate on Land	Poll tax Rate
	(1)	(2)	(3)	(4)	(5)
Commodity Index (log)	0.030 (0.086)	0.240** (0.101)	0.312** (0.137)	0.038** (0.016)	0.045* (0.023)
Commodity Index × MS River	-0.109 (0.130)	-0.090 (0.151)	-0.211 (0.183)	-0.051** (0.024)	0.010 (0.035)
Additional covariates	Yes	Yes	Yes	Yes	Yes
Observations	263	263	126	228	268
R <sup>2</sup>	0.103	0.060	0.187	0.091	0.140
States	14	14	14	14	14

Table A17: Alternative Moderator: Coastal State Indicator

<i>Two-Way Fixed Effects</i>					
	Taxes/ Income	Taxes per White Capita	Tax Rate on Slaves (log)	Tax Rate on Land	Poll tax Rate
	(1)	(2)	(3)	(4)	(5)
Commodity Index (log)	0.278 (0.366)	-0.064 (0.425)	0.677 (1.204)	0.138 (0.149)	0.088 (0.146)
Commodity Index × Coastal State	0.278 (0.211)	0.682** (0.279)	-0.009 (0.489)	0.093 (0.059)	-0.093 (0.097)
State FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Additional covariates	Yes	Yes	Yes	Yes	Yes
Observations	277	277	154	242	282
R <sup>2</sup>	0.188	0.163	0.272	0.224	0.355
States	14	14	14	14	14
<i>First Difference</i>					
	Taxes/ Income	Taxes per White Capita	Tax Rate on Slaves (log)	Tax Rate on Land	Poll tax Rate
	(1)	(2)	(3)	(4)	(5)
Commodity Index (log)	-0.058 (0.111)	0.093 (0.129)	0.166 (0.191)	-0.014 (0.020)	0.071** (0.030)
Commodity Index × Coastal State	0.062 (0.137)	0.164 (0.159)	0.085 (0.204)	0.048* (0.025)	-0.033 (0.037)
Additional covariates	Yes	Yes	Yes	Yes	Yes
Observations	263	263	140	228	268
R <sup>2</sup>	0.101	0.063	0.154	0.088	0.142
States	14	14	14	14	14

Table A18: Poll Tax Revenue per White Male Capita (\$)

<i>Two-Way Fixed Effects</i>						
Poll tax revenue						
	(1)	(2)	(3)	(4)	(5)	(6)
Commodity Index (log)	0.257 (0.324)	0.825 (0.687)	0.364 (0.307)	0.245 (0.281)	0.374 (0.335)	0.144 (0.320)
Commodity Index (log) × Malapportionment		-0.083 (0.082)	-0.205** (0.092)	-0.223* (0.115)	-0.244** (0.100)	-0.153 (0.131)
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Additional covariates	Yes	No	Yes	Yes	Yes	Yes
Observations	191	191	191	133	158	146
R <sup>2</sup>	0.512	0.121	0.562	0.673	0.602	0.642
<i>First Difference</i>						
Poll tax Revenue						
	(1)	(2)	(3)	(4)	(5)	(6)
Commodity Index (log)	0.072** (0.032)	0.114*** (0.042)	0.135*** (0.040)	0.167*** (0.058)	0.132*** (0.045)	0.127*** (0.047)
Commodity Index × Malapportionment		-0.123* (0.064)	-0.151** (0.062)	-0.185** (0.090)	-0.175** (0.084)	-0.155* (0.083)
Only seceding states	No	No	No	Yes	No	No
States w/o vote-tax link	No	No	No	No	Yes	No
Without urbanized states	No	No	No	No	No	Yes
Additional covariates	Yes	No	Yes	Yes	Yes	Yes
Observations	178	178	178	123	147	136
R <sup>2</sup>	0.109	0.041	0.139	0.152	0.145	0.153
States	14	14	14	11	12	11

Table A19: State Taxes PWC and Tax Rates on Slaves with 1-, 2-, 3-, and 4-Year Lags

	<i>Two-Way Fixed Effects</i>				
	Tax Revenue/White Population				
	(1)	(2)	(3)	(4)	(5)
Tax rate on slaves	0.440*** (0.162)				
Tax rate on slaves (1-year lag)		0.418** (0.201)			
Tax rate on slaves (2-year lag)			0.334* (0.202)		
Tax rate on slaves (3-year lag)				0.251 (0.173)	
Tax rate on slaves (4-year lag)					0.116 (0.162)
State FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Additional covariates	Yes	Yes	Yes	Yes	Yes
Observations	239	227	214	201	194
R <sup>2</sup>	0.265	0.259	0.243	0.217	0.167
States	14	14	14	14	14
	<i>First Difference</i>				
	Tax Revenue/White Population				
	(1)	(2)	(3)	(4)	(5)
Tax rate on slaves	0.152** (0.066)				
Tax rate on slaves (1-year lag)		0.216*** (0.069)			
Tax rate on slaves (2-year lag)			0.122* (0.072)		
Tax rate on slaves (3-year lag)				0.052 (0.074)	
Tax rate on slaves (4-year lag)					-0.001 (0.076)
Additional covariates	Yes	Yes	Yes	Yes	Yes
Observations	225	213	200	187	180
R <sup>2</sup>	0.040	0.061	0.028	0.037	0.031
States	14	14	14	14	14

*Note:* Estimation methods are two-way fixed effects and first-difference. Main variables measured as 5-year moving averages. All regressions include state population (log), urban population (log), and income (log). In parentheses are state-level clusters standard errors for FE models and robust standard errors for FD models. \*p<0.1; \*\*p<0.05; \*\*\*p<0.01

Table A20: State Taxes PWC and Land Tax Rate with 1-, 2-, 3-, and 4-Year Lags

	<i>Two-Way Fixed Effects</i>				
	Tax Revenue/White Population				
	(1)	(2)	(3)	(4)	(5)
Tax rate on land	2.116*				
	(1.144)				
Tax rate on land (1-year lag)		0.600**			
		(0.261)			
Tax rate on land (2-year lag)			0.599**		
			(0.286)		
Tax rate on land (3-year lag)				0.564**	
				(0.282)	
Tax rate on land (4-year lag)					0.418
					(0.289)
State FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Additional covariates	Yes	Yes	Yes	Yes	Yes
Observations	217	202	188	175	167
R <sup>2</sup>	0.302	0.360	0.347	0.324	0.215
States	14	14	14	14	14
	<i>First Difference</i>				
	Tax Revenue/White Population				
	(1)	(2)	(3)	(4)	(5)
Tax rate on land	1.475***				
	(0.382)				
Tax rate on land (1-year lag)		0.396***			
		(0.081)			
Tax rate on land (2-year lag)			0.275***		
			(0.089)		
Tax rate on land (3-year lag)				0.177*	
				(0.091)	
Tax rate on land (4-year lag)					-0.033
					(0.095)
Additional covariates	Yes	Yes	Yes	Yes	Yes
Observations	203	188	174	161	153
R <sup>2</sup>	0.094	0.134	0.067	0.066	0.025
States	14	14	14	14	14

*Note:* Estimation methods are two-way fixed effects and first difference. Main variables measured as 5-year moving averages. All regressions include state population (log), urban population (log), and income (log). In parentheses are state-level clusters standard errors for FE models and robust standard errors for FD models. \*p<0.1; \*\*p<0.05; \*\*\*p<0.01

Table A21: Collective Goods: State Support for Railroads, 1860

	State Government Railroad Spending			Railway Mileage	
	Railroad spending pwc (\$) (1)	Railroad spending sh. income (%) (2)	State sh. public RR spending (%) (3)	Railway mileage pwc (4)	Railway mileage sh. income (%) (5)
<b><u>Malapportioned</u></b>					
Florida	62.7	39.0	89.4	0.52	0.003
Georgia	11.7	7.8	53.9	0.24	0.002
Louisiana	9.5	3.7	38.7	0.09	0.004
Maryland				0.07	0.006
North Carolina	16.9	13.6	89.3	0.14	0.001
South Carolina	33.2	17.2	70.6	0.34	0.002
Virginia	22.7	18.6	75.0	0.17	0.002
<b>AVERAGE</b>	30.6	16.7	72.0	0.22	0.003
<b><u>Non-malapportioned</u></b>					
Alabama	4.1	3.0	36.6	0.14	0.001
Arkansas	0.6	0.4	47.4	0.02	0.000
Kentucky	0.8	0.8	4.5	0.06	0.001
Mississippi	6.0	2.1	50.8	0.25	0.001
Missouri	6.6	6.6		0.08	0.001
Tennessee	20.9	20.7	66.9	0.14	0.001
Texas	0.0	0.0	0.0	0.07	0.001
<b>AVERAGE</b>	5.6	4.8	34.3	0.11	0.001

*Note:* Railroad spending comes from Heath (1950), except for Maryland and Missouri. While Million (1894) provides comparable data for Missouri, we could not find anything for Maryland. Railway mileage was estimated from Atack (2015*b*). *pwc* indicates per white capita.

Table A22: Railroad Miles and Malapportionment, Urbanization, Manufacturing, and Enslaved Share of the Population, 1860

	<i>Dependent variable:</i>			
	Railroad Miles/Income (log)			
	(1)	(2)	(3)	(4)
Malapportionment	0.001** (0.0003)	0.001*** (0.0003)	0.001** (0.0003)	0.001** (0.0004)
Enslaved Population (%)	0.001 (0.001)			-0.0002 (0.001)
Urban Population (%)		-0.002 (0.001)		-0.003 (0.002)
Value of Manufacturing Products (log)			-0.0001 (0.0002)	0.00002 (0.0002)
Observations	14	14	14	14
Adjusted R <sup>2</sup>	0.307	0.410	0.312	0.284
	Railroad Miles per White Capita (log)			
	(1)	(2)	(3)	(4)
	Malapportionment	0.083** (0.036)	0.130*** (0.039)	0.114** (0.038)
Enslaved Population (%)	0.289** (0.129)			0.215 (0.179)
Urban Population (%)		-0.360 (0.209)		-0.168 (0.260)
Value of Manufacturing Products (log)			-0.031 (0.020)	-0.004 (0.026)
Observations	14	14	14	14
Adjusted R <sup>2</sup>	0.491	0.414	0.389	0.415

Table A23: County Enslaved-Population Share and Miles of Railroads in 1860

	<i>County Railroad Miles</i>	
	MS (1)	NMS (2)
Enslaved Pop. Share	2.19*** (0.71)	2.94*** (0.68)
State FE	Yes	Yes
Additional covariates	Yes	Yes
Observations	440	601
States	7	7
R <sup>2</sup>	0.13	0.12

*Note:* Miles of Railway in each county in 1860 was located from Atack (2015*b*). Each model controls for the distance of the county from the nearest coast (either the Atlantic or the Gulf of Mexico), and the miles of navigable rivers in each county.

## Appendix B: Commodity Prices

Figure B1: Cotton Prices (\$) and Commodity Price Index, 1840-1860 (5-year moving average)

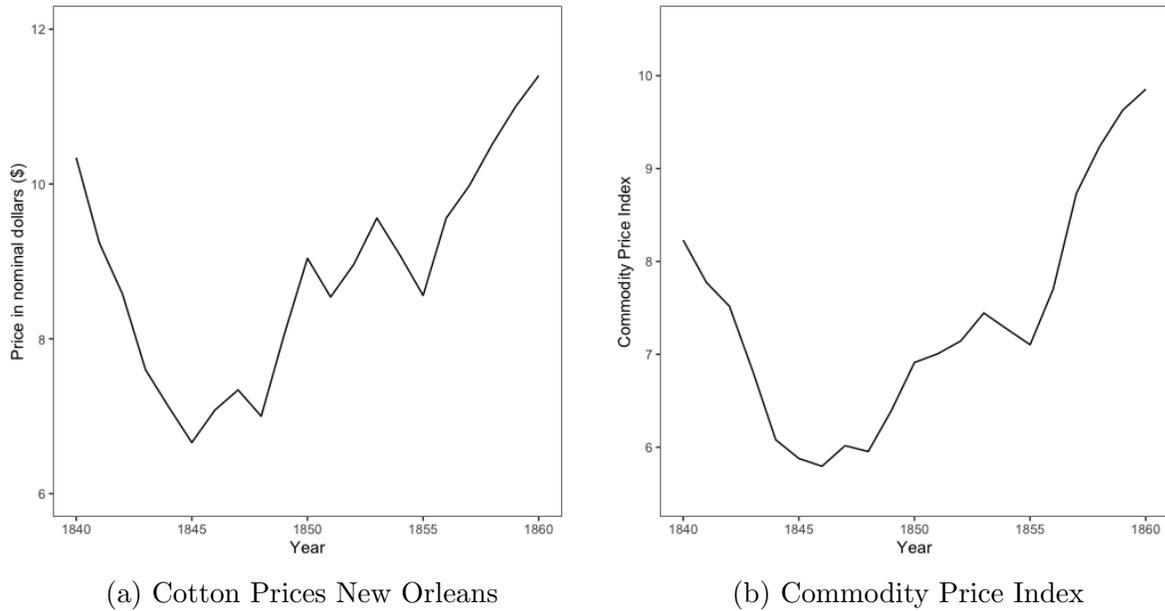
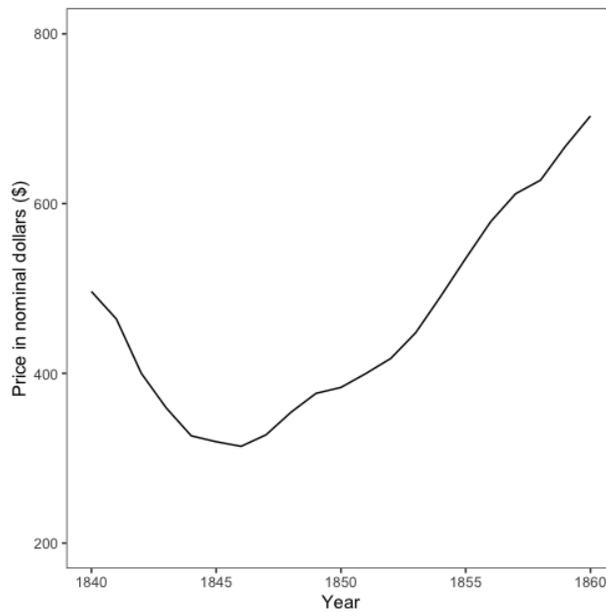


Figure B2: Average Slave Prices



## B.1 Commodity Price Index

The commodity price index reflects the exposure of states to fluctuations in international commodity prices at any given point in time based on their suitability to the cultivation of each crop relative to other Southern states:

$$P_{it} = \frac{Cot_t \times S_i^{cotton}}{Average S^{cotton}} + \frac{Sug_t \times S_i^{sugar}}{Average S^{sugar}} + \frac{Tob_t \times S_i^{tobacco}}{Average S^{tobacco}}$$

$Cot_t$ ,  $Sug_t$ , and  $Tob_t$  represent the average price of cotton, sugar, and tobacco (respectively) at time  $t$ ;  $S_i$  is the suitability of state  $i$  to the cultivation of each crop based on local geographic and climatic conditions. Finally, *Average S* is the average suitability of Southern states for growing each of the three selected crops. Agricultural suitability measures were obtained from the UN Food and Agriculture Organization (FAO) and commodity prices from Gray and Thompson (1933). See table C1 for a full description of sources.

## Appendix C: Data Construction Details

In this appendix, we provide information on the sources for each variable in this paper. This information can be found in Tables C1 (state-level data) and C2 (county-level data). We also provide additional details on the construction of our five primary tax variables that we omitted from the main text due to space constraints.

To construct two of our main dependent variables, we created an annual panel of the amount of state tax revenues in each Southern slave state between 1840 and 1860. This information was collected primarily from census, state treasurer, and auditor reports found in state legislative journals. If we could not locate a state-level report indicating the total amount of taxes in 1860, we used the 1860 Census (which was the only federal source for sub-national tax data in the antebellum period). The sources used to obtain each observation are listed below in Table C3.

We focused specifically on taxes collected and excluded other types of revenue, such as loans and income earned from assets (e.g., land sales, dividends from banks, railroads). In addition to direct taxes (e.g., property taxes, capitation taxes), our data also include indirect taxes, such as licensing fees on occupations and sales taxes. Unfortunately, most states did not include this information in each journal for all sessions. As a result, we have an unbalanced panel. Of the 294 possible state years in our panel, we have located 241 reports. Critically, for each state we have adequate coverage across the period, with at least 13 observations per state and a minimum of two observations for each 5-year period from 1840 to 1860 for almost every state.<sup>1</sup>

With these data, we create two aggregate measures of taxation across states: tax revenue per white capita and tax revenue as a share of state income (i.e., state tax/GSP). Population

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<sup>1</sup>Florida and Texas were admitted in 1845, therefore these cases only have 15 possible state-years (1846-1860), rather than 21 like other states. We only have one observation for Alabama before 1846 and one for Maryland between 1846 and 1850.

figures are taken from US Censuses for 1840, 1850, and 1860, with missing values filled-up via linear interpolation. State income figures are taken from economic historians, who have constructed reliable estimates of total income for each state for 1840 (Easterlin 1960) and 1860 (Gunderson 1974). We use linear interpolation for the intervening period. Two factors may potentially limit the accuracy of our annual estimates. First, reliable annual price deflators for this period, especially at the sub-national level, are difficult to obtain (Lindert and Williamson 2012). Fortunately, inflation seems to have been very low between 1840 and 1860<sup>2</sup>; we thus use nominal values. Second, we know that economic growth was much more rapid in the 1850s than during the first decade of our sample. In particular, the South suffered a severe depression, as exhibited by the decline in cotton prices in the late 1830s, through the mid 1840s.<sup>3</sup> Nonetheless, because this measurement error arguably affects all states equally, it is unlikely that it accounts for the systematic differences we observe in taxation outcomes across malapportioned and non-malapportioned states.

To determine the tax rates on slaves, land, and polls, we consulted the session laws of each state’s legislative sessions between 1840 and 1860 to find the relevant state tax statutes to create an annual panel covering the entire period. Taxes on slaves took two forms. Some states treated slaves as property to which an ad valorem tax rate was applied. The remaining states specified a capitation tax (i.e., tax per slave). For these states, we combined

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<sup>2</sup>Gallman (1966) (Table A-2), for example, estimates that inflation increased only 6% between 1839 to 1860.

<sup>3</sup>The depression was severe enough that four Southern states (Arkansas, Louisiana, Maryland, and Mississippi, as well as the then territory of Florida) defaulted on their state debts. We test whether a state previously defaulted could explain the observed patterns of taxation, as these five states may have been less able to tap debt markets and therefore were forced to rely more on tax revenues. Using the same strategy employed in our various robustness tests (e.g., see Figure 3), we find no meaningful relationship between a prior state default and any of the four tax variables.

the capitation tax (as specified by state law) with census data on the size of the enslaved population to estimate the amount of tax revenue levied on slaves in a given year. Using this information, we then derive the implied tax rate for the state.

To make rates comparable, we made an adjustment depending on whether taxes in a given state applied to all slaves or some portion. For instance, in North Carolina in 1845, males between the ages of 20 and 50 were taxed at 20 cents each, while all other slaves went untaxed. We used our interpolated census data to calculate the number of slaves liable for this tax and multiplied it by the state's capitation tax. In order to derive the tax rate for a given year, we then normalized this value by that of all slaves in the state – i.e., the average value of slaves in 1845 multiplied by the total number of slaves, and not just those liable to taxation. By comparison, Louisiana's capitation tax in 1845 was \$1 per slave, and thus the implied tax rate on slaves was much higher than simply five times more – that is, greater than just 20 cents versus one dollar as written in the statutes. We used this procedure for each relevant state-year to estimate the tax rate on slaves.

It is important to note that this method makes the measure of *tax rate on slaves* more susceptible to the fluctuations in slave prices in the states that used capitation taxes than in those relying on ad valorem rates. This can be problematic for periods in which there is a lot of volatility in slave prices, which was the case during most of the 1840s. Eight of the fourteen states began the period using a capitation tax.<sup>4</sup> By the late 1840s, however, a majority of states had shifted to an ad valorem system *and* slave prices began to exhibit a secular increase with reduced volatility. Therefore, we only use this measure as a dependent variable from 1848 (the first year in which ad valorem taxes are adopted by a majority of states) to 1860.

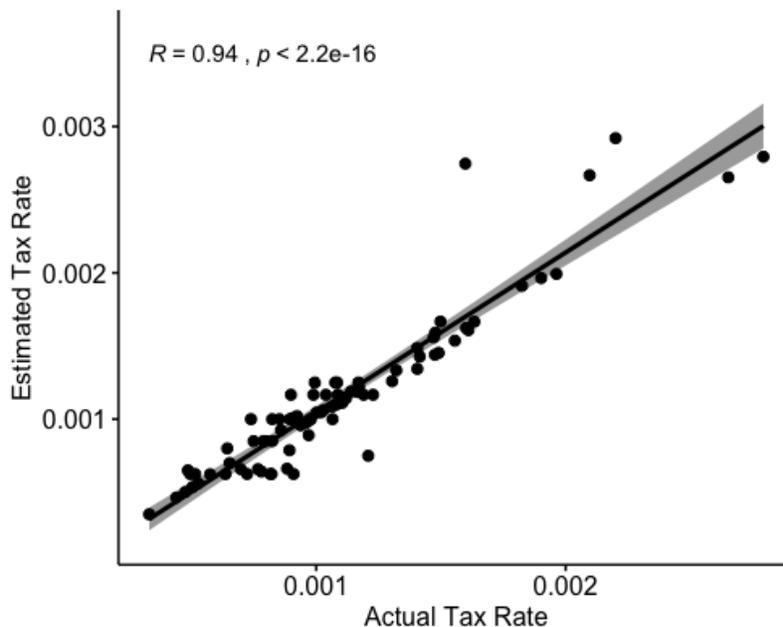
Given the process involved in creating a comparable measure of tax rates on slaves across

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<sup>4</sup>The states that began the period using a capitation tax are: AL, FL, GA, LA, MS, NC, SC, and VA. Three states (LA, GA, and FL) switched to an ad valorem property tax system during the period. No states switched from an ad valorem system to a capitation tax system.

states and years, it is important to validate the efficacy of this measure. We do this by comparing our *estimated tax rate* variable to a measure of *actual (effective) tax rates* on slaves. In many of the reports which we used to collect total state tax revenue, the precise amount levied on slaves was also reported. We then use each state’s slave population and the estimated value of slaves (from the slave prices data reported in Figure B2 in Appendix B)<sup>5</sup> to calculate the *estimated tax rate on slaves*. While these data are not available for all years/states, the available data shows that our estimated variable is very closely correlated with actual tax rates (see Figure C1). This provides confidence that our measure of tax rate on slaves is reasonably accurate.

Figure C1: Actual and Estimated Slave Tax Rate



Second, we use the ad valorem rate of taxation on land, again as specified in the statutes for each state and year. The assessments of taxable property for municipal, county, and state taxation were made by local officials, who had financial incentives to increase collections (by receiving a percentage of the amount of tax revenues they collected). They also had increasingly higher quality information over time. First, almost all states conducted their own

<sup>5</sup>The data from this figure was located in (Ransom and Sutch 1988), Table A.1.

censuses in this period. These censuses often included counts of taxable property (e.g., acres of farm land, farm animals). Furthermore, the US Census provided additional information that state governments used (for state legislative reapportionment, but also tax collections).<sup>6</sup> Beginning in 1840, the Census became increasingly detailed in the information it collected (i.e., information on economic production). The combination of pecuniary incentives and information may, therefore, have mitigated the power of elites to manipulate land values and sunset them in the event of a power transition, but it almost certainly did not eliminate assessment problems. Since we lack consistent data on the value of land in each year, we do not attempt to estimate the actual rate of taxation on land for each state-year as we did with slaves. Instead this measure demonstrates whether states changed the rate of taxation on land during the period under analysis. In short, given the difficulty in assessing real estate at a level commensurate with its true value, our land tax rate measure is our weakest. However, it is important to note that the strategic under-assessment of (land) property values is not a Southern-specific problem and not something we would expect to differ systematically across the MS and NMS; in fact this has been shown to be an endemic problem across the US as whole (e.g., Vollrath 2013; Seligman 1925).

By contrast, because there was a South-wide market for slaves (who were movable), and we have good measures of the price of slaves, we have quality estimates of the underlying true value of this form of property. The question then is whether the official counts of the enslaved are credible and largely accurate. On this, we feel confident that slave counts (the basis of taxes on slaves) were not systematically biased across time or malapportionment status, and that they would have been difficult to sunset. Both states and individual slaveowners had powerful incentives to have a complete count of the enslaved, arguably the largest source of wealth in the American South (Piketty and Zucman 2014). States had frequent reapportionment of the state legislature (often every four years), and local elites had no

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<sup>6</sup>The note by the comptroller/auditor, which typically served as a preface to the report, often referred to information provided by the federal Censuses.

incentive to undermine local population counts (including of slaves) for this purpose. Second, because slaves counted in federal apportionment, Southern states had a strong incentive to count the actual number of enslaved.

Individual slaveowners also had strong incentives against hiding their enslaved property. Because of the extremely liquid and high value of slaves, slaveowners regularly used their slaves as collateral.<sup>7</sup> The value of slavery was also fundamentally enhanced by how much the Southern state protected this form of property. Being able to retrieve runaway enslaved people and to legally pass on slave property required that these assets be legally owned by the proprietor.

Lastly, we construct a measure of the tax incidence on whites from poll taxes (i.e., capitation taxes). We created an annual state panel of poll tax rates from same state statutes used to create the tax rate for land and slaves. For each year and state, this is measured as the amount of poll tax owed by each eligible person (typically an adult male (though, often males above some age threshold - often 50 or 60 years old - were excluded).

For a robustness check, we also created a measure of tax revenues derived solely from poll taxes (just as some reports provided the amount of revenues stemming from taxes on slaves). Where this information was available, we created a panel of tax revenues collected specifically from poll taxes. This measure was used to test whether our tax revenue measures were due to unmeasured changes in income (i.e., “mechanical drift”).

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<sup>7</sup>See González et al. (2017) for a recent and thorough review of the large economic history literature exploring the importance of slaves to the the enslavers’ economic interests as legal property (i.e., beyond being a source of exploited labor).

Table C1: Variables and Sources: Annual State-level Data

	<i>Description</i>	<i>Source</i>
<i>Tax Outcomes</i>		
Tax revenue per white capita, 1840-1860	state tax revenue/total state white population, 1840-1860	see Table B3 for tax revenue sources, US Census (1840, 1850, 1860)
Tax revenue/Income, 1840-1860	state tax revenue/state income, 1840-1860	see Table B3, Easterlin (1960), Gunderson (1974)
Tax rate on slaves, 1848-1860	effective ad valorem tax rate on the value of slaves, 1848-1860	see Appendix C, State session laws, US Census (1840, 1850, 1860)
Tax rate on land, 1840-1860	ad valorem tax rate on the assessed value of land, 1840-1860	State session laws
Poll tax rate, 1840-1860	state-level poll tax (\$) owed by each eligible adult white male, 1840-1860	State session laws
<i>Outcomes: Robustness</i>		
Poll tax revenue, 1840-1860	state revenue collected from poll taxes, 1840-1860	see Table B3
<i>Variables of Interest</i>		
Apportionment status	dummy variable indicating whether the basis of representation in the state legislature is malapportioned in favor of high enslaved-share districts (1) or not (0)	Thorpe (1909)
Cotton Prices, 1840-1860	average cotton prices in New Orleans, 1840-1860	Gray and Thompson (1933)
Commodity Price Index, 1840-1860	average cotton, tobacco, and sugar prices, 1840-1860; maximum potential cotton, tobacco, and sugar yields based on agroclimatic conditions (averages are based on the 'intermediate' and 'high' levels of inputs)	Gray and Thompson (1933), UN Food and Agriculture Organization (FAO)
<i>Main Controls</i>		
State income, 1840-1860	total gross state product (GSP), 1840-1860	Easterlin (1960), Gunderson (1974)
Urbanization, 1840-1860	percentage of state population living in urban areas of at least 2500 residents, 1840-1860	US Census (1840, 1850, 1860)

Table C1: Variables and Sources: Annual State-level Data (*cont.*)

	<i>Description</i>	<i>Source</i>
<i>Balance Test</i>		
Total population, 1840	total state population, 1840	US Census 1840
Enslaved population, 1840	percentage of state enslaved population, 1840	US Census 1840
Urban population, 1840	percentage of state population living in urban areas of at least 2500 residents, 1840	US Census 1840
State income, 1840	total gross state product, 1840	Easterlin (1960), Gundersen (1974)
Cotton suitability	maximum potential cotton yield based on state agroclimatic conditions. Averages are based on the ‘intermediate’ and ‘high’ levels of inputs, 1961-1990	UN Food and Agriculture Organization (FAO)
Sugar suitability	maximum potential sugar yield based on state agroclimatic conditions. Averages are based on the ‘intermediate’ and ‘high’ levels of inputs, 1961-1990	UN Food and Agriculture Organization (FAO)
Tobacco suitability	maximum potential tobacco yield based on state agroclimatic conditions. Averages are based on the ‘intermediate’ and ‘high’ levels of inputs, 1961-1990	UN Food and Agriculture Organization (FAO)
Dispersion of cotton suitability	quartile coefficient of dispersion of cotton suitability across counties within a given state	UN Food and Agriculture Organization (FAO)
Cotton production, 1840	cotton farm production of cotton (pounds)	US Census 1840
State capacity, 1850	number of public officials per white capita	US Census 1850
Density of navigable rivers	length of navigable rivers over state surface area	Atack (2015a)

Table C2: Variables and Sources: County-level Data

	<i>Description</i>	<i>Source</i>
Total population, 1840-1860	total state population, 1840-1860	US Census (1840, 1850, 1860)
State revenue per white capita	state tax revenue/total county white population, circa 1860	See Table B3
Slave population	size of enslaved population, 1860	US Census (1860)
Apportionment status	dummy variable indicating whether the basis of representation in the state legislature is malapportioned in favor of high enslaved-share districts (1) or not (0)	Thorpe (1909)
Urbanization	proportion of total county population living in towns of at least 2,500 residents, 1860	US Census (1860)
Farm values per capita	total value of farms over total county population, 1860	US Census (1860)
Land inequality	Gini coefficient of land ownership in 1860; we aggregate the farm acreage categories of the census into : (1) 3 to 9, (2) 10 to 19, (3) 20 to 49, (4) 50 to 99, (5) 100 to 499, (6) 500-999, and (7) more than 1,000 acres, and use the median acreage in each to estimate the total number of farms correspondingly	US Census (1860)
Manufacturing output per white capita	value of county manufacturing output over white county population, 1860	US Census (1860)
County area	total county area, 1860	US Census (1860)

Table C3: Sources: State Tax Revenue Data

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**Alabama**

*Annual Report of the Comptroller of Public Accounts, 1844*

*Report of the Comptroller on the State of the Finances, 1847*

*Report of the Comptroller on the Subject of Taxation, 1848*

*Biennial Report of the Comptroller of Public Accounts of the State of Alabama*  
(1849, 1851, 1853, 1855, 1857, 1859)

**Arkansas**

*Biennial Report of the Auditor of Public Accounts of the State of Arkansas* (1856  
(Tables I and J, state taxes from 1840-1856), 1858)

**Florida**

“Report of the Comptroller of Public Accounts of the State of Florida.” (Found in  
the *Journal of the Proceedings of the House of Representatives of the General  
Assembly of the State of Florida*, 1848, 1851, 1853, 1859, and 1861).

“Treasurer’s Report.” (Found in the *Journal of the Proceedings of the House of  
Representatives of the General Assembly of the State of Florida*, 1854, 1856,  
1858).

**Georgia**

“Message from the Governor,” (Found in the *Journal of the House of  
Representatives of the State of Georgia at the Annual Session of the General  
Assembly*, 1843, 1845.)

*Annual Report of the Comptroller General of the State of Georgia to the Governor*  
(1847, 1849, 1853, 1854, 1857, 1860, 1861.)

**Kentucky**

*Annual Report of the Auditor of Public Accounts* (1843, 1846, 1850-1859)

**Louisiana**

Prior to change to an ad valorem tax system in 1848, taxes revenues constructed  
from state statutes (as amount of taxation specified by statutes)

*Annual Report of the Auditor of Public Accounts, to the Legislature of the State of  
Louisiana* (1849, 1853, 1854, 1855, 1856, 1858, 1860, 1861)

**Maryland**

“Statement of the Amount of Taxes Due from each county, Howard District &  
Baltimore City, for the years 1841, 1842 & 1843.” [House Document L] (Found  
in the *Journal of Proceedings of the House of Delegates of the State of  
Maryland, 1844*)

*Report of the Comptroller of the Treasury Department to the Governor of  
Maryland* (1850, 1852, 1856, 1858, 1860)

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Table C3 (*cont.*)

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**Mississippi**

“Report of the Treasurer’s Office” (found in the *Journal of the House of Representatives of the State of Mississippi, 1843*)

“Statement of the Auditor of Public Accounts” (found in the *Journal of Senate of the State of Mississippi, 1846*)

“Report of the Receipts and Disbursements of the Public Moneys” (found in the *Journal of Senate of the State of Mississippi, 1850*)

*Report of the Auditor of Public Accounts of the State of Mississippi* (1852, 1856, 1859)

**Missouri**

*Report of the Auditor of Public Accounts of the State of Missouri* (1843, 1846, 1850-1860)

**North Carolina**

*Statement of the Comptroller of Public Accounts of Public Revenue and Expenditure of the State of North Carolina* (1843, 1845, 1849, 1851, 1855, 1857, 1859, 1861)

**South Carolina**

*Report of the Comptroller General to the Legislature of South Carolina* (1841-1861)

**Tennessee**

*Report of the Comptroller of the Treasury of Tennessee* (1841, 1845, 1847, 1849, 1851, 1853, 1857, 1859, 1861)

**Texas**

“Taxable Property, Revenues and Expenditures of Texas.” (1847, 1851-1855)  
(Found in *Texas Almanac for 1857, with Statistics, Historical and Biographical Sketches, Relating to Texas*)

“Treasurer’s Report for the Year 1859.” (1856-1859) (Found in *Texas Almanac and State Industrial Guide for 1860. Statistics of the Counties (Texas) for the Year 1859.*)

**Virginia**

*Annual Report of the Auditor of Public Accounts of the Commonwealth of Virginia* (1841, 1843, 1845, 1847, 1849, 1853, 1859, 1861)

“Message III (of the Governor).” [Doc. No. 1] (Found in *Journal of the House of Delegates of the Commonwealth of Virginia, 1857*)

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## Appendix D: The Origins and Advantages to Slaveowners of Malapportionment

In this section we address two concerns with malapportionment status that space constraints precluded us from detailing in the main text. First, we focus on the origins of malapportionment in the Southern states and whether it can be taken as exogenous. Second, we provide evidence of the importance of malapportionment, and specifically the advantages this *de jure* bias provided to slaveowners in these states.

### The origins and exogeneity of malapportionment

One obvious question is whether malapportionment can be taken as exogenous, as states were not randomly selected into being malapportioned versus non-malapportioned. This concern is mitigated by two unambiguous factors. First, in each of the seven Southern malapportioned states (hereafter, MS), this *de jure* bias in favor of high slave share areas precedes statehood and was transferred to their first state constitution – predating railroads, the invention of the cotton gin and commercialization of cotton, and the end of slave imports into the US in all but two states (Florida and Louisiana). As we discuss later in this section, and while the plantation owners were imminently aware of the protections against redistribution that were being locked in (Green 1966), it is hard to imagine that any could have conceived of the economic riches that technologies, such as the cotton gin, would unlock. More importantly, these biases were incredibly difficult to reform, and required slaveowner consent. Namely, for this source of slave owner political power to be removed would require constitutional reforms which the legislative majorities created by this system of representation were well positioned to block (Chacon and Jensen 2020).

The difficulty in removing this distortion is evident by the fact that none of the states that were initially founded with a biased system reformed to a “one (white) man, one vote” system during the antebellum period (Thorpe 1909). In Figure D1 below, we provide empirical evidence of the persistence of this advantage in the MS. This is not say that reforms to

representative institutions did not occur in this period; rather, none of the many reforms equalized representation to the “one adult white male, one vote” principle. North Carolina, for instance, switched in 1835 from a fixed system (fixed number of members per county without reapportionment) to one that explicitly incorporated slavery and taxes paid into formula for reapportionment (which now occurred every 10 years). Other MS reformed their systems during this period; but, none moved to a white-population basis with regular reapportionment during the antebellum period.<sup>8</sup>

Likewise, in the seven non-malapportioned states, a white population-basis of apportionment with regular reapportionment was adopted from the outset and persisted throughout the antebellum period. Put differently, just as none of the MS reformed to a “one white man, one vote” basis during the antebellum period, none of the NMS deviated from “one white man, one vote” during the period (Thorpe 1909). That is, each state across both types of apportionment remained either malapportioned in favor slave-dependent districts or non-malapportioned from their initial constitution through to the Civil War. See Table 1 of the main text for the basis of apportionment in each state at the end of the antebellum period.

The question then is why did states initially adopt either an inegalitarian or egalitarian system of representation? The historical record suggests that the most important factor was the power of the slaveholding elite at the time of statehood. In turn, the presence of an entrenched pre-statehood slaveowning elite was determined by whether the state had been a

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<sup>8</sup>This failure to “democratize” or equalize representation is unsurprising, as malapportionment of state legislatures across the US was rarely reformed to a “one man, one vote” basis by politics internal to the state. Rather, it was only removed due to the US Supreme Court case of *Reynolds v. Sims* (377 U.S. 533, 1964), which ruled that state legislative malapportionment violated the “equal protection clause” of the 14<sup>th</sup> Amendment. See, for instance, Ansolabehere and Snyder (2008) regarding the importance of this ruling on state politics.

recently settled frontier or if the area had been a long-settled colony of a European power in which slavery was long-established. If the latter, we observe that the new state began with a system of legislative representation that gave disproportionate power to the high slave-share areas in that state. For instance, each of the five original Southern slave states had a fixed system during the British colonial era, which they carried over after independence.<sup>9</sup> The subsequent reforms to these states' systems to base representation on total population (including slaves) or taxation was made when high-slave share regions had disproportionate power and therefore control of the constitutional reform process in these states' various state constitutional conventions.<sup>10</sup> While Louisiana was only admitted as a state in 1812, the French had established chattel slavery in the area more than 100 years earlier. The 1810 Census reveals that the slave population of Orleans Territory (as it was then called) on the eve of statehood was greater than its white population. Similarly, plantation slavery was introduced in Florida, a state admitted in 1845, in the 1760s during British rule. In 1804, it is estimated that nearly half the population was enslaved (Williams 1949). And, the 1830 and 1840 U.S. Censuses both show that approximately the same proportion of what was a rapidly growing population remained enslaved.

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<sup>9</sup>Green (1966, p. 97-98) said that the “framework of the colonial governments and the constitutional usages of the colonial period were to a large extent embodied in the (first) state constitutions...The unequal system established in 1776 gave the eastern sections of the South Atlantic States a majority in and, therefore, control of the legislatures and governments. And it was this same eastern section which was the home of the wealthy, conservative, and aristocratic planter class.”

<sup>10</sup>States across the U.S. used the same basis of apportionment in the state legislature to select delegates to state constitutional conventions, where any reforms would need to be adopted. This meant the legislative majorities in favor of high slave-share areas carried over to the bodies tasked with reforming a state's fundamental political institutions (Chacon and Jensen 2020).

On the other hand, if the area was a recently settled part of the frontier, then it adopted a system of regular reapportionment based on white population in the state's initial constitution. Not only did these territories lack a long-established elite, the dependence on enslaved labor was lower than it would later become.<sup>11</sup> It is also important to note that unlike the prevalence of malapportioned state legislatures in the late 19<sup>th</sup> and early 20<sup>th</sup> century, a "one adult white male, one vote" system was the norm across the US in this period (Ansolabehere and Snyder 2008). Of the 21 (free and slave) non-Original states admitted prior to the Civil War, only Louisiana (1812) and Florida (1845) began with systematically malapportioned state legislatures.<sup>12</sup> While the journals from the initial conventions of the later admitted states indicate that slaveowners wanted to implement biased legislative representation, they tended to lack the political power at the time of statehood to deviate from the prevailing constitutional norms of this period. Once implemented, slaveowners in the non-malapportioned states were unable to alter during the antebellum era their state's fidelity to the "one white man, one vote" principle.

### **The advantages of malapportionment to slaveowners**

Another concern is whether legislative malapportionment actually conferred significant political control to slaveowners in their states. There is a large historical and social science

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<sup>11</sup>Censuses conducted during the territorial period show that slavery was far lower than it would later be in each of the six NMS for which there is pre-statehood data (AL (1810) - 28% slave share, AR (1830) - 15%, KY (1790) - 17%, MS (1810) - 47%, TN (1790) - 10%, TX (1840) - 16%). Even in Alabama and Mississippi, where the incidence of slavery grew rapidly, the territories were mostly a wilderness devoid of white settlers and slaves even twenty years prior to statehood.

<sup>12</sup>Ansolabehere and Snyder (2008) argue that malapportionment of state legislatures only became pervasive after the Civil War, which they attribute to rapid urbanization and its effects on partisan politics.

literature demonstrating the importance of malapportionment in protecting slave interests (Schaper 1901; Green 1966; Watson 1985; Chacon and Jensen 2020). Because this was an era in which governors and the judiciary were institutionally weak (e.g., Thornton (2014, p. 59) said of Alabama that “the legislature was nearly the whole state government”), maintaining majorities in the legislature was the key to controlling Southern state governments.<sup>13</sup>

This literature unambiguously demonstrates that the slaveholding minority in the original slave states saw malapportionment as the primary mechanism for limiting redistributive taxation. This was, for instance, clearly articulated by a state legislator in South Carolina soon after independence in response to calls from under-represented areas away from the coast to use a white basis of population “If representation were apportioned equally,...the system might be built up, the tax on lands might be entirely taken off, and laid wholly on negroes...It might be arranged that... no estate below a certain value should pay any tax at all, while the tax on estates should be raised to equal the wants of the government (as cited in DeSaussure (1795, p. 19)).” Approximately 35 years later, former US president James Madison expressed the same sentiment in a speech at the 1830 Virginia constitutional convention (in which the basis apportionment was the primary issue in dispute): “It is apprehended, if the power of the Commonwealth shall be in the hands of the Majority who have no interest in this species of property (slaves), that, from the facility with which it may be oppressed by excessive taxation, injustice may be done to its owners.”<sup>14</sup> In another Virginia convention held 20 year later in which the equalization of legislative representation remained the key issue of dispute, a slaveowning delegate echoed the same logic: “Congregated in one corner of the State are 400,000 slaves, worth near \$150,000,000. Between the owners of this property and

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<sup>13</sup>For instance, governors typically lacked veto power, served shorter terms, and in a few cases were actually selected by the legislature. Legislatures also often possessed appointment power of judges and other key bureaucrats (Tarr 2000).

<sup>14</sup>Excerpted from Madison’s speech as cited in *The Proceedings and Debates of the Virginia State Convention of 1829-1830* [Richmond: S. Shepherd: 1830], p. 538.

that portion of the State containing a majority of the white population, mountains interpose, and no particular tie of business or of social intercourse binds them in inseparable identity of feeling and interest. I tremble when I anticipate the day when the unrestricted control over the powers of this government shall pass into hands not interested in the preservation of that property (as cited in Dinan (2014)).” In each instance, the delegates from the high slave-dependent areas used their majorities to block reforms to fully equalize representation (Green 1966).

While the slaveowning elites’ fear that the majority non-slaveholding voters would increase taxation on their slaves to fund redistributive public goods is well documented by historians, our argument that slaveowners will want to increase taxation on themselves when they have political control is different. Specifically, these distortions to representation did not simply give elites the ability to block pro-redistributive efforts of the majority. Instead, the fact that both chambers of the state legislatures were malapportioned in ways that benefited the high slaveholding regions gave them effective control of their state governments.

We now empirically demonstrate the persistent benefits to slaveowners in the MS - and the absence of this bias to legislative representation in the NMS. We first construct a measure of county-level representation based on the number of members to a state’s upper and lower houses each county elected between 1790 and 1860. This information was coded from the relevant statutes or constitutional provisions specifying the apportionment for each chamber of each legislature for each decade from 1790 to 1860. Following Ansolabehere et al. (2002), we use a measure of representation which is relative to the ”fair” level of each particular state (which they call the Relative Representation Index-*RRI*). Formally, this measure is:

$$RRI_i = \frac{R_{j(i)}/N_{j(i)}}{R_j/N_j}, \quad (2)$$

where the subscript  $j(i)$  indicates that district  $i$  is located in state  $j$ .  $R$  is the number of representatives and  $N$  denotes the voting population. This index creates a common metric across conventions by normalizing the representation of each locality by the voting power

specific to each state. Voters in districts with an index of less (more) than 1 were under-represented (over-represented) in their legislature. An index value close to one corresponds to a level of representation consistent with the “one person, one vote” principle. We take the log to reduce the weight of outliers and the right skew of this variable and use the adult white male (hereafter *AWM*) population as a proxy for the number of eligible voters. As each state legislature was bicameral, we follow Ansolabehere et al. (2002) and take the average across the chambers.

In Figure 3 we present a regression for the counties in the MS (left) and NMS (right) predicting this index based on the level of county slave-population share for each decade from 1830 to 1860. A positive relationship shows that counties with greater slave dependency tend to be overrepresented in their state legislature. All demographic data for each variable was taken from the relevant decennial Census. All models include state fixed effects; therefore all coefficients have a within-state interpretation. Since states entered the Union at different times, each model varies in the number of states included.

Each figure represents the results from each Census-decade by state malapportionment status between 1830 and 1860 and presents a partial-regression leverage plot with the predicted *RRI* on the y-axis against the predicted county slave share on the x-axis. Each marker represents a single county in the corresponding state. Hence, in the figure corresponding to county-level data in the MS in 1830 (upper left panel), the estimated slope of 1.01 (S.E.=0.12) indicates that a percentage point increase in a county’s slave-population share was associated with a nearly 1% increase in the county’s *RRI* in 1830. This implies that a district having roughly 50% of its population enslaved (i.e., one standard deviation above the sample mean), is predicted to have a representation per voter that is more than three times the “fair” level in the particular state. A similarly large distortion is evident in each of the four Census decades for the counties of the MS (left column of figures). If anything, the slope of this relationship increases over time indicating that the bias in favor of higher slave-share counties is increasing over time.

By comparison, this relationship is never significant for any of the four Census decades in the counties of the NMS (right column). In fact, the sign on the coefficient is actually negative in three of the four models.

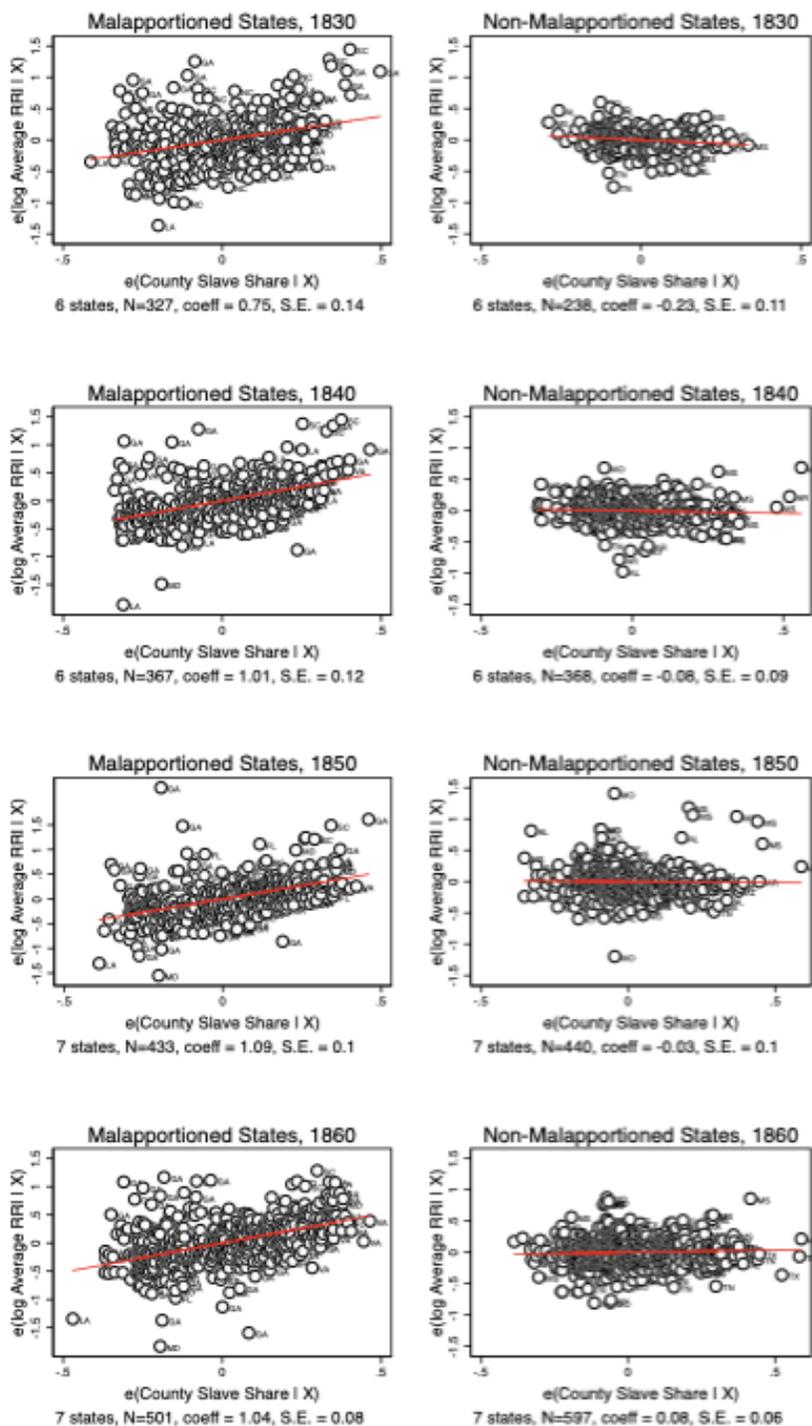
To be clear, our argument does not suggest that slaveowners in the seven NMS, especially the more slave dependent ones, did not also possess political power that was far in excess to their numbers. It is well documented that they exercised disproportionate power and even control of their state governments (Wooster 1969, 1975; Thornton 2014). Yet, in an electoral system in which representation is based on white population, suffrage among white males was unrestricted, and in which a majority of voters were not slaveholders, the ability of slaveowners to control their state's political system likely depended heavily on their ability to persistently act collectively and effectively use their economic resources.<sup>15</sup> As opposed to a persistent and self-enforcing source of *de jure* political power, such as malapportionment, *de facto* power would require elites to continue to expend resources to maintain their preferences over those of the majority non-slaveowners.

In other words, the fact that the system of representation was not biased in their favor meant that their power was always contestable and never guaranteed. As such, elites in the NMS had to weigh the benefits of more collective goods in the current period against the fear that their tax revenues and the state's fiscal apparatus more generally would be in the hands of the non-slaveholding white majority, whose interests might diverge from theirs.

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<sup>15</sup>Acemoglu and Robinson (2008) describe this ability to influence politics despite their small numbers as the *de facto* power that elites can possess. Donnelly (1965) described this as the “traditional powers of the planter oligarchy.”

Figure D1: Malapportionment Status and Overrepresentation of Slaveowners, 1830–1860



*Note:* Each figure presents a regression plot with the predicted (log) *RRI* against predicted county-level slave share (slaves/population) for a corresponding Census decade by malapportionment status (left vs. right). Each dot represents a single county. All models include state fixed effects, and all relevant states then in existence.

## Appendix E: Taxation and Public Spending in Georgia and Maryland

We noted in Section 3.2 that of the seven malapportioned states (hereafter, MS) only two, Maryland and Georgia, either maintained or decreased their rate of taxation during the 1850s. Both states acted similarly to the other MS by raising their taxes significantly in the late 1840s (Maryland) and early 1850s (Georgia). In fact, Georgia even adopted a uniform ad valorem property tax system (i.e., all property, including land and slaves, were subject to the same tax rate) in 1852 that substantially increased the incidence of taxation on the state's slaveowning elite (Wallenstein 1985). Georgia did not, however, maintain these higher taxes for long, as the tax rate was reduced from 0.1% per \$100 of value in 1852 to 0.065% by 1859.

Georgia's story is worth summarizing. Recognizing the potential of its incipient railroad network, state lawmakers sought to publicly support its construction. Despite significantly increasing the capitation tax on slaves in the late 1840s, the difficulties of raising sufficient revenue (given the inefficiencies of the existing tax system) led the state legislature to adopt an ad valorem property tax system in 1852. This immediately increased both overall tax revenues and the relative burden (i.e., vis-à-vis urban areas) shouldered by the state's planters. While total state tax revenues increased by 33% in one year at the same time that the incidence of taxation on urban assets declined by more than half! Much of the new tax revenues were plowed back into the railway system. This new transportation network, in particular, the Western & Atlantic line that pierced the Appalachian Mountains, generated so much revenue that by the late 1850s roughly half the state's budget was met by dividends from railroads. According to (Wallenstein 1985), by the late 1850s, "[public] investments in the Western and Atlantic Railroad began to generate the long-awaited revenue, and tax rates turned down again. Georgia's degree of success, not the quest itself, proved unique among American states." This unique success also explains why Georgia's elites were able to generate collective goods without sustained increases in their tax burden.

Maryland is also an interesting case. Like other malapportioned states, they also raised their property taxes in the late 1840s (from 0.2% per \$100 of value to 0.25% in 1847). Maryland's railroad (and canal) infrastructure also increased substantially over this period, as the state sought to make Baltimore a commercial hub that could compete with other Eastern cities. Unlike other malapportioned states, however, Maryland reduced its property tax rate substantially in 1854 (to 0.15%). Unlike in Georgia, however, this property tax rate cut did not result in a decline in state tax revenues. In 1853, total state tax revenues were roughly \$996,000; in 1856, they totaled roughly \$1,004,000. Yet the share of state taxes coming from property taxes (which fell more heavily on wealthy slaveowners) declined from 48% in 1853 to 35% by 1856. By 1858, this had declined further to 26%.

Maryland's ability to continue to raise substantial revenues while lowering taxes on the slaveowning elites was likely unique among these fourteen states. For one, Maryland's economy was much less dependent on slave labor than other slave states. Furthermore, slavery in Maryland was in decline. The number of slaves in Maryland actually decreased between 1840 and 1860, and the enslaved share of the state's population fell from 19% in 1840 to 13% in 1860. At the same time, the population of Baltimore, which was the nation's fourth largest city in 1860, more than doubled. Unlike other Southern states, the slaveowning elites used malapportionment to pass an increasing share of the state's tax burden onto the growing urban/industrializing sector, largely in the form of indirect taxes (e.g., sales taxes, license/occupation taxes, taxes on banks, etc.). Much state expenditure was, however, directed at enhancing Baltimore's commercial prospects, notably via railroads and canals that expanded the city's connections with the interior and other parts of the Eastern seaboard, maintaining a correspondence between tax and spending incidence.

## Appendix F: Initial Differences in State Capacity

One important question pertains to initial state capacity differences across the MS and NMS and the development of state capacity over time. We use five different approaches to test whether differences in initial state capacity could explain the divergence in observed taxation between the MS and NMS. While none of these approaches to assessing initial capacity rules out differences in initial state capacity on its own, together they provide strong evidence that the differential trends we observe in this period are not attributable to differences in starting points. The evidence pointing to endogenous state capacity is somewhat positive, but less compelling outside of taxes and railroads.

First, to examine starting points, we have extended the period of study back to 1835. This allows us to show that in the ten years prior to the commodity shock of the mid-1840s, both sets of states behaved similarly in terms of taxation. A key assumption required to interpret these results causally is that, in the absence of an increase in commodity prices, taxation outcomes in the MS and NMS would have followed a similar trajectory. Although untestable, this assumption implies that fiscal trends in these two groups of states should be parallel prior to the price shock. Figures A4 and A5 show that this is the case for both state tax revenues as a share of income, and taxes per white capita. In both figures, the trajectories of tax revenues in the MS and NMS are almost identical prior to the rise in commodity prices. By contrast, after 1844, these two groups diverge noticeably, with the MS experiencing larger increases in taxation. In Table A4, we directly investigate whether there are differential taxation trends across groups prior to the commodity price shock. We find that the interaction of a trend variable with malapportionment status has a coefficient that is not statistically distinguishable from zero, indicating that taxes in the MS and NMS have the same slope during this period, which supports the plausibility of the parallel-trends assumption.

While assuaging concerns about parallel taxation trajectories across the MS and NMS, this does not of course directly address the question of differential initial levels of state ca-

capacity. Our second approach is, thus, to use an alternative measure of state capacity (that is separate from taxation) and verify whether groups of states display systematic differences along this dimension. To do so, we focus on a key (input) component of state strength that captures the resources each state has available to implement policies within their territories: the density of government officials in the population, a commonly used measure of state capacity in the PE literature (e.g., Garfias 2018; Suryanarayan and White 2020). The 1850 Census provides a breakdown of occupations for white adults by state, which includes government employees (unfortunately, this information is not available before 1850). For each state, we normalize the number of government officials by the state's white population. We now include a test of differences in means between the MS and NMS that incorporates this variable as well as other state characteristics (the full balance test is presented in Figure A3). While the average is slightly higher in the MS, there is no statistically significant difference in the share of government employees per white capita across these two groups (the result remains unchanged if we normalize by the size of the labor force instead).

A third approach is to consider the actual taxation regimes. Perhaps, the NMS could not increase tax revenues because they relied on less sophisticated tax assessment and collection systems. If anything, we observe that the MS were less sophisticated at the beginning of our study. Colonial and early Republic-era tax systems tended to be extremely unsophisticated (Einhorn 2008; Rabushka 2010). While property taxes were a sizeable portion of tax revenues, they rarely entailed an attempt to systematically assess each individual household's value of real estate and personal property. Instead, certain taxable property (farm animals and equipment, slaves) would be assessed on a fixed/prescribed per item basis, and land would be assigned to a few categories based primarily on its geographic location (i.e., based on soils in the region, access to water, etc.). This was the system that prevailed for assessing land in five of the seven MS at the beginning of our sample (if Florida is counted at 1846) and in six of seven MS for taxes on the enslaved. By comparison, five of the seven NMS had tax regimes that attempted to individually assess the value of household's real estate

and non-enslaved property. Most MS moved to an ad valorem property tax system over the course of our study. They did so precisely because it was a much more efficient and politically fairer way to raise more revenue (Wallenstein 1985).<sup>16</sup> By 1860, all states used an ad valorem system for assessing and taxing land (and non-enslaved private property), and only five states continued to tax slaves on a per capita basis – 3 MS (NC, SC, and VA) and 2 NMS (AL and MS). We take this convergence in taxation systems as additional evidence that the progressive divergence in tax revenues was not due to an initial advantage of malapportioned states with respect to their adopted tax regime – if anything, the opposite was the case.

Fourth, we create a proxy that might capture some aspects of state capacity circa 1840. We use the detail and thoroughness of the auditor, comptroller, and treasurer reports we used to collect the tax revenue data as an alternative measure of state capacity. By the early 1830s, all Southern states had reports that were typed (i.e., not handwritten).<sup>17</sup> Yet, there was great variation across states in terms of the detail they provided (e.g., county-level breakdowns of assessed property by asset, detailed receipts and expenditures, etc.). We counted the number of pages of each report. We found that on average between 1835 and 1844, the MS averaged 23 pages per report. In the NMS, the average report was roughly 45 pages. This was driven primarily by Kentucky, who was already providing the kind of detail that would be the norm by 1860 and especially the post-Civil War period. If we remove Kentucky, the average report in the NMS was 22 pages, almost identical to the average in the MS. Although we think that, on its own, this is very flawed measure – if for no other

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<sup>16</sup>It is clear from Georgia that the change to an ad valorem system not only substantially increased the amount of revenues collected (by roughly 35% in the first year), but shifted the incidence of taxation away from the smaller ‘urban’ sector onto rural elites, especially slaveowners (Wallenstein 1985).

<sup>17</sup>See Emmenegger et al. (2021) for an example showing that this was not the case in Switzerland in the late 19<sup>th</sup> century. The authors use this as a proxy for state capacity across Swiss cantons.

reason that font size varied quite a bit, some used multiple columns per page, reporting structures varied, etc. – in combination with the other three pieces of evidence above, it provides additional supportive evidence that divergence in tax revenues collected does not simply result from differences in initial levels of capacity.

Finally, we examine changes in state capacity over time; namely, whether there was an increase in the size of the bureaucracy per white capita between 1850 and 1860. While this measure increases over time and remains higher in the MS (0.12 vs. 0.09 in 1860; compared to 0.11 vs. 0.07 in 1850), the rate of change is lower and the difference between the MS and NMS is never statistically significant. In other words, outside of railroads and taxes, the MS do not seem to be developing more general capacities, at least as measured by bureaucracy. Furthermore, to the extent that the MS tax assessment and collection systems become more sophisticated over time, these need to be understood as endogenous to the preferences of the elite. Because changes to taxing regimes are durable and not easily reversible, they suggest that elites in the MS were not principally concerned about the risk of future extraction – which is consistent with our theoretical expectations about the effects of political stability in lengthening the time horizons of rulers.<sup>18</sup>

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<sup>18</sup>Other work has emphasized the strategies elites can use to block extraction when it does not serve their interests. Suryanarayan and White (2020), for example, argue that in societies where social status is a cleavage, wealthy and poor members of high-status groups may unite to undermine taxation and bureaucratic capacity. The authors show that, after Reconstruction, taxes and bureaucratic capacity fell in areas where slavery was widespread. Sánchez-Talaquer (2020), in turn, highlights how elites can make themselves “visible” to the state (in order to obtain legal validation of property rights and access enforcement), while simultaneously manipulating assessments of property value to evade taxation.

## Appendix G: The Within-State Incidence of Taxation

In this section, we provide a second incidence test (as a complement to our state-level tests): cross-sectional analysis using county-level data of the within-state distribution of *state* taxes. Specifically, we perform an interaction model which tests whether the incidence of state taxation is rising in county slave population and is greater in the slave-dependent counties of the malapportioned states (hereafter, MS) compared similar counties in the non-malapportioned states (hereafter, NMS). Using the aforementioned reports, we calculated the total amount of state taxes levied across counties within each state circa 1860. We combine state taxes levied in each county with its white population to create a county-level measure of state taxes per white capita (pwc).<sup>19</sup> Using this data, we run a series of linear models where we investigate the association between the size of the enslaved population at the county level and tax revenues.

Table G1 reports the results. Restricting the sample to counties in the MS, columns 1 and 2 investigate the association between the size of the enslaved population and the amount of state taxes collected per white capita across counties. Both specifications account for state fixed effects and column 2 includes county-level economic and demographic covariates – manufacturing output per white capita, farm values per white capita, total population (log), urban population (log), county area (log), and land ownership inequality (Gini). State-level clustered standard errors are used in all models. Using the full sample of counties across all fourteen slave states, columns 3 and 4 investigate the moderating effect of malapportionment. Overall, these results show that state taxes per white capita are significantly higher in the high slave share counties. Specifically, a one standard deviation increase in slave population leads to an increase in taxes per white capita of roughly one third of a standard deviation in MS, versus a mere 0.06 standard deviation expansion in NMS. Critically, the interaction term indicates that high slave population counties in the MS collect more taxes than similar

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<sup>19</sup>We are unaware of any county-level measures of income from this period.

counties in the NMS.

Table G1: Cross-County Models: Enslaved Population and State Taxes per White Capita across Counties, 1860

	<i>Dependent variable: State Taxes pwc</i>			
	7 Malapportioned States		14 Slave States	
	(1)	(2)	(3)	(4)
Enslaved population (log)	0.851*** (0.326)	0.859*** (0.305)	0.305*** (0.068)	0.194* (0.112)
Malapportionment			-1.522 (2.588)	-2.795 (2.083)
Enslaved population (log) × Malapportionment			0.547* (0.320)	0.550** (0.235)
State fixed effects	Yes	Yes	Yes	Yes
Economic covariates	No	Yes	No	Yes
R <sup>2</sup>	0.427	0.669	0.452	0.674
Counties	439	379	1,015	867

Note: OLS county-level cross-sectional estimates with state-level clustered standard errors in parentheses. Models with economic covariates include county-level manufacturing output per white capita, farm values per white capita, urban population (log), total population (log), county area (log), and land ownership inequality (Gini). \*p<0.1; \*\*p<0.05; \*\*\*p<0.01

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