Erasmus University Rotterdam

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MSc Financial Economics

Limited liability and education:

did homestead exemptions boost school funding?

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1. INTRODUCTION

Limited liability is considered one of the most relevant inventions in modern economics. Its importance has been compared to that of the steam engine and likened to the discovery of electricity. It has been presented as an essential precondition for the development of widely held corporations, stock markets and industrial economies (Harris, 2020). In 1948, Merick Dodd asserted that "limited liability may have been a substantial stimulus to large-scale business". By providing individuals the ability to walk away from certain debts and limiting their downside risk, it affects borrowing, investment, and entrepreneurial activity.

The market revolution during the first half of the 19th century brought drastic economical and legislative changes to the United States. It was the era of transitioning from an agrarian into an industrial economy. The transition was a complex process, and hence faced ambivalence and resistance from various interest groups. A free market posed a threat to the material security of small, independent property holders. It was feared to lead to the emergence of a propertyless working class, monopoly, and speculative mentality. On the other hand, many Americans wanted to utilize the increase in wealth and consumption promised by the market revolution. Homestead exemptions emerged as a compromise between the two – a promise that the state stood ready to protect families and their homes against the perils of the market (Goodman, 1993). Other important methods of limiting liability also emerged during this period, such as laws on marital property and bankruptcy.

Empirical research has found a positive link between these laws, investment and entrepreneurship. Bankruptcy laws were confirmed to spur venture capital investment, business start-up, and self-employment within the United States and based on evidence across countries (Xue and Klein, 2010; Armour and Cumming, 2008; Fan and White, 2002). Most researchers agree about the positive effects of limited liability on investment, however there is no consensus on the levels of protection that should be provided. While one group argues that more forgiving bankruptcy laws incentivize undertaking risky investments and motivate risk-averse entrepreneurs to enter the market, others claim that higher levels of exemption limit the supply

of credit and increase interest rates to smaller firms. There seems to be a trade-off between the opposing sides, which will be further investigated in the next section.

Through the mid-19th century, the educational system of the United States also underwent a significant transformation. These changes led to the U.S. becoming the leader in enrollments, in share of GDP and share of public tax money invested in education (Go and Lindert, 2010). The goal of this thesis is to assess whether limiting liability – more specifically homestead exemptions – contributed to this transformation by stimulating investment in education, using data from the mid-19th century United States.

2. LITERATURE REVIEW

2.1. HOMESTEAD EXEMPTIONS

2.1.1. Historical overview

During the long history of commerce, its partakers have sought ways to reduce or even fully remove their liabilities and losses. Roman slaveholders were advised to use slaves instead of employing free agents in their commercial affairs. The masters would in that case only be held liable to the extent of the *peculium* – the assets entrusted to the slave – instead of against all their property. The Rhodian Sea Law and the Byzantine *Chreokoinonia*, the Arabian "licensed slave" and *qirad*, the Italian *commenda* and *compagnia* were all results of creative efforts in limiting liability (Hillman, 1997). *Las Siete Partidas* and *El Fuero Real*, both thirteen-century Castilian codes, were designed to protect certain movables and agricultural goods. Essential necessities such as bedding, clothing, and tools of the trade were shielded from seizure. The provisions were subject to periodic restatements and extensions. Plow animals and implements, seeded lands, breeding stock, horses and arms of knights and gentry, their pay and land, tools of trade of artisans and craftsmen were all gradually included in the provisions.

McKnight (1983) is of the opinion that Castilian exemption principles might have been the predecessors of the homestead exemptions introduced in 19th century United States. In 1829, the Mexican state of Coahuila y Texas formulated an act on the bases of these Castilian laws,

further extending the protection to sovereign grants of land. Only a decade later, the independent Republic of Texas adopted the Texas exemption act of 1839. This act was found to have significant similarities with the 1829 act of Coahuila y Texas. At the same time, Anglo-American practices related to exemptions of movable property seem to have been interweaved into the Hispanic concepts. The generosity of both the 1829 and 1839 acts caused serious issues to American creditors. During this period, Texas gained the reputation of a safe haven for debtors, by protecting the lands, equipment, and machinery from prior liabilities. The laws were, undoubtedly, designed to attract settlers from other states into the sparsely populated areas of Texas (Hynes et al, 2004). The phrase used by contemporaries – Gone to Texas – further backs this intention (Rothman, 2016, as cited in Koudijs and Salisbury, 2019).

The promise of Texan provisions became even more appealing after the Panic of 1837 hit the Southern states. In attempts to prevent high resettling of residents, in 1841, Georgia and Mississippi introduced the first homestead exemption acts in the United States. This led to a movement rapidly sweeping through the South, where states introduced exemption laws to retain population and protect families from financial hardships. By 1859, ten out of fourteen Southern states passed homestead laws. Following the trend, most northeastern and midwestern states had a law enacted by the end of 1850s. During the 1860s, far-western states also began to follow suit (Morantz, 2006).

In the 19th century, homestead exemption statutes primarily had and archaic purpose (Hynes et al, 2004). The main policy behind the acts was to shield certain property from creditors, thereby protecting the family and its home – “an asylum, a refuge which cannot be invaded, nor its tranquility or serenity disturbed” (Rivera, 2004). The wife and children (and with time, other innocent dependents) got protection from the improvidence of those whom they depended on. Through these laws, the debtor himself was also given a chance to start over and avoid incarceration, which was the previous practice (Breitenstein, 2010). All these safeguards assisted in deterring poverty and homelessness.

The laws varied during time and between states. The rules defined who can claim the exemption, based on whether the head of the household is married or single, whether he is elderly or a veteran. Some states required without exception that the debtor have a family, that the property is occupied as a residence, and formally declared as a homestead (Aycock, 1951). With time, the laws also restricted the debtor from waiving the rights to the exemption – in order to borrow – without the consent and signature of both husband and wife (Hynes et al, 2004). The type of debt that could get covered was also defined. In most states, the homestead exemptions did not shield from obligations arising from purchasing, improving or repairing the homestead. Taxes were formerly excluded from the protection of the statutes, but this rule got gradually abandoned (Haskins, 1950).

2.1.2. Benefits

The main benefits of homestead exemptions were summarized by Resnick (1978): 1. providing the debtor with property needed for survival, 2. protecting the dignity, culture, and religious identity of the debtor, 3. enabling the debtor to rehabilitate, 4. protecting the debtor's family, 5. shifting the burden of welfare from society to creditors.

The fundamental purpose of homestead exemptions is to protect the debtor in case of bankruptcy, by allowing him to remove an asset (property) from the prebankruptcy estate, thereby supporting his subsequent rehabilitation. Rivera (2004) opines that requiring debtors to surrender all their property would make them dependent on the state – to provide financial support for their families' basic needs. Hence, preserving a certain proportion of debtors' assets reduces the burden on state finances (Kemner, 1991). By safeguarding their homes, the laws give debtors and their family members financial stability and independence. They can then put money back into the local economy, which benefits the state. Additionally, it is presumed that "debtors whose homes are a protected interest will fulfill obligations more often than debtors whose homes are not protected" (Denzer and Prendergast, 1964; cited in Rivera, 2004).

Conversely, the interests of creditors are opposing to these benefits: their goal is to have as much of their receivables settled as possible. Exemption laws therefore have to balance these conflicting interests, to ensure social welfare and stability. Oftentimes, someone else down the line will have to pay for what the debtor did not (Breitenstein, 2010).

2.1.3. Levels

In the 19th century, the amount or size of the exemption was determined in dollars or acres (or both). Based on the comprehensive review by Goodman (1993), the level of homestead exemption laws at their first introduction ranged between 200 and 5,000 dollars, and between 25 to 160 acres. Certain states additionally distinguished rural and town property (farms and town lots), assigning different maximum values of protection. Subsequent liberalization and refining were common, especially in the Southern states and after the Civil War. A number of more contemporary laws only define an acreage limit (and no dollar limit). These exemptions are considered unlimited: a house of unlimited value can be exempted, as long as the acreage limit is not exceeded (Hynes et al).

As previously mentioned, the level of protection is an important determinant and is widely discussed in the existing literature. Fan and White (2002) assumed that higher (bankruptcy) exemption levels provide a partial wealth insurance and incentivize potential – but risk averse – entrepreneurs to set up an enterprise. They found that the chances of a household owning a business are 35% higher if it is located in a state with unlimited exemptions, compared to a low exemption state.

Cumming and Li (2013) also predicted that higher homestead exemptions encourage undertaking risky investments. However, they found the highest positive impact from the homestead exemption among the bottom quartile homestead exemption states. This is consistent with Koudijs and Salisbury (2019), who emphasize the trade-off between the costs and benefits of limited liability depending on the degree of protection offered. Looking into the passage of marital property laws protecting married women's assets from creditors, they found that

investments of the household increased if most property came from the husband. This means that only shielding a small share of household assets (the wife's) from liability leads to an increase in investments, while higher protection increases the agency costs.

Looking at prior studies, Hynes et al (2004) assert that many states – when considering the introduction of homestead laws – worry that even though they bring protection from income shocks, they increase the cost of credit, especially for the poor. If the protection is too high, and people never have to own up to their debts, no-one would be willing to lend to them. The same is found by (Berkowitz and White, 2002) who examined small firms and their access to credit. If located in a state with higher exemptions, their demand for credit rises, but smaller loans with higher interest rates are provided to them. This credit rationing is likewise confirmed by Armour and Cumming (2008).

Reducing access to credit makes it harder for individuals to obtain funds for profitable investments. Therefore, a balance in the law between protecting debtors and assisting creditors to collect valid debts is highly required. Finding the adequate level then allows for risk sharing between lenders and borrowers and stimulates investment and entrepreneurship. Koudijs and Salisbury (2019) estimate the optimal protection to be around 25% of household assets. A protection of more than 45% eliminates the beneficial impact of limited liability.

2.2. EDUCATION

Nineteenth century public schools evolved from schools built in Massachusetts and Connecticut during colonial times. During the 17th century, the main goal of these schools was teaching basic literacy and arithmetic. They were open to all children but were not obligatory. Financing and management were done collectively and voluntarily. Voters in town meetings decided the local property tax rate which would be allocated to public school funding. However, limited tax bases and resistance to taxation frequently led to insufficient resources for proper functioning of schools (Go, 2009). To ensure adequate levels of funding, parents of pupils were charged a tuition fee in the form of rate bills. Until 1840, more than half of schooling costs were covered by parents and other private sources (Go and Lindert, 2010). This set-up automatically excluded children of poor families from getting an education.

Around 1850, the "common school crusade" swept through the states: a movement of abolishing rate bills and advocating free schooling for every child, entirely funded by local and state revenues (Goldin and Katz, 2003). State and township property taxes, as well as poll taxes were introduced in legislations. Consequently, the tax revenue was redistributed from rich to poor communities, and large property taxpayers removed the burden off parents (with no or small property) to educate their children. There was a transition period, as many districts had free schooling before the official rate bill abolition. Between 1850 and 1870, most American states achieved a free school system supported by property taxes rather than private funding. In this period, the rate bills' share in total school revenues dropped from 22.4 to 7.9 percent. Starting from almost non-existent state taxes, by 1873 they provided 17.3% of total US public school income. Local and state tax support to schools kept rapidly increasing, to reach 100% around 1925. Total public-school income rose from 9.6 million (1850) to 64 million (1870) to 1.8 billion dollars (1925), further illustrating the astounding growth of tax revenues (Go, 2009).

2.3. REGIONAL DIFFERENCES

This section overviews regional differences, which were most pronounced between the Northern and Southern states. During mid-19th century, the South faced greater challenges both in its homestead exemption legislation and education funding.

Texas entered the Union in 1845 and followed the footsteps of Mexican rule in attracting settlers from Southern states: by offering them free land, homestead exemption and family protection. The exemption provision was incorporated into the Texas State Constitution to protect it from legislators (Kemner, 1991). As other states started implementing homestead exemptions, Texas continued liberalizing and refining its legislation, even offering a business "homestead" in 1876 (Goodman, 1993). These circumstances resulted in competition between Southern states to retain their citizens. Liberalizations were consequently more common and generous compared to the North. The postbellum era further intensified legislative enhancements, as the no longer slaveholding South tried to protect the lands and property of former slaveholders from freedmen (Morantz, 2006). For the same reasons, several Southern states included homestead exemptions in their Reconstruction constitutions (Breitenstein, 2010).

When it comes to schooling, Southern states spent more on education compared to the North (with rural regions attracting the fewest dollars). Even so, over 70% of the nation's students and teachers were located in the rural North. According to Go and Lindert (2010), education in the North was more affordable because of higher labor income and more abundant and cheaper teacher supply. To demonstrate the difference: with their daily wages, Massachusetts farm workers could afford nearly double the food supplies that West Virginian farmers could. With regards to teacher supply, they were mostly women due to the early lead of Northern states in female literacy. Preceding the Civil War, almost all white adult females in the North were literate, compared to an illiteracy rate of nearly 20% in the South (Vinovskis, 1987). Additionally, women were pushed towards teaching due to reduced employment opportunities for them in Northern agriculture.

Slaveholding and the resulting elitism played an important role in schooling matters too. Go and Lindert (2010) found that local slave owning was the only significant influence on educational outcomes within the South. The elitist mentality was also spotted by Smith (1980, as cited in Vinovskis, 1987). Education in itself was not highly valued, but planters sent their sons to school in order to gain respect for their families. Slaveholding states allocated less taxes to schools and had lower enrollments. This could be explained by high centralization of Southern policymaking, and limited local voting and autonomy. Northerners therefore had higher chances to raise their own taxes for schools. The interaction of all these conditions led to an uneven rise of public schools in the antebellum United States.

3. EMPIRICAL APPROACH

3.1. CONCEPTUAL FRAMEWORK

This section introduces the conceptual framework and hypotheses.

The main goal of the thesis is to evaluate whether the introduction of homestead exemptions in the mid-19th century United States had an impact on investment in education. The figure below illustrates the proposed conceptual framework.

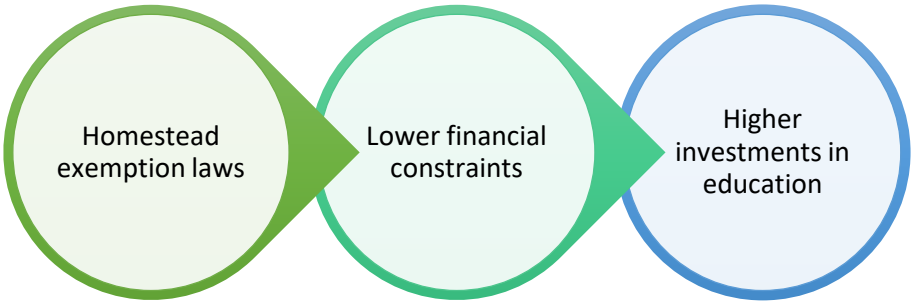


Figure 1.: *Conceptual framework*

The fundamental assumption is that homestead exemption laws played a meaningful role in the transformation of school funding, regardless of them not being the only contributor.

As previously described, homestead exemptions provided higher financial security. Fewer financial constraints supposedly led to more families sending their children to school. More adults chose to enroll in higher education. Citizens voted to pay school taxes since they also had a safety net provided. Reasons should be assessed why parents and taxpayers might have increased their support for education.

In colonial times, settlers had to educate their children at home as no schools were available. By the 19th century, the average parent considered the classroom to be the main place to learn reading and writing, basic skills, and attitudes. Broad public support and faith were given to common schools (Vinovskis, 1987). Most parents preferred sending their children to school, on the condition that they could afford it. Many schools in the first half of the century required additional funding from parents, primarily in the form of rate bills. While they might be viewed as lower costs, education also entailed spending on school supplies, clothing, etc. Considering the financial burden, homestead exemptions were a possible stimulant for parents to invest in their children's schooling.

Farm-owning families and those making a living from agriculture faced another challenge. The assistance of children was indispensable to making the necessary income, particularly during the season. Sending them to school, therefore, meant foregoing a part of the earnings. The school vacation was only harmonized with the agricultural working year after 1880 (Fishlow, 1966). Before the harmonization took place, providing a financial safety net might have overturned the opportunity costs in favor of education.

Various effects of exemption laws on education can also be observed when looking at the average voter. During the 19th century, voting rights gradually became more dispersed between income ranks. As reported by Go and Lindert (2010), ordinary white Americans gained more power against the elites, and extensive voting led to an increase in local tax support for education. A question remained unanswered: how decisions were made in town meetings (or even in referendums), and what incentivized voters to support taxing for education. While an "ends

against the middle" redistributive policy usually prevails, where poor and rich voters unite against the middle class, Stoddard (2012) argues that this was not the case when it comes to school taxes. One might expect that the elite would not have accepted taking over the financial burden from parents to educate their children. Nevertheless, the "free" schooling equilibrium turned out to be a redistribution from wealthy to poorer voters. At the same time, older property owners, even though their children concluded their education, ended up paying for the schooling of younger members of the community (Goldin and Katz, 2003). A financial safety net could have played a role in convincing both middle class and rich taxpayers to accept higher taxation.

Revisionist scholars argue that the wave of introducing free schools was initiated by the elite. During industrialization, capitalists gained benefits from an educated labor force (Vinovskis, 1987). As found in a letter from 1850, contemporaries recognized that "property is deeply interested in the education of all. There is no farm, no bank, no mill, no shop … which is not more valuable and more profitable to its owner if located among a well-educated, than if surrounded by an ignorant population." (Finegan (1921), as cited in Stoddard, 2012).

Finally, positive externalities of public education were emphasized by many authors: faster growth, reduced crime rates, and assimilation of immigrants (Go and Lindert, 2010; Stoddard, 2012). Democratic participation was also positively impacted. More educated lower-income voters benefited the middle class when it comes to redistribution policies. Reviewing these social gains, it is rational to assume that wealthier voters agreed to transfer higher tax prices in order to realize them.

Taking into account all of the above, parents paying rate bills and the later shift to taxation still entailed a higher financial burden. The hypothesis is that by mitigating this constraint, homestead exemptions were a tailwind to education funding.

Figure 2 illustrates the sources of U.S. public schools' revenue 1850-1995, created by Sun Go (2009). Nearly half of the states introduced homestead exemption laws at the starting point of the graph. As the 1840 census does not provide information on educational revenues, it is not possible to observe previous trends. The transition period from rate bills to local and state taxes is clearly visible, including the almost non-existent state taxes in 1850. A 100% tax funding to schools was achieved around 1925. Finally, even though not observable on Figure 3, the striking expansion of funding for education from 9.6 million (1850) to 64 million (1870) is relevant for the hypothesis formulation.

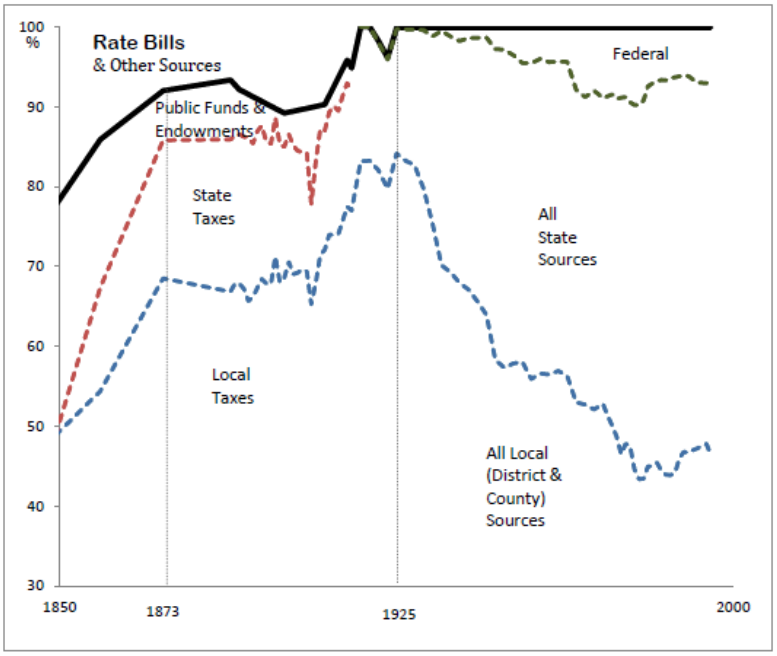


Figure 2.: *Sources of U.S. public schools' revenue 1850-1995*

Source: Go (2009) using the 1850 and 1860 censuses, U.S. Commissioner of Education Reports for 1874-1917, and Historical Statistics of the United States for 1917-1995.

3.2. HYPOTHESES

Two hypotheses will be tested using a difference-in-difference analysis:

H1. States with homestead exemption laws had a higher increase in investments in education, compared to states that did not incorporate the law.

H2. States that introduced homestead exemption provisions had a higher increase in tax funding for education, relative to states with no exemption laws.

The equation below shows the regressions used to test the hypotheses:

$$Y = \beta_0 + \beta_1 D^{Post} + \beta_2 D^{Tr} + \beta_3 D^{Post} D^{Tr} + \gamma + \delta + \varepsilon$$

The dependent variable is the level of investments in education. For testing of the second hypothesis only tax funding will be considered. D^{Post} is the time dummy variable (whether the investment level is from before or after the law introduction). D^{Tr} is the treatment dummy (whether there is a law introduced or not). $D^{Post}D^{Tr}$ is the time^treatment interaction. The coefficient on this interaction (β_3) is the DiD estimate. It is expected to have a positive sign for both hypotheses. γ and δ are state and slaveholding fixed effects, and ε is the error term.

For testing the hypotheses, no independent variables will be included in the regressions. Instead, for H1, three separate regressions are run with the following dependent variables related to education on state level:

- Total educational income scaled by the number of students (Total income/total students),
- Total educational income scaled by the state's population (Total income/population),
- Number of students scaled by population (Total students/population).

To test the impact on tax funding, only data regarding public schools is considered, as college and academy funding mostly came from private sources. This is confirmed in the 1850/1860 decennial censuses where tax funding for colleges and academies is minimal. The dependent variables are the following:

- Public school income from taxation scaled by the number of public schools,
- Public school income from taxation scaled by the number of public-school teachers,
- Public school income from taxation scaled by the number of public-school students,
- Public school income from taxation scaled by population.

4. DATA

Data from two sources should be merged, to obtain sufficient information on:

(1) Investments in education. The ICPSR Census of Population and Housing database contains the relevant breakdowns by types of schools and types of income.

(2) Homestead exemption laws. In his 1993 article, Goodman provides a comprehensive summary, listing the dates of first exemption laws by territory, amounts and acres/town lots exempted in the first law, and subsequent liberalizations.

The first census year to report on educational income is 1850. Census data from 1860 reports according to the same structure of educational institutions. The 1870 census is presented with a different breakdown. Therefore, only the 1850 and 1860 data will be used to avoid potential miscalculations. The uniformity of these two datasets is favorable, considering that the largest number of homestead exemption laws was passed between 1848 and 1852, when eighteen states introduced their first laws. Table 1 lists the relevant years and amounts exempted by state.

Various types of public and private schools are included in the ICPSR census definition of a "school": common schools, grammar schools, academies, high schools, colleges, etc. (Goldin and Katz, 2003). The ICPSR dataset reports three main groups of educational institutions: public schools, academies and colleges. Total income for each group is presented, followed by an additional breakdown to sources which are: endowment, taxation, public funds, and other sources. Also, the number of institutions, teachers, and students is reported in total and by institutional group. Table 2 contains the summary statistics for the main educational data. Additional formulas illustrating how the main variables were calculated can be found in Appendix 1. Summary statistics related to the institutional subgroups are presented in Appendix 2.

Table 1.: *First homestead exemption laws by state*

State	Region	Slaveholding	First Law	Amount exempt in $	Status
Connecticut	North	N	1847*	500	control
Maine	North	N	1849	500	treatment
Massachusetts	North	N	1851/1855	500/800**	treatment
New Hampshire	North	N	1851	500	treatment
Rhode Island	North	N	N/A	-	control
Vermont	North	N	1849	500	treatment
Delaware	South	Y	N/A	-	control
New Jersey	North	N	1852	1000	treatment
New York	North	N	1850	1000	treatment
Pennsylvania	North	N	1849	300	control
Illinois	Midwest	N	1851	1000	treatment
Indiana	Midwest	N	1852	300	control
Michigan	Midwest	N	1848	1500	treatment
Ohio	Midwest	N	1849	500	treatment
Wisconsin	Midwest	N	1848	400***	treatment
Iowa	Midwest	N	1849	500	treatment
Minnesota Territory	Midwest	N	1858	1000***	treatment
Missouri	South	Y	1863	unclear	control
Virginia	South	Y	1867	1200	control
Alabama	South	Y	1843	400***	
Arkansas	South	Y	1852	960***	treatment
Florida	South	Y	1845	200	
Georgia	South	Y	1841	200***	
Louisiana	South	Y	1852	1000	treatment
Mississippi	South	Y	1841	500	
North Carolina	South	Y	1859	500	control
Texas	South	Y	1839/1845	500/2000	
Kentucky	South	Y	1866	1000	control
Maryland	South	Y	N/A	-	control
Tennessee	South	Y	1852	500	treatment
Utah Territory	Far West	Y	1870	1000	control
California	Far West	N	1851	5000	treatment
Oregon Territory	Far West	N	1868	1000	control

** The first law was introduced in Connecticut in 1847. According to Goodman (1993), it was repealed in 1848.*

*** After the first introduction in 1851, Massachusetts liberalized the law in 1855. An average of $650 is considered for the tests.*

**** Where states only determined an exemption limit in acres, the dollar amounts were calculated according to the average farm real estate value in 1850, on state level. Source: Barnard and Jones (1987): Farm real estate values in the United States by counties, 1850-1982.*

The 1850 census contains data on 35 states. New Mexico Territory is excluded from the thesis dataset due to missing data. South Carolina introduced a homestead exemption law in 1851, but recalled it in 1858, so this state is also omitted. Seeing that sufficient information is provided for the remaining 33 states, their status regarding exemption laws between 1850 and 1860 is examined.

Rhode Island, Delaware, and Maryland did not pass exemption laws during the 19th century. Connecticut repealed its law in 1848. As observable in Table 1, a two-year cut-off is applied to define the treatment and control groups. States that introduced the law in and after 1848 are labelled as treated. If the state had no exemption law at all, or introduced it after 1858, it is included in the control group. Pennsylvania and Indiana passed exemption laws in 1849 and 1852. Regardless, they are also classified as controls since the level of exemption is below the median.

The treatment-control split of the main dataset including 28 states is presented in Table 1, and the summary statistics in Table 2. However, to test the robustness of the results, two more datasets are created. One dataset is stricter than the main one, and it contains only states with homestead exemptions introduced between 1849-1852 (26 states including controls). This way Michigan (1848) and Wisconsin (1848) are removed from the dataset. Minnesota Territory (1858) is moved to the control group. Next, a more liberal dataset is created, which includes all states where data are available. A two-year cut-off is used for determining the treatment group, as was done for the main dataset. States that never had or always had exemption laws between 1850-1860 are classified as controls. This results in a dataset of 33 states: adding Alabama, Florida, Georgia, Mississippi, and Texas to the main dataset. All three datasets are then additionally modified using different cut-offs related to exemption levels (0, 300, and 500 dollars) - as a way to further test the robustness. The results are presented in the appendices.

Table 2.: *Summary statistics of main variables*

Variable	N	Mean	SD	Min	Median	Max
Total income	56	1,047,362	1,164,526	280	764,092	5,057,971
Total institutions	56	4,834	5,361	2	3,645	24,970
Total teachers	55	6,122	6,711	2	4,344	32,550
Total students	55	221,414	277,495	24	157,534	1,454,444
Population	56	1,665,919	1,636,396	12,154	1,300,314	7,761,470
Total income / institutions	48	270.64	197.97	75.07	223.88	1,158.78
Total income / teachers	48	208.55	128.79	55.29	196.89	734.67
Total income / students	48	6.3082	4.2904	1.8397	5.6082	22.8551
Total income / population	56	0.6422	0.3239	0.0230	0.5592	1.8666
Students / population	56	0.1205	0.0770	0	0.1025	0.3425
Public school income from taxation	54	392,624	593,636	500	153,505	2,239,742
Public school income from taxation / public schools	54	87.09	80.59	0.56	61.34	303.70
Public school income from taxation / teachers	52	62.97	54.07	0.51	49.59	223.78
Public school income from taxation / students	52	1.8199	1.5347	0.0226	1.6442	6.3218
Public school income from taxation / population	56	0.2095	0.1848	0.0000	0.1662	0.9403
Homestead exemption level	56	615	953	0	500	5,000
Post	56	0.5000	0.5045	0	0.5	1
Treat	56	0.5714	0.4994	0	1	1
Post * treat	56	0.2857	0.4558	0	0	1
Slaveholding	56	1.3571	0.4835	1	1	2

Figure 3 illustrates the division of states into treatment and control. It simultaneously shows slaveholding and free states (American Social History Project, 1996). A complete map of free and slave states is included in Appendix 3.

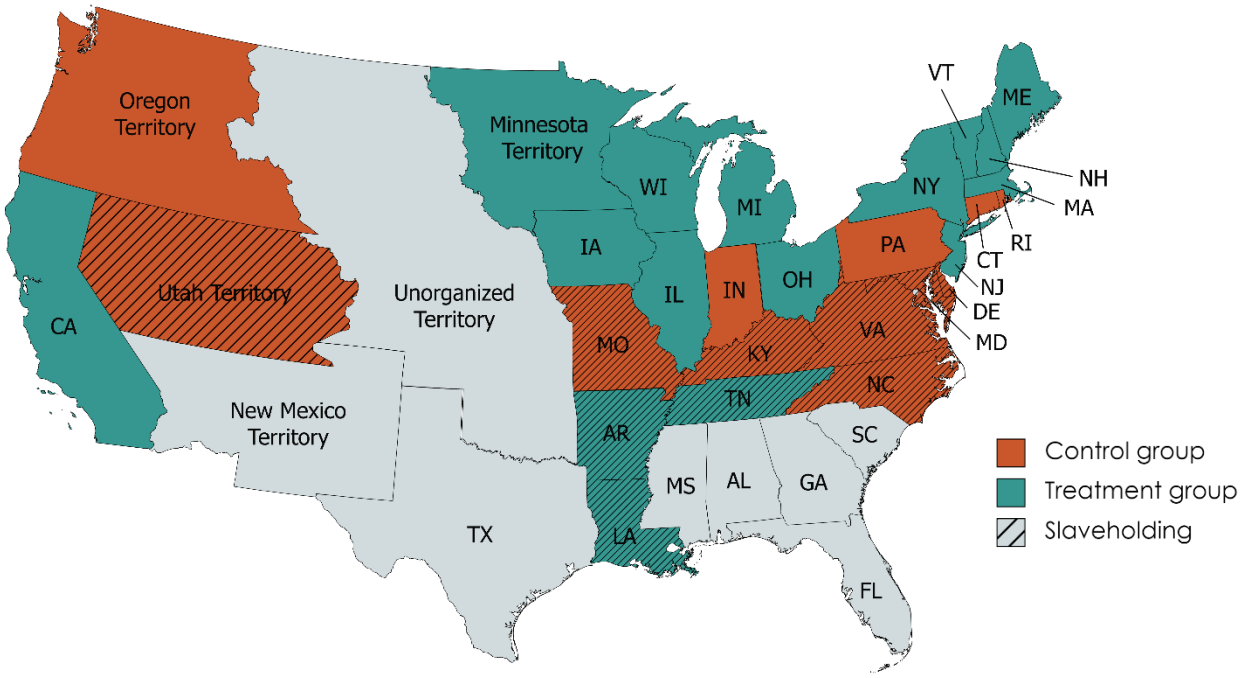


Figure 3.: *Treatment and control group, slaveholding and free states*
Created using: https://historicalmapchart.net/usa-historical

5. RESULTS

This section presents the key results related to the hypotheses.

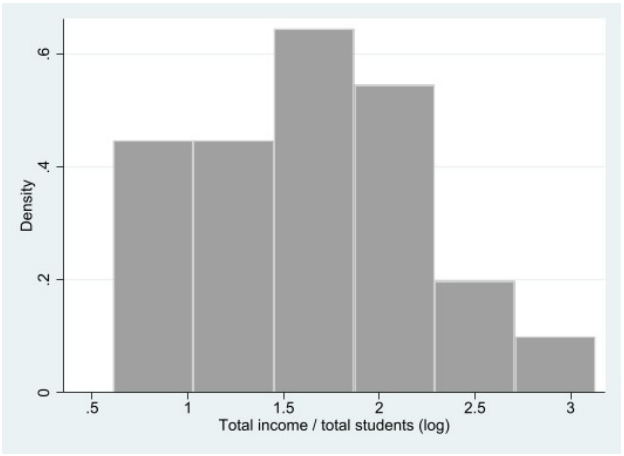
5.1. HYPOTHESIS 1

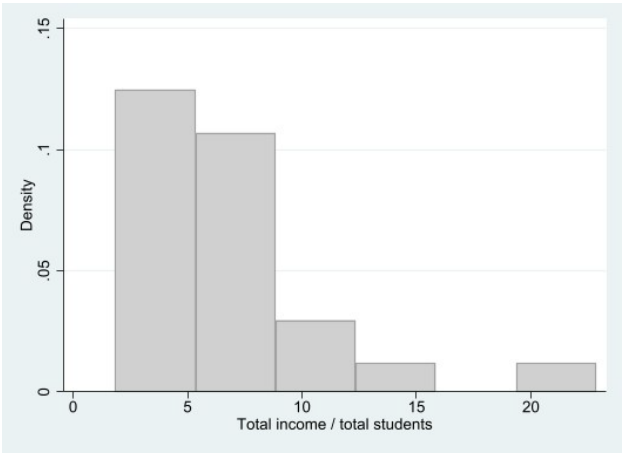
A difference-in-difference analysis is performed on three variables related to education, to evaluate how homestead exemptions impact them. A log transformation of each variable is performed to partially reduce their skewness and improve the validity of the results. Figure 4 contains the histograms illustrating the transformation. The distribution of Total income/total students becomes less skewed after the log transformation. For the other two variables, Total income/population and Total students/population, the distribution changes from right skewed to left skewed. This can be explained by the fact that scaling by (high-valued) population leads to many near-zero values.

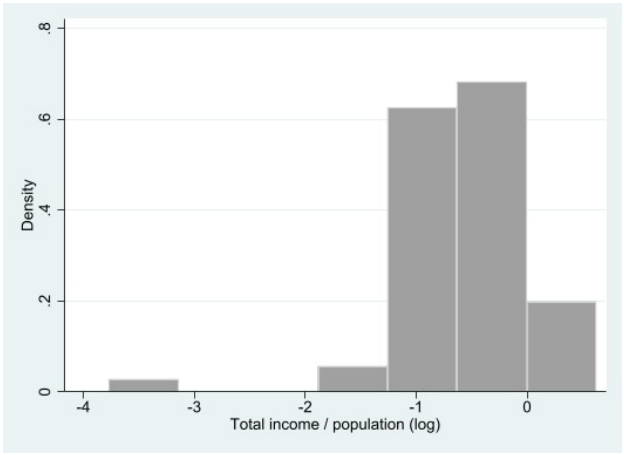
Next, state fixed effects are added to the regressions. As the importance of slaveholding in the Southern states was previously described, a slaveholding fixed effect is also included in the analysis. Assumably, the significant regional differences elaborated previously are also incorporated in this variable: as mainly the Southern states were slave states.

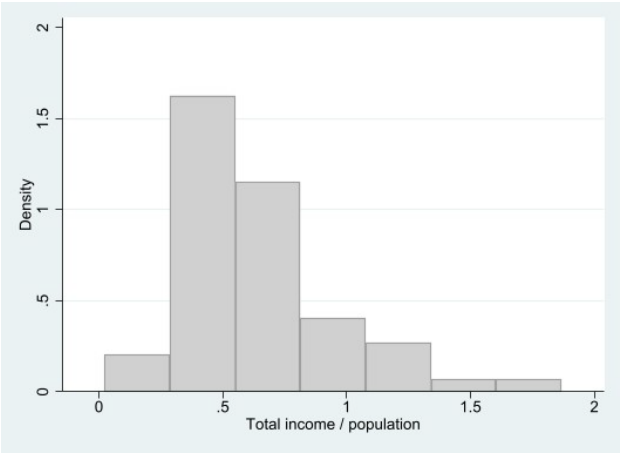
Table 3 contains the output of the difference-in-difference regressions performed on the three main variables. Positive impact of homestead exemptions on total income is found both when scaling by the number of students and by population. The most significant effect is found in relation to the log-transformed total income/total students variable. The diff-in-diff coefficient shows that the introduction of exemptions laws changed the educational income. Particularly, the income per student increased 34% more than it would have without limiting the liability of debtors.

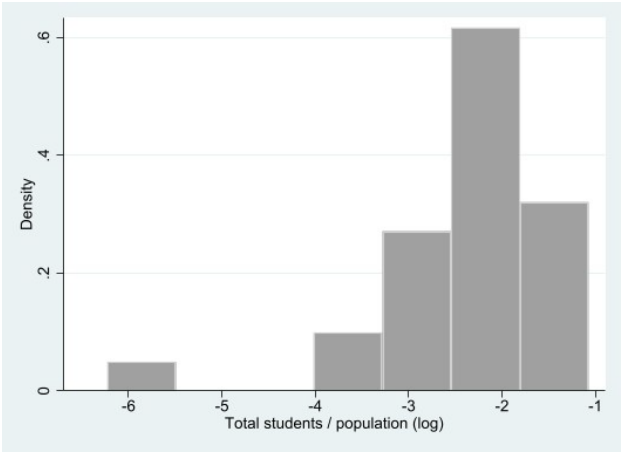
Figure 4.: *Histograms of the main variables related to H1 (log and log-normal)*

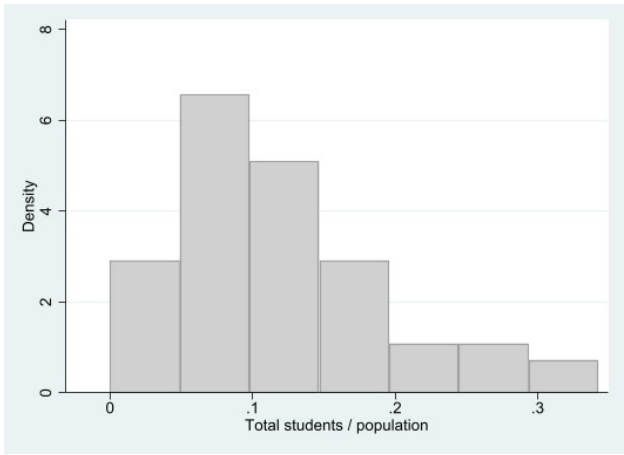












A similar, even though slightly lower effect can be derived from the log-normal output. The constant is significant and different from zero. In 1850, states in the control group had an average educational income of 6.62 (240.79)* dollars per student. The coefficient on the treatment dummy shows us that in 1850, states in the treatment and control group had different incomes per student. Specifically, income in the treatment group was 1.95 (70.93) dollars less than in the control group. The average income in treated states was therefore 4.67 (169.86) dollars per student. The post-treatment coefficient is significant and different from zero when state fixed effects are introduced. Educational income in the control group increased by 1.12 (40.74) dollars on average after 1850, reaching 7.74 (264.57) dollars in 1860. Finally, the β_3 coefficient of the log-normal regression shows that in states that introduced homestead exemptions, the educational income per student increased by 0.90 (30.76) dollars more, than it would have if the laws were not implemented.

When it comes to educational income per capita, the results should be interpreted with caution due to the non-normal distribution of the variable, illustrated in Figure 4. The log-transformed output shows an 87% higher increase of income per capita, compared to a scenario if a law was not implemented in the state. An increase of 0.33 (11.28) dollars is found using the log-normal dataset. Interestingly, the treatment group had a lower income per capita than the control group before the law introductions happened.

* *Values in brackets show the estimated dollar value in 2022 - converted using the CPI inflation calculator, which is based on raw data from the Bureau of Labor Statistics' Consumer Price Index (CPI).*

Table 3.: *Regression output related to H1*

Total income / total students (log)	(1)	(2)	(3)	(4)
post	0.179	0.179**	0	0
	(0.250)	(0.075)	(.)	(.)
treat	-0.528**	0	-0.528**	0
	(0.232)	(.)	(0.219)	(.)
post*treat	0.295	0.295***	0.528	0.228**
	(0.328)	(0.098)	(0.324)	(0.103)
Constant	1.790***			
	(0.177)			
Observations	48	48	48	48

Total income / total students	(1)	(2)	(3)	(4)
post	1.122	1.122***	0	0
	(1.912)	(0.390)	(.)	(.)
treat	-1.951	0	-1.951	0
	(1.770)	(.)	(1.649)	(.)
post*treat	0.895	0.895*	2.848	0.843
	(2.503)	(0.510)	(2.436)	(0.567)
Constant	6.624***			
	(1.352)			
Observations	48	48	48	48

Total income / population (log)	(1)	(2)	(3)	(4)
post	-0.487*	-0.487**	0	0
	(0.245)	(0.197)	(.)	(.)
treat	-0.541**	0	-0.541**	0
	(0.229)	(.)	(0.229)	(.)
post*treat	0.627*	0.627**	0.510	0.531*
	(0.324)	(0.260)	(0.339)	(0.287)
Constant	-0.210			
	(0.173)			
Observations	56	56	56	56

Total income / population	(1)	(2)	(3)	(4)
post	-0.378***	-0.378***	0	0
	(0.123)	(0.0937)	(.)	(.)
treat	-0.279**	0	-0.279**	0
	(0.115)	(.)	(0.115)	(.)
post*treat	0.331**	0.331**	0.273	0.324**
	(0.163)	(0.124)	(0.170)	(0.138)
Constant	0.896***			
	(0.0869)			
Observations	56	56	56	56

Total students / population (log)	(1)	(2)	(3)	(4)
post	-0.477	-0.476	0	0
	(0.401)	(0.328)	(.)	(.)
treat	-0.113	0	-0.113	0
	(0.377)	(.)	(0.367)	(.)
post*treat	0.366	0.366	0.054	0.248
	(0.526)	(0.427)	(0.537)	(0.465)
Constant	-2.170***			
	(0.290)			
Observations	55	54	55	54

Total students / population	(1)	(2)	(3)	(4)
post	-0.048*	-0.048**	0	0
	(0.028)	(0.019)	(.)	(.)
treat	0.051*	0	0.051*	0
	(0.026)	(.)	(0.026)	(.)
post*treat	-0.024	-0.023	-0.046	-0.012
	(0.037)	(0.025)	(0.038)	(0.027)
Constant	0.125***			
	(0.020)			
Observations	55	54	55	54

State FE	NO	YES	NO	YES
Slaveholding FE	NO	NO	YES	YES

State FE	NO	YES	NO	YES
Slaveholding FE	NO	NO	YES	YES

Standard errors in parentheses.
** p < 0.10, ** p < 0.05, *** p < 0.01*

For both variables, adding the slaveholding fixed effect seems to be reducing the impact of homestead exemptions, but it still remains positive. Significant regional differences between the Northern and Southern states are possibly the explanation for such outcomes.

The number of students per total population does not show significant changes after the law introduction. However, it can be seen from the results that this number decreased in the control group after 1850. The treatment group had a slightly higher number of students per capita compared to control group states, even before the laws were introduced. As the effect holds when a slaveholding fixed effect is included, it might be explained by the Northern lead in enrollments during mid-century.

As previously noted, the regressions were also run with variations of the dataset. The states included in the treatment/control group were selected in a different manner when it comes to the year of first law introduction (strict or liberal dataset), and the level of exemption provided (0-, 300- or 500-dollar cut-off). The results can be found in Appendix 4. The effect of homestead exemptions remains positive (and mainly significant) in most dataset variations for the total income per student and per capita variables. The total students per capita variable seems to decrease; however, it is uncertain whether this outcome is due to a faster increase in population compared to student enrollments.

Noticeably, the impact level and its statistical significance gradually fade as the dollar cut-off for the control group is increased. The β_3 coefficient is positive but has lower significance in the main and liberal dataset with a 500-dollar cut-off. It is non-significant for all variables in the strict dataset with 500-dollar cut-off. Once the 500-dollar exemptions are removed from the treatment group, the impact seems to lessen. Even though considerably higher levels of exemption are still considered as treated, it appears that the 500-dollar exemptions are of great importance. This is consistent with Cumming and Li (2013) and Koudijs and Salisbury (2019), finding that liability should be limited only to a certain level in order to spur investments.

5.2. HYPOTHESIS 2

To test whether homestead exemption provisions led to a higher increase in tax funding for education, we start by observing the histograms of the main variables, presented on Figure 5. As both the log-normal and log-transformed variables appear skewed, the results should be interpreted with caution.

The output of the regressions is included in table 4. β3 coefficients show a positive impact of homestead exemptions on all variables. The highest effect is found in regard to public school tax income scaled by the number of public schools, with state fixed effects included. On average, taxes were a source of 70.57 (2,601.17)* dollars of income per public school in the control group in 1850. The treatment group received a lower amount of taxes per school, namely 55.59 (2,049.02) dollars. While the control group received a tax increase of 10.21 (353.66) dollars by 1860, this rise was much higher in states that introduced homestead exemptions during the preceding decade.

Per public school, tax income increase was by 59.05 (2,045.44) dollars higher than it would have been without the exemption laws enacted. Income per teacher and per student also increased more with the laws put into force, by 21.60 (748.20) and by 0.57 (19.74) dollars respectively. In the same way as with Hypothesis 1, slaveholding fixed effects seem to be reducing the impact.

* *Values in brackets show the estimated dollar value in 2022 - converted using the CPI inflation calculator, which is based on raw data from the Bureau of Labor Statistics' Consumer Price Index (CPI).*

Figure 5.: *Histograms of the main variables related to H2 (log and log-normal)*

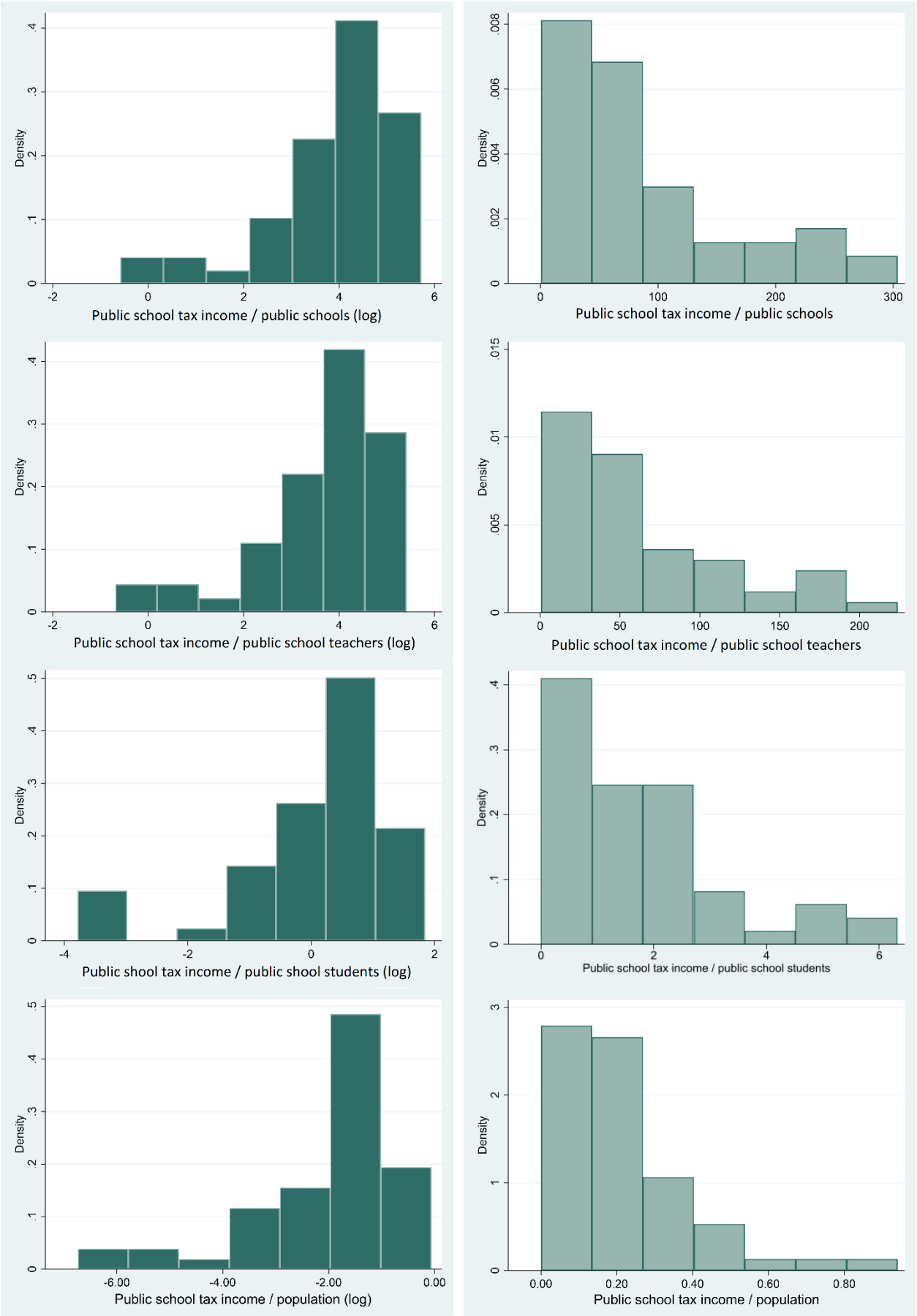


Table 4.: *Regression output related to H2*

Public school tax income / public schools (log)	(1)	(2)	(3)	(4)	**Public school tax income / public schools**	(1)	(2)	(3)	(4)
post	0.591	0.591**	0	0	post	10.21	10.21	0	0
	(0.559)	(0.234)	(.)	(.)		(31.87)	(21.40)	(.)	(.)
treat	-0.156	0	-0.156	0	treat	-14.98	0	-14.98	0
	(0.539)	(.)	(0.510)	(.)		(29.82)	(.)	(29.34)	(.)
post*treat	0.376	0.306	-0.200	0.260	post*treat	59.05	59.05**	37.50	32.91
	(0.751)	(0.319)	(0.745)	(0.351)		(42.17)	(28.31)	(43.53)	(28.93)
Constant	3.535***				Constant	70.57***			
	(0.395)					(22.54)			
Observations	54	52	54	52	Observations	54	52	54	52

Public school tax income / p.s. teachers (log)	(1)	(2)	(3)	(4)	**Public school tax income / p.s. teachers**	(1)	(2)	(3)	(4)
post	0.782	0.782***	0	0	post	25.95	25.95**	0	0
	(0.553)	(0.186)	(.)	(.)		(21.92)	(11.47)	(.)	(.)
treat	0.017	0	0.017	0	treat	3.737	0	3.737	0
	(0.522)	(.)	(0.491)	(.)		(20.14)	(.)	(19.56)	(.)
post*treat	0.114	0.032	-0.408	0.067	post*treat	21.60	21.60	5.828	9.975
	(0.728)	(0.248)	(0.711)	(0.269)		(28.48)	(14.90)	(28.75)	(15.10)
Constant	3.153***				Constant	39.04**			
	(0.391)					(15.50)			
Observations	52	50	52	50	Observations	52	50	52	50

Public school tax income / p.s. students (log)	(1)	(2)	(3)	(4)	**Public school tax income / p.s. students**	(1)	(2)	(3)	(4)
post	0.682	0.682***	0	0	post	0.604	0.604*	0	0
	(0.529)	(0.182)	(.)	(.)		(0.637)	(0.343)	(.)	(.)
treat	-0.030	0	-0.030	0	treat	0.111	0	0.111	0
	(0.500)	(.)	(0.475)	(.)		(0.585)	(.)	(0.578)	(.)
post*treat	0.135	0.050	-0.330	0.085	post*treat	0.566	0.566	0.226	0.241
	(0.697)	(0.244)	(0.687)	(0.264)		(0.827)	(0.445)	(0.850)	(0.456)
Constant	-0.277				Constant	1.217***			
	(0.374)					(0.450)			
Observations	52	50	52	50	Observations	52	50	52	50

Public school tax income / population (log)	(1)	(2)	(3)	(4)	**Public school tax income / population**	(1)	(2)	(3)	(4)
post	0.044	0.044	0	0	post	-0.062	-0.062	0	0
	(0.601)	(0.233)	(.)	(.)		(0.076)	(0.052)	(.)	(.)
treat	0.231	0	0.231	0	treat	0.033	0	0.033	0
	(0.579)	(.)	(0.522)	(.)		(0.071)	(.)	(0.068)	(.)
post*treat	0.112	0.087	-0.690	0.052	post*treat	0.066	0.066	-0.008	0.036
	(0.807)	(0.318)	(0.762)	(0.350)		(0.100)	(0.068)	(0.101)	(0.075)
Constant	-2.270***				Constant	0.203***			
	(0.425)					(0.054)			
Observations	54	52	54	52	Observations	54	52	54	52
State FE	NO	YES	NO	YES	State FE	NO	YES	NO	YES
Slaveholding FE	NO	NO	YES	YES	Slaveholding FE	NO	NO	YES	YES

Standard errors in parentheses.
** p < 0.10, ** p < 0.05, *** p < 0.01*

5.3. ADDITIONAL TESTS

To further examine the effects of homestead exemptions and their robustness, more tests were performed on a set of additional variables.

First, the total educational income was analyzed independently, and relative to the number of institutions and number of teachers. Appendix 6 contains the results and the histograms; the variables were log-transformed. A significant increase of total income per institution and total income per teacher is found as a result of exemption law enactments: both by approximately 40%. The distribution of the total income variable is not satisfactory, so the increase of almost 140% should be taken with reserve.

Next, the income by institutional subgroups is examined in more detail. The results related to public schools, colleges, and academies are reported in Appendix 7. Consistently with the previous findings, most diff-in-diff estimators show a positive impact of homestead exemptions on educational variables. Public school income per public school increased by 73% after the treatment, which is line with the previously discussed increase of tax income in public schools.

Even though mostly positive, no significant impact is found in the college related variables. Interesting results emerged in relation to the academy subgroup. The per capita values of academy income, academy teachers, and academy students seem to have increased significantly as a response to the exemption laws.

Despite these outcomes, it cannot be assumed that the academy income is the main contributor to the findings of this thesis. First, public school income makes up more than 60% of the total income, while academy income only partakes as the 25% of it. Additionally, there is a certain level of concern related to the classification of educational institutions as academies during the mid-19th century. The demand for education above the elementary level was not satisfied by the number of high schools in 1850 (Fishlow, 1966). To fill this gap, unregulated academies emerged: a form of privately financed secondary schools (Go and Lindert, 2010).

According to Goldin and Katz (2003), due to the rapid increase in the number of academies, this period is referred to as the “academy movement”. They explain why one should be cautious when interpreting data related to academies from the 1850 and 1860 decennial censuses. First, the census question included “academies and other schools”, so it is possible that other private schools and tutors were merged into this category. A regional difference also arises in relation to the definition of academies. In Northern and Midwestern states, only secondary level schools were considered academies. On the contrary, the Southern region seems to have included elementary schools in the definition as well. Many Southern counties did not list any common or elementary school but reported academies and its students. Hence, Goldin and Katz (2003) warn that the number of Southern academies and their students is overstated in the censuses.

6. CONCLUSION

The objective of this thesis was to determine whether investment in education was impacted by the introduction of homestead exemption laws in the United States during the 1850s. The first hypothesis looked into whether states that introduced homestead exemptions increased their investment in education more compared to states without such laws in force. The influence of exemptions on educational funding from taxes was further examined in hypothesis 2. Findings for both analyses show a positive impact on school funding when a financial safety net is provided. This research made a humble contribution to the existing literature by using the specific setup of the mid-19th century United States: the emergence of homestead exemptions as one of the mechanisms in limiting liability, beyond doubt played a role in the rise of public schooling in American education.

Limitations of the thesis are mostly related to the non-normal distribution of the variables, interpretation of log-transformed output, and possible inconsistencies in the dataset when it comes to institutional classification. Further work could be done to confirm the robustness of the results due to these limitations. Additionally, future research could examine the consistency of the education-related questions in the 1870 and 1880 censuses and include them in the dataset. As a wave of exemption law liberalizations happened after the Civil War, a deeper analysis could be performed on how the levels of homestead exemptions (and their change) impacted educational funding until the end of the 19th century. The effect of limited liability could be compared to that of other contributors which increased taxation and general interest in having more educated citizens.

To conclude, the unique mechanism of homestead exemption laws contributed to the level of investments in education. Indirectly, it improved the quality of education, bettered democracy, and led to industrialized growth and entrepreneurial activity. From their dawn in the 1850s, many authors confirmed that homestead exemptions are still of high importance to most states, with their modifications and variations spread internationally.

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8. APPENDICES

Appendix 1.: Composition of main variables

Total income	=	public school income = ***taxation*** *+ endowments* *+ public funds* *+ other sources*	+	college income = *taxation* *+ endowments* *+ public funds* *+ other sources*	+	academy income = *taxation* *+ endowments* *+ public funds* *+ other sources*
Total students	=	public school students	+	college students	+	academy students
Total institutions	=	public schools	+	colleges	+	academies
Total teachers	=	public school teachers	+	college teachers	+	academy teachers

Appendix 2.: Summary statistics by institutional subgroups

Table 1.: *Public schools*

Variable	N	Mean	SD	Min	Median	Max
Public schools	56	4,536	5,131	0	3,226	23,322
Public school income	55	681,970	812,693	7,200	423,704	3,341,088
Public school teachers	56	5,330	6,077	0	3,444	27,930
Public school students	56	200,489	259,074	0	136,843	1,350,442
Public school income / public schools	54	250.75	311.25	59.68	153.05	1,800.00
Public school income / p.s. teachers	52	206.56	276.56	42.20	133.00	1,800.00
Public school income / p.s. students	52	6.46	11.35	1.52	3.49	73.47
Public school income / population	55	0.3957	0.1924	0.0389	0.3776	1.0123
Public schools / population	55	0.0027	0.0019	0.0000	0.0023	0.0087
Public school teachers / population	54	0.0033	0.0025	0.0000	0.0026	0.0133
Public school students / population	54	0.1134	0.0739	0.0005	0.0976	0.3306

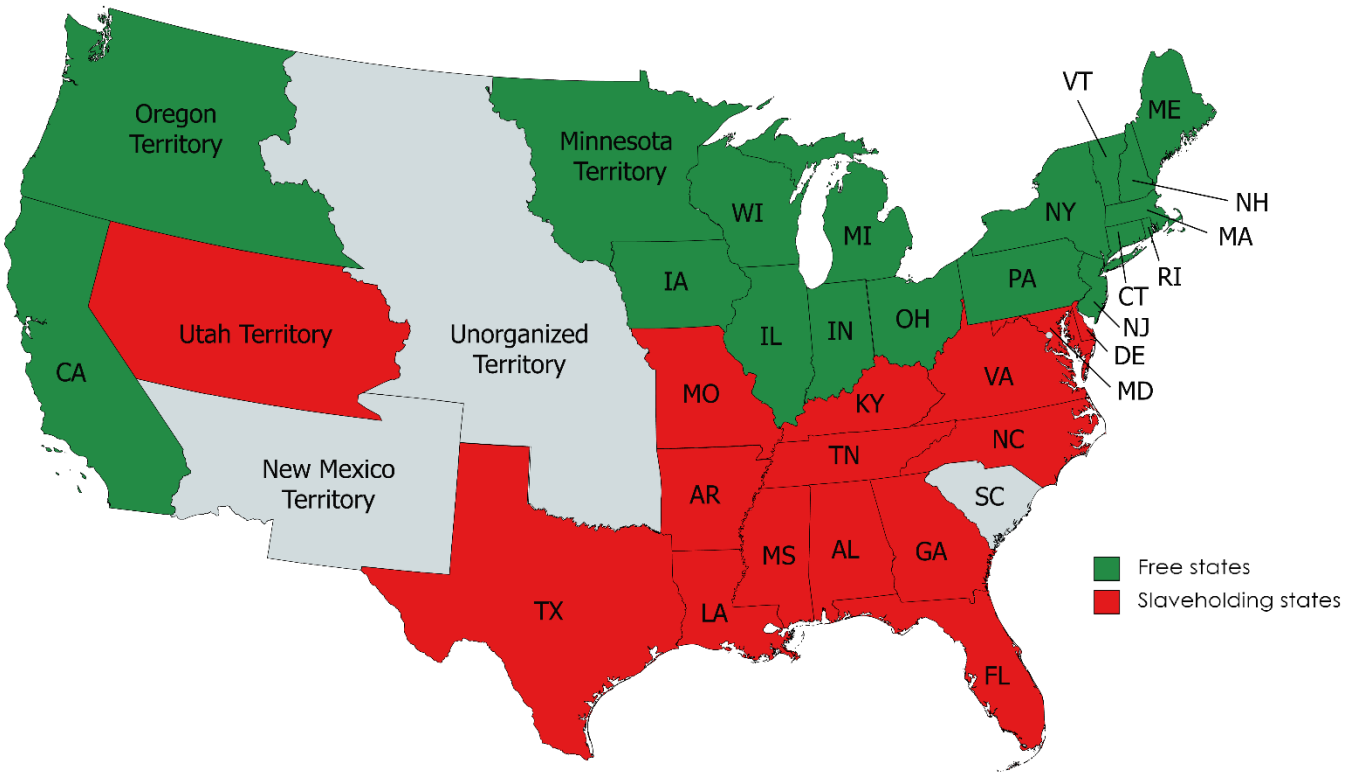
Table 2.: *Colleges*

Variable	N	Mean	SD	Min	Median	Max
Colleges	56	13	13	0	10	52
College income	51	110,775	108,642	3,250	86,700	564,410
College teachers	56	90	85	0	68	360
College students	56	1,633	1,774	0	939	7,242
College income / colleges	48	8,409.44	5,697.63	1,000.00	8,148.28	24,388.75
College income / college teachers	48	1,052.11	516.98	191.18	970.28	2,257.64
College income / college students	48	66.13	36.35	18.79	57.43	169.57
College income / population	51	0.06	0.05	0.01	0.05	0.19
Colleges / population	52	0.0000	0.0000	0.0000	0.0000	0.0001
College teachers / population	51	0.0001	0.0000	0.0000	0.0001	0.0002
College students / population	51	0.0010	0.0007	0.0002	0.0009	0.0043

Table 3.: *Academies*

Variable	N	Mean	SD	Min	Median	Max
Academies	56	285	307	2	210	1,774
Academy income	56	276,686	331,087	280	133,897	1,620,664
Academy teachers	56	593	717	0	444	4,272
Academy students	56	15,339	18,581	0	11,359	98,656
Academy income / academies	56	971.29	606.38	140.00	761.94	3,042.74
Academy income / academy teachers	54	486.95	381.04	140.00	421.27	2,854.00
Academy income / academy students	54	19.90	13.74	4.96	17.53	83.94
Academy income / population	56	0.1944	0.2194	0.0230	0.1561	1.5712
Academies / population	56	0.0003	0.0003	0.0000	0.0002	0.0022
Academy teachers / population	55	0.0004	0.0005	0.0001	0.0003	0.0033
Academy students / population	55	0.0104	0.0089	0.0018	0.0087	0.0633

Appendix 3.: Map of free and slave states in 1850-1860



Source: Brown, J.; American Social History Project/Center for Media and Learning (1996)

Created using: https://historicalmapchart.net/usa-historical

Appendix 4: Output of testing H1 with dataset variations

Appendix 4.1.: *Main dataset with 0-dollar cut-off for control group*

Total income / total students (log)	(1)	(2)	(3)	(4)
post	0.191	0.191**	0	0
	(0.265)	(0.090)	(.)	(.)
treat	-0.669***	0	-0.669***	0
	(0.230)	(.)	(0.221)	(.)
post*treat	0.241	0.241**	0.544	0.133
	(0.325)	(0.110)	(0.345)	(0.128)
Constant	1.927***			
	(0.188)			
Observations	48	48	48	48
State FE	NO	YES	NO	YES
Slavehold. FE	NO	NO	YES	YES

Total income / total students	(1)	(2)	(3)	(4)
post	1.262	1.262**	0	0
	(2.082)	(0.454)	(.)	(.)
treat	-2.830	0	-2.830	0
	(1.803)	(.)	(1.702)	(.)
post*treat	0.574	0.574	3.386	0.439
	(2.550)	(0.556)	(2.653)	(0.679)
Constant	7.373***			
	(1.472)			
Observations	48	48	48	48
State FE	NO	YES	NO	YES
Slavehold. FE	NO	NO	YES	YES

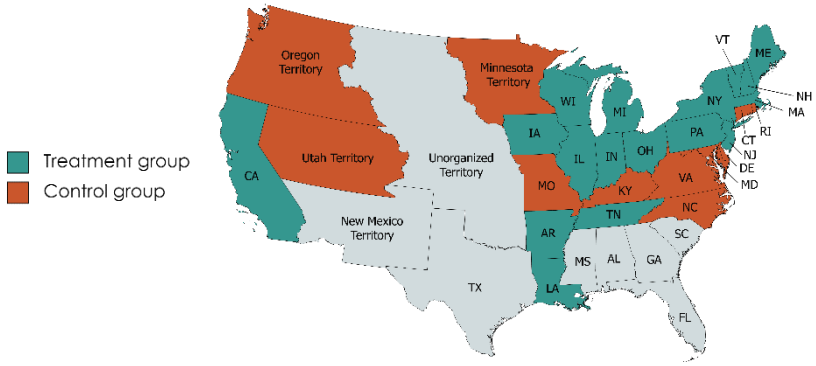
Total students / population (log)	(1)	(2)	(3)	(4)
post	-0.464	-0.473	0	0
	(0.440)	(0.365)	(.)	(.)
treat	0.052	0	0.052	0
	(0.391)	(.)	(0.382)	(.)
post*treat	0.311	0.321	-0.114	0.143
	(0.544)	(0.447)	(0.581)	(0.521)
Constant	-2.271***			
	(0.319)			
Observations	55	54	55	54
State FE	NO	YES	NO	YES
Slavehold. FE	NO	NO	YES	YES

Total students / population	(1)	(2)	(3)	(4)
post	-0.032	-0.032	0	0
	(0.030)	(0.021)	(.)	(.)
treat	0.075***	0	0.075***	0
	(0.026)	(.)	(0.026)	(.)
post*treat	-0.041	-0.041	-0.070*	-0.018
	(0.038)	(0.026)	(0.040)	(0.030)
Constant	0.101***			
	(0.021)			
Observations	55	54	55	54
State FE	NO	YES	NO	YES
Slavehold. FE	NO	NO	YES	YES

Total income / population (log)	(1)	(2)	(3)	(4)
post	-0.513*	-0.513**	0	0
	(0.269)	(0.219)	(.)	(.)
treat	-0.563**	0	-0.563**	0
	(0.237)	(.)	(0.235)	(.)
post*treat	0.598*	0.598**	0.402	0.488
	(0.335)	(0.273)	(0.364)	(0.327)
Constant	-0.157			
	(0.190)			
Observations	56	56	56	56
State FE	NO	YES	NO	YES
Slavehold. FE	NO	NO	YES	YES

Total income / population	(1)	(2)	(3)	(4)
post	-0.411***	-0.411***	0	0
	(0.133)	(0.102)	(.)	(.)
treat	-0.319***	0	-0.319***	0
	(0.117)	(.)	(0.117)	(.)
post*treat	0.345**	0.345**	0.249	0.368**
	(0.166)	(0.128)	(0.181)	(0.154)
Constant	0.942***			
	(0.0942)			
Observations	56	56	56	56
State FE	NO	YES	NO	YES
Slavehold. FE	NO	NO	YES	YES

Standard errors in parentheses.
** p < 0.10, ** p < 0.05, *** p < 0.01*



Appendix 4.2.: *Main dataset with 500-dollar cut-off for control group*

Total income / total students (log)	(1)	(2)	(3)	(4)
post	0.334	0.334***	0	0
	(0.200)	(0.0678)	(.)	(.)
treat	0.257	0	0.257	0
	(0.262)	(.)	(0.243)	(.)
post*treat	0.0584	0.0584	0.140	0.0257
	(0.370)	(0.126)	(0.345)	(0.114)
Constant	1.407***			
	(0.141)			
Observations	48	48	48	48
State FE	NO	YES	NO	YES
Slavehold. FE	NO	NO	YES	YES

Total income / total students	(1)	(2)	(3)	(4)
post	1.421	1.421***	0	0
	(1.438)	(0.307)	(.)	(.)
treat	2.036	0	2.036	0
	(1.883)	(.)	(1.719)	(.)
post*treat	0.766	0.766	1.407	0.716
	(2.662)	(0.568)	(2.439)	(0.578)
Constant	4.892***			
	(1.017)			
Observations	48	48	48	48
State FE	NO	YES	NO	YES
Slavehold. FE	NO	NO	YES	YES

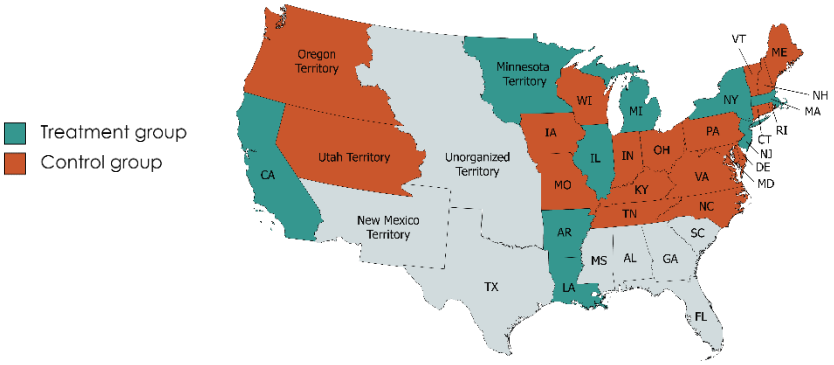
Total income / population (log)	(1)	(2)	(3)	(4)
post	-0.315	-0.315*	0	0
	(0.198)	(0.161)	(.)	(.)
treat	-0.461*	0	-0.461*	0
	(0.247)	(.)	(0.247)	(.)
post*treat	0.579	0.579*	0.525	0.505*
	(0.350)	(0.283)	(0.353)	(0.284)
Constant	-0.371**			
	(0.140)			
Observations	56	56	56	56
State FE	NO	YES	NO	YES
Slavehold. FE	NO	NO	YES	YES

Total income / population	(1)	(2)	(3)	(4)
post	-0.250**	-0.250***	0	0
	(0.102)	(0.0814)	(.)	(.)
treat	-0.0952	0	-0.0952	0
	(0.127)	(.)	(0.127)	(.)
post*treat	0.188	0.188	0.162	0.164
	(0.180)	(0.144)	(0.182)	(0.147)
Constant	0.767***			
	(0.0723)			
Observations	56	56	56	56
State FE	NO	YES	NO	YES
Slavehold. FE	NO	NO	YES	YES

Total students / population (log)	(1)	(2)	(3)	(4)
post	-0.526*	-0.515**	0	0
	(0.297)	(0.245)	(.)	(.)
treat	-0.976**	0	-0.976***	0
	(0.369)	(.)	(0.353)	(.)
post*treat	0.777	0.765*	0.611	0.714
	(0.520)	(0.424)	(0.502)	(0.435)
Constant	-1.912***			
	(0.213)			
Observations	55	54	55	54
State FE	NO	YES	NO	YES
Slavehold. FE	NO	NO	YES	YES

Total students / population	(1)	(2)	(3)	(4)
post	-0.0637***	-0.0637***	0	0
	(0.0235)	(0.0158)	(.)	(.)
treat	-0.0302	0	-0.0302	0
	(0.0293)	(.)	(0.0282)	(.)
post*treat	0.0181	0.0181	0.00574	0.0296
	(0.0414)	(0.0280)	(0.0403)	(0.0265)
Constant	0.159***			
	(0.0166)			
Observations	55	54	55	54
State FE	NO	YES	NO	YES
Slavehold. FE	NO	NO	YES	YES

Standard errors in parentheses.
** p < 0.10, ** p < 0.05, *** p < 0.01*



Appendix 4.3.: *Liberal dataset with 0-dollar cut-off for control group*

Total income / total students (log)	(1)	(2)	(3)	(4)
post	0.215	0.215**	0	0
	(0.213)	(0.0779)	(.)	(.)
treat	-0.817***	0	-0.817***	0
	(0.199)	(.)	(0.190)	(.)
post*treat	0.217	0.217**	0.605*	0.105
	(0.281)	(0.103)	(0.312)	(0.132)
Constant	2.076***			
	(0.150)			
Observations	48	48	48	48
State FE	NO	YES	NO	YES
Slavehold. FE	NO	NO	YES	YES

Total income / total students	(1)	(2)	(3)	(4)
post	1.868	1.868**	0	0
	(1.747)	(0.577)	(.)	(.)
treat	-4.090*	0	-4.090*	0
	(1.634)	(.)	(1.530)	(.)
post*treat	-0.0329	-0.0329	3.652	0.00987
	(2.311)	(0.763)	(2.512)	(1.013)
Constant	8.633***			
	(1.235)			
Observations	48	48	48	48
State FE	NO	YES	NO	YES
Slavehold. FE	NO	NO	YES	YES

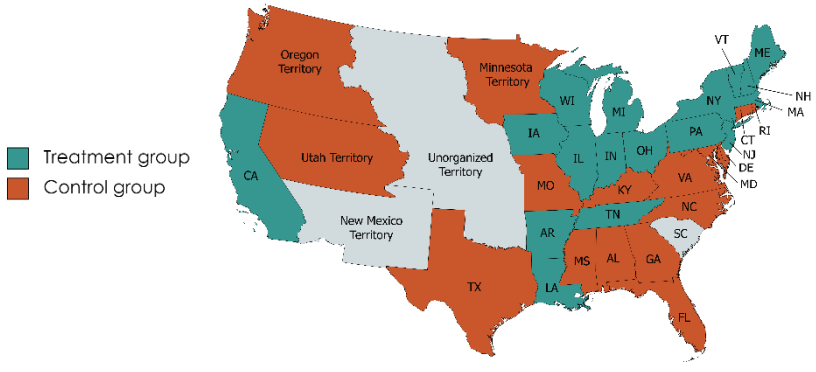
Total income / population (log)	(1)	(2)	(3)	(4)
post	-0.387*	-0.387**	0	0
	(0.209)	(0.169)	(.)	(.)
treat	-0.380*	0	-0.380*	0
	(0.200)	(.)	(0.198)	(.)
post*treat	0.472*	0.472**	0.231	0.398
	(0.283)	(0.229)	(0.323)	(0.300)
Constant	-0.340**			
	(0.148)			
Observations	56	56	56	56
State FE	NO	YES	NO	YES
Slavehold. FE	NO	NO	YES	YES

Total income / population	(1)	(2)	(3)	(4)
post	-0.297***	-0.297***	0	0
	(0.107)	(0.0829)	(.)	(.)
treat	-0.173*	0	-0.173*	0
	(0.103)	(.)	(0.102)	(.)
post*treat	0.231	0.231**	0.111	0.301**
	(0.145)	(0.112)	(0.167)	(0.146)
Constant	0.796***			
	(0.0759)			
Observations	56	56	56	56
State FE	NO	YES	NO	YES
Slavehold. FE	NO	NO	YES	YES

Total students / population (log)	(1)	(2)	(3)	(4)
post	-0.421	-0.446	0	0
	(0.340)	(0.267)	(.)	(.)
treat	0.345	0	0.345	0
	(0.326)	(.)	(0.316)	(.)
post*treat	0.269	0.293	-0.303	0.0998
	(0.457)	(0.356)	(0.510)	(0.458)
Constant	-2.565***			
	(0.245)			
Observations	55	54	55	54
State FE	NO	YES	NO	YES
Slavehold. FE	NO	NO	YES	YES

Total students / population	(1)	(2)	(3)	(4)
post	-0.0260	-0.0260	0	0
	(0.0232)	(0.0158)	(.)	(.)
treat	0.0934***	0	0.0934***	0
	(0.0222)	(.)	(0.0216)	(.)
post*treat	-0.0464	-0.0464**	-0.0841**	-0.0189
	(0.0314)	(0.0214)	(0.0353)	(0.0270)
Constant	0.0828***			
	(0.0164)			
Observations	55	54	55	54
State FE	NO	YES	NO	YES
Slavehold. FE	NO	NO	YES	YES

Standard errors in parentheses.
** p < 0.10, ** p < 0.05, *** p < 0.01*



Appendix 4.4.: *Liberal dataset with 300-dollar cut-off for control group*

Total income / total students (log)	(1)	(2)	(3)	(4)
post	0.203	0.203***	0	0
	(0.213)	(0.0684)	(.)	(.)
treat	-0.695***	0	-0.695***	0
	(0.213)	(.)	(0.198)	(.)
post*treat	0.271	0.271***	0.620**	0.202*
	(0.301)	(0.0967)	(0.302)	(0.111)
Constant	1.956***			
	(0.151)			
Observations	48	48	48	48
State FE	NO	YES	NO	YES
Slavehold. FE	NO	NO	YES	YES

Total income / total students	(1)	(2)	(3)	(4)
post	1.682	1.682***	0	0
	(1.699)	(0.532)	(.)	(.)
treat	-3.245*	0	-3.245**	0
	(1.699)	(.)	(1.552)	(.)
post*treat	0.336	0.336	3.356	0.488
	(2.402)	(0.752)	(2.372)	(0.884)
Constant	7.918***			
	(1.201)			
Observations	48	48	48	48
State FE	NO	YES	NO	YES
Slavehold. FE	NO	NO	YES	YES

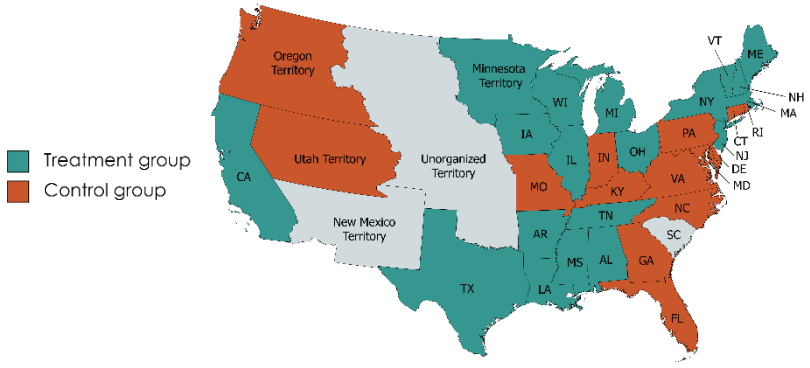
Total income / population (log)	(1)	(2)	(3)	(4)
post	-0.383*	-0.383**	0	0
	(0.195)	(0.156)	(.)	(.)
treat	-0.395*	0	-0.395**	0
	(0.198)	(.)	(0.197)	(.)
post*treat	0.523*	0.523**	0.367	0.455*
	(0.280)	(0.224)	(0.304)	(0.266)
Constant	-0.356**			
	(0.138)			
Observations	56	56	56	56
State FE	NO	YES	NO	YES
Slavehold. FE	NO	NO	YES	YES

Total income / population	(1)	(2)	(3)	(4)
post	-0.287***	-0.287***	0	0
	(0.101)	(0.0774)	(.)	(.)
treat	-0.164	0	-0.164	0
	(0.102)	(.)	(0.102)	(.)
post*treat	0.240	0.240**	0.161	0.271**
	(0.145)	(0.111)	(0.157)	(0.132)
Constant	0.781***			
	(0.0714)			
Observations	56	56	56	56
State FE	NO	YES	NO	YES
Slavehold. FE	NO	NO	YES	YES

Total students / population (log)	(1)	(2)	(3)	(4)
post	-0.436	-0.451*	0	0
	(0.324)	(0.249)	(.)	(.)
treat	0.175	0	0.175	0
	(0.329)	(.)	(0.316)	(.)
post*treat	0.325	0.341	-0.150	0.206
	(0.462)	(0.352)	(0.482)	(0.412)
Constant	-2.458***			
	(0.233)			
Observations	55	54	55	54
State FE	NO	YES	NO	YES
Slavehold. FE	NO	NO	YES	YES

Total students / population	(1)	(2)	(3)	(4)
post	-0.0311	-0.0311**	0	0
	(0.0228)	(0.0151)	(.)	(.)
treat	0.0813***	0	0.0813***	0
	(0.0231)	(.)	(0.0223)	(.)
post*treat	-0.0416	-0.0416*	-0.0733**	-0.0176
	(0.0327)	(0.0217)	(0.0344)	(0.0244)
Constant	0.0943***			
	(0.0161)			
Observations	55	54	55	54
State FE	NO	YES	NO	YES
Slavehold. FE	NO	NO	YES	YES

Standard errors in parentheses.
** p < 0.10, ** p < 0.05, *** p < 0.01*



Appendix 4.5.: *Liberal dataset with 500-dollar cut-off for control group*

Total income / total students (log)	(1)	(2)	(3)	(4)
post	0.321	0.321***	0	0
	(0.196)	(0.0633)	(.)	(.)
treat	0.0728	0	0.0728	0
	(0.277)	(.)	(0.250)	(.)
post*treat	0.0721	0.0721	0.262	0.0157
	(0.392)	(0.127)	(0.358)	(0.120)
Constant	1.591***			
	(0.139)			
Observations	48	48	48	48
State FE	NO	YES	NO	YES
Slavehold. FE	NO	NO	YES	YES

Total income / total students	(1)	(2)	(3)	(4)
post	1.737	1.737***	0	0
	(1.464)	(0.434)	(.)	(.)
treat	0.843	0	0.843	0
	(2.071)	(.)	(1.823)	(.)
post*treat	0.449	0.449	1.995	0.484
	(2.929)	(0.867)	(2.606)	(0.903)
Constant	6.085***			
	(1.035)			
Observations	48	48	48	48
State FE	NO	YES	NO	YES
Slavehold. FE	NO	NO	YES	YES

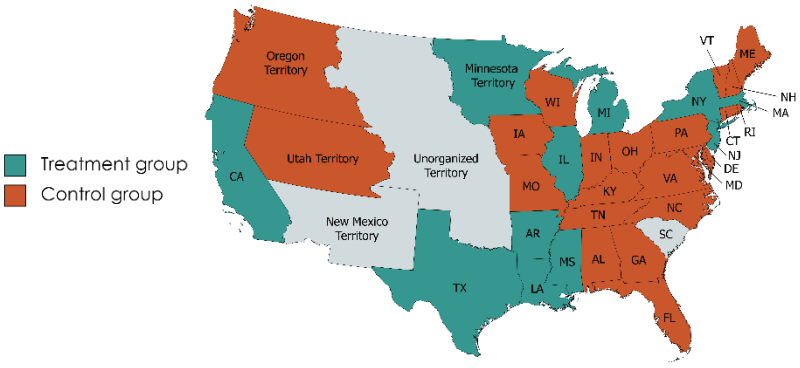
Total income / population (log)	(1)	(2)	(3)	(4)
post	-0.277*	-0.277**	0	0
	(0.165)	(0.133)	(.)	(.)
treat	-0.391*	0	-0.391*	0
	(0.224)	(.)	(0.223)	(.)
post*treat	0.541*	0.541**	0.455	0.461*
	(0.317)	(0.255)	(0.322)	(0.265)
Constant	-0.441***			
	(0.117)			
Observations	56	56	56	56
State FE	NO	YES	NO	YES
Slavehold. FE	NO	NO	YES	YES

Total income / population	(1)	(2)	(3)	(4)
post	-0.212**	-0.212***	0	0
	(0.0863)	(0.0684)	(.)	(.)
treat	-0.0406	0	-0.0406	0
	(0.117)	(.)	(0.116)	(.)
post*treat	0.150	0.150	0.108	0.136
	(0.165)	(0.131)	(0.168)	(0.139)
Constant	0.713***			
	(0.0611)			
Observations	56	56	56	56
State FE	NO	YES	NO	YES
Slavehold. FE	NO	NO	YES	YES

Total students / population (log)	(1)	(2)	(3)	(4)
post	-0.488*	-0.489**	0	0
	(0.269)	(0.198)	(.)	(.)
treat	-0.719*	0	-0.719**	0
	(0.362)	(.)	(0.338)	(.)
post*treat	0.739	0.739*	0.418	0.677*
	(0.511)	(0.374)	(0.486)	(0.393)
Constant	-2.168***			
	(0.192)			
Observations	55	54	55	54
State FE	NO	YES	NO	YES
Slavehold. FE	NO	NO	YES	YES

Total students / population	(1)	(2)	(3)	(4)
post	-0.0535**	-0.0535***	0	0
	(0.0214)	(0.0134)	(.)	(.)
treat	-0.00663	0	-0.00663	0
	(0.0289)	(.)	(0.0275)	(.)
post*treat	0.00790	0.00790	-0.0147	0.0279
	(0.0409)	(0.0257)	(0.0398)	(0.0240)
Constant	0.136***			
	(0.0151)			
Observations	55	54	55	54
State FE	NO	YES	NO	YES
Slavehold. FE	NO	NO	YES	YES

Standard errors in parentheses.
** p < 0.10, ** p < 0.05, *** p < 0.01*



Appendix 4.6.: *Strict dataset with 0-dollar cut-off for control group*

Total income / total students (log)	(1)	(2)	(3)	(4)
post	0.191	0.191**	0	0
	(0.266)	(0.0909)	(.)	(.)
treat	-0.589**	0	-0.589**	0
	(0.236)	(.)	(0.228)	(.)
post*treat	0.215	0.215*	0.487	0.121
	(0.334)	(0.114)	(0.352)	(0.131)
Constant	1.927***			
	(0.188)			
Observations	48	48	48	48
State FE	NO	YES	NO	YES
Slavehold. FE	NO	NO	YES	YES

Total income / total students	(1)	(2)	(3)	(4)
post	1.262	1.262**	0	0
	(2.139)	(0.475)	(.)	(.)
treat	-2.471	0	-2.471	0
	(1.896)	(.)	(1.800)	(.)
post*treat	0.592	0.592	3.169	0.454
	(2.682)	(0.596)	(2.776)	(0.715)
Constant	7.373***			
	(1.513)			
Observations	48	48	48	48
State FE	NO	YES	NO	YES
Slavehold. FE	NO	NO	YES	YES

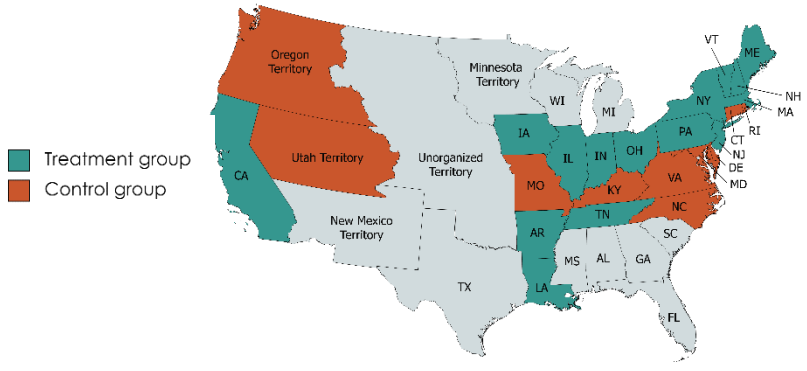
Total income / population (log)	(1)	(2)	(3)	(4)
post	-0.513**	-0.513***	0	0
	(0.193)	(0.151)	(.)	(.)
treat	-0.359**	0	-0.359**	0
	(0.177)	(.)	(0.171)	(.)
post*treat	0.427*	0.427**	0.227	0.378
	(0.250)	(0.195)	(0.261)	(0.229)
Constant	-0.157			
	(0.137)			
Observations	56	56	56	56
State FE	NO	YES	NO	YES
Slavehold. FE	NO	NO	YES	YES

Total income / population	(1)	(2)	(3)	(4)
post	-0.411***	-0.411***	0	0
	(0.133)	(0.105)	(.)	(.)
treat	-0.260**	0	-0.260**	0
	(0.122)	(.)	(0.120)	(.)
post*treat	0.307*	0.307**	0.207	0.341**
	(0.172)	(0.136)	(0.184)	(0.159)
Constant	0.942***			
	(0.0944)			
Observations	56	56	56	56
State FE	NO	YES	NO	YES
Slavehold. FE	NO	NO	YES	YES

Total students / population (log)	(1)	(2)	(3)	(4)
post	-0.464	-0.473*	0	0
	(0.367)	(0.252)	(.)	(.)
treat	0.214	0	0.214	0
	(0.337)	(.)	(0.324)	(.)
post*treat	0.0942	0.104	-0.295	0.00332
	(0.469)	(0.319)	(0.486)	(0.366)
Constant	-2.271***			
	(0.266)			
Observations	55	54	55	54
State FE	NO	YES	NO	YES
Slavehold. FE	NO	NO	YES	YES

Total students / population	(1)	(2)	(3)	(4)
post	-0.0316	-0.0316	0	0
	(0.0288)	(0.0189)	(.)	(.)
treat	0.0777***	0	0.0777***	0
	(0.0262)	(.)	(0.0256)	(.)
post*treat	-0.0477	-0.0477*	-0.0745*	-0.0236
	(0.0371)	(0.0244)	(0.0391)	(0.0269)
Constant	0.101***			
	(0.0203)			
Observations	55	54	55	54
State FE	NO	YES	NO	YES
Slavehold. FE	NO	NO	YES	YES

Standard errors in parentheses.
** p < 0.10, ** p < 0.05, *** p < 0.01*



Appendix 4.7.: *Strict dataset with 300-dollar cut-off for control group*

Total income / total students (log)	(1)	(2)	(3)	(4)
post	0.179	0.179**	0	0
	(0.251)	(0.0760)	(.)	(.)
treat	-0.434*	0	-0.434*	0
	(0.241)	(.)	(0.229)	(.)
post*treat	0.271	0.271**	0.472	0.215*
	(0.340)	(0.103)	(0.335)	(0.107)
Constant	1.790***			
	(0.178)			
Observations	48	48	48	48
State FE	NO	YES	NO	YES
Slavehold. FE	NO	NO	YES	YES

Total income / total students	(1)	(2)	(3)	(4)
post	1.122	1.122**	0	0
	(1.958)	(0.406)	(.)	(.)
treat	-1.510	0	-1.510	0
	(1.875)	(.)	(1.758)	(.)
post*treat	0.948	0.948	2.649	0.887
	(2.652)	(0.550)	(2.574)	(0.602)
Constant	6.624***			
	(1.385)			
Observations	48	48	48	48
State FE	NO	YES	NO	YES
Slavehold. FE	NO	NO	YES	YES

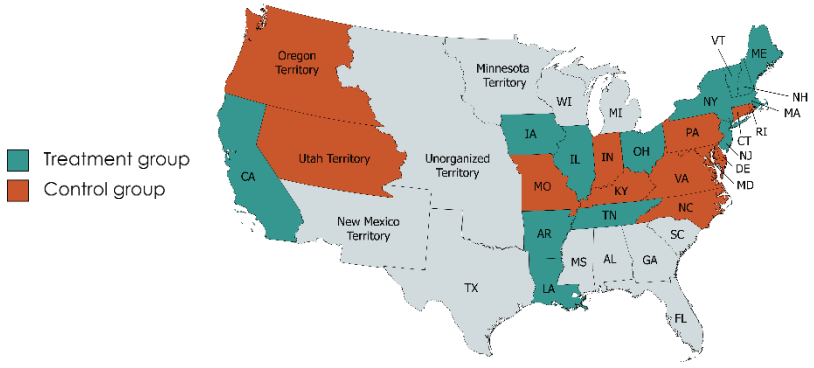
Total income / population (log)	(1)	(2)	(3)	(4)
post	-0.487***	-0.487***	0	0
	(0.177)	(0.136)	(.)	(.)
treat	-0.312*	0	-0.312*	0
	(0.174)	(.)	(0.170)	(.)
post*treat	0.442*	0.442**	0.326	0.391*
	(0.246)	(0.189)	(0.249)	(0.205)
Constant	-0.210			
	(0.125)			
Observations	56	56	56	56
State FE	NO	YES	NO	YES
Slavehold. FE	NO	NO	YES	YES

Total income / population	(1)	(2)	(3)	(4)
post	-0.378***	-0.378***	0	0
	(0.123)	(0.0965)	(.)	(.)
treat	-0.212*	0	-0.212*	0
	(0.121)	(.)	(0.120)	(.)
post*treat	0.290*	0.290**	0.232	0.292*
	(0.171)	(0.134)	(0.176)	(0.147)
Constant	0.896***			
	(0.0872)			
Observations	56	56	56	56
State FE	NO	YES	NO	YES
Slavehold. FE	NO	NO	YES	YES

Total students / population (log)	(1)	(2)	(3)	(4)
post	-0.477	-0.476**	0	0
	(0.337)	(0.228)	(.)	(.)
treat	0.0591	0	0.0591	0
	(0.331)	(.)	(0.316)	(.)
post*treat	0.125	0.125	-0.146	0.0637
	(0.462)	(0.309)	(0.458)	(0.332)
Constant	-2.170***			
	(0.243)			
Observations	55	54	55	54
State FE	NO	YES	NO	YES
Slavehold. FE	NO	NO	YES	YES

Total students / population	(1)	(2)	(3)	(4)
post	-0.0380	-0.0380**	0	0
	(0.0271)	(0.0175)	(.)	(.)
treat	0.0642**	0	0.0642**	0
	(0.0266)	(.)	(0.0259)	(.)
post*treat	-0.0428	-0.0428*	-0.0619	-0.0248
	(0.0376)	(0.0243)	(0.0379)	(0.0244)
Constant	0.114***			
	(0.0192)			
Observations	55	54	55	54
State FE	NO	YES	NO	YES
Slavehold. FE	NO	NO	YES	YES

Standard errors in parentheses.
** p < 0.10, ** p < 0.05, *** p < 0.01*



Appendix 4.8.: *Strict dataset with 500-dollar cut-off for control group*

Total income / total students (log)	(1)	(2)	(3)	(4)
post	0.324	0.324***	0	0
	(0.197)	(0.0697)	(.)	(.)
treat	0.394	0	0.394	0
	(0.267)	(.)	(0.249)	(.)
post*treat	0.0112	0.0112	0.0737	-0.0137
	(0.377)	(0.134)	(0.352)	(0.124)
Constant	1.445***			
	(0.139)			
Observations	48	48	48	48
State FE	NO	YES	NO	YES
Slavehold. FE	NO	NO	YES	YES

Total income / total students	(1)	(2)	(3)	(4)
post	1.422	1.422***	0	0
	(1.474)	(0.331)	(.)	(.)
treat	2.716	0	2.716	0
	(1.996)	(.)	(1.837)	(.)
post*treat	0.796	0.796	1.295	0.751
	(2.822)	(0.635)	(2.603)	(0.645)
Constant	5.060***			
	(1.042)			
Observations	48	48	48	48
State FE	NO	YES	NO	YES
Slavehold. FE	NO	NO	YES	YES

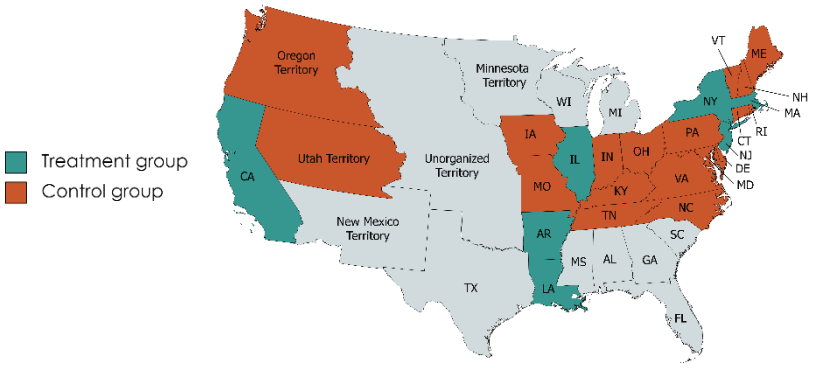
Total income / population (log)	(1)	(2)	(3)	(4)
post	-0.337**	-0.337***	0	0
	(0.149)	(0.120)	(.)	(.)
treat	-0.0909	0	-0.0909	0
	(0.199)	(.)	(0.195)	(.)
post*treat	0.286	0.286	0.237	0.246
	(0.281)	(0.226)	(0.277)	(0.226)
Constant	-0.346***			
	(0.105)			
Observations	56	56	56	56
State FE	NO	YES	NO	YES
Slavehold. FE	NO	NO	YES	YES

Total income / population	(1)	(2)	(3)	(4)
post	-0.266**	-0.266***	0	0
	(0.103)	(0.0852)	(.)	(.)
treat	0.00146	0	0.00146	0
	(0.138)	(.)	(0.137)	(.)
post*treat	0.138	0.138	0.113	0.125
	(0.195)	(0.161)	(0.195)	(0.165)
Constant	0.785***			
	(0.0731)			
Observations	56	56	56	56
State FE	NO	YES	NO	YES
Slavehold. FE	NO	NO	YES	YES

Total students / population (log)	(1)	(2)	(3)	(4)
post	-0.533**	-0.522***	0	0
	(0.260)	(0.178)	(.)	(.)
treat	-0.712**	0	-0.712**	0
	(0.345)	(.)	(0.323)	(.)
post*treat	0.398	0.387	0.272	0.365
	(0.485)	(0.330)	(0.458)	(0.338)
Constant	-1.930***			
	(0.186)			
Observations	55	54	55	54
State FE	NO	YES	NO	YES
Slavehold. FE	NO	NO	YES	YES

Total students / population	(1)	(2)	(3)	(4)
post	-0.0635***	-0.0635***	0	0
	(0.0233)	(0.0152)	(.)	(.)
treat	-0.0317	0	-0.0317	0
	(0.0311)	(.)	(0.0300)	(.)
post*treat	0.0116	0.0116	0.00222	0.0216
	(0.0440)	(0.0287)	(0.0427)	(0.0258)
Constant	0.157***			
	(0.0165)			
Observations	55	54	55	54
State FE	NO	YES	NO	YES
Slavehold. FE	NO	NO	YES	YES

Standard errors in parentheses.
** p < 0.10, ** p < 0.05, *** p < 0.01*



Appendix 5: Output of testing H2 with dataset variations

Appendix 5.1.: *Main dataset with 0-dollar cut-off for control group*

Public school tax income / schools (log)	(1)	(2)	(3)	(4)	**Public school tax income / schools**	(1)	(2)	(3)	(4)
post	0.549	0.549**	0	0	post	2.584	2.584	0	0
	(0.612)	(0.256)	(.)	(.)		(34.79)	(23.22)	(.)	(.)
treat	-0.0727	0	-0.0727	0	treat	-14.81	0	-14.81	0
	(0.551)	(.)	(0.522)	(.)		(30.68)	(.)	(30.35)	(.)
post*treat	0.395	0.336	-0.450	0.299	post*treat	64.35	64.35**	36.37	30.35
	(0.771)	(0.326)	(0.797)	(0.387)		(43.39)	(28.96)	(46.99)	(32.50)
Constant	3.496***				Constant	71.53***			
	(0.433)					(24.60)			
Observations	54	52	54	52	Observations	54	52	54	52

Public school tax income / teachers (log)	(1)	(2)	(3)	(4)	**Public school tax income / teachers**	(1)	(2)	(3)	(4)
post	0.790	0.790***	0	0	post	23.19	23.19*	0	0
	(0.609)	(0.206)	(.)	(.)		(23.98)	(12.64)	(.)	(.)
treat	0.185	0	0.185	0	treat	9.580	0	9.580	0
	(0.538)	(.)	(0.505)	(.)		(20.77)	(.)	(20.33)	(.)
post*treat	0.0825	0.0150	-0.699	0.0701	post*treat	23.33	23.33	2.446	6.870
	(0.754)	(0.257)	(0.763)	(0.298)		(29.37)	(15.48)	(31.06)	(16.96)
Constant	3.044***				Constant	34.87**			
	(0.431)					(16.96)			
Observations	52	50	52	50	Observations	52	50	52	50

Public school tax income / students (log)	(1)	(2)	(3)	(4)	**Public school tax income / students**	(1)	(2)	(3)	(4)
post	0.700	0.700***	0	0	post	0.552	0.552	0	0
	(0.584)	(0.202)	(.)	(.)		(0.702)	(0.379)	(.)	(.)
treat	0.0895	0	0.0895	0	treat	0.190	0	0.190	0
	(0.516)	(.)	(0.488)	(.)		(0.608)	(.)	(0.603)	(.)
post*treat	0.0884	0.0161	-0.624	0.0682	post*treat	0.581	0.581	0.117	0.106
	(0.723)	(0.252)	(0.738)	(0.293)		(0.859)	(0.464)	(0.921)	(0.512)
Constant	-0.351				Constant	1.156**			
	(0.413)					(0.496)			
Observations	52	50	52	50	Observations	52	50	52	50

Public school tax income / population (log)	(1)	(2)	(3)	(4)	**Public school tax income / population**	(1)	(2)	(3)	(4)
post	0.0295	0.0295	0	0	post	-0.0677	-0.0677	0	0
	(0.653)	(0.255)	(.)	(.)		(0.0822)	(0.0564)	(.)	(.)
treat	0.401	0	0.401	0	treat	0.0536	0	0.0536	0
	(0.589)	(.)	(0.531)	(.)		(0.0725)	(.)	(0.0700)	(.)
post*treat	0.116	0.0990	-1.044	0.0536	post*treat	0.0676	0.0676	-0.0298	0.0279
	(0.824)	(0.326)	(0.811)	(0.387)		(0.103)	(0.0704)	(0.108)	(0.0835)
Constant	-2.393***	-2.146***	-1.991***	-2.117***	Constant	0.187***	0.222***	0.185***	0.201***
	(0.462)	(0.112)	(0.314)	(0.144)		(0.0581)	(0.0238)	(0.0421)	(0.0317)
Observations	54	52	54	52	Observations	56	56	56	56
State FE	NO	YES	NO	YES	State FE	NO	YES	NO	YES
Slavehold. FE	NO	NO	YES	YES	Slavehold. FE	NO	NO	YES	YES

*Standard errors in parentheses. * p < 0.10, ** p < 0.05, *** p < 0.01*

Appendix 5.2.: *Main dataset with 500-dollar cut-off for control group*

Public school tax income / schools (log)	(1)	(2)	(3)	(4)	**Public school tax income / schools**	(1)	(2)	(3)	(4)
post	0.694	0.694***	0	0	post	20.69	20.69	0	0
	(0.442)	(0.188)	(.)	(.)		(24.13)	(16.52)	(.)	(.)
treat	0.145	0	0.145	0	treat	11.91	0	11.91	0
	(0.603)	(.)	(0.575)	(.)		(30.09)	(.)	(29.56)	(.)
post*treat	0.311	0.233	0.0587	0.205	post*treat	72.37*	72.37**	62.45	58.68**
	(0.817)	(0.362)	(0.787)	(0.370)		(42.56)	(29.14)	(42.21)	(26.79)
Constant	3.412***				Constant	58.18***			
	(0.313)					(17.06)			
Observations	54	52	54	52	Observations	54	52	54	52

Public school tax income / teachers (log)	(1)	(2)	(3)	(4)	**Public school tax income / teachers**	(1)	(2)	(3)	(4)
post	0.804*	0.804***	0	0	post	27.20*	27.20***	0	0
	(0.429)	(0.145)	(.)	(.)		(16.19)	(8.433)	(.)	(.)
treat	0.262	0	0.262	0	treat	17.94	0	17.94	0
	(0.574)	(.)	(0.544)	(.)		(19.83)	(.)	(19.21)	(.)
post*treat	0.0966	-0.0148	-0.117	-0.00649	post*treat	34.65	34.65**	27.97	29.41**
	(0.778)	(0.275)	(0.743)	(0.282)		(28.04)	(14.61)	(27.37)	(13.69)
Constant	3.089***				Constant	35.28***			
	(0.304)					(11.45)			
Observations	52	50	52	50	Observations	52	50	52	50

Public school tax income / students (log)	(1)	(2)	(3)	(4)	**Public school tax income / students**	(1)	(2)	(3)	(4)
post	0.720*	0.720***	0	0	post	0.677	0.677**	0	0
	(0.412)	(0.143)	(.)	(.)		(0.477)	(0.261)	(.)	(.)
treat	0.217	0	0.217	0	treat	0.526	0	0.526	0
	(0.550)	(.)	(0.526)	(.)		(0.585)	(.)	(0.577)	(.)
post*treat	0.0880	-0.0360	-0.102	-0.0285	post*treat	0.787	0.787*	0.640	0.638
	(0.746)	(0.270)	(0.719)	(0.276)		(0.827)	(0.452)	(0.822)	(0.431)
Constant	-0.354				Constant	1.107***			
	(0.291)					(0.338)			
Observations	52	50	52	50	Observations	52	50	52	50

Public school tax income / population (log)	(1)	(2)	(3)	(4)	**Public school tax income / population**	(1)	(2)	(3)	(4)
post	0.103	0.103	0	0	post	-0.0427	-0.0427	0	0
	(0.480)	(0.186)	(.)	(.)		(0.0611)	(0.0412)	(.)	(.)
treat	0.0672	0	0.0672	0	treat	0.0105	0	0.0105	0
	(0.654)	(.)	(0.590)	(.)		(0.0762)	(.)	(0.0721)	(.)
post*treat	0.0275	-0.0470	-0.348	-0.0637	post*treat	0.0573	0.0573	0.0196	0.0411
	(0.886)	(0.358)	(0.807)	(0.367)		(0.108)	(0.0726)	(0.103)	(0.0736)
Constant	-2.164***				Constant	0.218***			
	(0.339)					(0.0432)			
Observations	54	52	54	52	Observations	56	56	56	56
State FE	NO	YES	NO	YES	State FE	NO	YES	NO	YES
Slavehold. FE	NO	NO	YES	YES	Slavehold. FE	NO	NO	YES	YES

*Standard errors in parentheses. * p < 0.10, ** p < 0.05, *** p < 0.01*

Appendix 5.3.: *Liberal dataset with 0-dollar cut-off for control group*

Public school tax income / schools (log)	(1)	(2)	(3)	(4)
post	0.465	0.736**	0	0
	(0.540)	(0.280)	(.)	(.)
treat	0.294	0	0.294	0
	(0.532)	(.)	(0.501)	(.)
post*treat	0.479	0.149	-0.638	0.177
	(0.729)	(0.376)	(0.787)	(0.471)
Constant	3.129***			
	(0.395)			
Observations	54	52	54	52

Public school tax income / schools	(1)	(2)	(3)	(4)
post	4.678	4.678	0	0
	(27.00)	(17.43)	(.)	(.)
treat	5.698	0	5.698	0
	(25.85)	(.)	(25.36)	(.)
post*treat	62.25*	62.25**	23.92	25.16
	(36.55)	(23.59)	(41.43)	(29.09)
Constant	51.02***			
	(19.09)			
Observations	54	52	54	52

Public school tax income / teachers (log)	(1)	(2)	(3)	(4)
post	0.638	0.923***	0	0
	(0.532)	(0.254)	(.)	(.)
treat	0.512	0	0.512	0
	(0.516)	(.)	(0.482)	(.)
post*treat	0.235	-0.117	-0.845	-0.00852
	(0.706)	(0.336)	(0.748)	(0.413)
Constant	2.717***			
	(0.390)			
Observations	52	50	52	50

Public school tax income / teachers	(1)	(2)	(3)	(4)
post	17.39	17.39*	0	0
	(18.16)	(9.444)	(.)	(.)
treat	19.02	0	19.02	0
	(17.12)	(.)	(16.55)	(.)
post*treat	29.14	29.14**	-0.651	8.034
	(24.21)	(12.59)	(26.79)	(15.03)
Constant	25.43*			
	(12.84)			
Observations	52	50	52	50

Public school tax income / students (log)	(1)	(2)	(3)	(4)
post	0.570	0.822***	0	0
	(0.505)	(0.245)	(.)	(.)
treat	0.359	0	0.359	0
	(0.490)	(.)	(0.462)	(.)
post*treat	0.218	-0.106	-0.752	-0.00401
	(0.671)	(0.324)	(0.717)	(0.399)
Constant	-0.621*			
	(0.371)			
Observations	52	50	52	50

Public school tax income / students	(1)	(2)	(3)	(4)
post	0.430	0.430	0	0
	(0.532)	(0.283)	(.)	(.)
treat	0.474	0	0.474	0
	(0.501)	(.)	(0.492)	(.)
post*treat	0.703	0.703*	0.0159	0.112
	(0.709)	(0.378)	(0.797)	(0.456)
Constant	0.872**			
	(0.376)			
Observations	52	50	52	50

Public school tax income / population (log)	(1)	(2)	(3)	(4)
post	-0.116	0.210	0	0
	(0.592)	(0.284)	(.)	(.)
treat	0.888	0	0.888*	0
	(0.583)	(.)	(0.525)	(.)
post*treat	0.261	-0.0819	-1.278	-0.0646
	(0.799)	(0.382)	(0.824)	(0.479)
Constant	-2.879***			
	(0.433)			
Observations	54	52	54	52

Public school tax income / population	(1)	(2)	(3)	(4)
post	-0.0443	-0.0443	0	0
	(0.0638)	(0.0427)	(.)	(.)
treat	0.111*	0	0.111*	0
	(0.0611)	(.)	(0.0586)	(.)
post*treat	0.0442	0.0442	-0.0775	0.00896
	(0.0864)	(0.0578)	(0.0957)	(0.0752)
Constant	0.130***			
	(0.0451)			
Observations	56	56	56	56

State FE	NO	YES	NO	YES
Slavehold. FE	NO	NO	YES	YES

State FE	NO	YES	NO	YES
Slavehold. FE	NO	NO	YES	YES

*Standard errors in parentheses. * p < 0.10, ** p < 0.05, *** p < 0.01*

Appendix 5.4.: *Liberal dataset with 300-dollar cut-off for control group*

Public school tax income / schools (log)	(1)	(2)	(3)	(4)	**Public school tax income / schools**	(1)	(2)	(3)	(4)
post	0.495	0.745***	0	0	post	9.813	9.813	0	0
	(0.509)	(0.260)	(.)	(.)		(25.58)	(16.52)	(.)	(.)
treat	0.169	0	0.169	0	treat	2.832	0	2.832	0
	(0.534)	(.)	(0.500)	(.)		(25.98)	(.)	(25.24)	(.)
post*treat	0.472	0.153	-0.388	0.161	post*treat	59.44	59.44**	26.21	28.56
	(0.732)	(0.374)	(0.743)	(0.428)		(36.74)	(23.72)	(38.87)	(26.07)
Constant	3.210***				Constant	52.75***			
	(0.371)					(18.09)			
Observations	54	52	54	52	Observations	54	52	54	52

Public school tax income / teachers (log)	(1)	(2)	(3)	(4)	**Public school tax income / teachers**	(1)	(2)	(3)	(4)
post	0.635	0.897***	0	0	post	20.01	20.01**	0	0
	(0.501)	(0.235)	(.)	(.)		(17.37)	(8.906)	(.)	(.)
treat	0.321	0	0.321	0	treat	13.30	0	13.30	0
	(0.518)	(.)	(0.480)	(.)		(17.37)	(.)	(16.53)	(.)
post*treat	0.262	-0.0833	-0.571	0.00214	post*treat	27.54	27.54**	1.574	10.74
	(0.709)	(0.333)	(0.708)	(0.375)		(24.57)	(12.60)	(25.29)	(13.48)
Constant	2.849***				Constant	29.48**			
	(0.366)					(12.28)			
Observations	52	50	52	50	Observations	52	50	52	50

Public school tax income / students (log)	(1)	(2)	(3)	(4)	**Public school tax income / students**	(1)	(2)	(3)	(4)
post	0.562	0.791***	0	0	post	0.480	0.480*	0	0
	(0.473)	(0.227)	(.)	(.)		(0.502)	(0.265)	(.)	(.)
treat	0.218	0	0.218	0	treat	0.379	0	0.379	0
	(0.489)	(.)	(0.459)	(.)		(0.502)	(.)	(0.489)	(.)
post*treat	0.255	-0.0585	-0.472	0.0246	post*treat	0.689	0.689*	0.106	0.238
	(0.669)	(0.322)	(0.677)	(0.362)		(0.710)	(0.375)	(0.749)	(0.409)
Constant	-0.524				Constant	0.949***			
	(0.346)					(0.355)			
Observations	52	50	52	50	Observations	52	50	52	50

Public school tax income / population (log)	(1)	(2)	(3)	(4)	**Public school tax income / population**	(1)	(2)	(3)	(4)
post	-0.108	0.197	0	0	post	-0.0428	-0.0428	0	0
	(0.565)	(0.264)	(.)	(.)		(0.0612)	(0.0400)	(.)	(.)
treat	0.677	0	0.677	0	treat	0.0882	0	0.0882	0
	(0.592)	(.)	(0.528)	(.)		(0.0622)	(.)	(0.0589)	(.)
post*treat	0.264	-0.0669	-0.939	-0.0460	post*treat	0.0466	0.0466	-0.0549	0.0203
	(0.812)	(0.380)	(0.785)	(0.435)		(0.0879)	(0.0575)	(0.0908)	(0.0678)
Constant	-2.716***				Constant	0.148***			
	(0.412)					(0.0433)			
Observations	54	52	54	52	Observations	56	56	56	56
State FE	NO	YES	NO	YES	State FE	NO	YES	NO	YES
Slavehold. FE	NO	NO	YES	YES	Slavehold. FE	NO	NO	YES	YES

*Standard errors in parentheses. * p < 0.10, ** p < 0.05, *** p < 0.01*

Appendix 5.5.: *Liberal dataset with 500-dollar cut-off for control group*

Public school tax income / schools (log)	(1)	(2)	(3)	(4)	**Public school tax income / schools**	(1)	(2)	(3)	(4)
post	0.604	0.784***	0	0	post	18.23	18.23	0	0
	(0.423)	(0.215)	(.)	(.)		(20.49)	(13.53)	(.)	(.)
treat	0.350	0	0.350	0	treat	21.95	0	21.95	0
	(0.622)	(.)	(0.583)	(.)		(27.75)	(.)	(26.76)	(.)
post*treat	0.400	0.142	-0.0711	0.135	post*treat	74.83*	74.83***	55.86	55.57**
	(0.837)	(0.438)	(0.800)	(0.454)		(39.24)	(25.91)	(38.68)	(24.58)
Constant	3.207***				Constant	48.14***			
	(0.305)					(14.49)			
Observations	54	52	54	52	Observations	54	52	54	52

Public school tax income / students (log)	(1)	(2)	(3)	(4)	**Public school tax income / students**	(1)	(2)	(3)	(4)
post	0.627	0.787***	0	0	post	0.575	0.575**	0	0
	(0.389)	(0.186)	(.)	(.)		(0.403)	(0.217)	(.)	(.)
treat	0.370	0	0.370	0	treat	0.689	0	0.689	0
	(0.563)	(.)	(0.530)	(.)		(0.538)	(.)	(0.520)	(.)
post*treat	0.181	-0.103	-0.203	-0.0698	post*treat	0.889	0.889**	0.558	0.629
	(0.757)	(0.371)	(0.726)	(0.382)		(0.760)	(0.409)	(0.750)	(0.394)
Constant	-0.508*				Constant	0.945***			
	(0.281)					(0.285)			
Observations	52	50	52	50	Observations	52	50	52	50

Public school tax income / teachers (log)	(1)	(2)	(3)	(4)	**Public school tax income / teachers**	(1)	(2)	(3)	(4)
post	0.693*	0.878***	0	0	post	22.79	22.79***	0	0
	(0.413)	(0.192)	(.)	(.)		(13.75)	(7.048)	(.)	(.)
treat	0.456	0	0.456	0	treat	23.77	0	23.77	0
	(0.597)	(.)	(0.554)	(.)		(18.33)	(.)	(17.28)	(.)
post*treat	0.208	-0.0885	-0.237	-0.0522	post*treat	39.06	39.06***	24.82	29.53**
	(0.803)	(0.385)	(0.758)	(0.395)		(25.92)	(13.29)	(24.91)	(12.44)
Constant	2.896***				Constant	29.44***			
	(0.298)					(9.721)			
Observations	52	50	52	50	Observations	52	50	52	50

Public school tax income / population (log)	(1)	(2)	(3)	(4)	**Public school tax income / population**	(1)	(2)	(3)	(4)
post	-0.0360	0.200	0	0	post	-0.0333	-0.0333	0	0
	(0.483)	(0.218)	(.)	(.)		(0.0532)	(0.0338)	(.)	(.)
treat	0.386	0	0.386	0	treat	0.0527	0	0.0527	0
	(0.710)	(.)	(0.624)	(.)		(0.0720)	(.)	(0.0670)	(.)
post*treat	0.167	-0.144	-0.544	-0.134	post*treat	0.0479	0.0479	-0.0173	0.0308
	(0.955)	(0.443)	(0.857)	(0.460)		(0.102)	(0.0646)	(0.0968)	(0.0677)
Constant	-2.483***				Constant	0.176***			
	(0.349)					(0.0376)			
Observations	54	52	54	52	Observations	56	56	56	56
State FE	NO	YES	NO	YES	State FE	NO	YES	NO	YES
Slavehold. FE	NO	NO	YES	YES	Slavehold. FE	NO	NO	YES	YES

*Standard errors in parentheses. * p < 0.10, ** p < 0.05, *** p < 0.01*

Appendix 5.6.: *Strict dataset with 0-dollar cut-off for control group*

Public school tax income / schools (log)	(1)	(2)	(3)	(4)
post	0.549	0.549*	0	0
	(0.643)	(0.266)	(.)	(.)
treat	-0.122	0	-0.122	0
	(0.595)	(.)	(0.563)	(.)
post*treat	0.433	0.349	-0.373	0.308
	(0.836)	(0.348)	(0.853)	(0.407)
Constant	3.496***			
	(0.455)			
Observations	54	52	54	52

Public school tax income / schools	(1)	(2)	(3)	(4)
post	2.584	2.584	0	0
	(36.37)	(24.62)	(.)	(.)
treat	-9.503	0	-9.503	0
	(33.20)	(.)	(32.70)	(.)
post*treat	66.77	66.77**	37.38	33.38
	(46.95)	(31.78)	(49.95)	(34.72)
Constant				
Observations	54	52	54	52

Public school tax income / teachers (log)	(1)	(2)	(3)	(4)
post	0.790	0.790***	0	0
	(0.641)	(0.214)	(.)	(.)
treat	0.128	0	0.128	0
	(0.581)	(.)	(0.545)	(.)
post*treat	0.117	0.0348	-0.622	0.0816
	(0.816)	(0.274)	(0.817)	(0.314)
Constant	3.044***			
	(0.453)			
Observations	52	50	52	50

Public school tax income / teachers	(1)	(2)	(3)	(4)
post	23.19	23.19*	0	0
	(25.09)	(13.39)	(.)	(.)
treat	13.14	0	13.14	0
	(22.44)	(.)	(21.84)	(.)
post*treat	24.20	24.20	2.856	8.212
	(31.73)	(16.94)	(32.97)	(18.12)
Constant	34.87*			
	(17.74)			
Observations	52	50	52	50

Public school tax income / students (log)	(1)	(2)	(3)	(4)
post	0.700	0.700***	0	0
	(0.616)	(0.209)	(.)	(.)
treat	0.0448	0	0.0448	0
	(0.558)	(.)	(0.528)	(.)
post*treat	0.122	0.0394	-0.554	0.0822
	(0.784)	(0.267)	(0.791)	(0.307)
Constant	-0.351			
	(0.435)			
Observations	52	50	52	50

Public school tax income / students	(1)	(2)	(3)	(4)
post	0.552	0.552	0	0
	(0.734)	(0.399)	(.)	(.)
treat	0.313	0	0.313	0
	(0.657)	(.)	(0.649)	(.)
post*treat	0.595	0.595	0.109	0.138
	(0.929)	(0.505)	(0.980)	(0.544)
Constant	1.156**			
	(0.519)			
Observations	52	50	52	50

Public school tax income / population (log)	(1)	(2)	(3)	(4)
post	0.0295	0.0295	0	0
	(0.684)	(0.266)	(.)	(.)
treat	0.314	0	0.314	0
	(0.633)	(.)	(0.572)	(.)
post*treat	0.138	0.116	-0.953	0.0666
	(0.889)	(0.349)	(0.866)	(0.408)
Constant	-2.393***			
	(0.484)			
Observations	54	52	54	52

Public school tax income / population	(1)	(2)	(3)	(4)
post	-0.0677	-0.0677	0	0
	(0.0857)	(0.0589)	(.)	(.)
treat	0.0680	0	0.0680	0
	(0.0782)	(.)	(0.0752)	(.)
post*treat	0.0566	0.0566	-0.0379	0.0221
	(0.111)	(0.0760)	(0.115)	(0.0885)
Constant	0.187***			
	(0.0606)			
Observations	56	56	56	56

State FE	NO	YES	NO	YES
Slavehold. FE	NO	NO	YES	YES

State FE	NO	YES	NO	YES
Slavehold. FE	NO	NO	YES	YES

*Standard errors in parentheses. * p < 0.10, ** p < 0.05, *** p < 0.01*

Appendix 5.7.: *Strict dataset with 300-dollar cut-off for control group*

Public school tax income / schools (log)	(1)	(2)	(3)	(4)
post	0.591	0.591**	0	0
	(0.587)	(0.243)	(.)	(.)
treat	-0.221	0	-0.221	0
	(0.587)	(.)	(0.556)	(.)
post*treat	0.424	0.323	-0.0989	0.277
	(0.822)	(0.344)	(0.807)	(0.373)
Constant	3.535***			
	(0.415)			
Observations	54	52	54	52

Public school tax income / schools	(1)	(2)	(3)	(4)
post	10.21	10.21	0	0
	(33.31)	(22.67)	(.)	(.)
treat	-9.118	0	-9.118	0
	(32.66)	(.)	(31.96)	(.)
post*treat	62.38	62.38*	40.84	37.84
	(46.19)	(31.44)	(46.84)	(31.23)
Constant	70.57***			
	(23.55)			
Observations	54	52	54	52

Public school tax income / teachers (log)	(1)	(2)	(3)	(4)
post	0.782	0.782***	0	0
	(0.581)	(0.193)	(.)	(.)
treat	-0.0596	0	-0.0596	0
	(0.569)	(.)	(0.534)	(.)
post*treat	0.156	0.0565	-0.308	0.0838
	(0.797)	(0.268)	(0.770)	(0.287)
Constant	3.153***			
	(0.411)			
Observations	52	50	52	50

Public school tax income / teachers	(1)	(2)	(3)	(4)
post	25.95	25.95**	0	0
	(22.96)	(12.15)	(.)	(.)
treat	7.459	0	7.459	0
	(22.06)	(.)	(21.27)	(.)
post*treat	22.84	22.84	7.688	12.13
	(31.20)	(16.50)	(30.95)	(16.33)
Constant	39.04**			
	(16.24)			
Observations	52	50	52	50

Public school tax income / students (log)	(1)	(2)	(3)	(4)
post	0.682	0.682***	0	0
	(0.557)	(0.188)	(.)	(.)
treat	-0.0894	0	-0.0894	0
	(0.545)	(.)	(0.518)	(.)
post*treat	0.178	0.0796	-0.238	0.106
	(0.764)	(0.261)	(0.746)	(0.280)
Constant	-0.277			
	(0.394)			
Observations	52	50	52	50

Public school tax income / students	(1)	(2)	(3)	(4)
post	0.604	0.604	0	0
	(0.666)	(0.361)	(.)	(.)
treat	0.249	0	0.249	0
	(0.640)	(.)	(0.630)	(.)
post*treat	0.591	0.591	0.254	0.293
	(0.905)	(0.490)	(0.916)	(0.491)
Constant	1.217**			
	(0.471)			
Observations	52	50	52	50

Public school tax income / population (log)	(1)	(2)	(3)	(4)
post	0.0435	0.0435	0	0
	(0.628)	(0.243)	(.)	(.)
treat	0.121	0	0.121	0
	(0.628)	(.)	(0.568)	(.)
post*treat	0.137	0.107	-0.578	0.0707
	(0.880)	(0.344)	(0.823)	(0.373)
Constant	-2.270***			
	(0.444)			
Observations	54	52	54	52

Public school tax income / population	(1)	(2)	(3)	(4)
post	-0.0617	-0.0617	0	0
	(0.0791)	(0.0538)	(.)	(.)
treat	0.0488	0	0.0488	0
	(0.0776)	(.)	(0.0740)	(.)
post*treat	0.0536	0.0536	-0.0138	0.0292
	(0.110)	(0.0746)	(0.108)	(0.0804)
Constant	0.203***			
	(0.0560)			
Observations	56	56	56	56

State FE	NO	YES	NO	YES
Slavehold. FE	NO	NO	YES	YES

State FE	NO	YES	NO	YES
Slavehold. FE	NO	NO	YES	YES

*Standard errors in parentheses. * p < 0.10, ** p < 0.05, *** p < 0.01*

Appendix 5.8.: *Strict dataset with 500-dollar cut-off for control group*

Public school tax income / schools (log)	(1)	(2)	(3)	(4)
post	0.702	0.702***	0	0
	(0.476)	(0.201)	(.)	(.)
treat	0.195	0	0.195	0
	(0.674)	(.)	(0.642)	(.)
post*treat	0.355	0.203	0.145	0.178
	(0.927)	(0.403)	(0.888)	(0.411)
Constant	3.376***			
	(0.337)			
Observations	54	52	54	52

Public school tax income / schools	(1)	(2)	(3)	(4)
post	19.50	19.50	0	0
	(25.05)	(17.84)	(.)	(.)
treat	27.33	0	27.33	0
	(33.47)	(.)	(32.47)	(.)
post*treat	82.64*	82.64**	73.23	70.89**
	(47.33)	(33.71)	(46.16)	(30.28)
Constant	58.17***			
	(17.71)			
Observations	54	52	54	52

Public school tax income / teachers (log)	(1)	(2)	(3)	(4)
post	0.823*	0.823***	0	0
	(0.463)	(0.156)	(.)	(.)
treat	0.325	0	0.325	0
	(0.641)	(.)	(0.607)	(.)
post*treat	0.112	-0.0435	-0.055	-0.038
	(0.883)	(0.305)	(0.839)	(0.312)
Constant	3.037***			
	(0.328)			
Observations	52	50	52	50

Public school tax income / teachers	(1)	(2)	(3)	(4)
post	26.84	26.84***	0	0
	(16.74)	(9.123)	(.)	(.)
treat	30.38	0	30.38	0
	(21.91)	(.)	(20.88)	(.)
post*treat	39.35	39.35**	33.54	35.04**
	(30.99)	(16.89)	(29.63)	(15.49)
Constant	34.22***			
	(11.83)			
Observations	52	50	52	50

Public school tax income / students (log)	(1)	(2)	(3)	(4)
post	0.746	0.746***	0	0
	(0.446)	(0.152)	(.)	(.)
treat	0.274	0	0.274	0
	(0.617)	(.)	(0.589)	(.)
post*treat	0.078	-0.0832	-0.072	-0.079
	(0.849)	(0.297)	(0.814)	(0.305)
Constant	-0.395			
	(0.315)			
Observations	52	50	52	50

Public school tax income / students	(1)	(2)	(3)	(4)
post	0.689	0.689**	0	0
	(0.500)	(0.285)	(.)	(.)
treat	0.897	0	0.897	0
	(0.655)	(.)	(0.640)	(.)
post*treat	0.807	0.807	0.673	0.685
	(0.926)	(0.527)	(0.908)	(0.495)
Constant	1.090***			
	(0.354)			
Observations	52	50	52	50

Public school tax income / population (log)	(1)	(2)	(3)	(4)
post	0.115	0.115	0	0
	(0.514)	(0.199)	(.)	(.)
treat	0.0176	0	0.0176	0
	(0.727)	(.)	(0.659)	(.)
post*treat	0.00743	-0.0706	-0.294	-0.0863
	(1.000)	(0.398)	(0.911)	(0.407)
Constant	-2.214***			
	(0.364)			
Observations	54	52	54	52

Public school tax income / population	(1)	(2)	(3)	(4)
post	-0.0434	-0.0434	0	0
	(0.0653)	(0.0442)	(.)	(.)
treat	0.0476	0	0.0476	0
	(0.0872)	(.)	(0.0823)	(.)
post*treat	0.0344	0.0344	0.00310	0.0222
	(0.123)	(0.0836)	(0.117)	(0.0845)
Constant	0.215***			
	(0.0462)			
Observations	56	56	56	56

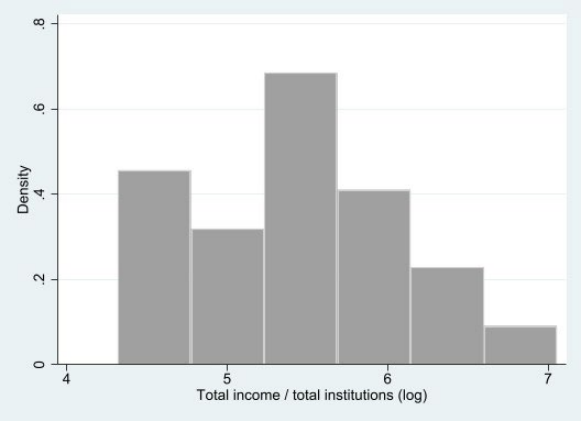
State FE	NO	YES	NO	YES
Slavehold. FE	NO	NO	YES	YES

State FE	NO	YES	NO	YES
Slavehold. FE	NO	NO	YES	YES

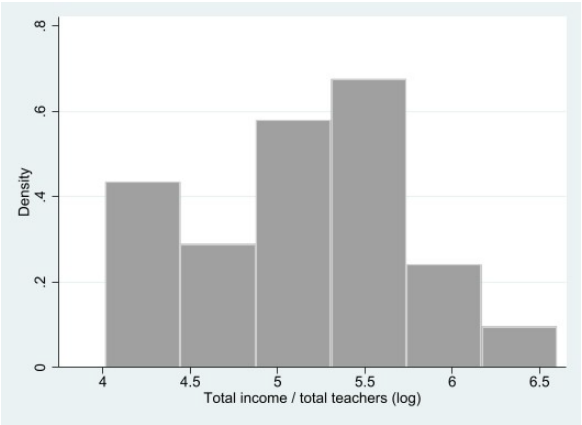
*Standard errors in parentheses. * p < 0.10, ** p < 0.05, *** p < 0.01*

Appendix 6.: Additional tests on total income variables (main dataset with 300-dollar cut-off)

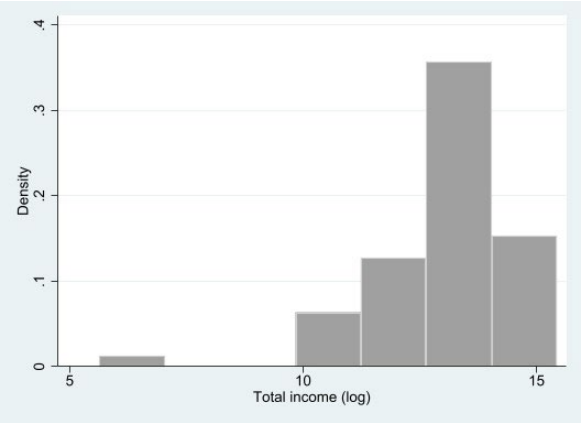
Total income / total institutions (log)	(1)	(2)	(3)	(4)
post	0.273	0.273***	0	0
	(0.266)	(0.0861)	(.)	(.)
treat	-0.480*	0	-0.480*	0
	(0.246)	(.)	(0.244)	(.)
post*treat	0.366	0.366***	0.507	0.295**
	(0.348)	(0.113)	(0.360)	(0.119)
Constant	5.427***			
	(0.188)			
Observations	48	48	48	48



Total income / total teachers (log)	(1)	(2)	(3)	(4)
post	0.229	0.229***	0	0
	(0.244)	(0.066)	(.)	(.)
treat	-0.527**	0	-0.527**	0
	(0.226)	(.)	(0.221)	(.)
post*treat	0.326	0.326***	0.493	0.284***
	(0.319)	(0.087)	(0.326)	(0.094)
Constant	5.266***			
	(0.173)			
Observations	48	48	48	48



Total income (log)	(1)	(2)	(3)	(4)
post	-0.0793	-0.0793	0	0
	(0.660)	(0.367)	(.)	(.)
treat	-0.335	0	-0.335	0
	(0.617)	(.)	(0.622)	(.)
post*treat	0.872	0.872*	0.767	0.697
	(0.873)	(0.485)	(0.923)	(0.535)
Constant	13.13***			
	(0.466)			
Observations	56	56	56	56
State FE	NO	YES	NO	YES
Slavehold. FE	NO	NO	YES	YES



Standard errors in parentheses
* p < 0.10, ** p < 0.05, *** p < 0.01

Appendix 7.: Additional tests on institutional subgroups (main dataset with 300-dollar cut-off)

Appendix 7.1.: *Public schools*

Public school income (log)	(1)	(2)	(3)	(4)
post	0.119	0.119	0	0
	(0.547)	(0.276)	(.)	(.)
treat	0.435	0	0.435	0
	(0.519)	(.)	(0.518)	(.)
post*treat	0.279	0.402	0.036	0.227
	(0.728)	(0.371)	(0.763)	(0.402)
Constant	12.37***			
	(0.387)			
Observations	55	54	55	54

Public school income / public schools (log)	(1)	(2)	(3)	(4)
post	-0.0197	-0.0197	0	0
	(0.321)	(0.197)	(.)	(.)
treat	-0.372	0	-0.372	0
	(0.304)	(.)	(0.308)	(.)
post*treat	0.549	0.549**	0.531	0.522*
	(0.431)	(0.264)	(0.454)	(0.293)
Constant	5.202***			
	(0.227)			
Observations	54	54	54	54

Public school income / teachers (log)	(1)	(2)	(3)	(4)
post	0.126	0.126	0	0
	(0.321)	(0.162)	(.)	(.)
treat	-0.283	0	-0.283	0
	(0.299)	(.)	(0.302)	(.)
post*treat	0.334	0.334	0.363	0.376
	(0.423)	(0.213)	(0.443)	(0.232)
Constant	4.967***			
	(0.227)			
Observations	52	52	52	52

Public school income / students (log)	(1)	(2)	(3)	(4)
post	0.0527	0.0527	0	0
	(0.332)	(0.195)	(.)	(.)
treat	-0.266	0	-0.266	0
	(0.309)	(.)	(0.311)	(.)
post*treat	0.304	0.304	0.372	0.350
	(0.437)	(0.256)	(0.456)	(0.279)
Constant	1.449***			
	(0.235)			
Observations	52	52	52	52

Public school income / population (log)	(1)	(2)	(3)	(4)
post	-0.289	-0.289	0	0
	(0.227)	(0.185)	(.)	(.)
treat	-0.047	0	-0.047	0
	(0.215)	(.)	(0.201)	(.)
post*treat	0.310	0.336	0.056	0.200
	(0.302)	(0.248)	(0.296)	(0.266)
Constant	-0.971***			
	(0.160)			
Observations	55	54	55	54

Public schools / population (log)	(1)	(2)	(3)	(4)
post	-0.269	-0.269	0	0
	(0.382)	(0.293)	(.)	(.)
treat	0.325	0	0.325	0
	(0.363)	(.)	(0.358)	(.)
post*treat	-0.191	-0.214	-0.437	-0.322
	(0.509)	(0.394)	(0.527)	(0.434)
Constant post	-6.173***			
	(0.270)			
Observations	55	54	55	54

Public school teachers / population (log)	(1)	(2)	(3)	(4)
post	-0.304	-0.343	0	0
	(0.404)	(0.304)	(.)	(.)
treat	0.325	0	0.325	0
	(0.384)	(.)	(0.376)	(.)
post*treat	-0.097	-0.070	-0.384	-0.215
	(0.533)	(0.401)	(0.547)	(0.430)
Constant	-6.027***			
	(0.292)			
Observations	54	52	54	52

Public school students / population (log)	(1)	(2)	(3)	(4)
post	-0.263	-0.270	0	0
	(0.430)	(0.341)	(.)	(.)
treat	0.308	0	0.308	0
	(0.409)	(.)	(0.398)	(.)
post*treat	-0.041	-0.040	-0.308	-0.188
	(0.568)	(0.449)	(0.578)	(0.483)
Constant	-2.509***			
	(0.311)			
Observations	54	52	54	52

State FE	NO	YES	NO	YES
Slaveholding FE	NO	NO	YES	YES

State FE	NO	YES	NO	YES
Slaveholding FE	NO	NO	YES	YES

Standard errors in parentheses

** p < 0.10, ** p < 0.05, *** p < 0.01*

Appendix 7.2.: *Colleges*

College income (log)	(1)	(2)	(3)	(4)
post	-0.423	-0.251	0	0
	(0.554)	(0.346)	(.)	(.)
treat	-0.872	0	-0.872	0
	(0.525)	(.)	(0.531)	(.)
post*treat	0.450	0.336	0.476	0.138
	(0.723)	(0.453)	(0.758)	(0.492)
Constant	11.62***			
	(0.401)			
Observations	51	48	51	48

College income / colleges (log)	(1)	(2)	(3)	(4)
post	0.108	0.108	0	0
	(0.345)	(0.228)	(.)	(.)
treat	-0.537	0	-0.537*	0
	(0.320)	(.)	(0.310)	(.)
post*treat	0.0375	0.0375	-0.223	-0.225
	(0.452)	(0.298)	(0.457)	(0.298)
Constant	9.026***			
	(0.244)			
Observations	48	48	48	48

College income / teachers (log)	(1)	(2)	(3)	(4)
post	0.119	0.119	0	0
	(0.230)	(0.213)	(.)	(.)
treat	-0.442**	0	-0.442**	0
	(0.213)	(.)	(0.213)	(.)
post*treat	0.0463	0.0463	-0.0356	-0.133
	(0.301)	(0.278)	(0.315)	(0.293)
Constant	7.014***			
	(0.163)			
Observations	48	48	48	48

College income / students (log)	(1)	(2)	(3)	(4)
post	0.0394	0.0394	0	0
	(0.238)	(0.171)	(.)	(.)
treat	-0.360	0	-0.360	0
	(0.220)	(.)	(0.222)	(.)
post*treat	-0.149	-0.149	-0.200	-0.327
	(0.312)	(0.223)	(0.328)	(0.228)
Constant	4.278***			
	(0.168)			
Observations	48	48	48	48

College income / population (log)	(1)	(2)	(3)	(4)
post	-0.407	-0.477	0	0
	(0.328)	(0.283)	(.)	(.)
treat	-0.829**	0	-0.829**	0
	(0.311)	(.)	(0.313)	(.)
post*treat	0.179	0.155	0.116	-0.0350
	(0.428)	(0.371)	(0.447)	(0.399)
Constant	-2.373***			
	(0.237)			
Observations	51	48	51	48

Colleges / population (log)	(1)	(2)	(3)	(4)
post	-0.587**	-0.585***	0	0
	(0.282)	(0.168)	(.)	(.)
treat	-0.480*	0	-0.480*	0
	(0.266)	(.)	(0.266)	(.)
post*treat	0.227	0.118	0.336	0.190
	(0.371)	(0.220)	(0.385)	(0.242)
Constant	-11.21***			
	(0.199)			
Observations	52	48	52	48

College teachers / population (log)	(1)	(2)	(3)	(4)
post	-0.493**	-0.596***	0	0
	(0.234)	(0.207)	(.)	(.)
treat	-0.387*	0	-0.387*	0
	(0.222)	(.)	(0.225)	(.)
post*treat	0.0862	0.109	0.0820	0.0981
	(0.306)	(0.271)	(0.321)	(0.301)
Constant	-9.387***			
	(0.170)			
Observations	51	48	51	48

College students / population (log)	(1)	(2)	(3)	(4)
post	-0.361	-0.516**	0	0
	(0.241)	(0.214)	(.)	(.)
treat	-0.469**	0	-0.469**	0
	(0.229)	(.)	(0.231)	(.)
post*treat	0.182	0.304	0.140	0.292
	(0.315)	(0.280)	(0.329)	(0.312)
Constant	-6.651***			
	(0.175)			
Observations	51	48	51	48

State FE	NO	YES	NO	YES
Slaveholding FE	NO	NO	YES	YES

State FE	NO	YES	NO	YES
Slaveholding FE	NO	NO	YES	YES

Standard errors in parentheses

** p < 0.10, ** p < 0.05, *** p < 0.01*

Appendix 7.3.: *Academies*

Academy income (log)	(1)	(2)	(3)	(4)
post	-0.302	-0.302	0	0
	(0.647)	(0.255)	(.)	(.)
treat	-0.628	0	-0.628	0
	(0.606)	(.)	(0.609)	(.)
post*treat	0.854	0.854**	1.038	0.848**
	(0.856)	(0.338)	(0.903)	(0.378)
Constant	12.02***			
	(0.458)			
Observations	56	56	56	56

Academy income / academies (log)	(1)	(2)	(3)	(4)
post	0.574**	0.574***	0	0
	(0.229)	(0.171)	(.)	(.)
treat	-0.159	0	-0.159	0
	(0.214)	(.)	(0.208)	(.)
post*treat	0.0849	0.0849	0.271	0.268
	(0.303)	(0.226)	(0.309)	(0.236)
Constant	6.475***			
	(0.162)			
Observations	56	56	56	56

Academy income / teachers (log)	(1)	(2)	(3)	(4)
post	0.135	0.135	0	0
	(0.214)	(0.133)	(.)	(.)
treat	-0.265	0	-0.265	0
	(0.197)	(.)	(0.190)	(.)
post*treat	0.165	0.165	0.331	0.302*
	(0.278)	(0.172)	(0.279)	(0.174)
Constant	6.075***			
	(0.152)			
Observations	54	54	54	54

Academy income / students (log)	(1)	(2)	(3)	(4)
post	-0.0549	-0.0549	0	0
	(0.266)	(0.152)	(.)	(.)
treat	-0.372	0	-0.372	0
	(0.244)	(.)	(0.227)	(.)
post*treat	0.130	0.130	0.405	0.309
	(0.346)	(0.197)	(0.333)	(0.193)
Constant	3.006***			
	(0.188)			
Observations	54	54	54	54

Academy income / population (log)	(1)	(2)	(3)	(4)
post	-0.710**	-0.710***	0	0
	(0.304)	(0.166)	(.)	(.)
treat	-0.833***	0	-0.833***	0
	(0.285)	(.)	(0.282)	(.)
post*treat	0.608	0.608**	0.781*	0.682***
	(0.402)	(0.219)	(0.419)	(0.242)
Constant	-1.318***			
	(0.215)			
Observations	56	56	56	56

Academies / population (log)	(1)	(2)	(3)	(4)
post	-1.284***	-1.284***	0	0
	(0.258)	(0.220)	(.)	(.)
treat	-0.674***	0	-0.674***	0
	(0.242)	(.)	(0.244)	(.)
post*treat	0.524	0.524	0.510	0.415
	(0.342)	(0.291)	(0.362)	(0.321)
Constant	-7.793***			
	(0.183)			
Observations	56	56	56	56

Academy teachers / population (log)	(1)	(2)	(3)	(4)
post	-0.937***	-0.810***	0	0
	(0.259)	(0.176)	(.)	(.)
treat	-0.604**	0	-0.604**	0
	(0.243)	(.)	(0.246)	(.)
post*treat	0.536	0.409*	0.510	0.353
	(0.340)	(0.229)	(0.359)	(0.250)
Constant	-7.358***			
	(0.187)			
Observations	55	54	55	54

Academy students / population (log)	(1)	(2)	(3)	(4)
post	-0.707***	-0.620***	0	0
	(0.252)	(0.165)	(.)	(.)
treat	-0.497**	0	-0.497**	0
	(0.237)	(.)	(0.235)	(.)
post*treat	0.531	0.444**	0.404	0.346
	(0.330)	(0.215)	(0.344)	(0.230)
Constant	-4.289***			
	(0.182)			
Observations	55	54	55	54

State FE	NO	YES	NO	YES
Slaveholding FE	NO	NO	YES	YES

State FE	NO	YES	NO	YES
Slaveholding FE	NO	NO	YES	YES

Standard errors in parentheses

** p < 0.10, ** p < 0.05, *** p < 0.01*