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The Finance-Education Nexus: Educational Consequences of US Interstate Bank Branching Deregulation

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Abstract

This paper studies the impact of US interstate bank branching deregulation on school finance and student achievement, leveraging the deregulation as a state tax revenue shock. Total revenue and expenditure increase following the deregulation. The revenue increase stems mainly from higher state aid, with spending gains concentrated in capital outlays. Deregulation subsequently improves student achievement, with no distributional effects evident across students' ability, race, or free lunch status. The findings highlight the spillover benefits of a centralized school finance system in channeling positive tax revenue shocks into public education funding and human capital formation.

JEL classification

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Keywords

banking deregulation, school finance system, student achievement

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1 Introduction

Public K-12 education funding represents a substantial share of government spending and GDP in the United States and worldwide.[1] Ensuring an adequate provision of public spending plays a crucial role in shaping students' outcomes and opportunities, forming a vital component of optimal social investments for policymakers (Hoxby, 2001; Jackson et al., 2016). Consequently, a large body of research has examined the impacts of school finance equalizations on student outcomes, addressing the classic question: "Does school spending matter?"(Jackson, 2018; Handel and Hanushek, 2023). At the same time, school finance equalizations have shifted the U.S. K-12 system from being predominantly locally funded to one in which state tax revenues have become a central funding source. By contrast, relatively little is known about how broader economic conditions — specifically, state revenue shocks — interact with the school finance system and, subsequently, influence the provision of public education funding and human capital formation.

This paper fills the gap by studying the impact of US interstate bank branching deregulation on school finance and student achievement. On the one hand, banking deregulation in the late 20th century, which eliminated geographic restrictions on bank expansion that had been in place since the 1930s, reshaped the banking industry and the economy as a whole — commonly referred to as the "real effects".[2] On the other hand, K-12 public education is primarily funded by state governments through sales and income

[1]In the fiscal year 2020, more than 21% of state and local government expenditure was allocated to elementary and secondary education (U.S. Census Bureau, 2020). On a global scale, in 2019, public spending on education from primary to postsecondary levels accounted for 11.7% of government expenditures in the United States, compared to an average of 10.6% among OECD countries. When considering the share relative to GDP, it represented 4.6% (4.4%) in the United States (OECD counties) (OECD, 2022).

[2]Relaxing geographic restrictions in the banking industry has led to a more consolidated yet less locally concentrated banking system. In a seminal study, Jayaratne and Strahan (1996) examine the effects of financial deregulation on the real economy, finding that real per capita income and output growth rates increased following the deregulation. Building on this finding, Strahan et al. (2003) identifies accelerated state economic growth after deregulation, particularly within the entrepreneurial sector. Subsequent research has explored the mechanisms behind these effects, showing that deregulation alleviates credit constraints for firms and reduces capital market frictions, thereby accelerating new business formation (Kerr and Nanda, 2009), fostering firm innovation (Chava et al., 2013), and enhancing total factor productivity among small businesses (Krishnan et al., 2015). See Kroszner and Strahan (2014) for a thorough review of this literature.

taxes (state revenue) and by local governments through local property taxes (local revenue).[3] As a result, deregulation could influence public school funding through two channels: (1) a *state revenue* channel, by influencing income and/or sales tax revenues as a result of its impact on aggregate growth (Strahan et al., 2003); and (2) a *local revenue* channel, by affecting local property tax revenues through its influence on the housing market (Favara and Imbs, 2015). Despite the relevance of these potential links, little is known about the causal relationship between banking deregulation and school finance. Furthermore, by examining the downstream effects of deregulation-induced school spending changes on student achievement, this paper offers new nationwide evidence to the resource-education literature.[4]

Our empirical analysis exploits the exogenous state-level reforms on interstate branching deregulation. The Interstate Banking and Branching Efficiency Act (IBBEA), implemented in 1994, deregulated the banking industry and permitted interstate bank branching. However, states had the discretion to maintain barriers to out-of-state bank entry. Consequently, over the period 1994 to 2005, deregulation occurred in a staggered manner across states.[5] We leverage the interstate branching deregulation as a shock to state government tax revenues, to study the impacts of deregulation on school finance and student achievement.[6] Exploiting the staggered implementation of deregulation documented by Rice

[3]The total revenue of a US public school district is composed of three main sources: state revenue, local revenue, and federal revenue. Between the 2000-01 and 2018-19 school years, state revenue accounted for 47% and local revenue for 44% of the per-pupil total revenue, together comprising over 90% of a typical school district's funding, with federal revenue contributing the remaining 9% (NCES, 2022).

[4]The resource-education relationship debate dates back to the 1960s (Coleman, 1968). Earlier studies suggested no significant relationship between school resources and student achievement (Hanushek, 2003). However, recent research using exogenous policy variation (e.g., school finance reforms) has shown positive impacts (Jackson, 2018). See Jackson (2018) and Handel and Hanushek (2023) for recent surveys.

[5]The interstate bank branching deregulation, prompted by the 1994 IBBEA, is widely studied in the finance literature. As documented in the literature, the timing of adopting interstate branching deregulation across states was plausibly exogenous, and was motivated by political factors, rather than by contemporaneous economic conditions (Kroszner and Strahan, 1999, 2014).

[6]In addition to prior findings indicating higher state economic growth following the deregulation (Strahan et al., 2003), our analysis shows that deregulation increased state government tax revenues — particularly income tax revenues (Figure 1), a key funding source for school finance. While deregulation may influence local housing markets (Favara and Imbs, 2015), we demonstrate that local revenue is not a driving channel for higher school resources, as it is less responsive to market fluctuations.

and Strahan (2010), we use a difference-in-differences (DiD) design, with both traditional two-way fixed effects (TWFE) and recent dynamic DiD estimators, to identify the dynamic treatment effects of deregulation on school finance and student achievement. In addition, we introduce a modified instrumental variables (IV) approach that incorporates dynamic treatment effects to estimate the school spending effect — how deregulation affects student achievement through school spending, while isolating other potential channels.

We use two main administrative datasets in our analysis. For school finance data, we use the School District Finance Survey (F-33) from the Common Core of Data (CCD), which provides comprehensive financial information for all US school districts since 1990, including detailed revenue, expenditure, and district characteristics. For student achievement, we employ restricted-use data from the National Assessment of Educational Progress (NAEP), commonly known as the Nation's Report Card. This dataset includes individual scores from nationwide assessments on mathematics and reading for students in grades 4 and 8, available biennially since the early 1990s. Together, the F-33 survey and NAEP datasets allow us to explore in detail the educational consequences of deregulation nationally over extensive pre- and post-periods.

Our results on school finance reveal that deregulation leads to increases in per-pupil total revenue and expenditure of school districts in the treated states of about 4.3% and 4.0%, respectively. Consistent with the evidence that deregulation leads to increased income tax revenues for state governments, the gains in total revenue are primarily driven by the increases in state tax revenue, where the income tax is a major funding source. While deregulation may impact local housing markets, we demonstrate that local revenue does not contribute to the increase in total revenue. The increases in total revenue and expenditure are most notable in low-income school districts. This pattern arises because, while both high- and low-income districts experience higher state revenue following deregulation, high-income districts offset these gains by receiving significantly less local revenue in response to the increase in state aid. Finally, the results are robust across various validity checks, including different dynamic DiD estimators, addressing concerns from potential confounding events, and a contiguous county-pairs design.

We further find that deregulation improves student achievement. Our reduced-form estimates indicate that student assessment scores increased by approximately 0.064 standard deviations following deregulation, about half the effect of post-1990 school finance reforms on students in low-income districts (Lafortune et al., 2018). These results remain robust across multiple checks that address confounding factors, contemporaneous events, and sensitivity to sample construction. In addition, we conduct a falsification test showing no effects on private school test scores, as well as additional tests that rule out selective migration of families to areas with increased school spending as a potential confounder. Further investigation of distributional effects shows that the achievement gains do not vary by student groups based on their ability, race, or free lunch status. These findings indicate that banking deregulation improves overall achievement without affecting inequality in educational outcomes.

Our findings shed new light on how the school finance system interacts with broader economic conditions to shape public education funding — one of the most important functions of state and local governments — and, in turn, student achievement. Since the 1970s, school finance reforms have shifted the system from being almost entirely locally determined to one in which states play a central equalizing role, with roughly equal shares of funding now coming from state and local tax revenues. While prior research has shown that this institutional evolution itself had major consequences (e.g., Murray et al. (1998); Jackson et al. (2016)), much less is known about how the post-equalization structure conditions the effects of external revenue shocks. Jackson et al. (2021) provide one of the few examples, showing that the financing system made student outcomes more vulnerable to negative state tax revenue shocks induced by the Great Recession. In contrast, our study highlights the spillover benefits of this increased centralization of school finance systems. We show that interstate banking deregulation increased state government tax revenues and, through equalization-based school finance systems, translated into higher per-pupil spending and improved student achievement, with larger increases in state aid flowing to lower-income districts.

Our study also extends the emerging literature on the broader consequences of financial deregulation. While earlier work documented the "real effects" of the US interstate bank branching deregulation (e.g., Rice and Strahan (2010); Krishnan et al. (2015)), more recent studies have examined its wider consequences.[7] Extending this line of research, we offer the first causal evidence linking banking deregulation to the provision of public education funding, thereby demonstrating its fiscal consequences as part of the broader social welfare implications of financial deregulation. Closely related, very few studies examine the impact of bank branching deregulation on human capital, and the existing evidence points in different directions. On the one hand, Hu et al. (2020) find that interstate branching deregulation reduces academic performance among children from low-income families, potentially because low-income parents reallocating time from childrearing to employment. On the other hand, Chang and Ravindran (2023) exploit branch expansion in underbanked districts in India using a regression discontinuity design and find significant improvements in students' test scores. Leveraging restricted-use individual-level NAEP datasets, this study provides one of the most comprehensive assessments to date and finds evidence consistent with the latter. We show that deregulation increased school spending and improved student achievement, without widening achievement gaps across groups based on ability, race, and free lunch status. In doing so, we also directly address the open question of whether financial deregulation alters educational inequality, complementing prior work that found deregulation reduced income inequality by raising incomes at the lower end of the distribution (Beck et al., 2010).

Finally, this study speaks to the literature on the effects of capital expenditures on student achievement. While prior studies have reached different conclusions (e.g, Cellini

[7]See studies on the impacts of interstate bank branching deregulation on housing markets (Favara and Imbs, 2015), the agricultural sector (Kandilov and Kandilov, 2018), household wealth accumulation (Célerier and Matray, 2019), fertility decisions (Yang, 2024), and mental health (Hu et al., 2024).

et al. (2010) find no effects, whereas Neilson and Zimmerman (2014) find positive effects on test scores), more recent work differentiates across project types and finds that increased capital spending on essential infrastructure improves test scores (Biasi et al., 2025). Although we cannot distinguish between types of capital projects, we find that deregulation-induced increases in capital outlays constitute one of the main drivers behind the improvements in student achievement. More broadly, this study contributes new nationwide evidence to long-standing debate over whether school spending matters (Jackson, 2018; Handel and Hanushek, 2023).[8] Existing studies typically draw on variation of school finance reforms (SFRs) of the 1970s-1990s, analyzing how redistributing educational resources within states — especially towards low-income districts — affects student outcomes. By contrast, our deregulation-based variation differs from SFRs in two respects: 1) we exploit more naturally occurring, tax-based variation rather than a large, discrete funding shift; and 2) whereas SFRs are often silent on the underlying sources of revenue changes, the deregulation-induced variation we exploit affects school finance explicitly through shocks to state tax revenues, and not through the local revenue channel.[9]

The paper proceeds as follows. Section 2 provides background on the 1990s US interstate branching deregulation and policy variation. Sections 3 and 4 describe the data and empirical strategy, respectively. Section 5 investigates the impact of deregulation on school finance, while Section 6 examines the impact on student achievement. Section 7 concludes.

[8] Many of these studies focus on single-state analyses. Among those conducting nationwide analyses, researchers have examined the impact of school spending on student achievement (Murray et al., 1998; Hoxby, 2001; Card and Payne, 2002; Sims, 2011; Lafortune et al., 2018), long-run adult outcomes (Jackson et al., 2016; Rothstein and Schanzenbach, 2022), and intergenerational mobility (Biasi, 2023).

[9] Exploiting non-SFRs variation also mitigates the confounding effects of contemporaneous educational initiatives when estimating the school spending effect. Court-mandated or legislative changes in school spending (i.e., school finance reforms) are often components of broader, multi-year initiatives aimed at improving educational outcomes. Many SFR-based studies acknowledge that spending and education policies may change contemporaneously, potentially confounding the school spending estimates. For detailed documentation of concurrent educational policy changes alongside school finance reforms in Arkansas and Michigan, see McGee (2023).

2 Interstate Branching Deregulation

Historically, states regulated geographic expansions of banking industries to enhance state revenues.[10] The McFadden Act of 1927 mandated that national banks adhere to state-level restrictions on branching, effectively prohibiting interstate banking. To avoid restrictions, banks formed multi-bank holding companies (MBHCs) that operated bank subsidiaries across state lines. As the MBHCs grew in size and market concentration, some states began to limit their intrastate branching. In 1956, the Douglas Amendment granted states the authority to further restrict bank acquisitions of MBHCs with out-of-state headquarters.

The regulation remained largely unchanged until the 1970s.[11] Beginning in the 1970s, there was a gradual deregulation of intrastate branching, allowing banks to expand within their respective states through mergers and acquisitions (M&A) or by opening *de novo* branches. In 1982, the Garn-St. Germain Act authorized out-of-state MBHCs to purchase failing banks or thrifts across state lines, further facilitating interstate banking deregulation. This wave of deregulation in the 1970s and 1980s introduced a series of changes to the banking sector, enabling intrastate branching and interstate banking.[12]

Following the deregulation of intrastate branching and interstate banking, another wave of deregulation permitting interstate branching emerged. In 1994, the Interstate Banking and Branching Efficiency Act (IBBEA) was passed by the federal government, which deregulated the banking industry and legalized interstate bank branching. However, states retained the authority to impose barriers along four dimensions that restricted out-of-state bank branch

[10]The ratification of the US Constitution in 1788 eliminated states' powers to issue paper money and tax imports/exports, reducing state public finance sources. In response, state governments turned to the banking industry as a revenue source, using strategies such as chartering banks, owning or acquiring bank shares, and levying taxes on banks (Sylla et al., 1987). This shift in focus gave the origin of many regulations on the geographical expansion of banks. During the first third of the nineteenth century, a dozen states had a bank-related share of total state revenues that surpassed 10 percent (Kroszner and Strahan, 2014).

[11]There are a few exceptions banks operated across state lines before the 1970s. One example is the Freedman's Savings Bank, which operated 37 branches across 17 states between 1865 and 1872, aiming to provide financial services to emancipated former slaves (Stein and Yannelis, 2020; Célérier and Tak, 2025). Some states also had multilateral reciprocity that relaxed restrictions on their banking industries, but such agreements were largely regional.

[12]See Kerr and Nanda (2009); Rice and Strahan (2010) for recent studies and Kroszner and Strahan (2014) for a detailed survey.

entry. Between 1994 and 2005, these barriers were gradually deregulated in a staggered way across states, resulting in expansions of interstate bank branching. The deregulation measures were implemented along four dimensions: (1) relaxed the requirement that the targeted bank be less than three years old, (2) allowed *de novo* branching without explicit state authority, (3) permitted the acquisition of individual branches without acquiring the entire bank, and (4) set the total amount of state-wide deposits controlled by a single bank or bank holding company to be the same or larger than the IBBEA default (i.e., 30%).[13]

Identifying Variation in Deregulation.— We obtained information on state-level variation in deregulation from US interstate branching laws enacted between 1994 and 2005 from data compiled by Rice and Strahan (2010). The staggered deregulation laws relaxed bank branching restrictions across the four dimensions described above. The interstate branching deregulation has been widely studies in the finance literature. As documented in the literature, the timing of adopting interstate branching deregulation across states was plausibly exogenous, and was motivated by political factors, rather than by contemporaneous economic conditions (Kroszner and Strahan, 1999, 2014).

Table 1 presents the timing of the four types of branching deregulation reforms between 1994 and 2005. Among the 50 US states and Washington, DC, 43 relaxed their intrastate branching barriers during the study period. The branching deregulation occurred nationwide but for different states at different times, providing sufficient cross-state policy variation to identify the causal effect of branching deregulation. Further disentangling policy variation by deregulation type underscores the significance of statewide deposit cap deregulation, particularly in terms of its geographic coverage and timing of implementation. In terms of geographic coverage, among the 43 deregulated states, the deregulation of the statewide deposit cap is the most widely adopted reform (38 states), with only five deregulated states adopting a non-statewide-deposit-cap reform.[14] Among the 38 states with statewide deposit

[13]The deregulation on statewide deposit caps allowed banks to hold a higher proportion of deposits within the deregulated state. This change was aimed at encouraging banks to engage in cross-state mergers or expand branch networks, thereby increasing market share and enhancing the provision of financial services.

[14]The five states are Kentucky, North Dakota, Oklahoma, Texas, and West Virginia (see Table 1).

cap deregulation, a little less than half (16) solely focused on the statewide deposit cap. The other three types of deregulation share similar reform timing, as they are often bundled and enacted in a single deregulation act issued by the state government.[15] In terms of reform timing, the statewide deposit cap also tended to be implemented earlier than the other three types of deregulation. For instance, among the 22 deregulated states that implemented the other three types of deregulation, four first regulated the statewide deposit cap and then other dimensions, while 18 deregulated the statewide deposit cap and others at the same time.

To undertake a more accurate investigation of the effects of deregulation, we disentangle the estimations based on the deregulation type in our analysis. As outlined in our discussion on reform variation, we subsequently show empirical evidence indicating that the deregulation related to the statewide deposit cap yields impacts on all the outcomes of this study (i.e., tax revenues, school finance, and student achievement), whereas the other three types of deregulation generate null effects.

[15] See Johnson and Rice (2008) Appendix A for the details of interstate branching laws (1994-2005).

Table 1: Identifying Policy Variation: Interstate Branching Laws

State	Reform timing: Minimum age requirement (1)	*De novo* branches (2)	Acquisition of branches (3)	**Statewide deposit cap (4)**	State	Reform timing: Minimum age requirement (1)	*De novo* branches (2)	Acquisition of branches (3)	**Statewide deposit cap (4)**
Alabama	x	x	x	**1997**	Montana	x	x	x	**x**
Alaska	x	x	1994	**1994**	Nebraska	x	x	x	**x**
Arizona	x	x	2001	**1996**	Nevada	x	x	x	**1995**
Arkansas	x	x	x	**x**	New Hampshire	2002	2000	2000	**2000**
California	x	x	x	**1995**	New Jersey	1996	x	1996	**1996**
Colorado	x	x	x	**x**	New Mexico	x	x	x	**1996**
Connecticut	x	1995	1995	**1995**	New York	x	x	1997	**1997**
Delaware	x	x	x	**1995**	North Carolina	1995	1995	1995	**1995**
District of Columbia	1996	1996	1996	**1996**	North Dakota	1997	2003	2003	**x**
Florida	x	x	x	**1997**	Ohio	1997	1997	1997	**1997**
Georgia	x	x	x	**1997**	Oklahoma	2000	2000	2000	**x**
Hawaii	2001	2001	2001	**1997**	Oregon	x	x	x	**1997**
Idaho	x	x	x	**1995**	Pennsylvania	1995	1995	1995	**1995**
Illinois	2004	2004	2004	**1997**	Rhode Island	1995	1995	1995	**1995**
Indiana*	x	1997	1997	**1997**	South Carolina	x	x	x	**1996**
Iowa	x	x	x	**x**	South Dakota	x	x	x	**1996**
Kansas	x	x	x	**x**	Tennessee	x	2001	1998	**1997**
Kentucky	2000	x	x	**x**	Texas	1999	1999	1999	**x**
Louisiana	x	x	x	**1997**	Utah	x	2001	1995	**1995**
Maine	1997	1997	1997	**1997**	Vermont	2001	2001	1996	**1996**
Maryland	1995	1995	1995	**1995**	Virginia	1995	1995	1995	**1995**
Massachusetts	x	1996	1996	**1996**	Washington	x	x	x	**1996**
Michigan	1995	1995	1995	**1995**	West Virginia	1997	1997	1997	**x**
Minnesota	x	x	x	**1997**	Wisconsin	x	x	x	**1996**
Mississippi	x	x	x	**x**	Wyoming	x	x	x	**1997**
Missouri	x	x	x	**x**					

Notes: The table shows state-level policy timing for interstate branching deregulation between 1994 and 2005 The 'x' indicates no deregulation during the period. Columns (1)-(4) refer to different aspects of branching deregulation: eliminating minimum age requirement, allowing banks to open entirely new branches, allowing acquisition of single branches, and relaxing the statewide deposit cap. The information on the Interstate Branching Laws is from Rice and Strahan (2010).
* Indiana removed minimum age restriction in 1997 but reintroduced the regulation back in the following year. We coded Indiana as no deregulation on the minimum age requirement.

3 Data

To investigate the educational consequences of banking deregulation, we construct two main datasets that combine multiple administrative data sources. The first dataset focuses on school district's finances, containing information on revenue, expenditure, and non-finance characteristics such as pupil-teacher ratio. The second dataset contains records on student achievement, with each observation corresponding to grade-by-district level aggregates of students' mathematics assessments in grades 4 and 8. We follow established approaches from the literature for data processing and sampling, with further details in Appendix A.

To study the impact of deregulation on school finance, we use the School District Finance Survey (or F-33) of the Common Core of Data (CCD) of the National Center for Education Statistics (NCES). The F-33 data contain the enrollment, revenue, expenditure, as well as the teacher salaries and benefits, of all school districts in the United States. The financial items are re-scaled in per-pupil terms in constant 2013 dollars, deflated with the US Consumer Price Index from the Bureau of Labor Statistics. The F-33 data are available in 1990, 1992, and every year since 1995. We combine F-33 with the Census of Government in 1993 and 1994, which surveys a large portion of US school districts. We exclude the District of Columbia and Hawaii from the analysis, as they only contain a single school district. We further drop districts with noise in enrollment numbers by applying a school district sampling procedure following Lafortune et al. (2018) (see details in Appendix A.2). The final estimation sample consists of a district-by-year sample reflecting 162,724 observations over 12,821 unique school districts in 49 states between 1992 and 2005.

We compute the pupil-teacher ratio (PTR) to measure class size for each school district. To do this, we obtain counts of full-time equivalent (FTE) teachers in each school district from the CCD School District Universe Survey between 1992 and 2005. Combined with the student enrollment number in the F-33 sample, we calculate the district-level PTR as the ratio between FTE teacher counts and enrolled student numbers. To mitigate the potential effect of outliers, we trim PTR at the top and bottom 2% and 98% within each state and year.

To examine the impact on student achievement, we leverage restricted-use National Assessment of Educational Progress (NAEP) data from the NCES. Known as the Nation's Report Card, the National Assessment of Educational Progress (NAEP) has conducted biennial surveys since 1990, providing comparable assessments of state-representative student achievement across various subjects.[16] We use individual-level mathematics and reading assessments for fourth- and eighth-grade students from the NAEP data. Since the eighth grade reading assessment did not exist until 1998, analyses based on this assessment lack pre-deregulation periods for almost all states in the treatment group.[17] Therefore, we focus on mathematics assessments. To facilitate interpretation, we standardize the test scores relative to the distribution of assessment scores in the first survey year (i.e., 1992). Then, we aggregate the standardized individual assessments to the grade-district-year level, weighted by NAEP individual weights. Finally, we retain school districts that are matched with the school finance estimation sample.[18] The student estimation sample consists of 8,260 unique school districts out of 26,560 observations, constructed from over 1.1 million original individual score records of grades 4 and 8 students between 1992 and 2005.[19]

Because sales and income taxes are the two major contributors to state revenue in school finance, we also verify how interstate branching deregulation affects state government tax revenues. To conduct the analysis, we obtain state government tax revenue data from the Annual Survey of State Government Tax Collections (STC), conducted by the Census Bureau. The STC provides annual statistics on the fiscal year tax collections of state

[16] Participation was voluntary for all states when the NAEP survey was launched in 1990. In 2003, all US states and Washington, DC, began participating in both mathematics and reading assessments for fourth and eighth-grade students. Table A1 displays the survey schedules and the number of participating states during our study period.

[17] Only one treated state, New Hampshire that had deregulation in 2000, has pre-periods for the grade 8 reading assessment.

[18] There were no unified identifiers for matching school districts between CCD F-33 surveys and pre-2000 NAEP data. We thank Julien Lafortune, Jesse Rothstein, and Diane Schanzenbach for sharing the NAEP -CCD crosswalk data.

[19] The sample size/number of observations (e.g., individual students and sampled school districts) from NAEP data are rounded to the nearest ten per IES disclosure guidelines.

governments in the United States. We use historical STC data for fiscal years between 1992 and 2005, which includes state-level total tax revenues as well as the breakdowns of tax revenues into five categories: sales and gross receipts taxes, income taxes, property taxes, license taxes, and other taxes. To facilitate comparisons and interpretation, all of the tax revenues are re-scaled and deflated to a common unit (constant 2013 dollars, in millions).

Finally, to assess potential heterogeneous effects across school districts, we incorporate two other datasets into our analysis. First, we merge the estimation samples with the 1990 School District Databook (SDDB). The SDDB allows us to identify if a school district is a low/high-income district. Specifically, we categorize a district into one of the quintiles based on its within-state distribution of mean household income in 1990. We designate the lowest-income school districts as the first quintile (Q1), and the highest-income school districts as the fifth quintile (Q5). Second, we combine the estimation samples with 1990 county-level land availability data developed in Lutz and Sand (2019).[20] These land availability data enable us to identify whether a county's housing supply is inelastic or elastic. By examining whether there are distinct effects of deregulation on school districts in counties with a different elasticity of housing supply, we aim to verify whether deregulation influences school finance through the local revenue channel, where local property taxes play a major role. We classify a county as having less elastic housing supply if the percentage of its developable land falls below the sample median; otherwise, the supply is considered more elastic.

Table 2 provides the summary statistics for characteristics of school districts, along with the mean values for districts in the low (Q1) and high (Q5) income quintiles. The estimation sample has an average enrollment of 3,763, with low-income districts having an average enrollment of about 2,000 and high-income districts around 6,500. In the baseline year of 1990, the mean household annual income in high-income districts was approximately $20,000

[20]Land (un)availability is commonly used as a predictor of housing supply in studies estimating the impact of housing prices on outcomes of interest. Lutz and Sand (2019) constructs a novel land availability dataset with extensive geographic coverage across the contiguous United States, using high-resolution satellite imagery combined with modern machine-learning techniques.

higher on average than in low-income districts. While per-pupil total revenue is similar between high- and low-income school districts, the primary funding source in low-income districts is state revenue, indicating an equalization-based distribution of school resources within the state. In contrast, the majority of total revenue in high-income districts comes from local sources, highlighting the significant contribution of local property taxes in these districts. Low-income districts spend similar amounts in terms of current expenditures compared to high-income districts, but less on capital outlays. In terms of non-finance characteristics, the pupil-teacher ratio in high-income districts is slightly larger than that in low-income districts. As for student achievement, the district-level average NAEP assessment score, standardized relative to the score distribution in the initial survey year (1992), is around 0.36 standard deviations. Notably, low-income districts have significantly lower average scores—about 0.45 standard deviations below those of high-income districts.

Table 2: Summary Statistics

	Overall			Mean by subgroup	
	Observations	Mean	SD	Q1	Q5
Enrollment	162,724	3,763	15,489	1,988	6,505
Log(mean income, 1990)	158,497	10.21	0.3546	9.872	10.59
Total revenue	162,724	10,844	3,752	11,049	11,505
State revenue	162,724	5,117	2,472	6,290	3,949
Local revenue	162,724	5,063	3,810	3,686	7,182
Federal revenue	162,724	663.2	774.6	1,073	373.6
Total expenditures	162,724	10,943	4,089	11,065	11,779
Current instructional exp.	162,724	5,681	1,843	5,804	5,957
Current non-instructional exp.	162,724	3,515	1,191	3,657	3,686
Capital outlays	162,724	1,064	1,869	1,007	1,303
Pupil teacher ratio	158,422	15.55	13.84	14.64	15.92
NAEP scores	26,560	0.3606	0.4820	0.1115	0.5572

Notes: Table reports summary statistics (mean and standard deviation) at the district-by-year level. Financial items are scaled in 2013 dollars per pupil terms. NAEP score is standardized relative to the distribution of assessment scores in the first survey year (i.e., 1992). The test scores are first standardized at the student level, and then aggregated to the district level using the NAEP individual weights. Q1/Q5 contains school districts in the first/fifth quintile based on the within-state distribution of mean household income in 1990. The number of observations from NAEP data is rounded to the nearest ten per IES disclosure guidelines.
NAEP Source: US Department of Education, Institute of Education Sciences, National Center for Education Statistics, National Assessment of Educational Progress (NAEP) 1992, 1996, 2000, 2003, and 2005 Mathematics Assessments.

4 Empirical Strategy

We start the empirical specification with a difference-in-differences (DiD) design using a two-way fixed effects (TWFE) estimator:

$$Y_{ist} = \beta \cdot Deregulation_{st} + \delta_s + \delta_t + \epsilon_{ist}, \tag{1}$$

where Y_{ist} is school finance items, as well as teacher-pupil ratio, of school district i in state s in year t. When investigating student achievement, Y_{ist} indicates the district-level aggregates of students' mathematics assessments in NAEP. $Deregulation_{s,t}$ is an indicator equal to one if state s has deregulated its statewide deposit cap by year t, and zero otherwise.[21] δ_s and δ_t are year and state fixed effects, respectively. We cluster error terms $\epsilon_{i,s,t}$ at the state level, as this represents the level of variation in deregulation.

Our identification strategy relies on the assumption that the timing of interstate branching deregulation across states was plausibly exogenous and not driven by economic factors that could influence student achievement (Kroszner and Strahan, 1999, 2014). Empirically, the central requirement for identification in our DiD strategy is the parallel-trend assumption: school resources and student achievement would have experienced similar trends across treated and control states in the absence of the deregulation.

To formally assess the validity of the parallel-trend assumption, we conduct an event study model that captures treatment effects over each of the pre- and post-periods. The specification is similar to equation (1) but replaces the single treatment indicator with a

[21]For reform timing, we use fiscal year (FY) to indicate event time for school finance analysis and survey year for student achievement. For example, for states that enacted deregulation in 1995, in the school finance analysis, the post-period equals one when the states reach FY 1996, which is between July 1st 1995 and June 30th 1996. For student achievement, the post-period equals one when the treated states reach the survey year 1996 in the NAEP data, where NAEP conducts its data collection between January and March in the survey year.

group of event-time indicators, allowing for differential treatment effects over time:

$$Y_{ist} = \sum_{k=-6, k\neq -1}^{k=8} \beta_k \cdot Deregulation_{st} \times \mathbb{1}\left(t = t^* + k\right) + \delta_s + \delta_t + \epsilon_{ist}, \tag{2}$$

where β_k represents the effect of deregulating statewide deposit cap in year t^* on outcomes k years later (or previously, for $k < 0$). These effects are measured relative to year $k = -1$, which is omitted as the reference year. We truncate the number of periods pre- and post-treatment k at -6 and 8, so β_{-6} represents average outcomes six or more years prior to the deregulation, and β_8 indicates average outcomes eight or more years following deregulation.

In addition, as informed by the recent DiD literature, when having multiple groups/periods in the DiD framework, TWFE estimators could fail to identify the average treatment effect on the treated (ATT) if there are heterogeneous treatment effects (i.e., ATTs vary by treatment cohort or calendar time). To address this identification challenge, we adopt a stacked DiD design, as outlined by Cengiz et al. (2019) and Deshpande and Li (2019).[22]

We implement the stacked DiD approach in three steps. First, we create cohort(j)-specific panel datasets, where each dataset contains a single treated cohort along with the never-treated cohorts as "clean controls." For example, states that experienced deregulation in 1995 (i.e., California, Connecticut, Delaware, Idaho, Maryland, Michigan, Nevada, North Carolina, Pennsylvania, Rhode Island, Utah, and Virginia) comprise one treated cohort, and the never-treated states are the controls. Next, we stack these datasets and line them up according to the dataset indicators. Finally, we estimate the model using DiD specifications on this stacked dataset, with fixed effects interacting with the dataset indicator group.

[22]In addition to the stacked DiD estimator, we also employ various dynamic DiD estimators to assess robustness, including the estimators of de Chaisemartin and D'Haultfoeuille (2020), Sun and Abraham (2021), Borusyak et al. (2024), and Callaway and Sant'Anna (2021). The results (i.e., event study estimates in Figures B2 and B3 and average treatment effects in Figures 3 and 5) indicate that our benchmark estimates are not sensitive to the choice of DiD estimators.

Formally, for event study estimates, we have the following specification:

$$Y_{isjt} = \sum_{j} \sum_{k=-6,k\neq-1}^{k=8} \beta_{jk} \cdot Deregulation_{st} \times \mathbb{1}\left(t = t^{*} + k\right) + \delta_{sj} + \delta_{jt} + \epsilon_{isjt}. \tag{3}$$

For average treatment effect estimates, we employ a pre-post version of equation (3):

$$Y_{isjt} = \beta \cdot Deregulation_{st} + \delta_{sj} + \delta_{jt} + \epsilon_{isjt}, \tag{4}$$

where δ_{sj} and δ_{jt} are the stack-by-state and stack-by-year fixed effects, respectively. The error terms are again clustered at the state level in these two equations.

5 Branching Deregulation and School Finance

We begin our analysis by examining the changes in state government tax revenue following the deregulation. We then investigate the impact of interstate branching deregulation on school finance.

5.1 Impact on State Tax Revenue

School finance is primarily funded by both state revenue and local revenue. While state revenue mostly comes from sales and income taxes collected by the state government, local revenue largely relies on local property taxes. The interstate branching deregulation potentially could affect school finance via both state revenue and local revenue channels. For the former channel, deregulation might affect it as a source of state tax revenue shocks (e.g., shocks on sales taxes and/or income taxes), which are investigated here. For the latter channel, Favara and Imbs (2015) finds that the interstate branching deregulation increases local housing prices, suggesting that deregulation also was likely to cause changes in local property tax revenues.[23] In this section, we investigate whether deregulation affects state government tax revenues, in particular sales taxes and/or income taxes.

The financial system of the United States has evolved and remained relatively stable since the 1930s, with sales and income taxes becoming the predominant sources of state government revenue (Wallis, 2000). As of 1992, sales taxes and income taxes accounted for approximately 48% and 40% of total taxes, respectively. In contrast, property taxes only accounted for around 2.1%, while license taxes (6.4%) and other taxes (3.3%) made up the remaining 10%. This revenue composition remained relatively consistent during our study period from 1992 to 2005.

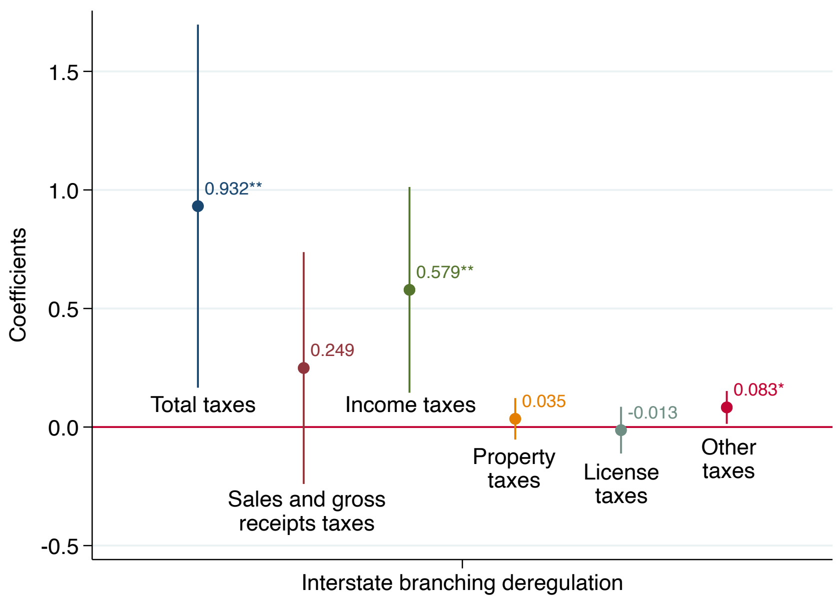
[23] However, shocks in local housing markets might not end up having impacts on local tax revenue. In particular, the local millage rate (i.e., property tax rate) may adjust endogenously in response to market shocks on the values of local housing assets, leading to limited effects on local property revenues. The limited effects are further attenuated by the year lags between a market shock and the ensuing impacts on appraised home values. We defer this discussion, along with empirical findings showing the impact of deregulation on local revenue, to the next section.

Leveraging the staggered state-level deregulation, we examine whether deregulation leads to changes in total tax revenues, as well as revenues from the five sub-categories. Examining impacts on sales taxes and income taxes is informative for capturing the state revenue channel, while investigations on the other three tax categories provide a fuller picture of the deregulation effects. Figure 1 plots the estimates of the impact of deregulation on tax revenues, which shows treated states exhibit an increase in total taxes following the deregulation. The estimates on the five tax sub-categories indicate that the effect was mainly driven by the significant increase in revenues from income taxes and other taxes, and there is no effect on sales taxes, property taxes, or license taxes. Relative to the mean of dependent variables (see Table C1), interstate branching deregulation leads to a 6.9% increase in total tax revenues and a 10.8% increase in income tax revenues.[24] Therefore, given the sizeable effects on income tax revenues, we should anticipate that deregulation would affect school finance primarily through the state revenue channel.[25]

[24]Between 1992 and 2005, four states (i.e., Nevada, Texas, Washington, and Wyoming) did not levy income taxes. Including these states may lead to downward estimates of the impact of deregulation on income tax revenues (as those in Figure 1). This could be especially true given that three of the four states are in the treatment group. Figure B1 presents estimates of the interaction terms between deregulation and a dummy variable indicating if the state levies income taxes, showing larger effects of deregulation on total tax and income tax revenues for states that levy income taxes.

[25]To further test if the effects were driven by the deregulation on the statewide deposit cap, rather than by variation that captures the general pattern of deregulation timing, we test whether the other three dimensions of deregulation affect state government tax revenues. Results in Table C1, Panels A to C, show that none of the other deregulation reforms have a significant impact on any of the tax items. This evidence is consistent with the school finance results in the next section, which show that the deregulation on statewide deposit caps affects school finance, while the other three types of deregulation do not.

Figure 1: Impacts on State Government Tax Revenues



Notes: The dependent variables are state government state tax revenues on total taxes, as well as revenues in five sub-categories: sales and gross receipts taxes, income taxes, property taxes, license taxes, and other taxes. The independent variables are interstate branching deregulation on statewide deposit caps. All dependent variables are expressed in constant 2013 million dollars. All regressions include a time-varying log population as the control, as well as state fixed effects and year fixed effects. The figure shows the 95% confidence interval with standard errors clustered at the state level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

5.2 Impact on School Finance

We now examine the impact of interstate bank branching deregulation on school finance. Figure 2 plots the event study estimates of the impact on school finance, using both TWFE and stacked DiD estimators. Regardless of the estimation approach, we observe a sharp and persistent increase in per-pupil total revenue (subfigure a) and expenditure (subfigure b) in the reformed states following the deregulation. The total revenues show significant increases three years after the deregulation, while it takes an additional year to yield a statistically significant increase in total expenditures.[26]

Figure 2 also shows no systematic deviation in the pre-trends under TWFE estimations, supporting the DiD identifying assumption. When using stacked DiD estimators, the graph shows flat pre-trends centered around zero, highlighting the virtue of dynamic DiD estimators that incorporate "clean controls" (i.e., never-treated states) in estimations.

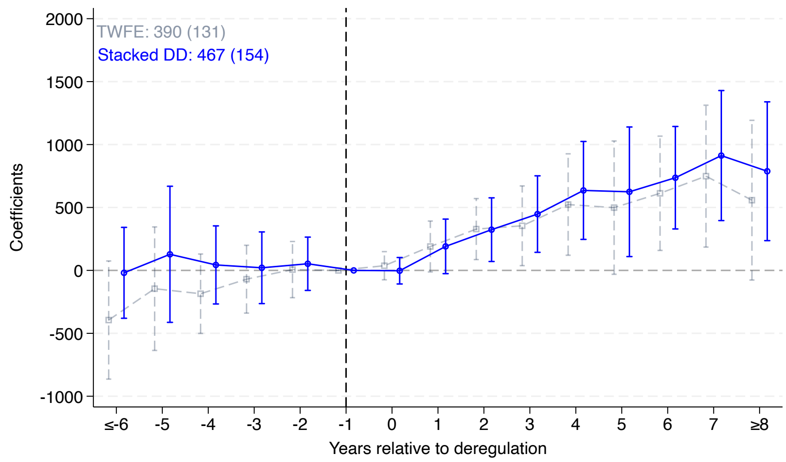
It is important to address potential issues of using TWFE estimators to conduct more accurate estimation, especially when we have dynamic treatment effects on total revenue and expenditure, as shown in Figure 2. In addition to the stacked DiD estimator, we employ a variety of dynamic DiD estimators to assess the robustness (de Chaisemartin and D'Haultfoeuille, 2020; Sun and Abraham, 2021; Borusyak et al., 2024; Callaway and Sant'Anna, 2021). Figures B2 and B3 show that the event study estimates are similar across all these estimators.

We report estimated average treatment effects using both specifications. Stacked DiD and TWFE estimates are in Tables 3 and C2, respectively. ATT estimates generated by the two specifications show similar patterns. In terms of effect size, based on stacked DiD results, deregulation leads to an average increase of $467 per pupil in total revenue throughout the eight-year post-reform period, which is a 4.31% increase compared to the mean of the dependent variables. Similarly, total expenditures increase by $435 per pupil on average following deregulation, amounting to a 3.98% increase relative to the mean of the dependent

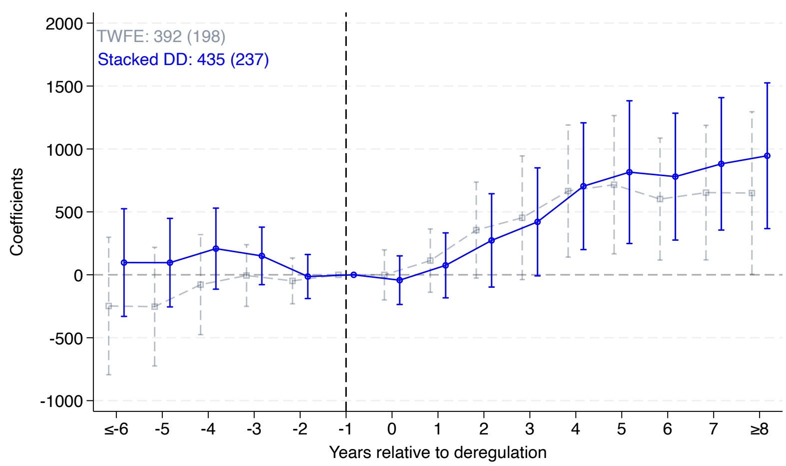
[26]Figure C2 presents the event study estimates for the breakdown of school finance outcomes.

variable.[27]

Figure 2: Event Study Figure for Total Revenue and Expenditure



(a) Total revenue per pupil



(b) Total expenditure per pupil

Notes: The figures show event study estimates of the impact of deregulation on school finance, using two-way fixed effects (in gray) and stacked DiD estimators (in blue). The dependent variables are total revenue and expenditure per pupil in 2013 dollars terms. Event period -1 is normalized to zero. The figures show the 95% confidence interval with standard errors clustered at the state level. The DD estimates are shown at the upper-left corner, with standard errors included in the parentheses.

[27]One might wonder whether deregulation impacts school enrollment. Our DiD estimates reveal neither statistically nor economically significant effects. The stacked DiD and TWFE estimates are 150 (159) and 75 (130), respectively, with the standard errors in parentheses. Given that the sample mean of enrollment is around 3,763, these estimates represent relatively small magnitudes.

Revenue Effects.— Panel A of Table 3 shows the impact of deregulation on the two major components of total revenue: state and local revenue. The increases in total revenue are driven by increases in state revenue and not from local revenue. A positive impact on state revenue is expected, reflecting that deregulation leads to increases in state government income tax revenues (as in Figure 1), a primary funding source for state revenue.[28]

For local revenue, the statistically insignificant and negative effects also are not surprising. Although deregulation is found to increase local housing supply/prices (e.g., Favara and Imbs (2015)), and local property taxes comprise the lion's share of the local revenue channel, the positive shocks in housing assessment values may not translate into increased local revenues. As documented in the literature, the elasticity of local property tax revenues to home values is low (estimated to be between 0 and 0.4). The low elasticity could be explained by two factors. First, taxes are collected on assessed values with a considerable lag (Lutz, 2008). Second, local policymakers often respond to shocks in assessed values by adjusting local property tax rates (Lutz et al., 2011).[29] A recent study by Jackson et al. (2021) on how negative economic shocks (i.e., the Great Recession) affect school finance shows a similar pattern: State revenue channel that relies on tax bases is more responsive to market fluctuations, while local revenue channel is more stable in response to these shocks.

The revenue effects could vary across different school districts. When focusing on low- vs. high-income school districts, the results in columns (3) and (4) show revenue effects are concentrated in low-income districts. This is because, although both quintile groups experience positive gains in state revenue following the deregulation, high-income districts also receive significantly less local and federal revenue in response to the increase in state aid. The decreased local revenue, which could be due in part to lower millage rates on housing assets following deregulation, contributes to the small magnitude of total revenue in

[28]Table C1, Panels A to C, show evidence that the other three types of deregulation (i.e., minimum age requirement, *de novo* branches, and acquisition of branches) do not have impacts on raising state government tax revenues. These three policies also generate null effects on total revenue and expenditure (Figure C1).

[29]Although there is no nationwide evidence on how local property tax rates respond to shocks in the housing market due to the lack of tax rate data, Brunner et al. (2022) document the case in Illinois and find school districts reduce their property tax rates after positive revenue shocks from wind energy installation.

high-income districts.[30]

To further validate that changes in housing markets did not affect school finance via local revenue channel, we examine how revenue effects could vary across counties with inelastic/elastic land availability. The underlying logic is straightforward: If changes in the housing market affect total revenue via the local revenue channel, the effect should be more concentrated in counties with an inelastic housing supply, as measured by less available developable land. A county is defined as having an inelastic housing supply if the percentage of its developable land is less than the sample median. Results of Table 3 Panel A, columns (5) and (6), show that patterns are similar for districts in counties with different housing supply elasticities. In addition, the results indicate that no significant positive effects on local revenue following the deregulation.[31] These results imply that, while deregulation might affect the local housing market, no evidence is found that local revenue is a channel for increasing total revenue.

Expenditure Effects.— Panel B of Table 3 presents results on expenditure effects. Column (2) shows that deregulation leads to increases in total expenditure. Columns (3) and (4) reveal that the expenditure effects are concentrated in low-income school districts, and there is no significant impact in high-income school districts. These findings align with the patterns of revenue effects, where deregulation primarily benefits low-income districts in terms of total revenue. The cross-district heterogeneity in total expenditure also reflects the post-1970s school funding formulas that aim at equalization in distributing state aid across school districts within a state.[32]

[30]Figure C4 presents the revenue effects by quintile. Q5 school districts experience substantial reductions in local revenue following deregulation relative to other quintiles. The pattern also suggests that lower millage rates are a key mechanism behind the reduction in local revenue for Q5 districts.

[31]Favara and Imbs (2015) find deregulation leads to increased housing prices in counties with inelastic housing supply, while the stock of housing increased in elastic counties. The negative effects on local revenue in inelastic counties (as in Table 3 Column (5)) are consistent with their findings, since the counties with increased shock-induced housing prices are more likely to lower their millage rates, potentially leading to reduced local revenues.

[32]Following the school finance reforms of the 1970s, the "minimum foundation plan" (MFP), which allocates more state aid to districts with lower levels of local revenue, became one of the most widely adopted components in the school funding formula. For example, in 1990-1991, 37 states incorporated the MFP mechanism in their state aid systems (Card and Payne, 2002).

To gain a fuller picture of how expenditure effects vary across school districts with different household income levels, we present event study estimates of the impact on total expenditure for school districts in all five quintiles in Figure C5.[33] The event study figures indicate that school districts in lower income quintiles have increases in total expenditure earlier and have a larger effect on spending in a given post-period, compared to those in the highest income quintile, although their differences are not statistically distinguishable.

We next decompose the expenditure effects into sub-categories. Distinguishing spending by type is important as it would be formative for student outcomes. While the literature has recognized the significance of instruction-relevant spending in improving human capital, the effects of capital spending are less settled.[34] We find that increases in total expenditure are mainly driven by increases in capital outlays, providing supportive evidence that capital spending can translate into gains in student achievement. Instruction-relevant expenditures (e.g., current instructional expenditure and teacher salaries and benefits) also rise following deregulation, as indicated by the event study estimates (Figure C2), although their ATEs are not precisely estimated.

When decomposing the expenditure effects by income quintile, we find that low-income school districts experience increases in both current instructional/non-instructional expenditure and capital outlays. Average teacher salaries (and benefits) also rise following deregulation. By contract, in high-income school districts, the null effect in total revenue leads to no significant spending expansions across all sub-categories. We further examine whether the impact of deregulation varies for school districts with different housing supply elasticities. Columns (5) and (6) of Panel B show that there are no systematic differences across various spending categories between inelastic and elastic counties.

[33]The corresponding event study estimates of the impact of deregulation on per-pupil state aid by quintile are in Figure C3, showing a pattern similar to that in Figure C5.

[34]For example, Baron (2022) uses evidence from referendum-induced revenue changes in Wisconsin and finds that increases in operational spending substantially improve educational outcomes, whereas capital expenditures have limited impact. However, Biasi et al. (2025) find heterogeneous effects of capital spending on student achievement, with investments in essential infrastructure yielding significant improvements and expenditures on athletic facilities showing no effects.

Table 3: Impacts of Branching Deregulation on School Finance

	Mean of dep var	Stacked DiD estimate	District income		Land availability	
			Q1	Q5	Inelastic	Elastic
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Revenue effects						
Total revenue	10,844	467***	849***	175	425**	473***
		(154)	(174)	(181)	(188)	(153)
State revenue	5,117	802***	976***	855**	960***	789**
		(257)	(219)	(351)	(311)	(299)
Local revenue	5,063	-294	-145	-646*	-471**	-262
		(233)	(178)	(345)	(230)	(353)
Federal revenue	663.2	-41	18	-34**	-64*	-54*
		(27)	(58)	(17)	(34)	(29)
Panel B. Expenditure effects						
Total expenditures	10,943	435*	798***	151	409*	465*
		(237)	(216)	(282)	(215)	(271)
Total current exp.	9,196	173	455***	1	162	124
		(140)	(150)	(151)	(135)	(174)
Current instructional exp.	5,681	145	314**	71	158*	79
		(116)	(123)	(118)	(93)	(141)
Current non-instructional exp.	3,514	27	141**	-71	5	45
		(49)	(54)	(62)	(75)	(58)
Capital outlays	1,064	283***	298***	166	285***	329***
		(75)	(93)	(108)	(86)	(81)
Teacher salaries + benefits	5,109	162	310***	70	174	125
		(106)	(113)	(120)	(107)	(122)
Teacher salaries	4,074	65	176*	-13	113	1
		(87)	(92)	(86)	(72)	(99)
Panel C. Class size						
Pupil teacher ratio	15.55	-0.006	-0.468**	0.010	-0.174	-0.556
		(0.012)	(0.228)	(0.171)	(0.295)	(0.372)

Notes: The table shows the deregulation effects on school finance using the stacked DiD model. Column (1) presents the mean of dependent variables, while column (2) shows the estimates for average effect size. Columns (3)-(4) look at school districts in the first/fifth quintile of the within-state household mean income: Q1/Q5 indicates school districts in the lowest/highest income quintile. Columns (5) and (6) separate school districts into counties with inelastic or elastic land availability in 1990. The relevant estimates using TWFE estimators are in Table C2. Standard errors clustered at the state level are in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Class Size.— Finally, we investigate the impact of deregulation on the pupil-teacher ratio in Panel C. On average, deregulation has no significant impact on pupil teacher ratio. However, this average effect masks decreases in pupil-teacher ratios of 0.468 (about 3% of the dependent variable mean) in low-income school districts, while no effects are observed in high-income school districts. The reduced class sizes in low-income districts likely reflect the increased spending on teacher salaries (and benefits) in these districts, as the expenditure effects indicated in Panel B.

5.3 Robustness Checks

We begin the robustness assessment by focusing on the average effect sizes of estimates obtained from different estimators. Figure 3 displays the benchmark estimates in black in the first two rows, which are close to each other. In addition to these estimates, the third row presents estimates using the "imputation" estimator of Borusyak et al. (2024). Average effect sizes are similar across different estimators — the TWFE, stacked DiD, and "imputation" estimators — indicating that our benchmark estimates are not sensitive to the choice of DiD estimators.

We next investigate whether potential confounding events would affect interpretations of our baseline results. One concurrent event (from the finance literature) is the other three dimensions of the interstate branching deregulation. Results in the fourth row of Figure 3 indicate that the benchmark results remain robust after controlling for these deregulation reforms. Although the impact on per-pupil total expenditure becomes statistically insignificant, the estimate largely overlaps with the benchmark estimate.[35]

The education literature suggests another potential confounding factor: the post-1990 school finance reforms, which aimed to provide adequate funding to low-income school districts. Research has documented the influence of SFRs in shaping within-state school

[35] Table B2 column (2) provides estimates of the impact of each deregulation dimension, showing consistent evidence that none of the other three dimensions significantly affect school finance when controlling for all four dimensions of deregulation simultaneously

finance allocations (e.g., Jackson et al. (2016) and Lafortune et al. (2018)). During the 1990s, states adopted SFRs in a staggered manner, which, as a concurrent event, could confound our baseline results. As Table B1 shows, the reform timing of deregulation largely differs from those of SFRs.[36] The lack of systematic overlap in the timing of the two policies is unsurprising, as they are distinct reforms. To firmly illustrate the distinct policy targets of the two reforms, we present additional evidence showing the distributional effect of deregulation on state revenues in Figure C3. The adequacy-based post-1990 SFRs specifically direct increased state aid toward low-income districts (Lafortune et al., 2018; Brunner et al., 2020). In contrast, banking deregulation, as a source of state tax revenue shocks, generates revenue effects across schools at all income levels (Figure C3).[37]

To formally assess how post-1990 SFRs might confound our results, we re-estimate the baseline regressions while controlling for the introduction of the SFRs. The results remain robust to accounting for the impact of SFRs, as indicated in the fifth row of Figure 3 (or Table B2 column 3).[38] To further validate that SFRs were not a potential confounding concern, we conduct an additional sub-sample analysis. The sixth row of Figure 3 (or Table B2 column 6) shows that deregulation has positive impacts on school finance in states that did not implement SFRs. The results further address concerns that the revenue and expenditure effects at the baseline were driven by the post-1990 school finance reforms.

As a final robustness exercise, we re-estimate the impact of deregulation on total revenue

[36] During the study period, among the 38 states that adopted deregulation, only about one-third (13 out of 38) of them implemented SFRs. Among the 13 overlaying states, nine of them adopted the SFRs later than deregulation, while four of them had SFRs the same year or earlier than the deregulation. Empirically, we regress the timing of deregulation on the timing of SFRs, controlling for state and year fixed effects, using a state-by-year sample. The estimated correlation is insignificantly negative, with a coefficient of -0.0006 and a standard deviation of 0.0370, suggesting a null effect.

[37] Figure C3 indicates that the impact on state revenues is slightly larger for school districts in the lowest two income quintiles (Q1 and Q2) compared to the other three quintiles, potentially reflecting some effects of post-1990 SFRs. However, state aid increases similarly across Q3-Q5 income quintile districts following deregulation, underscoring the key treatment difference between post-1990 SFRs and banking deregulation.

[38] In addition, we test if there are complementary relationships between the two reforms by examining the impact of their interaction terms in Table B2, columns (4) and (5). Column (4) reveals that low-income districts not only experienced increases in total revenue and expenditure following deregulation, but those in deregulated states that also implemented SFRs experienced even greater effects. In contrast, high-income districts did not experience any significant changes in total revenue or expenditure (column 5).

and expenditure using a contiguous county-pairs design. The contiguous county-pairs design is widely used in the finance literature (e.g., to study the real effect of intrastate banking deregulation in Huang (2008)) and the labor economics literature (e.g., to study the impact of the minimum wage in Dube et al. (2010)). The intuition behind this design is that contiguous counties on opposite sides of the state border share similar characteristics, where one county is from a treated state, and the other county is not.

Formally, in the specification, we use pair-specific time effects (δ_{pt}) with school district fixed effects (δ_i), which exploit variation in deregulation among de-meaned districts within each contiguous county-pair.[39] The last row of Figure 3 presents results based on contiguous county-pairs. Results are robust to implementing the contiguous county-pairs design; indeed, the design dramatically improves the estimation precision, highlighting the virtue of the design.[40]

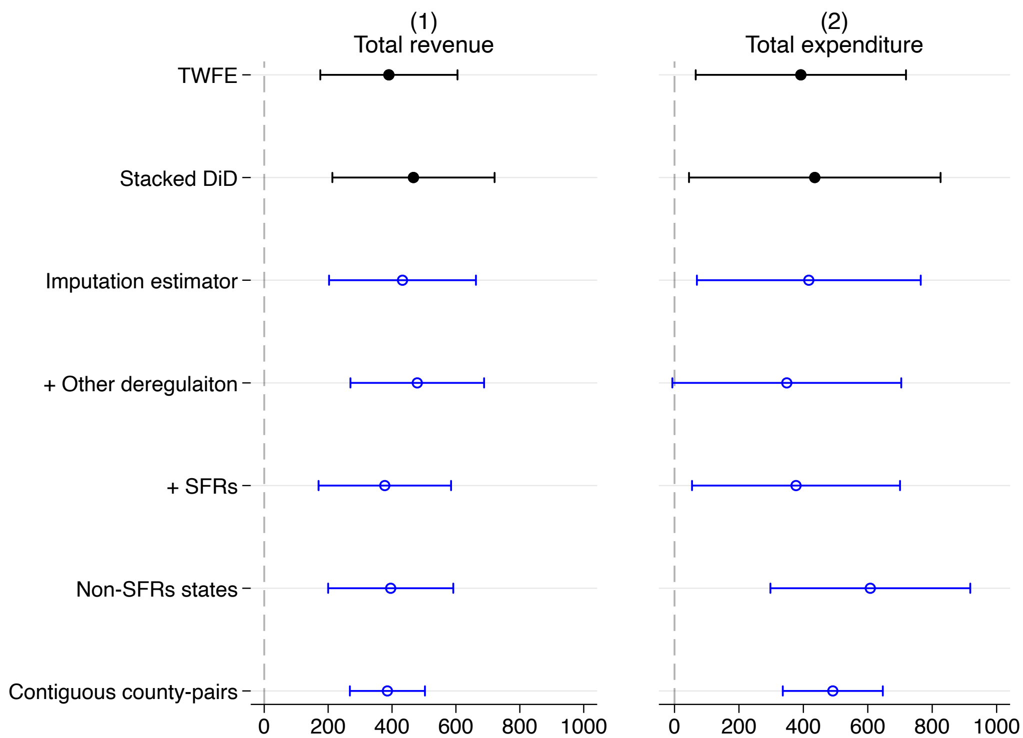
[39] The contiguous counties are obtained from the county adjacency files of NBER's public use data archive at https://www.nber.org/research/data/county-adjacency. We identify all contiguous county pairs (N = 1,303) in the contiguous United States in 2010 and merge them with the school finance data, leaving 1,264 contiguous county pairs (with at least two counties) in the estimation sample. The formal specification is:

$$Y_{ipst} = \beta \cdot Deregulation_{st} + \delta_i + \delta_{pt} + \epsilon_{ipst},$$

where Y_{ipst} are finance items in school district i, county-pair p, state s, and year t. δ_i and δ_{pt} are district and pair-year fixed effects, respectively. The standard errors are clustered at the state level.

[40] The results are similar when using the contiguous county-pair sample and the baseline specification (equation (1)). The estimated impacts on total revenue and expenditure are 315 (124) and 546 (153), respectively, with standard errors in parentheses.

Figure 3: Robustness: Total Revenue and Expenditure



Notes: The figure plots estimates for the impact on per-pupil total revenue (column 1) and expenditure (column 2) under various robustness checks, with a 90% confidence interval. The first two rows (in black) show the benchmark TWFE and stacked DiD estimates, followed by estimates: using the "imputation" estimator by Borusyak et al. (2024), adding the other three deregulation timing as control, controlling for the post-1990 school finance reforms, using states that do not implement any of the post-1990 SFRs, and under a contiguous county-pairs design.

6 Branching Deregulation and Student Achievement

So far, we have shown that school districts in deregulated states experience positive revenue and expenditure effects. We now examine whether banking deregulation translates into improvements in student achievement.

Overall Effects.— We first look at the overall effect of deregulation on student achievement. Figure 4 plots the event study estimates of the impact of deregulation on student achievement using TWFE and stacked DiD estimators. Subfigure (a) conducts the event study analysis at the district level, while subfigure (b) provides estimates at the individual-level with a set of student controls: indicators for whether the student is female, black, needs an individualized education plan (IEP), or has limited English proficiency (LEP).

Figure 4 exhibits flat and statistically insignificant pre-trends, supporting the DiD assumption. The post-periods show that school districts in reformed states begin to see significant increases in student achievement in the second post-period (i.e., third and fourth years), confirming that the divergence in test scores occurs only after the significant increases in per-pupil total expenditure (Figure 2b). In addition, compared to the district-level analysis (Figure 4a), the individual-level analysis with student-level controls (Figure 4b) yields estimates with improved precision, resulting in statistically significant and lasting improvements in student achievement throughout the eight-year post-reform period.[41] Using the individual-level analysis with student controls as our preferred specification, the stacked DiD analysis shows that deregulation leads to an overall improvement in student achievement of 6.4% of a standard deviation.[42]

How does the effect size of banking deregulation on student achievement compare to that

[41]Table C3 Panel A presents reduced-form estimates from the district-level analysis, individual-level analysis, and individual-level analysis with student controls. The sample in the individual-level analysis (N $\approx$ 1,143,920) is obtained from disaggregating the sample in the district-level analysis (N $\approx$ 26,560). The observation numbers are rounded to the nearest ten per IES disclosure guidelines.

[42]We also verify that the deregulation variation does not affect student composition. Specifically, we examine whether deregulation influences any of the student controls and find no impact. The estimates (means and standard deviations in the parentheses) are -0.001 (0.002), 0.006 (0.007), 0.005 (0.004), and -0.002 (0.009) for female, black, IEP, and LEP student indicators, respectively.

of post-1990 school finance reforms in the US and banking deregulation in other countries? When we compare our effect to post-1990 SFRs effects, we find that banking deregulation leads to a 0.064 standard deviation improvement in student mathematics achievement ten years after the reform, which is about half the effect of post-1990 SFRs on students in low-income districts (0.12 standard deviations) (Lafortune et al., 2018). When compared to the banking deregulation effect in other countries, our estimate is less than one-third of the 0.22 standard deviation effect found in the Indian context (Chang and Ravindran, 2023).

We next validate the robustness of reduced-form estimates of the impact on student achievement through a host of validity checks in Figure 5. The first three rows in Figure 5 present the benchmark estimates. Based on the specification used in the third row (i.e., individual-level analysis with student controls), the following rows incorporate additional adjustments — adding district fixed effects, controlling variation of other three types of deregulation, controlling variation of post-1990 SFRs, using non-SFRs states as the estimation sample, and using a sample include DC and Hawaii. Results remain robust to these additional adjustments, indicating that the estimated impact of deregulation on student achievement is not driven by alternative confounding factors, contemporaneous events, or sensitive to sample construction.

Additional concerns may arise if deregulation affects student achievement through channels other than school spending, such as housing (Favara and Imbs, 2015) or labor markets (Hu et al., 2020). To address this possibility, the next two rows in Figure 5 account for county-level housing prices and labor market conditions (employment and wages) to ensure that the estimated effects on student achievement are not driven by these potential channels.[43] Results remain robust to these additional adjustments, mitigating the concerns

[43]To control for housing prices, we use the housing price index from the Federal Housing Finance Agency (FHFA) (http://www.fhfa.gov). This index is a widely used measure that captures variation in housing prices and costs. For local labor market conditions, we obtain employment and wage data from the Quarterly Census of Employment and Wages (QCEW) provided by the US Bureau of Labor Statistics (https://www.bls.gov/cew/), which covers over 95% of jobs in the United States. Around 95.3% and 96.4% of the student estimation sample are matched to the FHFA and QCEW data, respectively. To address skewness in the distributions, we apply a log transformation to these measures.

of housing market and labor market as the alternative channels.

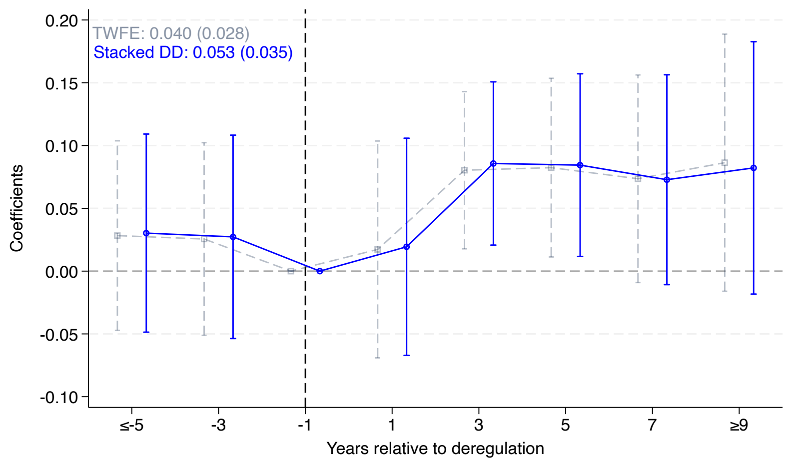
Still, several alternative channels may link deregulation and student achievement. First, because deregulation operates as a state tax revenue shock, student achievement could improve through increased state-funded social safety nets.[44] Second, as a credit supply shock, deregulation could relax borrowing constraints on school districts, facilitating bond issuance and subsequently increasing school spending. Third, parental investment may also respond to deregulation (Chang and Ravindran, 2023), potentially affecting student outcomes. To fully address these alternative interpretations, the last row of Figure 5 presents a falsification test using private school students. If these concerns were valid, we would expect to observe similar patterns in both public and private schools. By contrast, if the effects operate primarily through increases in public school spending, we should see impacts on public school achievement but not on private schools outcomes. The results confirm the latter: none of the estimates for private schools is statistically significant, and their magnitudes are close to zero.

Lastly, we address the concern that the observed effects are from families moving into resourceful school districts after deregulation, rather than reflecting the impact of spending increases. Using additional school datasets from the NAEP, we show that school characteristics — percentage of students who are black, Hispanic, eligible for the free lunch program, English learner, or in special education (Figure B4) — do not alter after the deregulation, indicating that migration is not the driving force behind the student achievement improvement.[45]

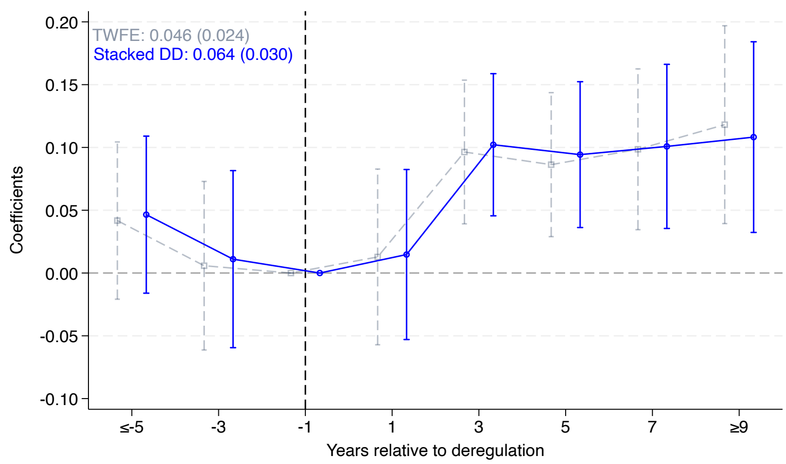
[44] The absence of differential effects between students participating and not participating free school lunch program in Figure 6 in next section helps mitigate this concern.

[45] The scale of these outcome variables varies across assessment years in the NAEP school data. The school-level percentage of Black or Hispanic students is measured on a 0-100% scale, while the other three variables are categorical, ranging from 1 to 7: 1 for 0%, 2 for 1-5%, 3 for 6-10%, 4 for 11-25%, 5 for 26-50%, 6 for 51-75%, and 7 for 76-100%.

Figure 4: Event Study Figure of Impact on Student Achievement



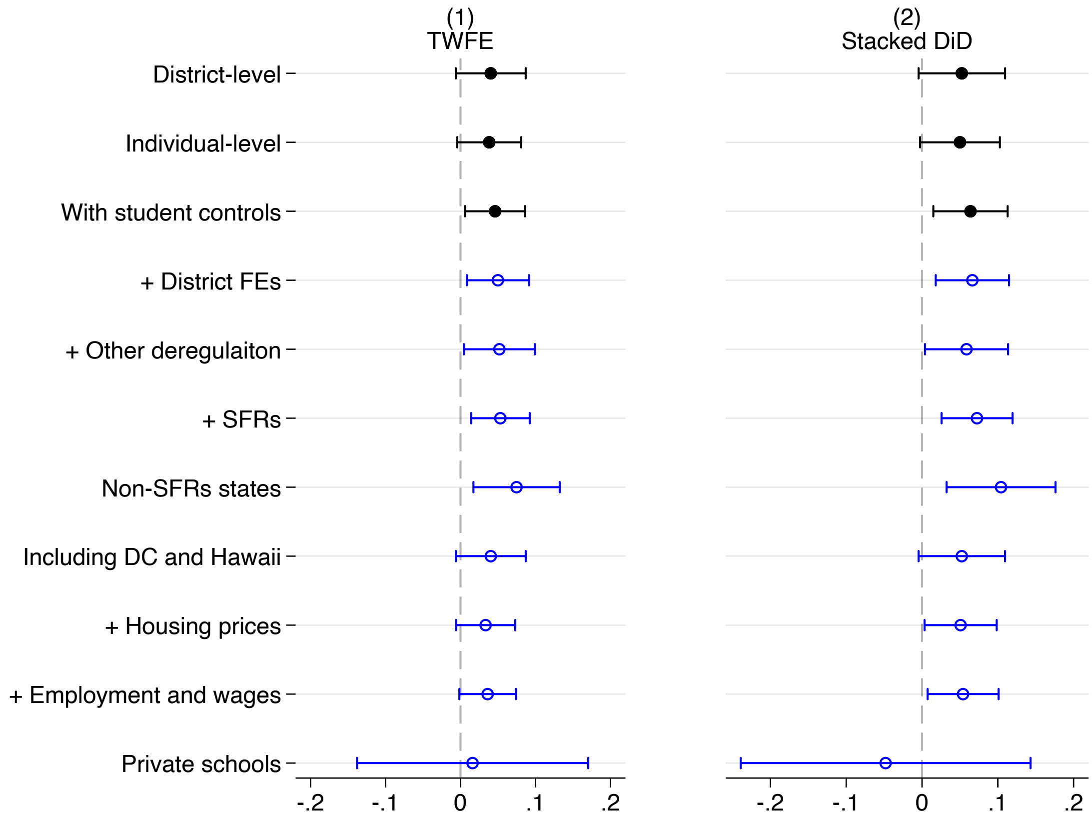
(a) District-level analysis



(b) Individual-level analysis with student controls

Notes: The figure shows event study estimates of the impact of deregulation on student achievement, using two-way fixed effects (in gray) and stacked DiD estimators (in blue). The dependent variables are standardized district-level aggregates of student achievement in grades 4 and 8 NAEP mathematics assessments. Subfigure (a) displays district-level analysis, and subfigure (b) shows individual-level analysis with student controls. Student controls include dummies indicating if the student is female, black, needs an individualized education plan, or is an English learner. Event period -1 is normalized to zero. The figures show the 95% confidence interval with standard errors clustered at the state level. The DD estimates are shown at the upper-left corner, with standard errors included in the parentheses.
NAEP Source: US Department of Education, Institute of Education Sciences, National Center for Education Statistics, National Assessment of Educational Progress (NAEP) 1992, 1996, 2000, 2003, and 2005 Mathematics Assessments.

Figure 5: Robustness: Impacts on Student Achievement



Notes: The figure plots estimates for the impact on student achievement using TWFE (column 1) and Stacked DiD estimators (column 2) under various robustness checks, with a 90% confidence interval. The first three rows (in black) display the benchmark estimates, followed by additional estimates that incorporate: school district fixed effects, controls for the timing of the other three types of deregulation, controls for the variance of post-1990 school finance reforms, a sample limited to states without implementing post-1990 SFRs, a sample including DC and Hawaii, county-level housing price index, controls for county-level employment and wages, and a sample of private school students.
NAEP Source: US Department of Education, Institute of Education Sciences, National Center for Education Statistics, National Assessment of Educational Progress (NAEP) 1992, 1996, 2000, 2003, and 2005 Mathematics Assessments.

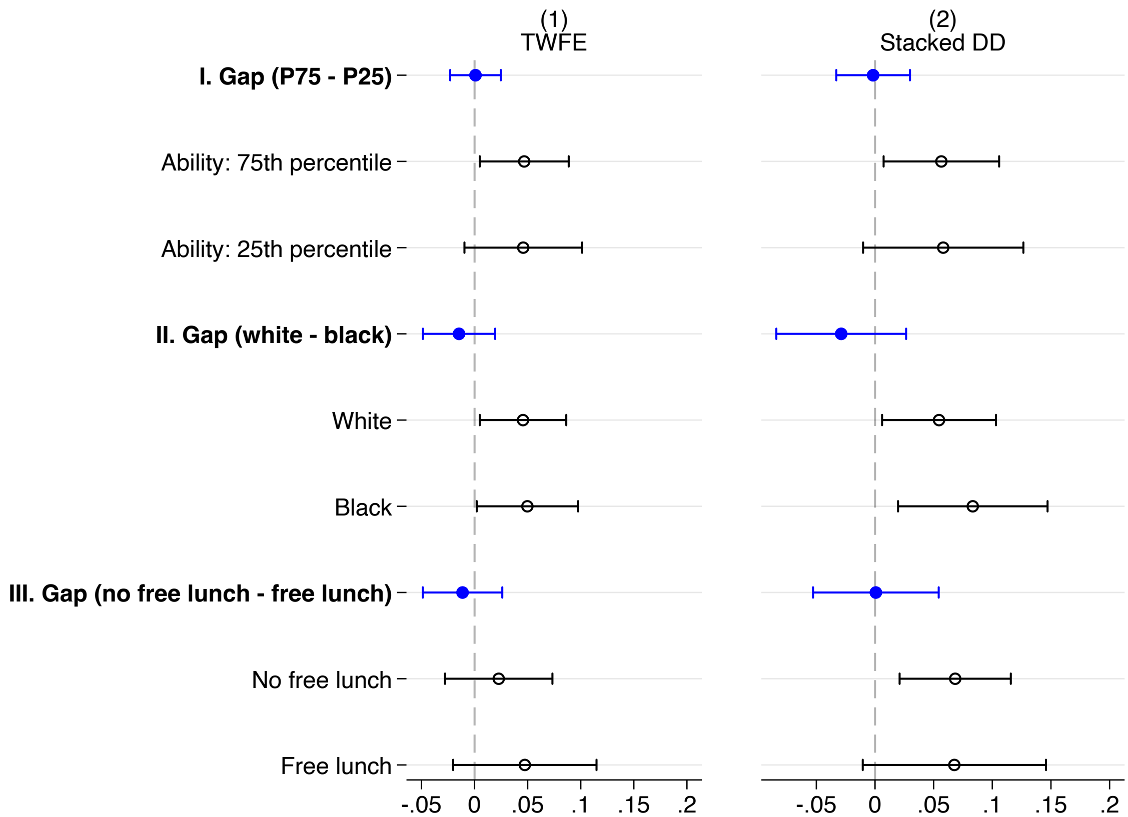
Distributional Effects.— We next investigate the distributional effect of deregulation on student achievement. The finance literature has found that intrastate branching deregulation reduced income inequality by increasing incomes at the lower end of the distribution (Beck et al., 2010). However, whether banking deregulation reduces inequality in educational outcomes remains an open question.

We examine the heterogeneous effect on achievement across student groups by their ability, race, and free lunch status, following the approach of Lafortune et al. (2018). To do this, we first obtain individual test scores for students in the specific group within each grade-state-year cell. Next, we compute the score averages for each group aggregated to the grade-state-year level, weighted by NAEP individual weights. We compute the difference of statewide score averages between groups as the gap for students with different abilities, races, or free lunch status. Finally, we weight each regression by the total count of NAEP-surveyed students in the corresponding group.

Figure 6 presents the heterogeneity effects.[46] Stacked DiD estimates show that, students situated in higher percentiles of achievement (75th percentile), those who are white, those who are black, or those not enrolled in the free lunch program exhibit statistically significant improvements in test scores following deregulation. However, none of the achievement gaps across groups are statistically different from zero, and all estimated magnitudes are close to zero. The results indicate that banking deregulation improves overall student achievement without generating differential effects across student groups by ability, race, or free lunch status.

[46]Table C3 Panel B presents reduced-form estimates for the distributional analysis.

Figure 6: Distributional Effects of Branching Deregulation on Student Achievement



Notes: The figure shows the distributional effects of deregulation on student achievement, using both two-way fixed effects and the stacked DiD model. The dependent variables are the specified summaries of the within-state student achievement distribution: the 75th and 25th percentiles scores; scores for white and black students; and scores for free/reduced-price lunch students and non-free/reduced-price lunch students. The mean of NAEP scores of students in each group is computed and then aggregated to the level of state-year-grade level, and standard errors are weighted by the count of NAEP-surveyed students in each group. See more details in the main text. Standard errors clustered at the state level are in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.
NAEP Source: US Department of Education, Institute of Education Sciences, National Center for Education Statistics, National Assessment of Educational Progress (NAEP) 1992, 1996, 2000, 2003, and 2005 Mathematics Assessments.

7 Conclusion

We examine how US interstate bank branching deregulation affects school finance and student achievement. By leveraging the staggered implementation of deregulation across states, we find that deregulation raises per-pupil total revenue and expenditure of treated school districts. The increases in total revenue are primarily driven by higher state aid, while the spending gains are concentrated in capital outlays. Our further investigation indicates improved student achievement following deregulation, with no distributional effects evident by students' ability, race, or free lunch status.

This study advances the finance-education nexus by studying how a centralized school finance system interacts with broader economic conditions to shape the provision of public education funding and student outcomes, in the context of the 1990s U.S.bank branching deregulation. The paper provides the first causal evidence on the impact of banking deregulation on K-12 public school finance and offers a comprehensive investigation of its overall and distributional impacts on educational outcomes. These findings demonstrate the fiscal and educational consequences of financial deregulation as part of its broader social welfare implications. In the end, by documenting the downstream effects on student achievement, the paper contributes to the school spending literature with a new nationwide analysis that exploits more naturally occurring tax-revenue-based variation.

Looking ahead, future research is encouraged to explore longer-term outcomes beyond test scores, such as high school completion, college attendance, and labor market performance. How deregulation-induced capital spending increases may be capitalized into housing prices remains an open question, speaking to the efficiency of these school infrastructure investments. In addition, the impacts of revenue shocks on local revenue crowd-out and on the tax prices of local spending remain understudied, offering promising public finance angles and opportunities to connect with broader literatures. We see these explorations as promising avenues for future research.

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Online Appendix to:

“The Finance-Education Nexus: Educational Consequences of US Interstate Bank Branching Deregulation”

Xi Yang **Jian Zou**

January, 2026

A Data: Additional Details

A.1 Data Sources

State Government Tax Collection.— The state government tax revenues are from the Annual Survey of State Government Tax Collections (STC) of the Census Bureau, which is available at https://www.census.gov/programs-surveys/stc/data/datasets.html. The STC is conducted by the Census Bureau to contain annual statistics on the fiscal year tax collections of all 50 state governments in the United States. We made use of STC's historical dataset for the fiscal years of 1992 and 2005, which gives information on the total tax revenues, as well as a summary of taxes for 5 categories: property taxes, sales and gross receipts taxes, license taxes, income taxes, and other taxes.

School Finance Data.— The school finance data are from the CCD School District Finance Survey (or F-33). We obtained the data for fiscal years 1992 and 1995-2005 via the CCD Data Files (https://nces.ed.gov/ccd/files.asp). For school district finance in fiscal years 1993 and 1994, we make use of the Census of Government data, following Lafortune et al. (2018). The Census of Government data are available from the replication files of Lafortune et al. (2018) at https://www.openicpsr.org/openicpsr/project/113709/version/V1/view. We appended them and constructed repeated cross-sectional data on school finance, as well as enrollment, for every school district during the fiscal years of 1992 and 2005.

The school finance items include total revenue and total expenditure. Total revenue is the summation of federal revenue, state revenue, and local revenue. Total expenditure primarily includes total current expenditure for elementary and secondary education (which can be further separated into current instructional and non-instructional expenditure), total non-elementary/secondary expenditure, and total capital outlays expenditure. F-33 data also contain expenditures on teacher salaries and benefits. All finance items are measured on per pupil base and in 2013 US dollar terms.

School Non-Finance Data.— Using the CCD School District Universe Survey, we further collect counts of full-time equivalent (FTE) teachers for each school district in fiscal years between 1992 and 2005. Combined with the enrollment number of the F-33, we calculate the pupil-teacher ratio (PTR) as the ratio between FTE teacher counts and enrolled student numbers for each district-by-year cell. The School District Universe Survey is available at the CCD Data Files (`https://nces.ed.gov/ccd/files.asp`).

NAEP data.— We applied for and obtained the restricted-use student-level data from the National Assessment of Educational Progress (NAEP) at the National Center for Education Statistics of the Institute of Education Sciences. Researchers who are interested in working with the restricted-use NAEP data would need to apply for and obtain the restricted-use data license from the IES/NCES (see `https://nces.ed.gov/pubsearch/licenses.asp#license`). We obtain a set of data including mathematics and reading assessments for students in fourth and eighth grades between 1990 and 2011. The surveyed states and years across the NAEP waves are documented in Table A1. NAEP datasets prior to 2000 do not include NCES school district identifiers that can match with the identifiers in CCD data. To process the data construction, we obtained the DELIVER.DAT (from ESRI) data shared by Julien Lafortune, Jesse Rothstein, and Diane Schanzenbach. The DELIVER.DAT was designed and developed originally by Westat, Inc., the major field administration contractor of the NAEP. Combined with the replication scripts of Lafortune et al. (2018), this dataset can be used to generate the NAEP-CCD crosswalk data.

Auxiliary Data.— 1) The state population totals are from Census Bureau's **State Intercensal Tables** for the periods of 1990-2000 and 2000-2010. The table 1990-2000 is available at `https://www.census.gov/data/tables/time-series/demo/popest/intercensal-1990-2000-state-and-county-totals.html`, and table 2000-2010 is available at `https://www.census.gov/data/tables/time-series/demo/popest/intercensal-2000-2010-state.html`. The tables provide intercensal estimates of the resident population (as of July 1) in the United States. 2) To assess the heterogeneous effects across different school districts, we merge the school district data with the **1990 School District Databook (SDDB)**. The 1990 SDDB allows us to identify a school district as a low-, medium-, or high-income district according to its within-state distribution of mean household income. We obtain the data from replication files of Lafortune et al. (2018), which is available at https://doi.org/10.3886/E113709V1. 3) The **land availability data** is a newly developed topological dataset from Lutz and Sand (2019), which measures county-level elasticity of housing supply in 1990. The data are available at https://github.com/ChandlerLutz/LandUnavailabilityData.

Table A1: NAEP Survey States and Years: 1990-2005

	Subjects and grades				Number of
Year	Mathematics G4	Mathematics G8	Reading G4	Reading G8	states
1990		✓			38
1992	✓	✓	✓		42
1994			✓		41
1996	✓	✓			45
1998			✓	✓	41
2000	✓	✓			42
2002			✓	✓	51
2003	✓	✓	✓	✓	51
2005	✓	✓	✓	✓	51

Notes: The table displays the NAEP survey schedules for mathematics and reading assessments of students in fourth and eighth grade, as well as the number of participating states between 1990 and 2011.

A.2 Data Sampling and Construction

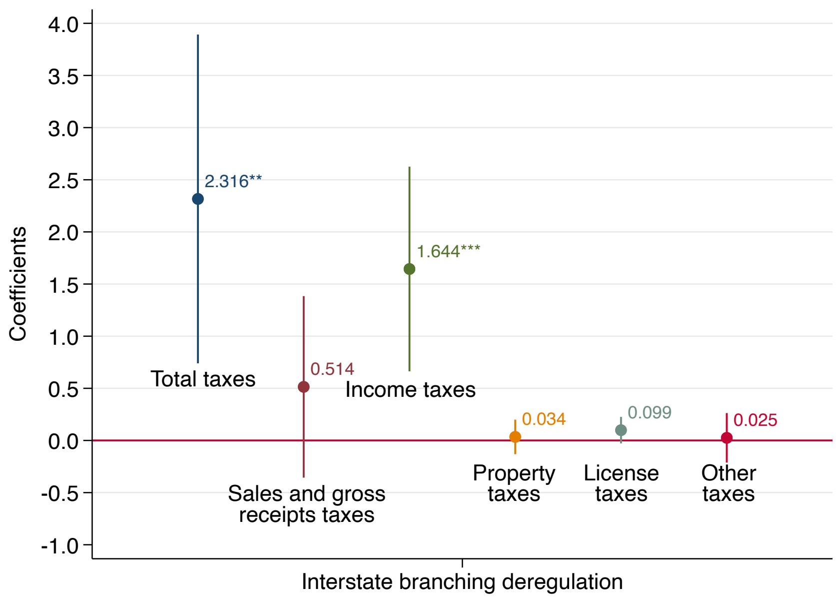
School Finance.— Our process for constructing our estimation sample of school districts is based on that of Lafortune et al. (2018). We construct the district-by-year data beginning with F-33 data between 1992 and 2005. We drop districts with missing a county fips code or LEA id, as well as districts with missing or negative enrollment. We kept only primary and secondary school districts and dropped administrative and charter-only districts. Then we merged the sample with the School District Universe Survey, with very few district-by-year cells unmatched (less than 0.2% of the F-33 sample). We dropped Hawaii and the District of Columbia from the sample, both of which only have one single school district.

Due to the noisy information on district enrollment over time, we also kept school districts with a reasonable enrollment. We first dropped districts with a small enrollment (less than 100) or a very large enrollment (larger than two times the average enrollment). We dropped school districts with an overly volatile enrollment – those with 15% year-on-year changes or 10% above/off the trend line. We also dropped the districts if more than 1/3 of observations were trimmed for one of the above. After trimming the school districts, none of the finance items have missing or a zero value. In the end, we obtain a district-by-year sample with 12,821 unique school districts out of 162,724 observations, corresponding to about 80% of all school district observations (N=203,846) in the 49 states between 1992 and 2005.

Student Achievement.— Although NAEP provides assessments in mathematics and reading, we employ mathematics assessments in this study because the eighth grade reading assessment was launched in 1998 and thus only one state in the treatment group has pre-trends. To obtain comparable estimates, we first standardize the individual assessment scores, within each subject and grade, to the distribution in the first tested year (i.e., 1992). We then aggregate the standardized individual scores to obtain student achievement at the grade-district-year level, weighting the student-level scores by the individual NAEP weights. Lastly, we merge the school district aggregates of student achievement with the school finance estimation sample constructed from the F-33 data.

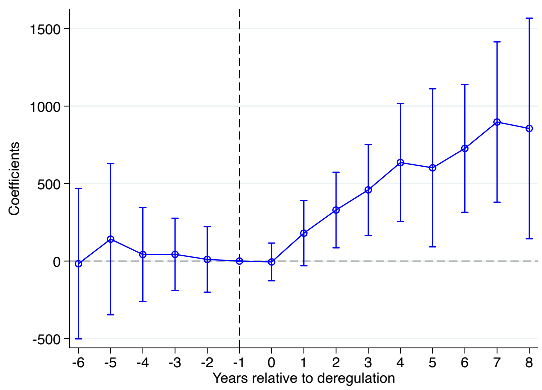
B Robustness Checks

Figure B1: Robustness: Impacts on State Government Tax Revenues

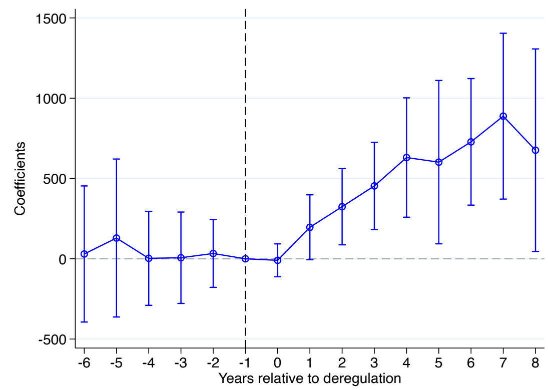


Notes: The dependent variables are state government state tax revenues on total taxes, as well as revenues in five sub-categories: sales and gross receipts taxes, income taxes, property taxes, license taxes, and other taxes. The coefficients are obtained from interaction terms between the deregulation (on the statewide deposit cap) and a dummy indicating states with non-zero income taxes. All dependent variables are expressed in constant 2013 million dollars. All regressions include a time-varying log population as the control, as well as state fixed effects and year fixed effects. The figure shows the 90% confidence interval with standard errors clustered at the state level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

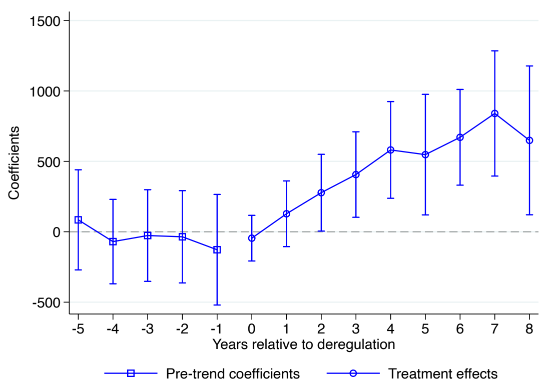
Figure B2: Revenue Effects: Heterogeneous DiD Treatment Effects



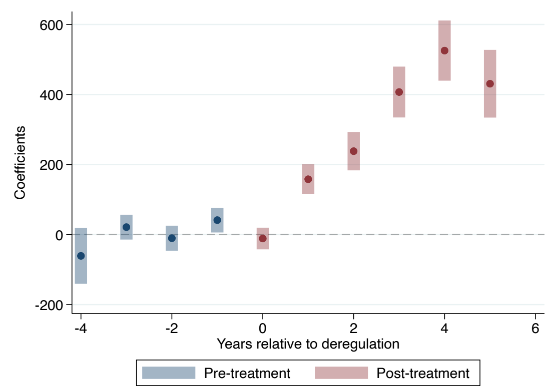
(a) De Chaisemartin and D'Haultfoeuille (2020)



(b) Sun and Abraham (2021)



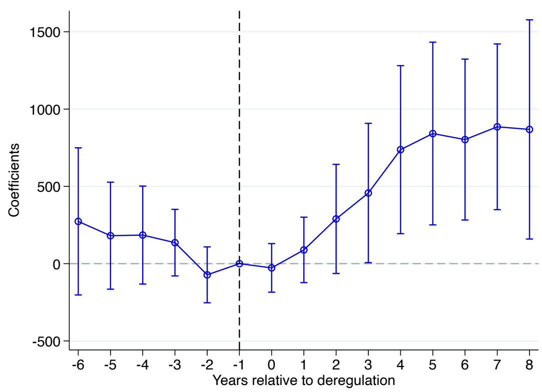
(c) Borusyak et al. (2021)



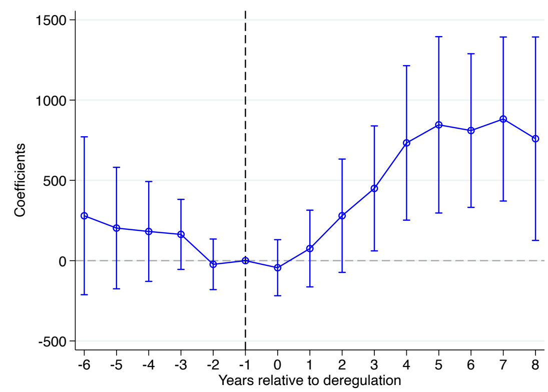
(d) Callaway and Sant'Anna (2021)

Notes: The subfigures (a) to (d) show estimates (with a 95% confidence interval) of the impact on per-pupil total revenue using de Chaisemartin and D'Haultfoeuille (2020), Sun and Abraham (2021), Borusyak et al. (2024), and Callaway and Sant'Anna (2021), respectively. Subfigure (a) is generated using 1,000 replications for the bootstrapped standard errors. The sample in Subfigure (d) is restricted to a balanced district-by-year panel which is required by the approach.

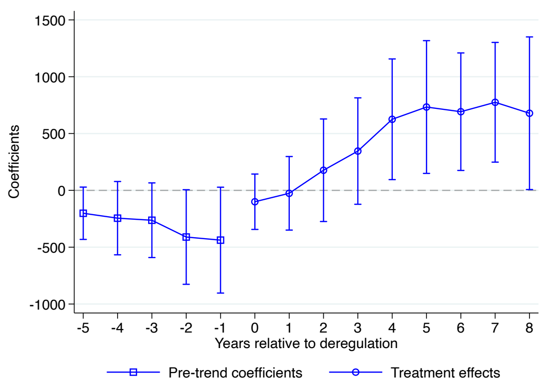
Figure B3: Expenditure Effects: Heterogeneous DiD Treatment Effects



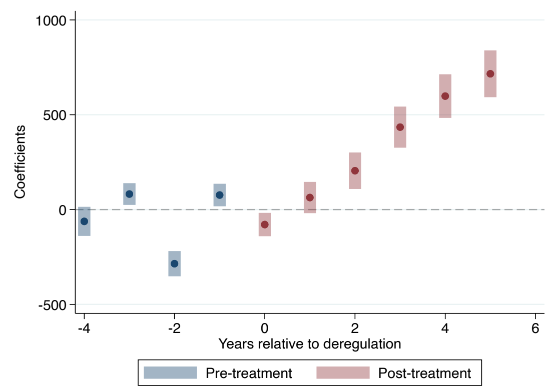
(a) De Chaisemartin and D'Haultfoeuille (2020)



(b) Sun and Abraham (2021)



(c) Borusyak et al. (2021)



(d) Callaway and Sant'Anna (2021)

Notes: The subfigures (a) to (d) show estimates (with a 95% confidence interval) of the impact on per-pupil total expenditure using de Chaisemartin and D'Haultfoeuille (2020), Sun and Abraham (2021), Borusyak et al. (2024), and Callaway and Sant'Anna (2021), respectively. Subfigure (a) is generated using 1,000 replications for the bootstrapped standard errors. The sample in Subfigure (d) is restricted to a balanced district-by-year panel which is required by the approach.

Table B1: Reform Timing Comparison: Interstate Branching Laws and Post-1990 SFRs

State	Reform timing		State	Reform timing	
	Interstate branching deregulation (1)	Post-1990 SFRs (2)		Interstate branching deregulation (1)	Post-1990 SFRs (2)
Alabama	1997	x	Montana	x	2005
Alaska	1994	1999	Nebraska	x	x
Arizona	1996	1998	Nevada	1995	x
Arkansas	x	2002	New Hampshire	2000	x
California	1995	2004	New Jersey	1996	1998
Colorado	x	2000	New Mexico	1996	1999
Connecticut	1995	x	New York	1997	x
Delaware	1995	x	North Carolina	1995	1997
District of Columbia	1996	x	North Dakota	x	x
Florida	1997	x	Ohio	1997	1997
Georgia	1997	x	Oklahoma	x	x
Hawaii	1997	x	Oregon	1997	x
Idaho	1995	1993	Pennsylvania	1995	x
Illinois	1997	x	Rhode Island	1995	x
Indiana	1997	x	South Carolina	1996	x
Iowa	x	x	South Dakota	1996	x
Kansas	x	2005	Tennessee	1997	1995
Kentucky	x	x	Texas	x	1992
Louisiana	1997	x	Utah	1995	x
Maine	1997	x	Vermont	1996	2003
Maryland	1995	2002	Virginia	1995	x
Massachusetts	1996	1993	Washington	1996	x
Michigan	1995	x	West Virginia	x	1995
Minnesota	1997	x	Wisconsin	1996	x
Mississippi	x	x	Wyoming	1997	2001
Missouri	x	1993			

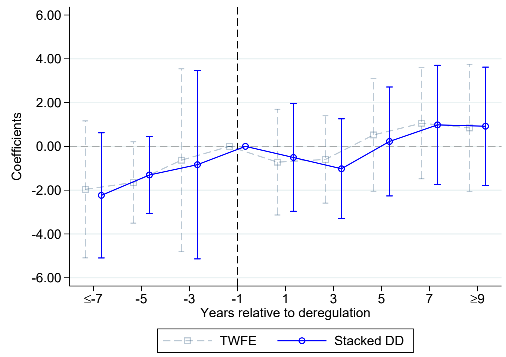
Notes: The table shows the policy timing variation for both deregulation and SFRs for states, while 'x' indicates no reform in the state during our sample period (1992-2005). Columns (1)-(2) refer to the deregulation on statewide deposit caps and post-1990 school finance reforms, respectively. Four states have SFRs beyond the 1992-2005 window. Three of them have SFRs implemented after 2005, thereby being as controls: Indiana in 2011, New Hampshire in 2008, New York in 2006, North Dakota in 2007, and Washington in 2010. Kentucky passed SFRs in 1990, which is considered as always treated. The deregulation data are from Rice and Strahan (2010), and the SFRs data are from Lafortune et al. (2018).

Table B2: Robustness: Potential Confounding Events

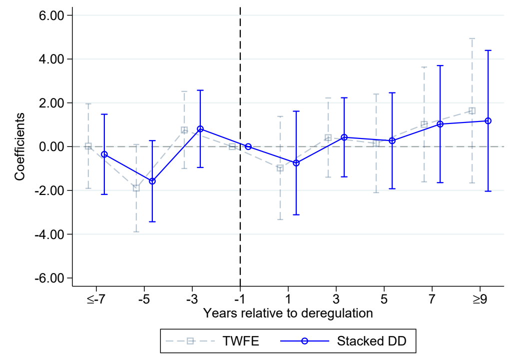
	School finance					
	Full sample			Q1	Q5	Non-SFR states
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Revenue effects						
Deregulation	390.147***	478.992***	377.411***	588.038***	155.135	395.876***
	(130.559)	(127.180)	(126.098)	(160.524)	(123.174)	(119.036)
Deregulation - minimum age requirement		-90.116				
		(270.080)				
Deregulation - *De novo* branches		99.505				
		(321.900)				
Deregulation - acquisition of branches		-267.274				
		(287.152)				
SFRs			333.236**	311.182**	252.419	
			(155.853)	(142.040)	(206.960)	
Deregulation*SFRs				452.576*	-113.825	
				(228.432)	(234.028)	
Mean dep. var.	10,844	10,844	10,844	11,050	11,505	10,940
Observations	162,724	162,724	162,724	32,011	31,395	92,264
Panel B. Expenditure effects						
Deregulation	392.195*	348.586	377.243*	578.306***	146.196	607.895***
	(198.418)	(216.019)	(196.291)	(201.834)	(205.056)	(188.612)
Deregulation - minimum age requirement		-81.071				
		(299.432)				
Deregulation - *De novo* branches		-168.446				
		(367.354)				
Deregulation - acquisition of branches		229.312				
		(404.879)				
SFRs			391.227**	391.518***	338.935	
			(174.089)	(145.963)	(225.133)	
Deregulation*SFRs				471.300*	-163.776	
				(241.961)	(261.784)	
Mean dep. var.	10,943	10,943	10,943	11,065	11,779	11,138
Observations	162,724	162,724	162,724	32,011	31,395	92,264
State FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓

Notes: The dependent variables are per-pupil total revenue and expenditure in Panel A and B, respectively. While column (1) replicates the baseline result, columns (2) and (3) include variation of potential confounding events: deregulation in the other three dimensions and school finance reforms (SFRs) during the study period. The SFRs variation follow the one complied in Lafortune et al. (2018). Columns (4) and (5) look at school districts in the first and fifth income quintiles, respectively. Column (6) replicates the main specification using states that do not implement SFRs. Standard errors clustered at the state level are in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

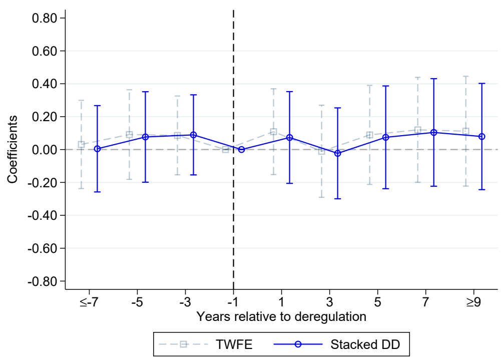
Figure B4: Robustness: No Impacts on School Characteristics



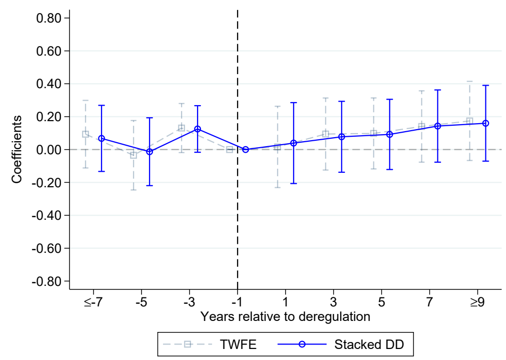
(a) Share black students



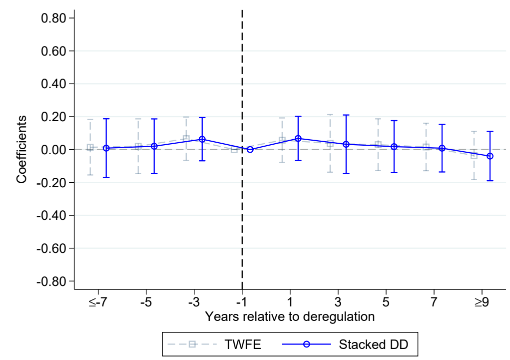
(b) Share Hispanic students



(c) Share students eligible for free lunch



(d) Share students with ESL instruction



(e) Share students in special education

Notes: The figures show event study estimates of the impact of deregulation on school characteristics, using two-way fixed effects and stacked DiD estimators. The analyses use school datasets from the NAEP and strict the sample to public schools. The dependent variables of the two subfigures in the top panel are the percentage of black and Hispanic students for each school. The dependent variables of the three subfigures in the bottom panel are the school-level percentage of students who are eligible for the free lunch program, English learners, and in special education, respectively. Event period -1 is normalized to zero. The figures show the 95% confidence interval with standard errors clustered at the state level.

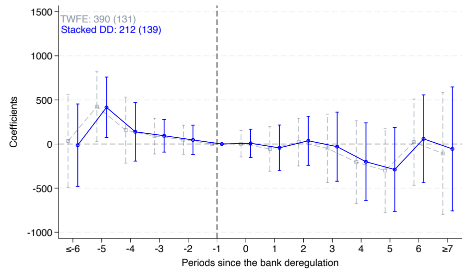
C Additional Results

Table C1: Impacts of Interstate Branching Deregulation on State Tax Revenue

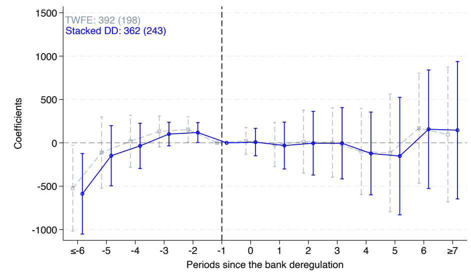
	Total Taxes	Sales and Gross Receipts Taxes	Income Taxes	Property Taxes	License Taxes	Other Taxes
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A						
Minimum age requirement	0.356	0.241	-0.132	0.112	0.043	0.093
	(0.729)	(0.348)	(0.478)	(0.085)	(0.066)	(0.057)
R-squared	0.981	0.989	0.961	0.902	0.951	0.888
Panel B						
De novo branches	0.306	0.366	-0.257	0.122	0.016	0.059
	(0.699)	(0.307)	(0.467)	(0.081)	(0.066)	(0.056)
R-squared	0.981	0.989	0.961	0.902	0.951	0.888
Panel C						
Acquisition of branches	0.246	0.210	-0.076	0.101	0.020	0.032
	(0.756)	(0.280)	(0.540)	(0.069)	(0.069)	(0.055)
R-squared	0.981	0.989	0.961	0.902	0.951	0.887
Panel D						
Statewide deposit cap	0.932**	0.249	0.579**	0.035	-0.013	0.083*
	(0.457)	(0.292)	(0.259)	(0.052)	(0.059)	(0.041)
R-squared	0.982	0.989	0.962	0.901	0.951	0.888
State FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Mean dep. var.	13.509	6.550	5.367	0.286	0.867	0.440
Observations	686	686	686	686	686	686

Notes: The dependent variables are state government state tax revenues on total taxes (Column 1), as well as revenues in five sub-categories: sales and gross receipts taxes, income taxes, property taxes, license taxes, and other taxes (Columns 2-6). The independent variables are interstate branching deregulation between 1994 and 2005 on minimum age requirement, *de novo* branches, acquisition of branches, and statewide deposit cap in Panel A, B, C, and D, respectively. All dependent variables are re-scaled in 1 million dollar terms and deflated to 2013 US dollars. All regressions include a time-varying log population as the control, as well as state fixed effects and year fixed effects. Standard errors clustered at the state level are in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

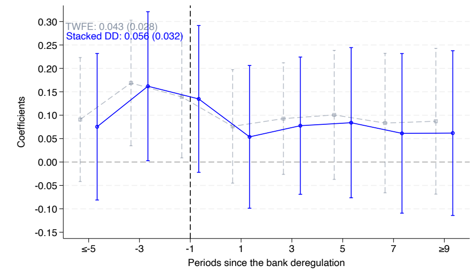
Figure C1: Event Studies on Impacts on School Finance and Student Achievement: Other Deregulation



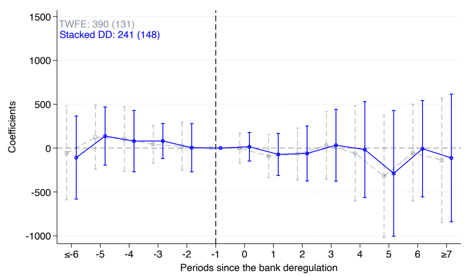
(a) Revenue: minimum age requirement



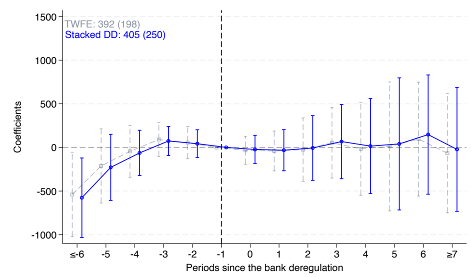
(b) Expenditure: minimum age requirement



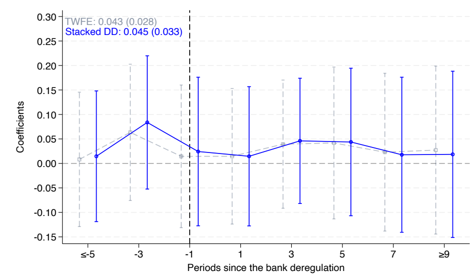
(c) NAEP: minimum age requirement



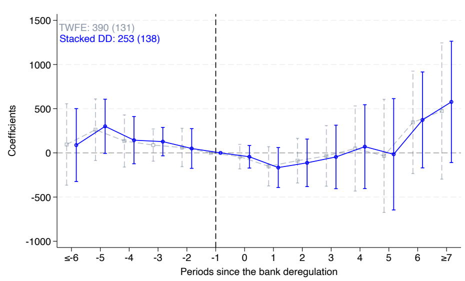
(d) Revenue: *de novo* branches



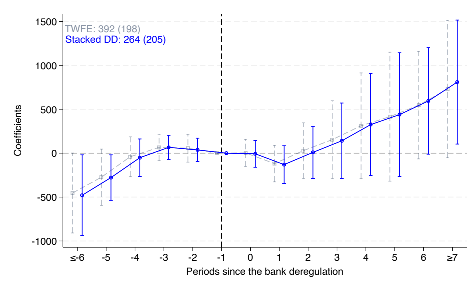
(e) Expenditure: *de novo* branches



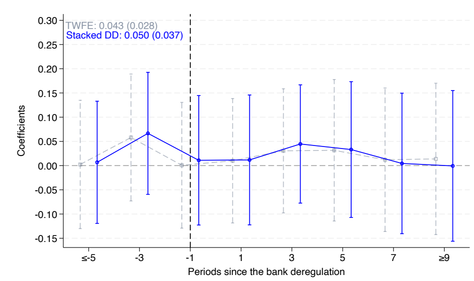
(f) NAEP: *de novo* branches



(g) Revenue: acquisition of branches



(h) Expenditure: acquisition of branches



(i) NAEP: acquisition of branches

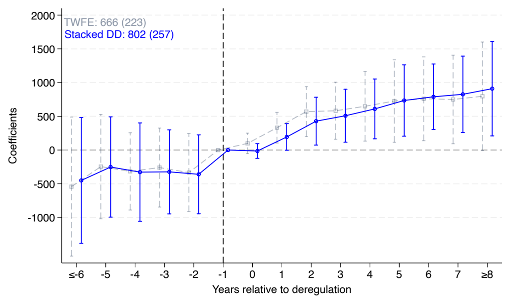
Notes: The figure shows event study estimates on school finance (per-pupil total revenue and expenditure) and student achievement (district-level analysis) using the deregulation in the other three dimensions: minimum age requirement (top panel), *de novo* branches (medium panel), and acquisition of branches (bottom panel). The DD estimates, obtained using two-way fixed effects (in gray) and stacked DiD estimators (in blue), are shown at the upper-left corner, with standard errors included in the parentheses.
NAEP Source: US Department of Education, Institute of Education Sciences, National Center for Education Statistics, National Assessment of Educational Progress (NAEP) 1992, 1996, 2000, 2003, and 2005 Mathematics Assessments.

Table C2: Impacts of Branching Deregulation on School Finance: TWFE

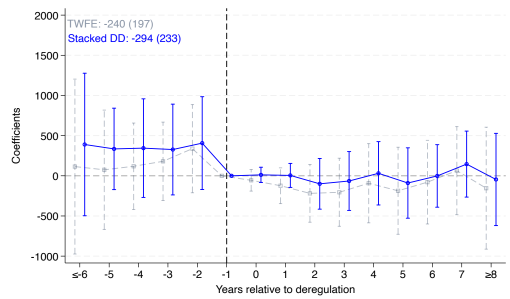
	Mean of dep var	TWFE estimate	District income		Land availability	
			Q1	Q5	Inelastic	Elastic
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Revenue effects						
Total revenue	10,844	390***	689***	142	310**	470***
		(131)	(168)	(130)	(128)	(148)
State revenue	5,117	666***	842***	720**	637***	731**
		(223)	(208)	(297)	(219)	(285)
Local revenue	5,063	-240	-106	-546*	-285*	-212
		(197)	(150)	(286)	(148)	(342)
Federal revenue	663.2	-36	-4	-32**	-42*	-49*
		(22)	(48)	(13)	(25)	(25)
Panel B. Expenditure effects						
Total expenditures	10,943	392*	686***	127	301	489*
		(198)	(187)	(214)	(192)	(258)
Total current exp.	9,196	169	374***	52	151	168
		(115)	(119)	(122)	(107)	(170)
Current instructional exp.	5,681	137	259**	88	119	114
		(93)	(94)	(95)	(77)	(138)
Current non-instructional exp.	3,514	33	115**	-36	32	54
		(40)	(44)	(48)	(47)	(54)
Capital outlays	1,064	241***	283***	99	173*	315***
		(74)	(96)	(82)	(92)	(77)
Teacher salaries + benefits	5,109	153*	260***	92	127	157
		(74)	(86)	(88)	(79)	(120)
Teacher salaries	4,074	61	142*	7	71	26
		(69)	(72)	(67)	(60)	(92)
Panel C. Class size						
Pupil teacher ratio	15.55	-0.328	-0.359*	0.088	-0.148	-0.451
		(0.196)	(0.183)	(0.151)	(0.207)	(0.311)

Notes: The table shows the deregulation effects on school finance using the two-way fixed effects model. Column (1) presents the mean of dependent variables, while column (2) shows the estimates for average effect size. Columns (3)-(4) look at school districts in the first/fifth quintile of the within-state household mean income: Q1/Q5 indicates school districts in the lowest/highest income quintile. Columns (5) and (6) separate school districts into counties with inelastic or elastic land availability in 1990. The relevant estimates using stacked DiD estimators are in Table 3. Standard errors clustered at the state level are in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

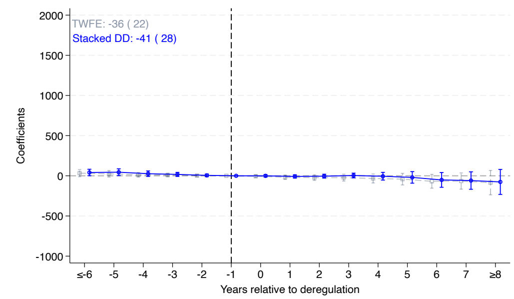
Figure C2: Event Study Estimates on Impacts on School Finance by Category



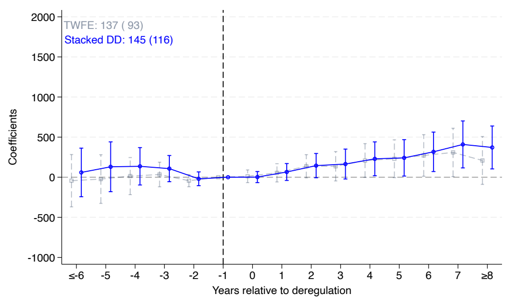
(a) State revenue



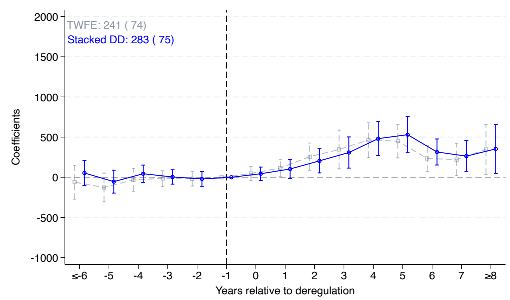
(b) Local revenue



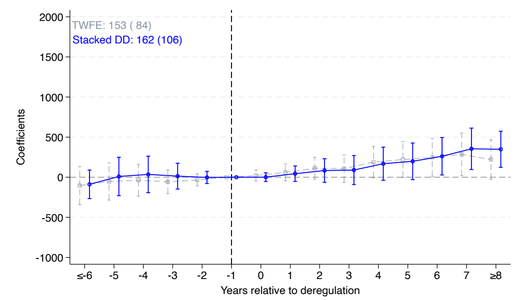
(c) Federal revenue



(d) Current instructional expenditure



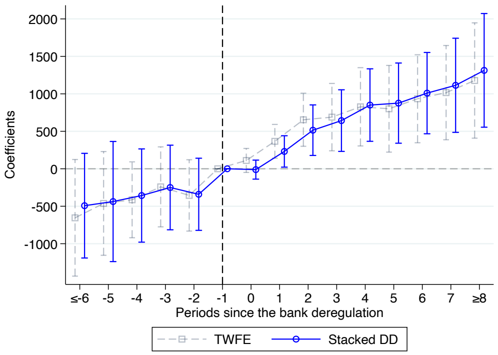
(e) Capital Outlays



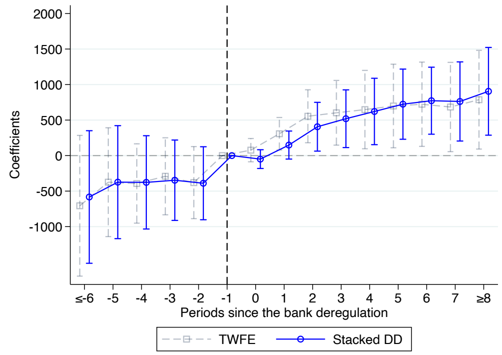
(f) Teacher salaries and benefits

Notes: The figures show event study estimates of the impact of deregulation on school finance by category, using two-way fixed effects (in gray) and stacked DiD estimators (in blue). The dependent variables on the top panel are state revenue (a), local revenue (b), and federal revenue (c), while the dependent variables on the bottom panel are current instructional expenditure (d), capital outlays (e), and teacher salaries and benefits (f). All finance items are measured in terms of constant 2013 dollars per pupil. Event period -1 is normalized to zero. The figures show the 95% confidence interval with standard errors clustered at the state level. The DD estimates are shown at the upper-left corner, with standard errors included in the parentheses.

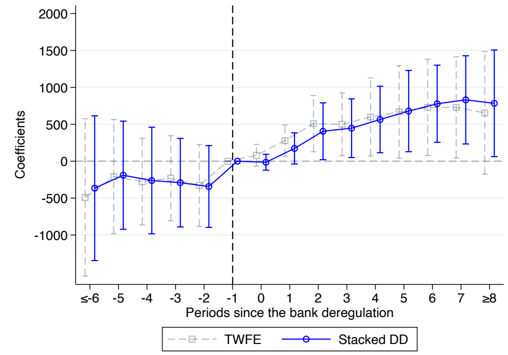
Figure C3: Event Study Estimates of Impacts on State Revenue by Quintile



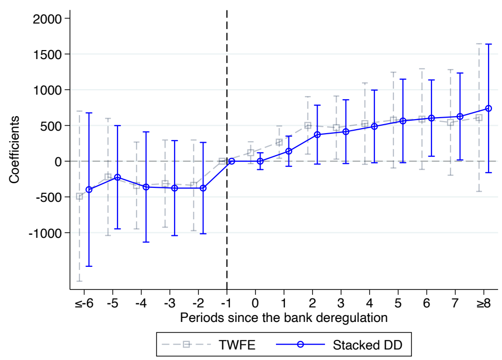
(a) First quintile



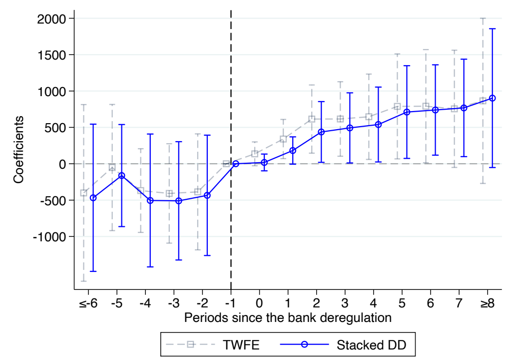
(b) Second quintile



(c) Thrid quintile



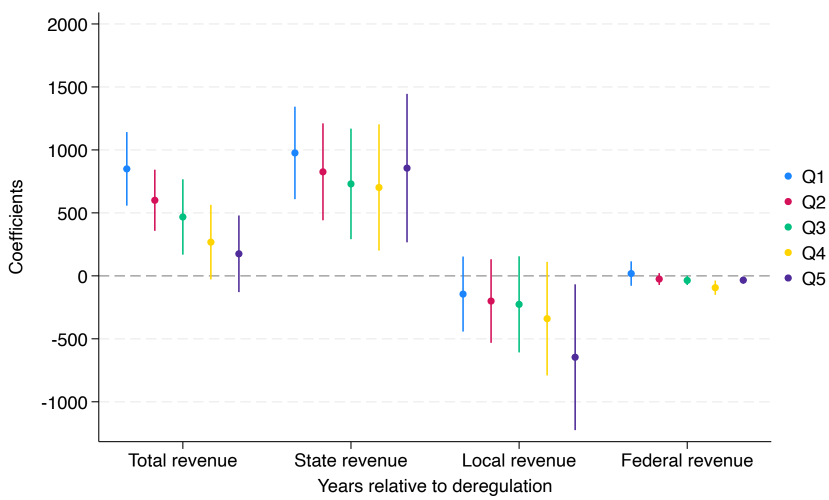
(d) Fourth quintile



(e) Fifth quintile

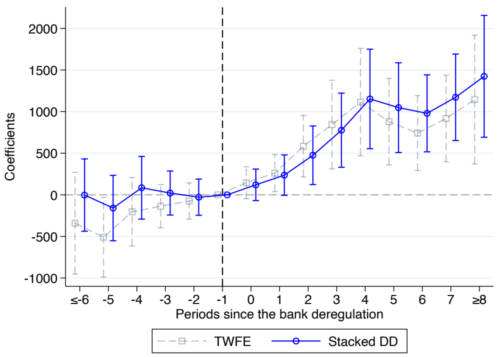
Notes: The figures show event study estimates of the impacts of deregulation on state revenue by quintile, using two-way fixed effects and stacked DiD estimators. A school district is identified in one of the quintiles based on its within-state distribution of baseline mean household income. School districts in the first/fifth quintile indicate those in the lowest-/highest- income quintile. The dependent variables are state revenue per pupil in 2013 dollars terms. Event period -1 is normalized to zero. The figures show the 95% confidence interval with standard errors clustered at the state level.

Figure C4: Revenue Effects by Quintile

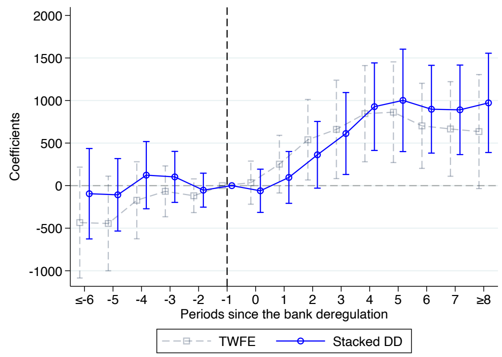


Notes: The figures show estimates of revenue effects by quintile (Q1-Q5), using stacked DiD estimators. A school district is identified in one of the quintiles based on its within-state distribution of baseline mean household income. The dependent variables are per-pupil total revenue (in 2013 dollars terms) and its sub-categories: state revenue, local revenue, and federal revenue. Event period -1 is normalized to zero. The figures show the 90% confidence interval with standard errors clustered at the state level.

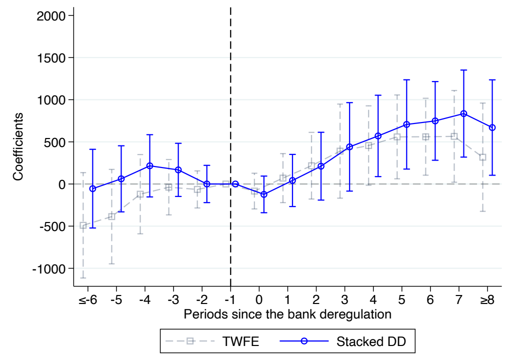
Figure C5: Event Study Figure on Expenditure Effects by Quintile



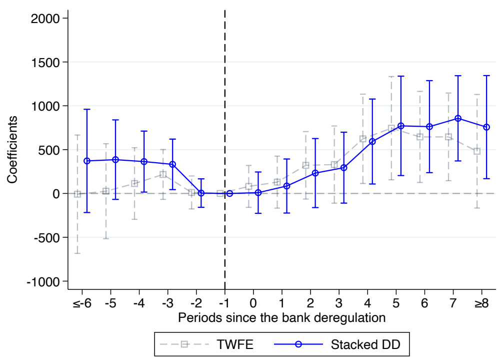
(a) First quintile



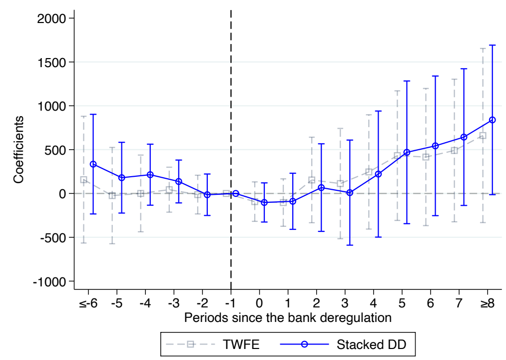
(b) Second quintile



(c) Thrid quintile



(d) Fourth quintile



(e) Fifth quintile

Notes: The figures show event study estimates of expenditure effects by quintile, using two-way fixed effects and stacked DiD estimators. A school district is identified in one of the quintiles based on its within-state distribution of baseline mean household income. School districts in the first/fifth quintile indicate those in the lowest-/highest- income quintile. The dependent variables are total expenditure per pupil in 2013 dollars terms. Event period -1 is normalized to zero. The figures show the 95% confidence interval with standard errors clustered at the state level.

Table C3: Overall and Distributional Effects of Branching Deregulation on Student Achievement

	Average NAEP score		
	Mean of dep var (1)	TWFE (2)	Stacked DiD (3)
Panel A. Overall effects			
District level analysis	–	0.040	0.053
		(0.028)	(0.035)
Individual level analysis	–	0.038	0.050
		(0.026)	(0.032)
Individual level analysis + student controls	–	0.046*	0.064**
		(0.024)	(0.030)
Panel B. Heterogeneity across groups			
Gap (P75-P25)	1.204	0.001	-0.002
		(0.015)	(0.019)
Ability: 75th percentile	0.921	0.047*	0.057*
		(0.025)	(0.030)
Ability: 25th percentile	-0.283	0.046	0.058
		(0.034)	(0.042)
Gap (white - black)	0.846	-0.015	-0.029
		(0.021)	(0.034)
White	0.536	0.046*	0.055*
		(0.025)	(0.030)
Black	-0.310	0.050*	0.083**
		(0.029)	(0.039)
Gap (no free lunch - free lunch)	0.653	-0.011	0.001
		(0.023)	(0.033)
No free lunch	0.587	0.023	0.068**
		(0.031)	(0.029)
Free lunch	-0.065	0.047	0.068
		(0.041)	(0.047)

Notes: The table shows the overall (Panel A) and distributional effects (Panel B) of deregulation on student achievement, using both two-way fixed effects and the stacked DiD model. Column (1) presents the mean of the dependent variable, and columns (2) and (3) display the estimates. In Panel A, student controls include dummies indicating if the student is female, black, needs an individualized education plan, or is an English learner. In Panel B, the dependent variables are the specified summaries of the within-state student achievement distribution: the 75th and 25th percentiles scores; scores for white and black students; and scores for free/reduced-price lunch students and non-free/reduced-price lunch students. The mean of NAEP scores of students in each group is computed and then aggregated to the level of state-year-grade level, and standard errors are weighted by the count of NAEP-surveyed students in each group. See more details in the main text. Standard errors clustered at the state level are in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

NAEP Source: US Department of Education, Institute of Education Sciences, National Center for Education Statistics, National Assessment of Educational Progress (NAEP) 1992, 1996, 2000, 2003, and 2005 Mathematics Assessments.