EdWorkingPaper No. 23-874

Financial Deregulation, School Finance, and Student Achievement

Xi Yang
University of North Texas

Jian Zou
University of Illinois Urbana-Champaign

This paper studies how school spending impacts student achievement by exploiting the US interstate branching deregulation as state tax revenue shocks. Leveraging school finance data from universal school districts, our difference-in-differences estimation reveals that deregulation leads to an increase in per-pupil total revenue and expenditure. The rise in revenue is primarily attributed to higher state revenues, while the expenditure increase is more prominent in low-income school districts. Using restricted-use student assessments from the Nation's Report Card, we find that deregulation results in improved student achievement, with no distributional effects evident across students' ability, race, or free lunch status. We introduce an instrumental variables approach that accounts for dynamic treatment effects and estimate that a one-thousand-dollar increase in per-pupil spending leads to a 0.035 standard deviation improvement in student achievement.

VERSION: November 2023

Suggested citation: Yang, Xi, and Jian Zou. (2023). Financial Deregulation, School Finance, and Student Achievement. (EdWorkingPaper: 23-874). Retrieved from Annenberg Institute at Brown University: https://doi.org/10.26300/k7ep-0t13

Financial Deregulation, School Finance, and Student Achievement*

Xi Yang Jian Zou (JMP)†

October, 2023

Click here for the latest version

Abstract

This paper studies how school spending impacts student achievement by exploiting the US interstate branching deregulation as state tax revenue shocks. Leveraging school finance data from universal school districts, our difference-in-differences estimation reveals that deregulation leads to an increase in per-pupil total revenue and expenditure. The rise in revenue is primarily attributed to higher state revenues, while the expenditure increase is more prominent in low-income school districts. Using restricted-use student assessments from the Nation's Report Card, we find that deregulation results in improved student achievement, with no distributional effects evident across students' ability, race, or free lunch status. We introduce an instrumental variables approach that accounts for dynamic treatment effects and estimate that a one-thousand-dollar increase in per-pupil spending leads to a 0.035 standard deviation improvement in student achievement.

Keywords: Interstate branching deregulation; School spending; Student achievement
JEL Classification: G21, G28, H75, I21, I22

*Zou particularly acknowledges Dan Bernhardt, Benjamin Marx, Rigissa Megalokonomou, and Russell Weinstein for their guidance and advice. We thank Taara Cason and Adam Todd at the Institute of Education Sciences (IES) for assistance with the NAEP data and Julien Lafortune, Jesse Rothstein, and Diane Schanzenbach for sharing the NAEP-CCD crosswalk data. We are thankful for the comments from Claire Célérier, Jiaying Gu, William Ridley, Michael Stepner, and seminar participants at the University of Illinois. The paper has been reviewed by the IES to ensure there are no disclosure risks. All errors remain ours.

†Yang: Department of Economics, University of North Texas, Denton, TX, 76203. xi.yang@unt.edu. Zou: Department of Economics, University of Illinois Urbana-Champaign, Urbana, IL, 61801. jianzou2@illinois.edu.

1 Introduction

Public K-12 education funding constitutes a significant portion of government spending and GDP in the United States and worldwide.[1] Ensuring the 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). Indeed, the policy relevance extends beyond its impact on educational quality; school finance metrics, including equity and adequacy, have been fundamental considerations in school finance reforms since the 1970s (Corcoran and Evans, 2015). Academically, economists have long been engaged in debates regarding the impact of school spending on student outcomes (Jackson, 2018; Handel and Hanushek, 2022). Building on a growing body of literature that addresses the classic question, "Does school spending matter?", our study advances the discussion by providing new evidence in the context of the 1990s nationwide interstate bank branching deregulation in the United States. We leverage the staggered implementation of deregulation as state government tax revenue shocks, together with comprehensive administrative data on school finance and student achievement, to examine how financial deregulation affects public school finance provision and student achievement.[2]

We exploit the exogenous state-level reforms on interstate branching deregulation. The Interstate Banking and Branching Efficiency Act (IBBEA), implemented in 1994, deregulated the banking industries 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.[3] We leverage

[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] US public schools nowadays are mainly financed through state governments' sales and income taxes and local property taxes. During school years 2000-01 and 2018-19, state revenue (47%) and local revenue (44%) take up over 90% of the per-pupil total revenue of a typical US school district, with federal revenue contributing the remaining 9% (NCES, 2022).

[3] The interstate bank branching deregulation, prompted by the 1994 IBBEA, is widely utilized in the finance literature. As documented in the literature, the timing of adopting interstate branching deregulation

the interstate branching deregulation, as a shock on state government tax revenues, to study the impact of increased school spending on student achievement.[4] Exploiting the staggered implementation of deregulation documented by Rice 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, to estimate how deregulation affects student achievement solely through school spending, while isolating other potential channels (e.g., human capital investment changes when families get rid of credit constraints), we construct dosage-based instrumental variables (IV) and introduce a modified IV approach that accounts for dynamic treatment effects.

We use two main administrative datasets. For information on school finance, we exploit administrative data from universal school districts, obtained from the School District Finance Survey (F-33) of the Common Core of Data (CCD). This dataset offers comprehensive details on school finances, covering all school districts in the United States since 1990. It provides detailed information on revenue, expenditure, and other characteristics of each district. For student achievement, we use 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 math and reading for students in grades 4 and 8, available biennially since the early 1990s. Both the F-33 survey and NAEP datasets are unique in their coverage and historical depth, allowing us to explore the educational consequences of financial 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

across states was plausibly exogenous, which was motivated by political factors, rather than economic conditions (Kroszner and Strahan, 1999, 2014).

[4]Financial deregulation relaxes financial constraints of firms/households and removes frictions in the capital markets, leading to accelerated new business formation (Kerr and Nanda, 2009), increased innovation of firms (Chava et al., 2013), and spurred the total factor productivity of small business (Krishnan et al., 2015), which could generate impacts on the real economy (Strahan et al., 2003).

tax revenues for state governments, the gains in total revenue are primarily driven by the increases in the state revenue channel, where income tax revenues are a major funding source. The increased total expenditure is more prominent in low-income school districts, which aligns with school funding formulas that aim at equalization in within-state resource distribution, shaped by School Finance Reforms since the 1970s. The results are robust across a host of validity checks, including different dynamic DiD estimators, addressing concerns from potential confounding events, and a contiguous county-pairs design.

The findings on student achievement show reduced-form estimates indicating that deregulation improves student assessment scores by approximately 0.064 standard deviations. The results are resilient to a battery of robustness checks. Further investigations show that these effects do not vary by student groups based on their ability, race, or free lunch status. To estimate the school spending effect through which deregulation affects student achievement while isolating other potential channels, we employ a dosage-based instrumental variable (IV) approach, in the spirit of Jackson et al. (2021). We further adjust the IV approach to incorporate dynamic treatment effects, where we adopt a stacked DiD estimator in the first stage and employ a control function approach for the two-stage estimation. The dosage-based IV approach capitalizes on variation in post-period spending slope changes across school districts with different baseline household income levels. Within the context of the equalization-based school funding system, low-income school districts experience a more rapid increase in their spending (referred to as "dosage") relative to high-income school districts following the deregulation. The IV estimations reveal that a one-thousand-dollar increase in per-pupil spending leads to an average 0.035 standard deviation improvement in student performance.

This study makes three significant contributions. First, the study complements the literature on school spending and student outcomes. Existing studies that investigate the question "Does school spending matter?" typically rely on credibly exogenous variations in either the *parameters* (i.e., changes in the rules governing resource distribution) or the *values* (i.e., change in the amount of allocated funds) of a school funding formula (Jackson, 2018). Studies that focus on changes in *parameters* primarily exploit the School Finance Reforms implemented in the 1970s and 1990s. These studies investigate how distribution changes of educational resources within a state, particularly towards low-income school districts, affect student outcomes.[5] Vis-à-vis these studies, our paper explores policy variations that, while adopted nationwide, impact spending in numerous school districts, not just in low-income districts. Moreover, by examining variations from non-SFR policies, our study mitigates potential concerns raised by concurrent educational policies or legislation. Recent studies exploit nationwide variations in the *values*, capitalizing on positive shocks in local revenue channels on school finance (Brunner et al., 2022).[6] However, as discussed in Brunner et al. (2022), funding raised from local revenue may be directed towards spending on capital outlays, leading to limited impacts on student achievement. In contrast, our study uses deregulation, which increases spending on both capital outlays and instructional dimensions, as the policy shock, thereby likely affecting student achievement.

More broadly, our study speaks to the literature on understanding the role of investments play in fostering human capital accumulation. Human capital theory has long lent perspectives from family investment, focusing on how changes in incentives and the ability of parental investment could affect children's outcomes (Becker, 1960, 2009). In the context of the nexus between finance and education, researchers have explored how financial policies that increase family income can enhance children's human capital formation.[7] One relevant study by Sun and Yannelis (2016) exploits a similar reform – the intrastate bank branching deregulation – and investigates how financial deregulation could influence the

[5]Many of these studies conduct their analysis based on single states. Studies that have a nationwide analysis have investigated 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 social mobility of the next generation (Biasi, 2023). See Jackson (2018) and Handel and Hanushek (2022) for recent surveys.

[6]In addition, a recent study by Jackson et al. (2021) examines the impact of negative economic shocks on the state revenue channel, showing how the Great Recession led to school spending cuts and weakened student educational outcomes. In contrast to their study, our paper focuses on a positive economic shock that results in increased school spending, subsequently improving student achievement.

[7]For example, see Dahl and Lochner (2012), Aizer et al. (2016), and Bailey et al. (2023), among others.

demand for college education by relaxing household credit constraints, thereby ruling out family-endowment effects. However, it is important to note that financial deregulation could impact human capital through the provision of K-12 public spending. From this perspective, our study establishes the first causal link between financial deregulation and school finance, which operates as an additional channel that affects student achievement.

Lastly, our study contributes to the literature on the consequences of the interstate bank branching deregulation. Previous research has examined the real effects of the post-1990 bank branching deregulation on firm performance (Rice and Strahan, 2010; Krishnan et al., 2015) and household wealth accumulation (Célerier and Matray, 2019).[8] Yet, almost no studies have investigated the educational consequences of the 1990s interstate branching deregulation.[9] Our study provides a comprehensive assessment of the educational consequences of interstate branching deregulation on student achievement via the school finance channel. The finance literature has also demonstrated that deregulation in the banking industry, in general, reduces income inequality by increasing the income of individuals at the lower end of the distribution (Beck et al., 2010). However, the question of whether financial deregulation could affect educational inequality remains open. Our paper addresses the question by conducting investigations on whether interstate branching deregulation has distributional impacts on student achievement across groups categorized by ability, race, and free lunch status. These investigations contribute to a more comprehensive understanding of the consequences of financial deregulation.

The paper proceeds as follows. Section 2 provides background on the 1990s US interstate branching deregulation and policy variations. 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] Recent studies have explored the impact of interstate branching deregulation on housing markets (Favara and Imbs, 2015; Yang, 2023).

[9] An exception is Hu et al. (2020), who find that interstate branching deregulation reduces children's academic performance in low-income families, possibly through the mechanism that low-income parents substitute out of childrearing and into employment. Unlike theirs, we highlight the channel of school spending that operates between financial deregulation and student achievement.

2 Interstate Branching Deregulation

Historically, states regulated geographic expansions of banking industries to enhance public finance.[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 in 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 that operated banking 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, 2023). 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 between 1994 and 2005, from data compiled by Rice and Strahan (2010). The staggered deregulation enacted by the states relaxed bank branching restrictions across the four dimensions described above. While the deregulation constitutes exogenous variation driven by political factors, rather than economic reasons, the use of this interstate branching deregulation is more concentrated within the finance literature and has been less widely employed in other fields.

Table 1 presents the timing of the four types of branching deregulation reforms between 1994 and 2005. Among the 51 states, 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 variations to identify the causal effect of branching deregulation. Further exploiting the policy variations by deregulation type, we can identify the significance of deregulation on the statewide deposit cap, in terms of its geographic coverage and reform timings. 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 non-statewide-deposit-cap reform.[14] Among the 38 states with statewide deposit cap deregulation, a little less than half (16) solely focused on the statewide deposit cap. The other three types of deregulation share

[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).

similar reform timings, as they are often bundled and enacted in a single deregulation act issued by the state government.[15] In terms of reform timings, 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 variations, 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 insignificant effects.

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

Table 1: Identifying Policy Variations: Interstate Branching Laws

State	Reform timings				State	Reform timings			
	Minimum age requirement (1)	*De novo* branches (2)	Acquisition of branches (3)	**Statewide deposit cap (4)**		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 timings 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 *de novo* 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 bank branching deregulation, we construct two main datasets that combine multiple administrative data sources. The first dataset focuses on the 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' math 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 contains 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, which are deflated with the US Consumer Price Index from the Bureau of Labor Statistics. The F-33 data is 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 the universal school districts. We exclude the District of Columbia and Hawaii from the analysis, as they only contain a single school district. To address potential noise in enrollment numbers, we implement a school district sampling process as in Lafortune et al. (2018). 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 of 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 RTP 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 use the restricted-use National Assessment of Educational Progress (NAEP) data from the NCES. Known as the Nation's Report Card, NAEP has conducted biennial surveys that collect comparable assessments of student achievement in various subjects since 1990.[16] The restricted-use NAEP data we exploit contains individual achievement in Math and Reading assessments for a state-level representative sample of fourth and eighth grade students. 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 Math 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

[16]Participation was voluntary for all states when the NAEP survey was launched in 1990. In 2003, all 51 states began participating in both math and reading assessments for fourth and eighth-grade students. Appendix 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. See equations in Section 4 for how treatment timing is specified.

[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 is rounded to the nearest ten per IES disclosure guidelines.

Bureau. The STC provides annual statistics on the fiscal year tax collections of all 50 state governments in the United States. We use historical data on STC 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 the tax revenues are re-scaled and deflated to a common unit (constant 2013 dollars, in millions).

Finally, to assess the heterogeneity 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] This land availability data enables 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 define a county as having an inelastic housing supply if the percentage of its developable land is less than the sample median; otherwise, the supply is said to be 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

[20]Compared to the popular measure for housing price developed in Saiz (2010), Lutz and Sand (2019) provides land availability data with more accurate measures and extensive geographic coverage of the contiguous United States.

1990, the mean household annual income in high-income districts was approximately \$20 thousand 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 revenue, highlighting the significant contribution of local property taxes in these districts. Low-income districts spend similar amounts of current expenditure 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, district-level aggregates of the NAEP assessment score in low-income districts are substantially lower (about 0.45 standard deviations) than those in 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 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 Math Assessments.

4 Empirical Strategy

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

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

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' math 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 variations in deregulation.

An associated event study specification below is used to capture treatment effects over each of the pre- and post-periods. The event study specification shares parameters with those in equation (1) but replaces the timing dummy with a set of timing indicators to allow 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 the

[21] For reform timings, 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.

deregulation.

As informed by the recent DiD literature, when having multiple groups/periods in the DiD framework, TWFE estimators would 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) among others.[22]

The stacked DiD approach is based on the following 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 run the estimations using DiD specifications on this stacked dataset, with fixed effects interacting with the dataset indicator group.

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.

[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. (2021), and Callaway and Sant'Anna (2021).

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 was also 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.

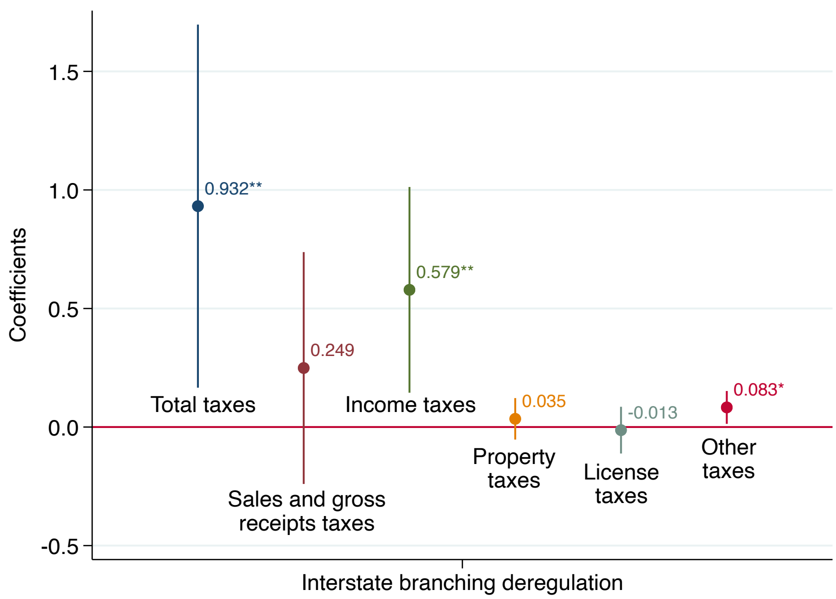
[23]However, shocks in local housing markets might not end up having impacts on the local revenue channel. In particular, the local millage rate (i.e., property tax rate) may adjust endogenously in response to market shocks on the values of local housing assets, leading to limited effects on local property 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 impacts 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 on the impact of deregulation tax revenues, which shows treated states meet a positive effect on 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 of income taxes and other taxes, and there is no effect on income taxes, property taxes, and license taxes. Relative to the mean of dependent variables (see Appendix 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. Appendix Figure B1 presents estimates of the interaction terms between deregulation and a dummy 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 variations that capture the general pattern of deregulation timings, we also test whether the other three dimensions of deregulation affect state government tax revenues. Results in Appendix 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 90% 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

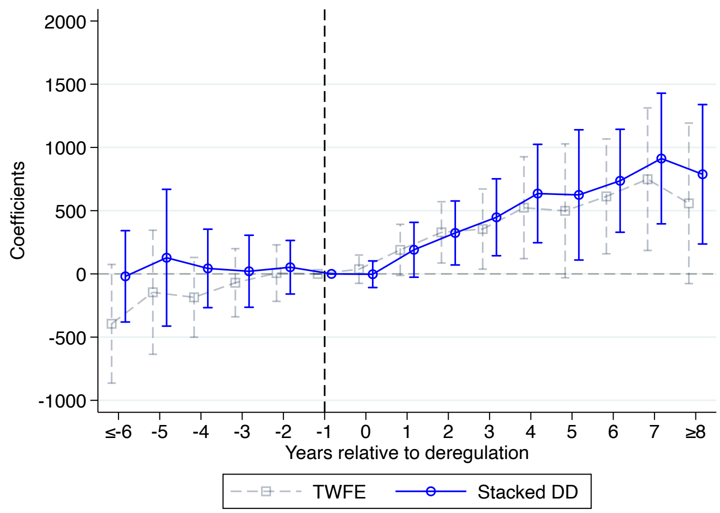
We now study the impact of interstate bank branching deregulation on school finance. Figure 2 plots the event study estimates (with a 95% confidence interval) of the impact on school finance, using both TWFE and stacked DiD estimators. Regardless of the estimation framework, we observe a sharp and persistent increase in per-pupil total revenue (left panel) and expenditure (right panel) 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.

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

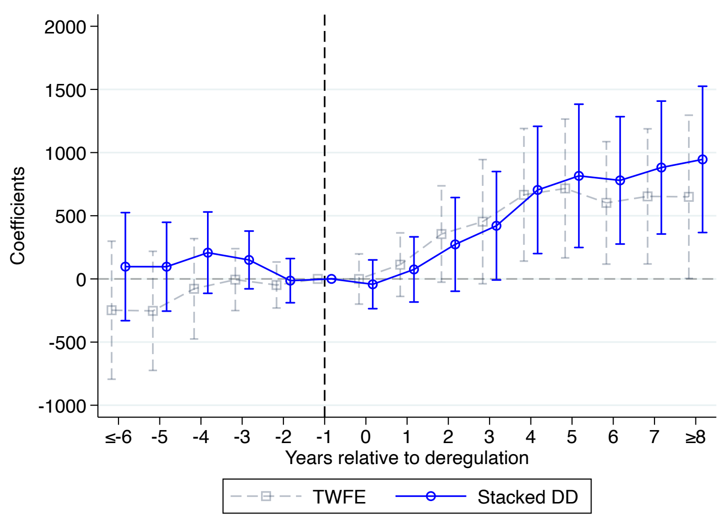
It is important to address potential issues of using TWFE estimators to conduct more accurate estimations, especially when we have dynamic treatment effects on total revenue and expenditure, as shown in Figure 2. In addition to the stacked DiD estimator, we employ a variety of dynamic DiD estimators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

When estimating the average treatment effect size, we report estimates using both specifications. Results obtained from stacked DiD estimations are in Table 3, while estimates based on TWFE estimations are in Appendix Table C2. ATT estimates generated by the two specifications show similar patterns. In terms of effect size, based on stacked DiD estimations, deregulation leads to total revenue increases of $467 on average throughout the 8-year post-reform period, which is a 4.31% increase compared to the mean of the dependent variables. Similarly, total expenditures are increased by $435 on average following the deregulation, amounting to a 3.98% increase relative to the mean of the dependent variable.

Figure 2: Event Study Figure on Total Revenue and Expenditure



(a) Total revenue



(b) Total expenditure

Notes: The figures show event study estimates of the impact of deregulation on school finance, using two-way fixed effects and stacked DiD estimators. 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.

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.[26]

For local revenue, the insignificant and negative effects are also 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 reasons. First, taxes are collected on assessed values with a considerable year lag (Lutz, 2008). Second, local policymakers often respond to shocks in assessed values by adjusting local property tax rate (Lutz et al., 2011).[27] 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 that is based on tax bases is more responsive to market fluctuations, while local revenue is more stable in response to 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 on low-income school districts. In addition, while school districts from both quintile groups have positive gains in state revenue following the deregulation, high-income school districts also receive significantly less local and federal revenues. The decreased local revenue, which could be partially due to lower millage rates on housing assets following

[26]Appendix 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 deregulation also generate null effects on total revenue and expenditure (Appendix Figure C1).

[27]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 cases in Illinois and finds that property tax relief leads to reduced property tax rates after positive shocks of wind energy installation.

deregulation, contributes to the small magnitude of total revenue in high-income districts.

To further validate that changes in housing markets did not affect school finance via the 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 in 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.[28] 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 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 in revenue effects, where deregulation primarily benefits low-income districts in terms of total revenue. The cross-district heterogeneity on total expenditure also reflects the post-1970s school funding formulas that aim at equalization in distributing school resources across school districts within a state.[29]

[28] Favara and Imbs (2015) finds deregulation leads to increased housing prices in counties with the 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, leading to reduced local revenues.

[29] One of the most widely adopted components in the school funding formula after the 1970s School Finance Reforms is the "minimum foundation plan" (MPF). For example, in 1990-1991, 37 states incorporated the MPF mode in their state aid systems (Card and Payne, 2002). In this mode, state governments first set a per-pupil spending goal to ensure all school districts within the state can reach it, which is subject to the state government budget. State governments next calculate the potential local revenue of each district, based on local factors such as the housing asset base and millage rate. Under this mode, districts with less local revenue support would receive more aid from the state government.

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 3.[30] The event study figures indicate that, school districts in the lower income quintile take up increases in total expenditure earlier, and have a larger effect on the spending in a given post-period, compared to districts in the higher income quintile.

We next decompose the expenditure effects into sub-categories. Distinguishing spending by type is important as it would be formative on student outcomes. Recent literature has recognized the significance of instruction-relevant spending in improving human capital, while indicating a more limited influence of non-instructional spending, such as capital outlays.[31] When decomposing the expenditure effects in low-income school districts, the boom in total expenditure increases school spending in both total current instructional/non-instructional expenditure and capital outlays. Average teacher salaries (and benefits) also rise post-deregulation. For high-income school districts, the null effect in total revenue leads to no significant spending expansions in all the sub-categories.

The increases in current instructional expenditure and teacher salaries (including benefits) in low-income districts are relevant to enhanced educational inputs, such as reduced class sizes, which might be linked to improvements in student academic achievement. This also highlights a virtue of our policy variations. As Brunner et al. (2022) discuss, shocks to local revenues put additional constraints on spending, which leads to larger spending on capital outlays but limited improvement in student achievement. Unlike the shock on local revenue, we find that deregulation-induced shocks to state revenue lead to increased school spending on instructional inputs, which could generate gains in student achievement.

[30]The corresponding event study estimates of the impact of deregulation on per-pupil state aid by quintile are in Appendix Figure C3, which share a similar pattern with event study estimates on total expenditure by quintile (Figure 3).

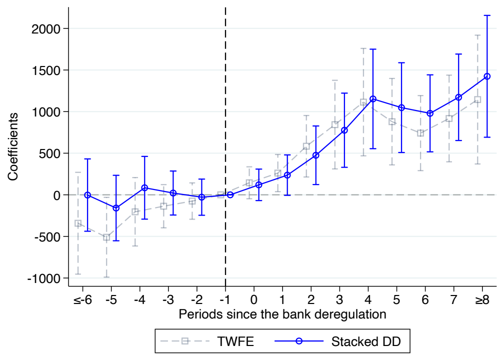
[31]For example, as discussed in a recent study by Baron (2022), which exploits evidence from the referendum-induced revenue changes in Wisconsin, increases in operational spending substantially improve educational outcomes, while capital expenditures generate limited impacts.

Table 3: Impacts of Branching Deregulation on School Finance: Stacked DiD

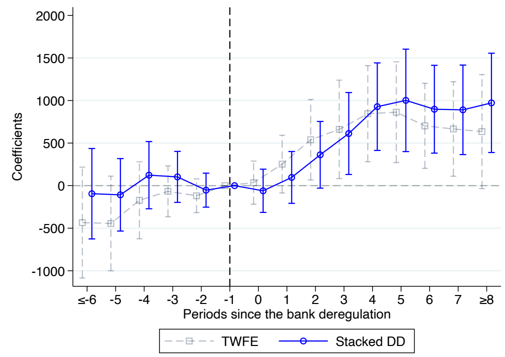
	Mean of dep var	Stacked DiD estimate	District income		Land availability	
			Q1	Q5	Inelastic	Elastic
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Revenue effects						
Total revenue	10,844	467***	849***	175	425**	473***
		(154)	(174)	(181)	(188)	(153)
State revenue	5,117	802***	976***	855**	960***	789**
		(257)	(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 Appendix Table C2. Standard errors clustered at the state level are in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

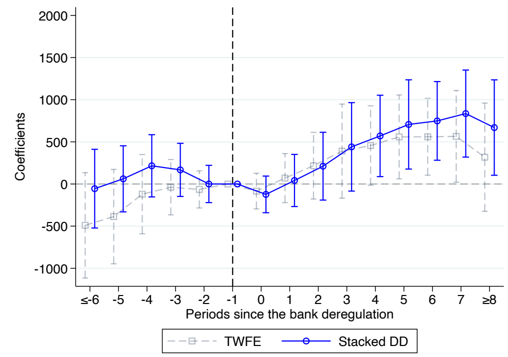
Figure 3: Event Study Figure on Expenditure Effects by Quintile



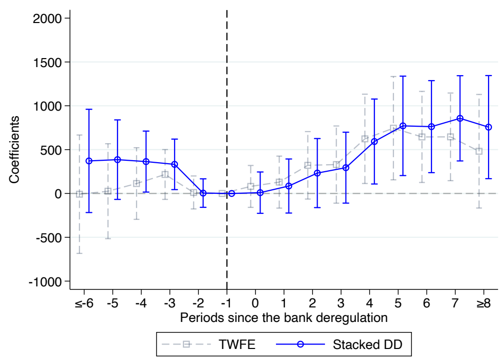
(a) First quintile



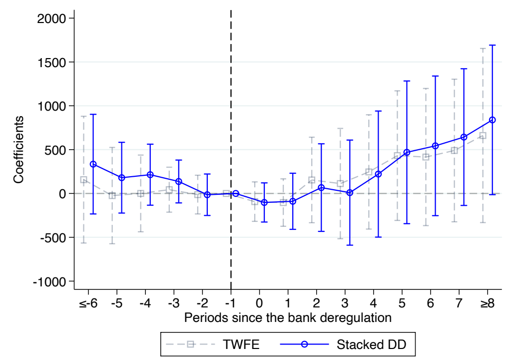
(b) Second quintile



(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.

We also look at whether the impact of deregulation would vary for school districts with different housing supply elasticities. Columns (5) and (6) of Panel B show that there are no systematical differences across various spending categories between inelastic and elastic counties.

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 reducing class size. 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 findings in Panel B indicate.

5.3 Robustness

We begin the robustness assessment by focusing on the average effect sizes of estimates obtained from different estimators. Figure 4 displays the benchmark estimates in black in the first two rows, which are close to each other. In addition to the Stacked DiD, the third row presents estimates using the "imputation" estimation of Borusyak et al. (2021). 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 estimator.

We next investigate whether potential confounding events would affect interpretations of our baseline results. One concurrent event (from the finance literature) is the interstate branching deregulation in the other three dimensions. As Table 1 shows, states adopt deregulation in dimensions other than the statewide deposit cap during our study period. Results in the fourth row of Figure 4 indicate that the benchmark results remain robust after controlling for these deregulations. Although the impact on per-pupil total expenditure becomes insignificant, the estimate largely overlaps with the benchmark estimate. Table B2 column (2) provides estimates of the impact of each deregulation, showing consistent evidence that none of the other three dimensions generate significant effects on school finance, in the specification that controls for all four dimensions of deregulation simultaneously.

The education literature suggests another potential confounding event could be the post-1990 School Finance Reforms that sought to provide adequate funding to low-income school districts. The literature has documented the impact of SFRs on school finance allocation within the state (e.g., Jackson et al. (2016) and Lafortune et al. (2018)). During the 1990s, states adopted SFRs in a staggered way, which, as a simultaneous event, could confound our baseline results. Importantly, the reform timings of deregulation largely differ from those of SFRs, as Appendix Table B1 shows.[32] The observation that there are no systematic overlaying patterns between the timings of the two policies is not surprising, as they are distinct reforms from different domains.

To formally assess the concern, we re-run the baseline regressions while controlling for the introduction of the SFRs. The results remain robust after accounting for the impact of SFRs, as indicated in the fifth row of Figure 4 (or Appendix Table B2 column 3). In addition, since deregulation increases both total revenues and expenditures, and post-SFRs reallocate adequate resources to lower-income school districts, in Appendix Table B2 columns (4) and (5) we further test if there are complementary relationships between the two reforms by looking at the impact of their interaction terms. Column (4) reveals that not only did low-income districts receive increased total revenue and expenditure, but low-income districts in deregulated states that also implemented SFRs had even greater total revenue and expenditure effects. In contrast, high-income districts did not experience any significant impacts on total revenue and expenditure (Column 5).

To further validate that SFRs were not a potential confounding concern, we conduct an additional sub-sample analysis. The sixth row of Figure 4 (or Appendix Table B2 column

[32]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.

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 School Finance Reforms.

As a final robustness exercise, we re-estimate the impact of deregulation on total revenue 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 simple: We compare contiguous counties on opposite sides of the state border that share similar characteristics, where one county is from a treated state, and the other county is not.

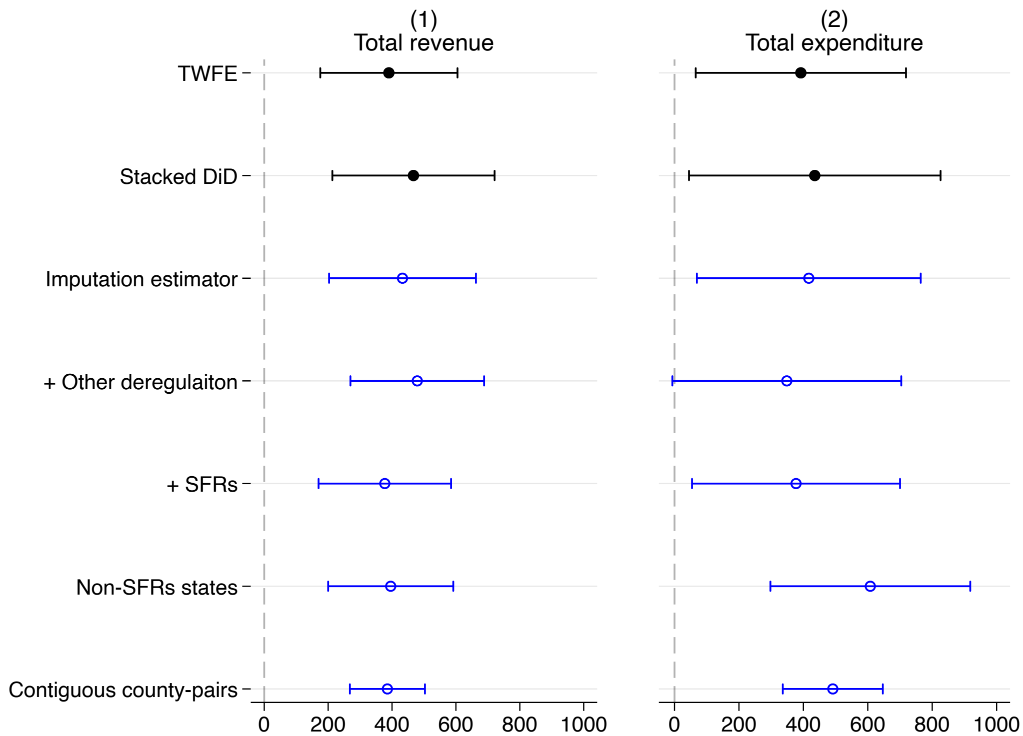
Formally, in the specification, we use pair-specific time effects (δ_{pt}) with school district fixed effects (δ_i), which exploit variation in deregulation among de-meaned districts within each contiguous county-pair.[33] The last row of Figure 4 presents results based on contiguous county-pairs. Results are robust under the contiguous county-pairs design; indeed, the design dramatically improves the estimation precision, highlighting the virtue of the design.

[33] 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.

Figure 4: 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. (2021), adding the other three deregulation timings 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. To investigate the educational consequences, we now examine the impact of deregulation on student achievement.

6.1 Overall and Distributional Effects: Reduced-Form Estimates

Overall Effects.— We first look at the overall effect of interstate branching deregulation on student achievement. Figure 5 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) re-runs the estimation at individual-level with a set of student controls: dummies indicating if the student is female, black, needs an individualized education plan (IEP), or has limited English proficiency (LEP).

Both estimations present flat and insignificant pre-trends, supporting the DiD assumption. In addition, post-periods in both subfigures indicate that school districts in reformed states begin having significant increases in student achievement in the second post-period (i.e., third and fourth years), when per-pupil total expenditure gets significant increases after deregulation (as in Figure 2b). As Figure 5b shows, adding student controls enhances the similarity between the treatment and control group and improves the estimation precision, leading to significantly lasting improvements in student achievement over the remaining eight-year post-period window.

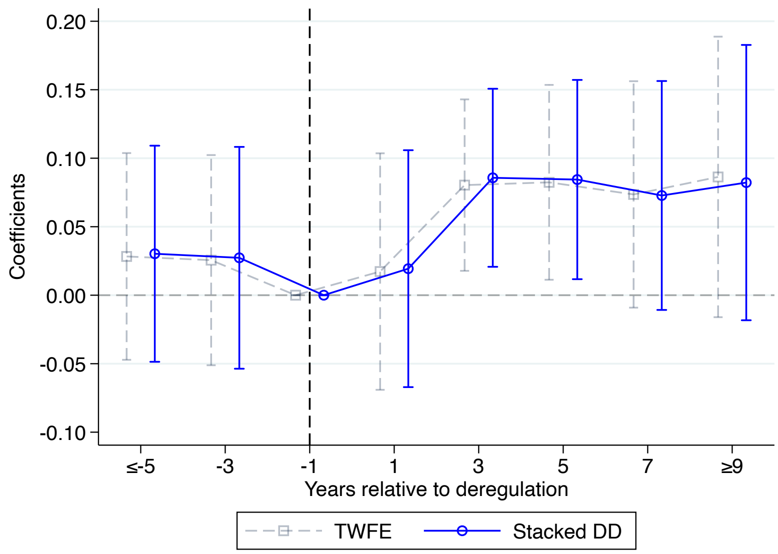
Table 4 Panel A presents reduced-form estimates of the overall effect. The district-level analysis compares the district-level aggregates of NAEP scores between treated and control states both before and after deregulation. Both TWFE and stacked DiD estimates indicate improved student achievement following deregulation, although the effect size does not differ significantly from zero. We then re-run the estimations at the individual level, obtaining estimates that closely resemble those obtained from the district-level analysis.[34] Similar to the event study analysis in Figure 5, we then re-estimate the individual-level analysis adding student controls. Conditional on these student characteristics, the stacked DiD estimation shows that deregulation leads to an improvement in student achievement of 6.4% of a standard deviation.[35]

We validate the robustness of reduced-form estimates of impacts on student achievement through a host of validity checks, as Append Figure B4 displays. In Appendix Figure B4, the first three rows 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, and using non-SFRs states as the estimation sample. Results remain robust with these additional adjustments, indicating that the overall impact of deregulation on student achievement was not driven by other potential confounding events or factors.

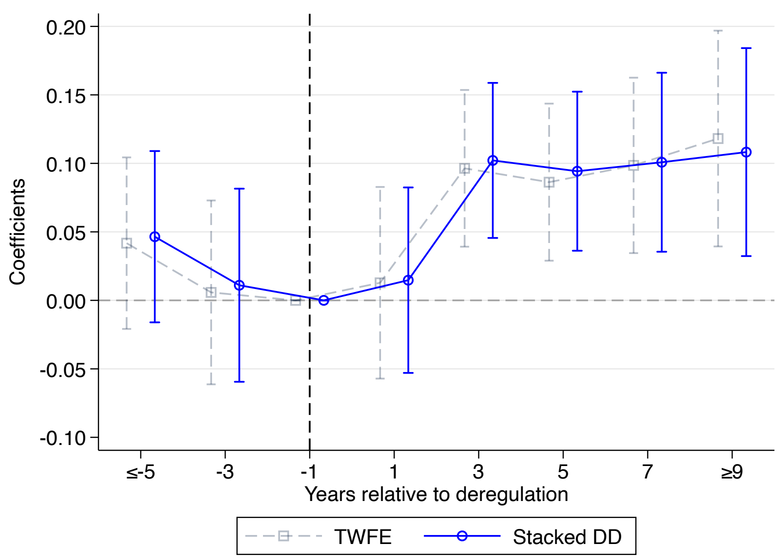
[34]The sample in the individual-level analysis (N ≈ 1,143,920, rounded to the nearest ten per IES disclosure guidelines) is obtained from disaggregating the sample in the district-level analysis (N ≈ 26,560).

[35]Appendix Figure C4 presents event study estimates of the impact on student achievement in school districts of different income quintiles. Unlike the pattern found for expenditure effects in Figure 3, where lower-income school districts experience higher per-pupil total expenditure, Appendix Figure C4 shows that there are no such differential effects on student achievement across school districts. This finding may not be surprising, as deregulation, unlike School Finance Reforms, is not a resource-equalization reform. In addition, deregulation can influence student achievement through channels beyond school finance. For instance, Hu et al. (2020) find that interstate branching deregulation reduced the academic performance of children from low-income families, and they suggest that low-income parents substitute out of childrearing and into employment as one potential mechanism.

Figure 5: 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 and stacked DiD estimators. The dependent variables are standardized district-level aggregates of student achievement in grades 4 and 8 NAEP math 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.
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 Math 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 in the lower part of the distribution (Beck et al., 2010). However, whether banking deregulation reduces inequality in educational outcomes remains an open question.

We follow Lafortune et al. (2018) and examine the heterogeneous effect on achievement across student groups by their ability, race, and free lunch status. 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 aggregated to the grade-state-year level, weighted by NAEP individual weights. We also 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.

The heterogeneity analyses are in Table 4 Panel B. Stacked DiD estimates in Column (3) 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 demonstrate significantly improved test scores after deregulation. However, none of the gaps are statistically different from zero, indicating that deregulation does not lead to distributional effects across student groups by ability, race, or free lunch status.

Table 4: 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 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 Math Assessments.

6.2 Spending Effects: An Instrumental Variable Approach

The reduced-form estimates capture the overall effects of interstate branching deregulation on student achievement, which could operate through channels other than school spending. For example, deregulation could also affect student achievement through household human capital investment changes when families get rid of credit constraints (Sun and Yannelis, 2016) or experience a positive shock on their housing values (Favara and Imbs, 2015). To estimate the spending effect that operates between deregulation and student achievement solely through school finance, while isolating other channels, we propose a dosage-based IV approach, in the spirit of Jackson et al. (2016, 2021).

The intuition underlying the dosage-based IV approach is simple: Figure 3 shows that low-income school districts in treated states have greater increases in school spending after the deregulation than their higher-income counterparts. Specifically, Figure 3 indicates that school districts in the first to fourth income quintiles (Q1-Q4) have prominent increases in school spending following deregulation, while districts in the fifth quintile (Q5) do not. Based on this pattern, we construct a group indicator I_{gs} that equals one for school districts in Q1-Q4, and zero for school districts in Q5. Therefore, the extra spending between districts in Q1-Q4 and Q5 consists of the increased dosage that could be used to construct the instrumental variables for spending effects.

However, the sharp and permanent change in spending may not perform well to capture the effect, given the idea that achievement is cumulative – the spending effect should enhance scores gradually as students are longer exposed (Lafortune et al., 2018). Following Jackson et al. (2021), we rely on a more precise layer of the additional spending as the dosage: the gradual spending increases over the post-period time trends in Q1-Q4 and Q5 districts (i.e., two excluded instrumental variables), while accounting for the overall spending shift during the entire post period. The dosage design essentially exploits the difference in the slope of the spending change across Q1-Q4 and Q5 school districts, rather than directly comparing an average change in the level of spending across the district group.

Formally, we have the following specifications.

$$\begin{aligned} Expenditure_{ist} = & \sum_{g=\{Q1-Q4,Q5\}} [\pi_{1g} \cdot (Deregulation_{st} \times I_{gs} \times (T - T_s^*))] \\ & + \sum_{g=\{Q1-Q4,Q5\}} [\phi_{1g} \cdot (Deregulation_{st} \times I_{gs})] + \theta_{1t} + \alpha_{1i} + \varepsilon_{1ist}, \end{aligned} \tag{5}$$

$$\begin{aligned} Y_{ist} = & \beta \cdot (Expenditure_{st}) + \sum_{g=\{Q1-Q4,Q5\}} [\phi_{2g} \cdot (Deregulation_{st} \times I_{gs})] \\ & + \theta_{2t} + \alpha_{2i} + \varepsilon_{2ist}, \end{aligned} \tag{6}$$

where equations (5) and (6) are specifications for the first- and second-stage of the IV estimation, respectively. $Expenditure_{ist}$ in the first stage is the per-pupil total expenditure in school district i, state s, and year t, while Y_{ist} in the second stage is the district-level aggregates of student achievement. $Deregulation_{st}$ is an indicator for deregulation, which equals one if the state s experiences deregulation and zero otherwise, which is the same as the one in baseline specifications. T is the calendar year and T_s^* is the deregulation timings for statewide deposit cap as listed in Table 1. θ_{1t} are the school-district fixed effects, and α_{1i} are the year fixed effects. Standard errors are clustered at the school district level.

The IV design implicitly assumes marginal dollar has the same impact across various districts, while exploiting the difference in the slope of the spending change across districts as the dosage. The exclusion restriction is that the instrumental variables affect student achievement only through the difference in slope of increased spending across districts (specifically, Q1-Q4 vs Q5) during the post-period, and not through other inter-district differences in resources.

One remaining issue is that the above IV specification relies on the TWFE estimator in its first-stage estimations. To improve the estimations and address dynamic treatment issues, we replace the first-stage TWFE estimator with the stacked DiD estimator. Since there are no readily available analytical methods for two-stage least squares (2SLS) estimations that incorporate a dynamic DID estimator in the first stage, we adopt the control function approach for the IV two-step estimation (Wooldridge, 2015). The control function approach shares the statistical properties of the 2SLS, but it is more flexible to adjust.

Specifically, in our adjusted IV specification, we adopt stacked DiD estimators in the first stage and obtain the residuals; in the second stage, we run an OLS estimation with the first-stage residuals as an additional regressor. In the end, we compute bootstrapped standard errors using 1,000 replications. Formally, we have the following adjusted IV specifications, where the first stage features an equation similar to equation (5) but in the stacked DiD framework:

$$\begin{aligned} Expenditure_{isjt} = & \sum_{g=\{Q1-Q4,Q5\}} [\pi_{1g} \cdot (Deregulation_{st} \times I_{gs} \times (T - T_s^*))] \\ & + \sum_{g=\{Q1-Q4,Q5\}} [\phi_{1g} \cdot (Deregulation_{st} \times I_{gs})] + \theta_{1jt} + \alpha_{1ij} + \varepsilon_{1isjt}. \end{aligned} \tag{7}$$

In the second-stage, instead of having equation (6), we have:

$$\begin{aligned} Y_{isjt} = & \beta \cdot (Expenditure_{st}) + \sum_{g=\{Q1-Q4,Q5\}} [\phi_{2g} \cdot (Deregulation_{st} \times I_{gs})] \\ & + \theta_{2jt} + \alpha_{2ij} + \rho \times \hat{\varepsilon}_{1isjt} + \varepsilon_{2ijst}, \end{aligned} \tag{8}$$

where $\hat{\varepsilon}_{1isjt}$ are the residuals from the first-stage estimation. Equation (8) is a stacked DiD version of equation (6) with $\hat{\varepsilon}_{1isjt}$ taken into account.

Table 5 presents IV estimates of spending effects on student achievement. Panel A shows results using traditional TWFE estimators and 2SLS in the IV estimations, while Panel B shows estimates using stacked DiD and control function approach. Results in the two panels are similar. When looking at the first-stage F-statistics in column (1), while both pass the weak IV test, the stacked DiD estimators improve the goodness-of-fit relative to that estimated by the TWFE estimators (F-statistic of 18.43 versus 15.03).

We interpret the findings using results in Table 5 Panel B. Column (1) presents first-stage outcomes, which indicate that lower-income school districts (I_{Q1-Q4}) on average undertake

119 more dollars in per-pupil total spending among their post-period time trends, relative to highest-income (I_{Q5}) districts. The estimates of the instrumental variable on spending increases for Q5 districts are positive but economically small (9 dollars) and statistically insignificant. Column (3) shows the reduced-form estimates for the relationship between average NAEP scores and deregulation that are across the school district group and post-period time trend. The results indicate that deregulation on average leads to a 0.008 standard deviation increase in the lower-income districts group (I_{Q1-Q4}), while a 0.005 standard deviation increase in high-income districts group (I_{Q5}), over each of the five post periods, corresponding to a cumulative total improvement of 0.065 standard deviations.

Column (4) gives the IV estimates of school spending effects on student achievement, where the control function approach estimates that a $1 thousand increase in spending leads to a 0.035 standard deviation improvement (about 1 percentile points change) on the district-level average of student achievement.[36] This estimate is close to the spending effect estimate found in related recent literature (e.g., Jackson and Mackevicius (2021)).

We close the section with a concluding remark on the IV approach that we develop to estimate the spending effect, which incorporates dynamic DiD estimators in the first stage. The adjusted IV approach is simple yet could be very useful. It is especially true in the education literature in which studies heavily rely on staggered roll-outs of SFRs as identifying variations to understand the role of school spending on human capital (Jackson, 2018). Broadly speaking, our proposed approach can be easily adopted by researchers to improve their estimations in settings that exploit staggered-type variations (e.g., SFRs) to estimate effects of a specific channel (e.g., spending effects) using an IV framework.

[36] Columns (2) and (5) present the results of the first- and second-stage estimations that control for school district-level linear time trends (interacted with stack fixed effects). By adding the district linear time trends, the estimation imposes restrictions that further absorb factors varying along with district-specific linear time trends. While controlling for district time trends leads to a higher F-statistic in the first stage (19.19), the exercise does not improve the precision of IV estimates in the second stage (6.1% standard deviations). Therefore, we choose the specification without district time trend controls as our preferred one for the IV estimation.

Table 5: Spending Effects on Student Achievement: IV Approach

	First stage		Reduced form	Second stage	
	Per-pupil spending (thousands)		Average NAEP	Average NAEP	
	(1)	(2)	(3)	(4)	(5)
Panel A. TWFE + 2SLS					
Deregulation $\times (I_{Q5})$	0.199	0.074	-0.028		
	(0.127)	(0.168)	(0.017)		
Deregulation $\times (I_{Q1-Q4})$	0.141	-0.057	0.009		
	(0.117)	(0.100)	(0.013)		
Deregulation $\times (I_{Q5}) \times (T - T_s^*)$	-0.026	0.094**	0.009***		
	(0.018)	(0.045)	(0.002)		
Deregulation $\times (I_{Q1-Q4}) \times (T - T_s^*)$	0.084***	0.250***	0.006***		
	(0.017)	(0.041)	(0.002)		
Per-pupil spending (thousands)				0.033*	0.053**
				(0.019)	(0.026)
Kleibergen-Paap Wald F-statistic	15.03	18.29	10.92	15.03	18.29
Panel B. stacked DiD + CF					
Deregulation $\times (I_{Q5})$	-0.036	0.105	-0.018		
	(0.132)	(0.195)	(0.018)		
Deregulation $\times (I_{Q1-Q4})$	-0.092	-0.024	0.018		
	(0.133)	(0.130)	(0.014)		
Deregulation $\times (I_{Q5}) \times (T - T_s^*)$	0.009	0.111**	0.008***		
	(0.019)	(0.049)	(0.002)		
Deregulation $\times (I_{Q1-Q4}) \times (T - T_s^*)$	0.119***	0.268***	0.005***		
	(0.020)	(0.043)	(0.002)		
Per-pupil spending (thousands)				0.035**	0.061***
				(0.016)	(0.021)
Kleibergen-Paap Wald F-statistic	18.43	19.19	6.78	–	–
(Stack-)Grade FE	✓	✓	✓	✓	✓
(Stack-)District FE	✓	✓	✓	✓	✓
(Stack-)Year FE	✓	✓	✓	✓	✓
(Stack-)District trends		✓			✓
Observations	23,810	23,810	23,810	23,810	23,810

Notes: Panel A (B) uses TWFE (stacked DiD) estimators in the first stage and the 2SLS (control function) method in the two-step estimation. The first-stage models (columns 1-2) regress per-pupil spending (in 2013 dollars) on the exogenous instruments and controls. The reduced-form results (column 3) regress student achievement on the same exogenous instruments and controls. The 2SLS results (columns 4-5) regress student achievement on the same exogenous instruments and controls, with per-pupil spending as the endogenous variable. Specification details are in equations (5) and (6). All models in Panel A (B) control for (stack-)grade fixed effects, (stack-)district fixed effects, and (stack-)year fixed effects. District trends indicate district-level linear time trends. Standard errors (bootstrapped with 1,000 replications) clustered at the district level are in parentheses in Panel A (B). Observations indicate the size of the estimation sample in Panel A. The number of observations from NAEP data is rounded to the nearest ten per IES disclosure guidelines. Standard errors clustered at the district 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 Math Assessments.

7 Conclusion

We examine how school spending impacts student achievement using the US interstate branching deregulation as a source of state tax revenue shocks. By leveraging the staggered implementation of deregulation across states, we find that deregulation raises per-pupil total revenue and expenditure of treated school districts. Our further investigation indicates improved student achievement following deregulation, with no distributional effects evident by students' ability, race, or free lunch status. We propose a dosage-based IV approach that incorporates dynamic treatment effects, which estimates the spending effect that a \$1 thousand increase in per-pupil spending yields a 0.035 standard deviation average improvement in student achievement.

This study advances discussions on the impact of school spending on student achievement with a new nationwide analysis in the context of the US interstate branching deregulation. Through the analysis of school finance of universal US school districts, this paper provides the first causal evidence of the impact of financial deregulation on K-12 public school finance. Using restricted-use student assessments from the Nation's Report Card, this paper offers a comprehensive investigation of the overall and distributional impacts of financial deregulation on educational outcomes. The study proposes an IV approach that incorporates dynamic treatment effects, which can be easily adopted in settings that exploit staggered-type variations in the first stage within an IV framework. This adjusted approach is especially important when estimating the spending effect, given that most studies in the literature rely on staggered roll-outs of School Finance Reforms as identifying variations.

Looking ahead, future research is encouraged to explore longer-term outcomes beyond test scores, including high school completion, college graduation, and labor market wages. Previous research has documented the long-term effects of school spending increases resulting from School Finance Reforms, and similar investigations could provide insights in the context of financial deregulation.

References

Aizer, Anna, Shari Eli, Joseph Ferrie, and Adriana Lleras-Muney, "The long-run impact of cash transfers to poor families," *American Economic Review*, 2016, *106* (4), 935–971.

Bailey, Marthaj, Hilary Hoynes, Maya Rossin-Slater, and Reed Walker, "Is the Social Safety Net a Long-Term Investment? Large-Scale Evidence From the Food Stamps Program," *The Review of Economic Studies*, 2023.

Baron, E Jason, "School spending and student outcomes: Evidence from revenue limit elections in Wisconsin," *American Economic Journal: Economic Policy*, 2022, *14* (1), 1–39.

Beck, Thorsten, Ross Levine, and Alexey Levkov, "Big bad banks? The winners and losers from bank deregulation in the United States," *The Journal of Finance*, 2010, *65* (5), 1637–1667.

Becker, Gary S, "An economic analysis of fertility," in "Demographic and economic change in developed countries," Columbia University Press, 1960, pp. 209–240.

_ , *Human capital: A theoretical and empirical analysis, with special reference to education*, University of Chicago press, 2009.

Biasi, Barbara, "School finance equalization increases intergenerational mobility," *Journal of Labor Economics*, 2023, *41* (1), 1–38.

Borusyak, Kirill, Xavier Jaravel, and Jann Spiess, "Revisiting event study designs: Robust and efficient estimation," *arXiv preprint arXiv:2108.12419*, 2021.

Brunner, Eric, Ben Hoen, and Joshua Hyman, "School district revenue shocks, resource allocations, and student achievement: Evidence from the universe of US wind energy installations," *Journal of Public Economics*, 2022, *206*, 104586.

Callaway, Brantly and Pedro H.C. Sant'Anna, "Difference-in-differences with multiple time periods," *Journal of Econometrics*, 2021, *225* (2), 200–230.

Card, David and A. Abigail Payne, "School finance reform, the distribution of school spending, and the distribution of student test scores," *Journal of Public Economics*, 2002, *83* (1), 49–82.

Célerier, Claire and Adrien Matray, "Bank-branch supply, financial inclusion, and wealth accumulation," *The Review of Financial Studies*, 2019, *32* (12), 4767–4809.

Célérier, Claire and Purnoor Tak, "Finance, Advertising, and Fraud: The Rise and Fall of the Freedman's Savings Bank," Technical Report, Working Paper. University of Toronto, 2023.

Cengiz, Doruk, Arindrajit Dube, Attila Lindner, and Ben Zipperer, "The effect of minimum wages on low-wage jobs," *The Quarterly Journal of Economics*, 2019, *134* (3), 1405–1454.

Chava, Sudheer, Alexander Oettl, Ajay Subramanian, and Krishnamurthy V Subramanian, "Banking deregulation and innovation," *Journal of Financial Economics*, 2013, *109* (3), 759–774.

Corcoran, Sean P and William N Evans, "Equity, adequacy, and the evolving state role in education finance," *Handbook of Research in Education Finance and Policy*, 2015, pp. 353–371.

Dahl, Gordon B and Lance Lochner, "The impact of family income on child achievement: Evidence from the earned income tax credit," *American Economic Review*, 2012, *102* (5), 1927–1956.

de Chaisemartin, Clément and Xavier D'Haultfoeuille, "Two-way fixed effects estimators with heterogeneous treatment effects," *American Economic Review*, 2020, *110* (9), 2964–96.

Deshpande, Manasi and Yue Li, "Who is screened out? Application costs and the targeting of disability programs," *American Economic Journal: Economic Policy*, 2019, *11* (4), 213–248.

Dube, Arindrajit, T William Lester, and Michael Reich, "Minimum wage effects across state borders: Estimates using contiguous counties," *The Review of Economics and Statistics*, 2010, *92* (4), 945–964.

Favara, Giovanni and Jean Imbs, "Credit supply and the price of housing," *American Economic Review*, 2015, *105* (3), 958–92.

Handel, Danielle V and Eric A Hanushek, "US School Finance: Resources and Outcomes," Technical Report, NBER Working Paper. 30769. 2022.

Hoxby, Caroline M, "All school finance equalizations are not created equal," *The Quarterly Journal of Economics*, 2001, *116* (4), 1189–1231.

Hu, Qing, Ross Levine, Chen Lin, and Mingzhu Tai, "Finance and Children's Academic Performance," Technical Report, NBER Working Paper. 26678. 2020.

Huang, Rocco R, "Evaluating the real effect of bank branching deregulation: Comparing contiguous counties across US state borders," *Journal of Financial Economics*, 2008, *87* (3), 678–705.

Jackson, C Kirabo, "Does school spending matter? The new literature on an old question," Technical Report, NBER Working Paper. 25368. 2018.

_ **and Claire Mackevicius**, "The distribution of school spending impacts," Technical Report, NBER Working Paper. 28517. 2021.

— , **Cora Wigger, and Heyu Xiong**, "Do school spending cuts matter? Evidence from the Great Recession," *American Economic Journal: Economic Policy*, 2021, *13* (2), 304–35.

— , **Rucker C Johnson, and Claudia Persico**, "The effects of school spending on educational and economic outcomes: Evidence from school finance reforms," *The Quarterly Journal of Economics*, 2016, *157*, 218.

Johnson, Christian A and Tara Rice, "Assessing a Decade of Interstate Bank Branching," *Washington and Lee Law Review*, 2008, *65* (1), 73.

Kerr, William R and Ramana Nanda, "Democratizing entry: Banking deregulations, financing constraints, and entrepreneurship," *Journal of Financial Economics*, 2009, *94* (1), 124–149.

Krishnan, Karthik, Debarshi K Nandy, and Manju Puri, "Does financing spur small business productivity? Evidence from a natural experiment," *The Review of Financial Studies*, 2015, *28* (6), 1768–1809.

Kroszner, Randall S and Philip E Strahan, "What drives deregulation? Economics and politics of the relaxation of bank branching restrictions," *The Quarterly Journal of Economics*, 1999, *114* (4), 1437–1467.

— **and** — , "Regulation and deregulation of the US banking industry: Causes, consequences, and implications for the future," in "Economic regulation and its reform: what have we learned?," University of Chicago Press, 2014, pp. 485–543.

Lafortune, Julien, Jesse Rothstein, and Diane Whitmore Schanzenbach, "School finance reform and the distribution of student achievement," *American Economic Journal: Applied Economics*, 2018, *10* (2), 1–26.

Lutz, Byron F, "The connection between house price appreciation and property tax revenues," *National Tax Journal*, 2008, *61* (3), 555–572.

Lutz, Byron, Raven Molloy, and Hui Shan, "The housing crisis and state and local government tax revenue: Five channels," *Regional Science and Urban Economics*, 2011, *41* (4), 306–319.

Lutz, Chandler and Ben Sand, "Highly disaggregated land unavailability," *Available at SSRN 3478900*, 2019.

Murray, Sheila E, William N Evans, and Robert M Schwab, "Education-finance reform and the distribution of education resources," *American Economic Review*, 1998, pp. 789–812.

NCES, "NCES 2022 digest table 235.10. Revenues for public elementary and secondary schools, by source of funds: Selected school years, 1919-20 through 2019-20," https://nces.ed.gov/programs/digest/d22/tables/dt22_235.10.asp, 2022.

OECD, "Education at a Glance 2022: OECD Indicators, OECD Publishing, Paris," https://doi.org/10.1787/3197152b-en, 2022.

Rice, Tara and Philip E Strahan, "Does credit competition affect small-firm finance?," *The Journal of Finance*, 2010, *65* (3), 861–889.

Rothstein, Jesse and Diane Whitmore Schanzenbach, "Does money still matter? Attainment and earnings effects of post-1990 school finance reforms," *Journal of Labor Economics*, 2022, *40* (S1), S141–S178.

Saiz, Albert, "The geographic determinants of housing supply," *The Quarterly Journal of Economics*, 2010, *125* (3), 1253–1296.

Sims, David P, "Lifting all boats? Finance litigation, education resources, and student needs in the post-Rose era," *Education Finance and Policy*, 2011, *6* (4), 455–485.

Stein, Luke CD and Constantine Yannelis, "Financial inclusion, human capital, and wealth accumulation: Evidence from the Freedman's Savings Bank," *The Review of Financial Studies*, 2020, *33* (11), 5333–5377.

Strahan, Philip E et al., "The real effects of US banking deregulation," *Review-Federal Reserve Bank Of Saint Louis*, 2003, *85* (4), 111–128.

Sun, Liyang and Sarah Abraham, "Estimating dynamic treatment effects in event studies with heterogeneous treatment effects," *Journal of Econometrics*, 2021, *225* (2), 175–199.

Sun, Stephen Teng and Constantine Yannelis, "Credit constraints and demand for higher education: Evidence from financial deregulation," *The Review of Economics and Statistics*, 2016, *98* (1), 12–24.

Sylla, Richard, John B Legler, and John J Wallis, "Banks and state public finance in the new republic: The United States, 1790–1860," *The Journal of Economic History*, 1987, *47* (2), 391–403.

U.S. Census Bureau, "2020 Annual Surveys of State and Local Government Finances.," https://www.census.gov/data/datasets/2020/econ/local/public-use-datasets.html, 2020.

Wallis, John Joseph, "American government finance in the long run: 1790 to 1990," *Journal of Economic Perspectives*, 2000, *14* (1), 61–82.

Wooldridge, Jeffrey M, "Control function methods in applied econometrics," *Journal of Human Resources*, 2015, *50* (2), 420–445.

Yang, Xi, "More Credit, Fewer Babies? Bank Credit Expansion, House Price, and Fertility," *Unpublished Manuscript*, 2023.

Online Appendix to:

"Financial Deregulation, School Finance, and Student Achievement"

Xi Yang **Jian Zou**

October, 2023

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 is 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 (RTP) 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 Math 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 Appendix 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 is available at https://github.com/ChandlerLutz/LandUnavailabilityData.

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

	Subjects and grades				Number of
Year	Math G4	Math 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 math 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

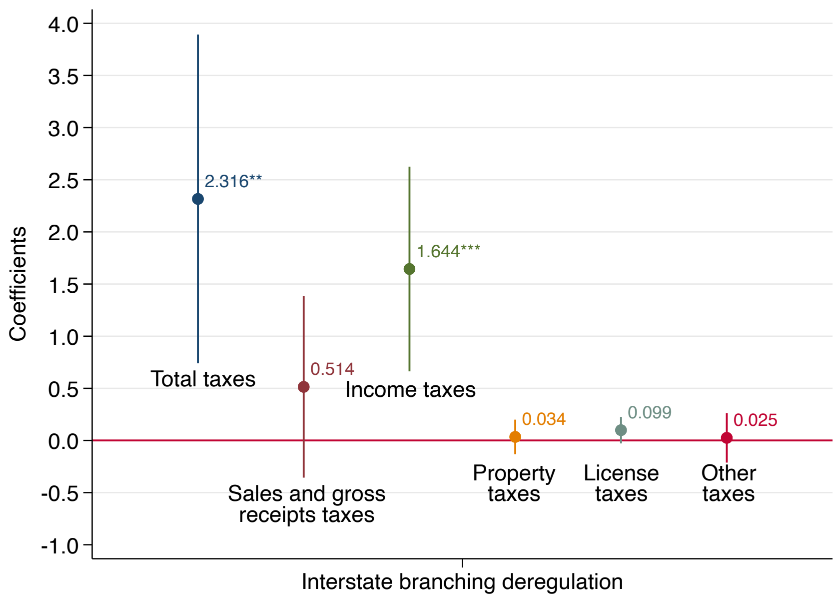
School Finance.— Our process for constructing our estimation sample of school districts is based on that of Lafortune et al. (2018). We construct the district-by-year data beginning with F-33 data between 1992 and 2005. We drop districts with missing a county fips code or LEA id, as well as districts 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 Math and Reading, we employ Math 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.

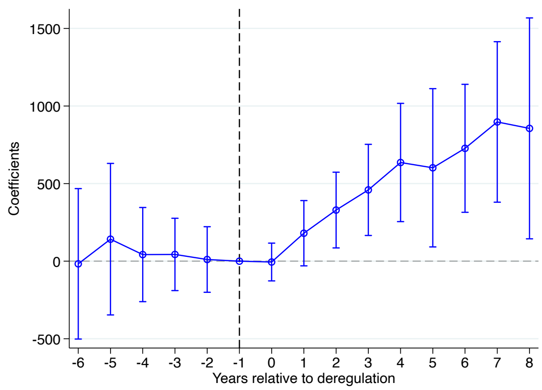
B Robustness Checks

Figure B1: Robustness: Impacts on State Government Tax Revenues

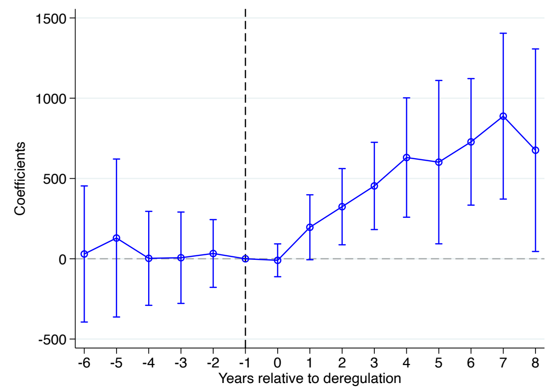


Notes: The dependent variables are state government state tax revenues on total taxes, as well as revenues in five sub-categories: sales and gross receipts taxes, income taxes, property taxes, license taxes, and other taxes. The coefficients are obtained from interaction terms between the deregulation (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. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

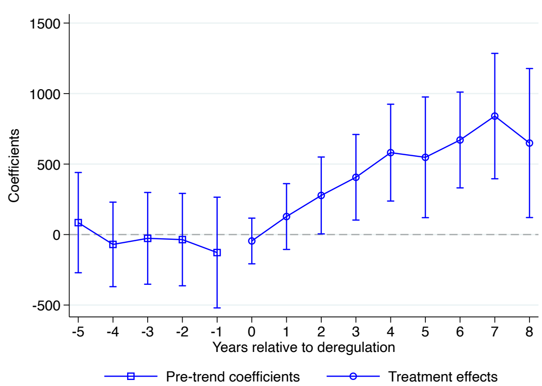
Figure B2: Revenue Effects: Heterogeneous DiD Treatment Effects



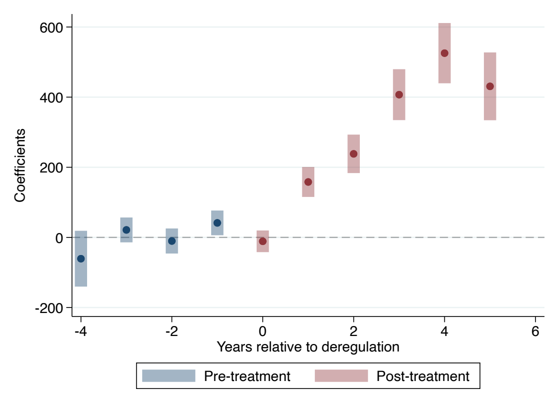
(a) De Chaisemartin and D'Haultfoeuille (2020)



(b) Sun and Abraham (2021)



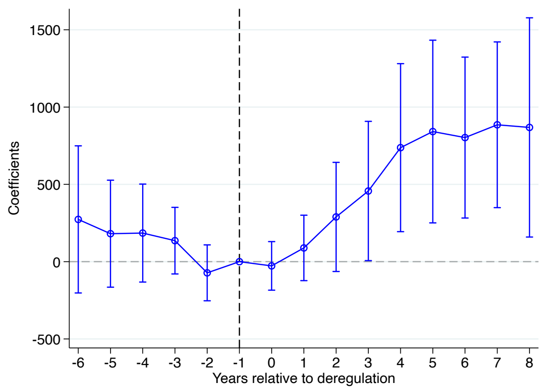
(c) Borusyak et al. (2021)



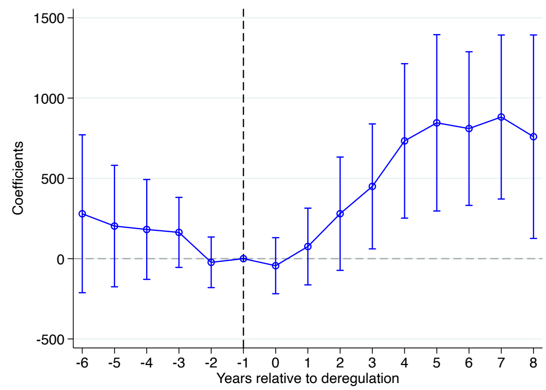
(d) Callaway and Sant'Anna (2021)

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. (2021), 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.

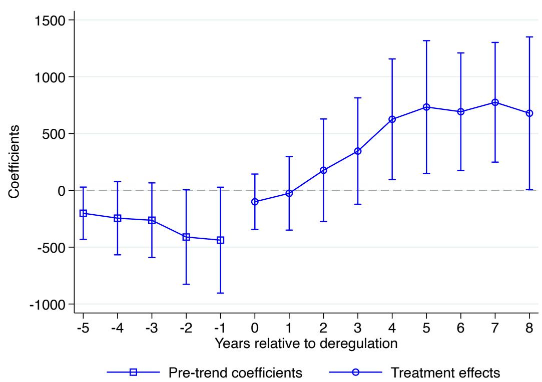
Figure B3: Expenditure Effects: Heterogeneous DiD Treatment Effects



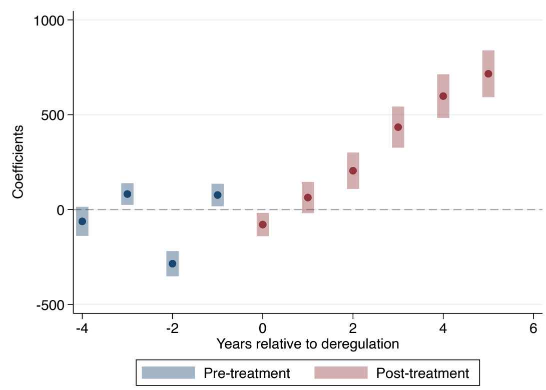
(a) De Chaisemartin and D'Haultfoeuille (2020)



(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. (2021), 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 Timings Comparison: Interstate Branching Laws and Post-1990 SFRs

State	Reform timings		State	Reform timings	
	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			

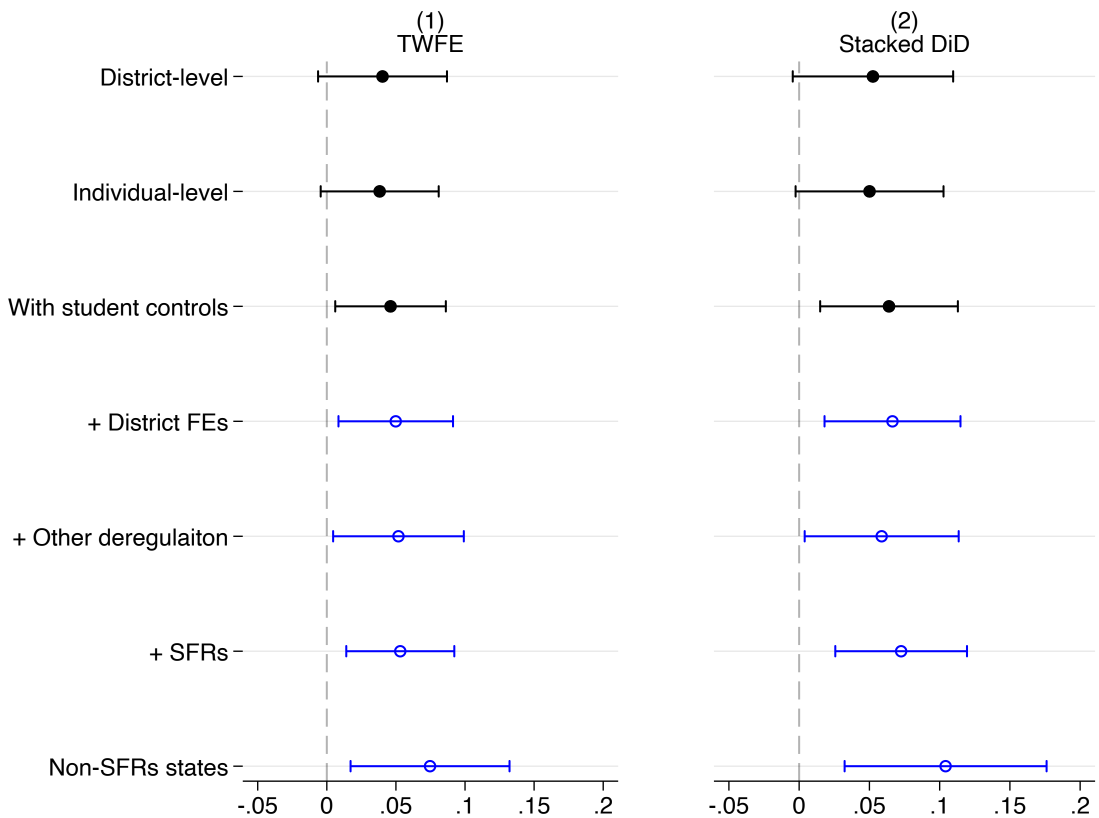
Notes: The table shows the policy timing variations for both deregulation and SFRs for states, while 'x' indicates no reform in the state during our sample period (1992-2005). Columns (1)-(2) refer to the deregulation on statewide deposit caps and post-1990 school finance reforms, respectively. Four 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 is from Rice and Strahan (2010), and the SFRs data is 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 variations of potential confounding events: deregulation in the other three dimensions and School Finance Reforms (SFRs) during the study period. The SFRs variations 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: 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) show the benchmark estimates, followed by estimates: under the individual-level analysis, adding school district fixed effects, including the other three deregulation timings as control, controlling for the post-1990 School Finance Reforms, and using states that do not implement any of the post-1990 SFRs.
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 Math Assessments.

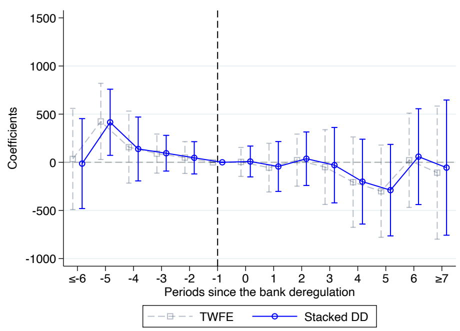
C Additional Results

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

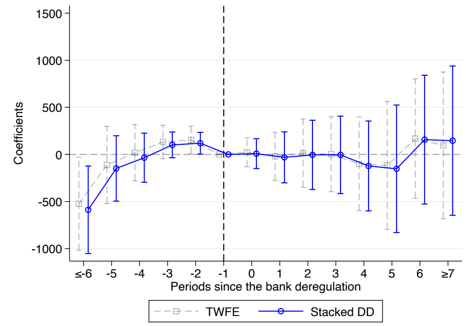
	Total Taxes	Sales and Gross Receipts Taxes	Income Taxes	Property Taxes	License Taxes	Other Taxes
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A						
Minimum age requirement	0.356	0.241	-0.132	0.112	0.043	0.093
	(0.729)	(0.348)	(0.478)	(0.085)	(0.066)	(0.057)
R-squared	0.981	0.989	0.961	0.902	0.951	0.888
Panel B						
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. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

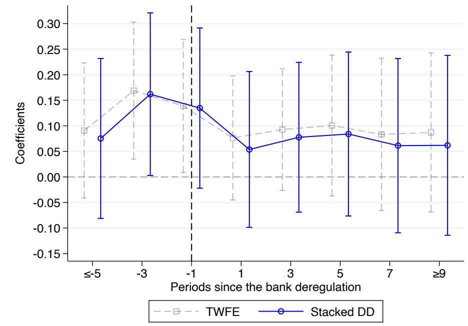
Figure C1: Event Studies on Impacts on School Finance and Student Achievement: Other Deregulation



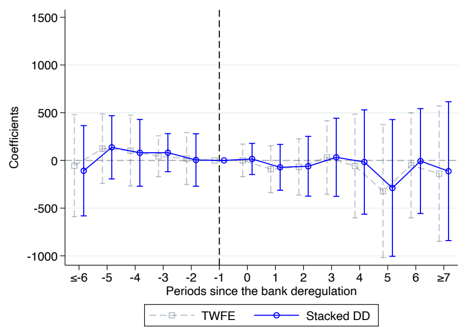
(a) Revenue: minimum age requirement



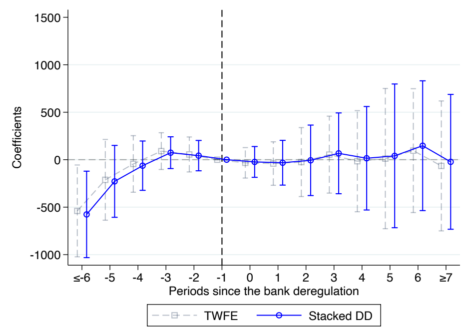
(b) Expenditure: minimum age requirement



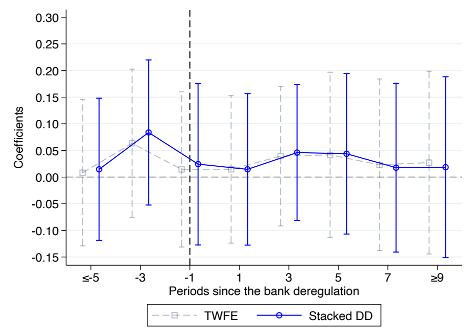
(c) NAEP: minimum age requirement



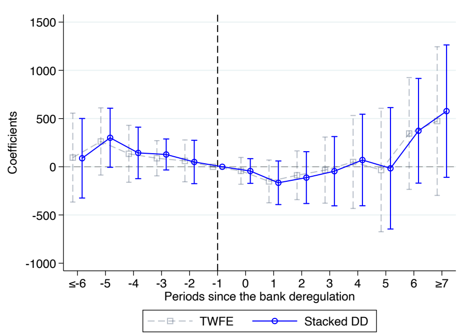
(d) Revenue: *de novo* branches



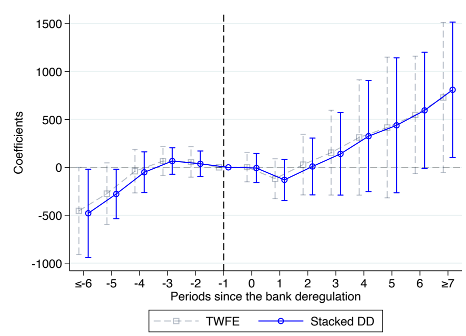
(e) Expenditure: *de novo* branches



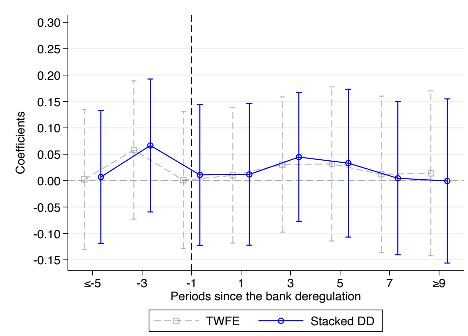
(f) NAEP: *de novo* branches



(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).

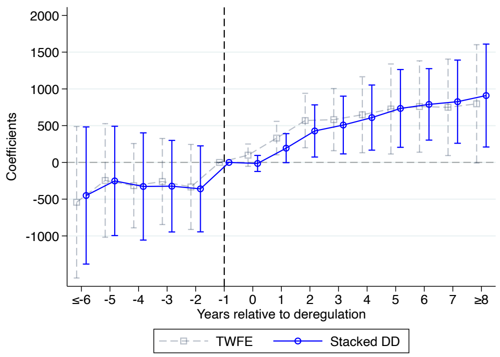
NAEP Source: US Department of Education, Institute of Education Sciences, National Center for Education Statistics, National Assessment of Educational Progress (NAEP) 1992, 1996, 2000, 2003, and 2005 Math Assessments.

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

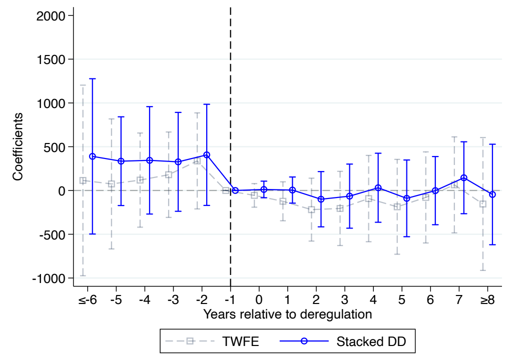
	Mean of dep var	TWFE estimate	District income		Land availability	
			Q1	Q5	Inelastic	Elastic
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Revenue effects						
Total revenue	10,844	390***	689***	142	310**	470***
		(131)	(168)	(130)	(128)	(148)
State revenue	5,117	666***	842***	720**	637***	731**
		(223)	(208)	(297)	(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. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

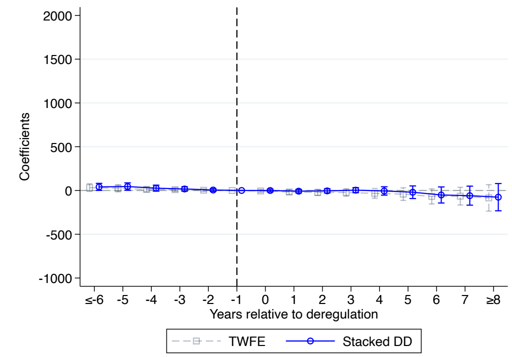
Figure C2: Event Studies on Impacts on School Finance by Category



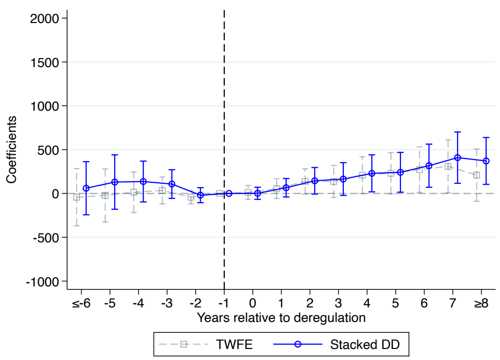
(a) State revenue



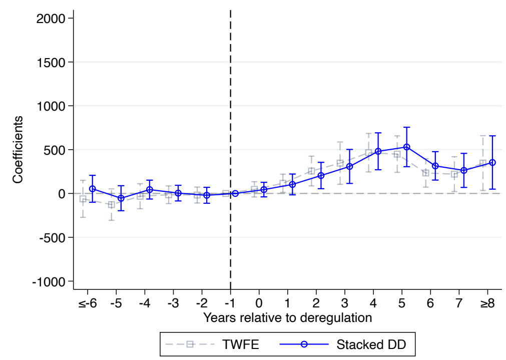
(b) Local revenue



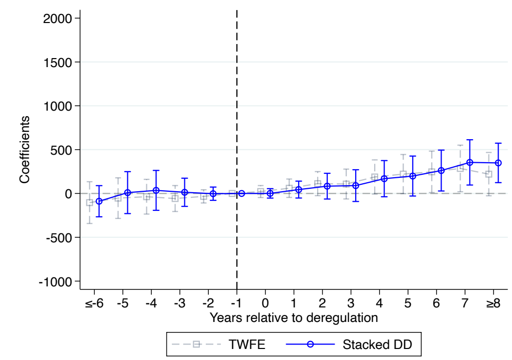
(c) Federal revenue



(d) Current instructional expenditure



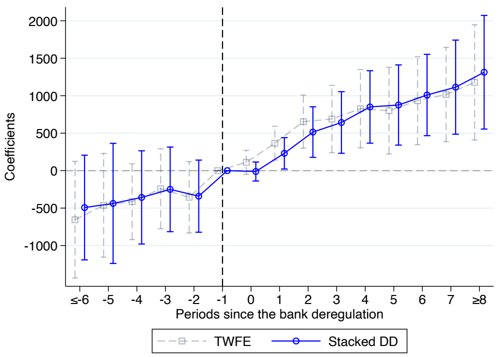
(e) Capital Outlays



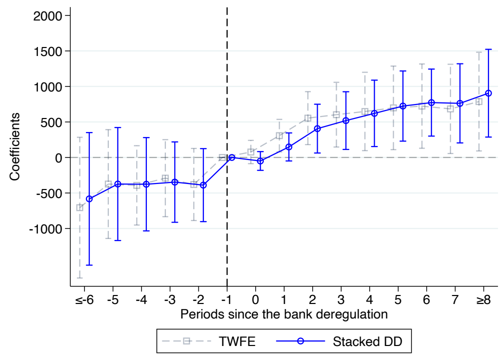
(f) Teacher salaries and benefits

Notes: The figures show event study estimates of the impact of deregulation on school finance by category, using two-way fixed effects and stacked DiD estimators. 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.

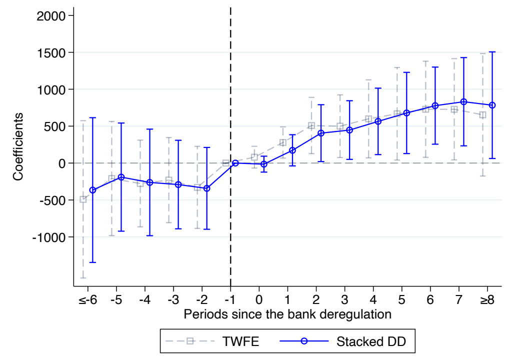
Figure C3: Event Study Estimates of Impacts on State Revenue by Quintile



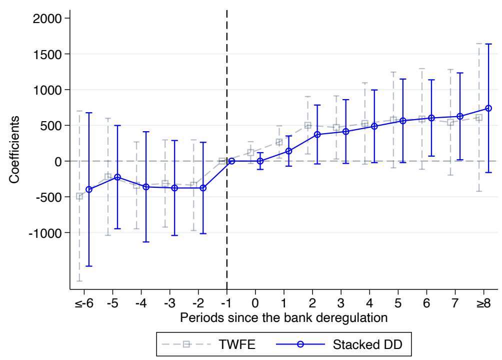
(a) First quintile



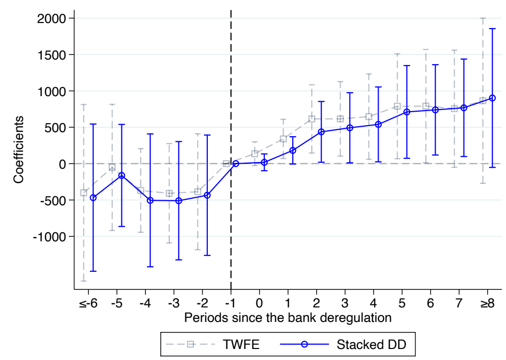
(b) Second quintile



(c) Thrid quintile



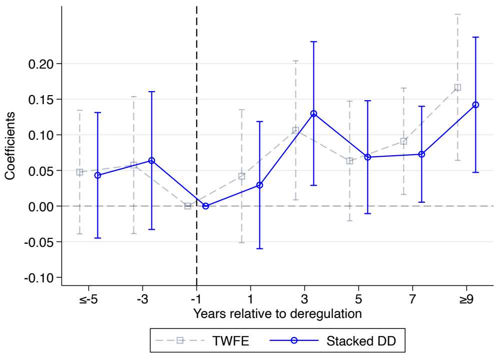
(d) Fourth quintile



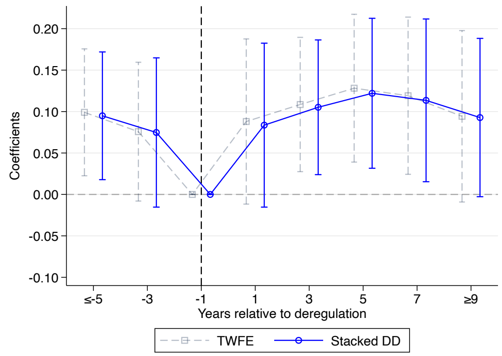
(e) Fifth quintile

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.

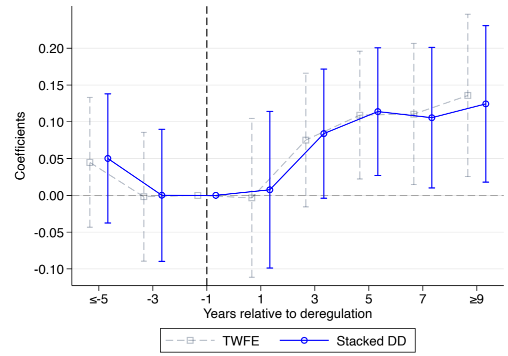
Figure C4: Event Study Figure on Impacts on Student Achievement by Quintile



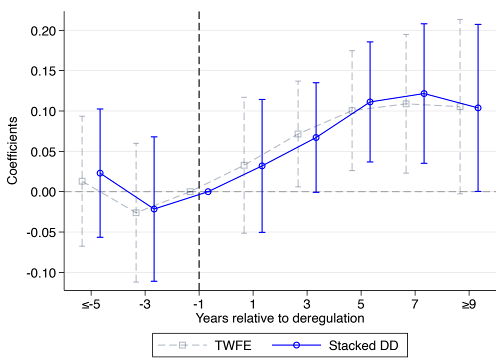
(a) First quintile



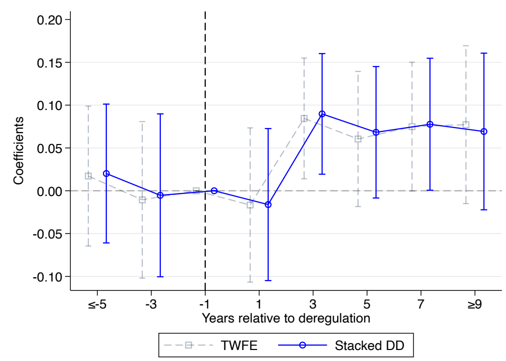
(b) Second quintile



(c) Thrid quintile



(d) Fourth quintile



(e) Fifth quintile

Notes: The figures show event study estimates of impact on student achievement 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 standardized district-level aggregates of student achievement. Event period -1 is normalized to zero. The figures show the 95% confidence interval with standard errors clustered at the state level.
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 Math Assessments.