University of Connecticut

Department of Economics Working Paper Series

The Impact of School Spending on Civic Engagement: Evidence from School Finance Reforms

by

Erdal Asker
Georgia Institute of Technology

Eric J. Brunner
University of Connecticut

Stephen L. Ross
University of Connecticut

Working Paper 2022-16
November 2022

365 Fairfield Way, Unit 1063
Storrs, CT 06269-1063
Phone: (860) 486-3022
Fax: (860) 486-4463
http://www.econ.uconn.edu/

The Impact of School Spending on Civic Engagement: Evidence from School Finance Reforms

Erdal Asker, Eric Brunner & Steve Ross*

Abstract

A primary rationale for public provision of K-12 education and state financing of school spending is that education fosters civic engagement and the development of social capital. However, limited evidence exists on whether and how school spending affects civic engagement. Virtually all studies focus on the impact of educational attainment (as opposed to school spending) on political activity. We provide the first causal evidence on how school spending affects volunteerism as well as voting. The court-ordered and legislative school finance reforms that occurred throughout the United States over recent decades led to large and plausibly exogenous shocks to K-12 school spending. We estimate difference-in-difference-in-differences (DDD) models to isolate the causal impact of school spending on civic engagement. Using data from the National Education Longitudinal Study of 1988 (NELS), the Education Longitudinal Study of 2002 (ELS), and the High School Longitudinal Study of 2009 (HSLS), we find that exogenous increases in school spending led to increases in the probability that young adults volunteer and the amount of time they spend volunteering. In contrast, we find little evidence that school spending impacts voting. Consistent with prior studies, we find that increases in school spending increase high school graduation and college attendance.

Acknowledgements: We are grateful to Elizabeth Cascio, Thomas Downes, Holger Sieg and John Yinger for insightful comments on earlier drafts of this work. We also thank seminar participants at the American Economic Association, the Urban Economic Association and the Conference in Honor of John Yinger at Syracuse University. All errors are our own, and no external funding was provided to support this research.

JEL Codes: H42, H72, I22, I26

Key Words: Civic Engagement, Education Spending, Volunteerism, Voting**,** School Finance Reform

---

* Erdal Asker, School of Economics, Georgia Institute of Technology, Old C.E. Building 221 Bobby Dodd Way, Atlanta, GA, 30332, easker3@gatech.edu; Eric J. Brunner, Department of Public Policy, University of Connecticut, 10 Prospect Street, 4th Floor, Hartford, CT 06103, eric.brunner@uconn.edu; Stephen Ross, Department of Economics, University of Connecticut, 341 Mansfield Road, Unit 1063, Storrs, CT 06269-1063, stephen.l.ross@uconn.edu.

I. Introduction

One of the primary rationales for the public provision of K-12 education and the large subsidies provided by state governments to finance school spending is that education fosters civic engagement and the development of social capital. Indeed, as Dee (2004) notes, “the putative existence of such civic returns to education motivated the proliferation of common schools in the early 19th century.” Costa & Kahn (2004) and Dee (2020) describe three general categories of civic engagement: volunteering and membership, trust, and support for government activities. The widespread belief that education promotes all three categories of civic engagement is supported by a wealth of correlational evidence.[1] In fact, the ubiquitous nature of correlational evidence led Putnam (2001) to conclude that “education is by far the strongest correlate that I have discovered of civic engagement in all its forms.”

However, despite civic returns being one of the primary rationales for the public provision of K-12 education and the significant role played by state governments in financing school spending, very little causal evidence exists on whether and how school spending affects civic engagement. What evidence is available comes primarily from studies of how educational attainment, or years of schooling, affects political activity and voting, rather than other forms of engagement and that evidence is mixed. For example, using state and birth year variation in compulsory school laws as an instrument for years of schooling, Milligan, Moretti, & Oreopoulos (2004), and Dee (2004) both find that additional schooling leads to an increase in the probability of being registered to vote and engaging in political activities.[2] More recently, using data from the Perry Pre-School and Tennessee STAR Class Size experiments and the roll-out of a college scholarship program, Sondheimer & Green (2010) find that high school graduation increases voting. In contrast, using the Vietnam draft as an instrument for college attendance and completion, Berinsky & Lenz (2011) find little evidence that college increases voting. Similarly, based on propensity score matching, Kam & Palmer (2008) find little evidence that college attendance increases political participation.

[1] See Nie, Junn, & Stehlik-Barry (1996), and Dee (2020) for a review of studies providing correlational evidence on the relationship between education and multiple dimensions of civic engagement in the United States.
[2] Similarly, using an instrumental variable identification strategy, based on geographic proximity to community colleges, Dee (2004) finds that educational attainment increases the likelihood of being registered to vote and voting in the last presidential election. See Lochner (2011) for a more detailed review of these studies.

The purpose of this paper is to provide some of the first causal evidence on how exogenous increases in K-12 school spending impact civic engagement. The court-ordered and legislative school finance reforms that occurred throughout the United States over the last several decades led to large and plausibly exogenous shocks to K-12 school spending.[3] We leverage the timing and location of these reforms to estimate difference-in-difference-in-differences (DDD) models exploiting variation over time and across states in exposure to reforms and variation in the impact of reforms between low- and high-income districts to isolate the causal impact of K-12 school spending on civic engagement. Our analysis is based on multiple waves of three surveys conducted by the National Center for Educational Statistics (NCES) between 1988 and 2009 that follow cohorts of high school students as they graduate from high school, enroll in college and/or join the labor force. Each of the three surveys provides a sample of students from a broad cross-section of school districts across reform and non-reform states. Specifically, we combine data from the National Education Longitudinal Study of 1988 (NELS), the Education Longitudinal Study of 2002 (ELS), and the High School Longitudinal Study of 2009 (HSLS) with data on the timing and location of state-level school finance reforms and data on school district spending from 1986-2015 obtained from the NCES common core of data.

We begin by estimating event study models of the impact of school finance reforms on per-pupil revenues and expenditures among the school districts contained in the NCES high school surveys. Consistent with prior work, we provide evidence that school finance reforms led to large increases in per-pupil state aid and spending per-pupil, particularly among school districts located in the bottom quartile of the within-state income distribution in 1980. We then estimate our DDD models that relate the average level of school spending each cohort of students in a school district was exposed to while in grades K-12 to whether and when a cohort was exposed to a school finance reform and whether a school district was located in the first or fourth quartile of the within-state income distribution. We show that school districts in the bottom quartile of the within-state income distribution experienced large increases in state aid following a school finance reform relative to districts in the top quartile of the income

[3] Jackson, Johnson, & Persico (2016), Hyman (2017), Lafortune, Rothstein, & Schanzenbach (2018), Brunner, Hyman, & Ju (2020), Biasi (forthcoming), and Rothstein & Schanzenbach (2022), all exploit shocks to school spending that result from court-ordered and legislative school finance reforms to provide causal evidence on the effect of school spending on student achievement, high school graduation, college enrollment, adult earnings, and intergenerational income mobility.

distribution and that this increase in state aid also led to a sustained increase in school spending that grows the longer a cohort was exposed to a school finance reform (i.e., upward trend).

Having established that school finance reforms led to exogenous and policy-relevant increases in bottom income quartile school district spending, we next turn to examining the impact of school finance reforms on student outcomes. Consistent with the results of Jackson, Johnson, & Persico (2016), Hyman (2017), and Rothstein & Schanzenbach (2022), we find that school finance reforms led to an increase in high school graduation rates and college enrollment in the bottom income quartile districts, with the effects growing in magnitude with the length of time a cohort was exposed to a school finance reform. Turning next to measures of civic engagement, we find that exposure to school finance reforms led to increases in the probability that young adults volunteer and the amount of time they spend volunteering. In contrast, we find little evidence that exposure to a school finance reform increased the probability of being registered to vote or voting in the last presidential election.

We undertake a series of analyses to examine the validity and robustness of these results. First, we replicate our results using newly developed two-way fixed effects (TWFE) estimators that address potential bias in the presence of heterogeneous treatment effects with staggered timing of treatment (Callaway & Sant'Anna, 2020; Goodman-Bacon, 2021; Sun & Abraham, 2021). Second, while the nature of our survey data precludes us from estimating standard event study models, we are able to estimate models similar to those in Rothstein & Schanzenbach (2022) that include a linear trend variable that allows us to test whether our outcomes of interest were trending upward prior to a school finance reform. For all our outcomes, we find little evidence of any pre-trends. Third, we conduct a falsification test where we move the actual year of a school finance reform back 10 years and re-estimate our DDD models using these placebo reform years. Once again, for all of our outcomes, the estimated treatment effects from these falsification tests are statistically insignificant and typically small in magnitude.[4] Finally, we examine potential heterogeneity by gender and race/ethnicity in our average treatment effects. Overall, we find little evidence of heterogeneous treatment effects.

[4] As we discuss in detail later in the paper, to implement the falsification tests and the pre-trend analysis, we augment our analytic sample with data from the High School and Beyond Survey (HS&B) of 1980 and first show that our core results are robust to the addition of data from the HS&B survey.

Our study makes several contributions to the literature. First, as noted previously, prior studies have focused exclusively on how education affects political participation. In contrast, to our knowledge, our paper is the first to directly examine how education affects alternative measures of civic engagement specifically, the probability of volunteering and hours spent volunteering. Second, prior studies that examine the impact of education on civic engagement have focused on how years of educational attainment or college completion affect civic engagement. In contrast, our study provides some of the first direct causal evidence on how K-12 school spending affects civic engagement which we believe ties more closely to a core justification for large public investments in education. Third, our study provides validation from a unique and different data source that school finance reforms lead to increases in high school graduation and college enrollment.[5] Fourth, most prior studies that use school finance reforms to examine the effect of school resources on student outcomes have typically relied on cross-state variation in school spending induced by school finance reforms. In contrast, our study, which utilizes a triple difference identification strategy by comparing across income quartiles, is one of the few studies to focus exclusively on within-state variation in school spending attributable to school finance reforms to isolate the causal impact of school resources on educational attainment and civic engagement.[6] This is important since many school finance reforms not only alter spending levels but also involve state-wide changes to teaching standards, accountability structures, and school governance, making it difficult to directly isolate the impact of school spending on student outcomes.[7]

The paper proceeds as follows. Section II describes the data we assemble to examine the impact of school finance reforms on K-12 school spending, state aid and student educational attainment, and civic engagement. Section III outlines our empirical framework for isolating the causal effects of K-12 school spending on our outcomes of interest. We present our main

[5] Downes & Figlio (1997) use somewhat older data from the National Longitudinal Study of the High School Class of 1972 and from the National Education Longitudinal Study of 1988 (NELS) to examine the impact of earlier (equity-focused) school finance reforms and tax and expenditure limits on student achievement as measured by standardized test scores.
[6] The one exception to our knowledge is Lafortune, Rothstein, & Schanzenbach (2018) who also use a triple-difference identification strategy that focuses on the achievement (test score) gap between districts in the first and fifth quintile of the within-state income distribution.
[7] For example, in response to the Kentucky Supreme Court ruling in Rose v. Council for Better Education, Kentucky not only increased state aid among previously low-income districts but also revamped its curriculum and governance structure, introduced new student outcome goals, and developed new student assessment standards (Odden, 1993).

findings in Section IV, including balancing tests, robustness checks, falsification tests, and heterogeneity analysis, and conclude in Section V.

II. Data

Our primary source of data comes from multiples waves of three NCES longitudinal high school surveys: 1) the National Education Longitudinal Study of 1988 (NELS); 2) the Education Longitudinal Study of 2002 (ELS); and 3) the High School Longitudinal Study of 2009 (HSLS).[8] Figure 1 provides a graphical depiction of the longitudinal nature and timing of the NCES surveys.

From these surveys, we construct consistent measures of student and family background characteristics using responses from the baseline year of each longitudinal survey. For example, the NELS of 1988 consists of a cohort of 8th graders first surveyed in 1988, while the ELS of 2002 and HSLS of 2009 consist of a cohort of 10th graders first surveyed in 2002 and a cohort of 9th graders first surveyed in 2009, respectively. From each of the baseline years, we constructed measures of a student's mother and father's educational attainment, a student's race/ethnicity and gender, and family income.

We exploit the longitudinal nature of the NCES surveys to construct consistent measures of six outcomes: 1) an indicator for whether a respondent graduated high school as measured in the first follow-up survey after a cohort was scheduled to graduate from high school; 2) an indicator for whether a respondent ever was enrolled in a post-secondary institution; 3) an indicator for whether a respondent had ever volunteered; 4) hours spent volunteering per week; 5) an indicator for whether a respondent was registered to vote; and 6) and an indicator for whether a respondent voted in the last presidential election. Tables A1 and A2 of the Appendix provide more detailed definitions of these variables and information on when each of our outcomes was collected.

We obtained a comprehensive list of SFRs from Jackson, Johnson, & Persico (2016), Lafortune Rothstein, & Schanzenbach (2018), and Brunner, Hyman & Ju (2020). Our primary coding of these SFRs is based on the coding structure developed by Brunner, Hyman, & Ju (2020).[9] Appendix Table A3 shows the states that implemented a SFR and the year they implemented a reform, while Figure 2 panel A shows the geographical distribution of states that implemented a reform. Following Brunner, Hyman, & Ju (2020), we separate the effect of SFRs by the within-state distribution of 1980 household income because these adequacy era reforms were designed to differentially impact spending for low- and high-income

[8] We use the restricted use version of these surveys in order to obtain school district identifiers.

[9] Brunner, Hyman, & Ju (2020) omit Kansas, Kentucky, Missouri, and Texas from their core results due to the nature of their research question (crowd-out of intergovernmental grants) and the fact that those states implemented "reward for local effort" (matching grants) in response to their school finance reforms. They also omit Michigan and Wyoming because those states removed local discretion of school spending in response to their reforms. We include all those states in our analysis and use the same reforms and reform years found in Brunner, Hyman, & Ju (2020).

districts, with the goal of providing resources for an adequate education in all districts.[10] Specifically, we use data from the Special School District Tabulations of the 1980 Census to create quartiles of the within-state distribution of median household income in 1980 that are based on the universe of school districts in the U.S and then assign the school districts in our sample to these quartiles.

We use the school district identification code in each of the surveys to merge in annual data on school district per-pupil revenues and expenditures from the Local Education Agency (i.e., School District) Finance Survey (F-33) maintained by the National Center for Education Statistics (NCES). The F-33 surveys contain detailed annual revenue and expenditure data for all school districts in the United States for the period 1990-91 to 2016-17. We augment this data with earlier versions of the F-33 survey provided by the U.S. Census for the years 1986-87 through 1989-90. Using that data, we construct annual measures of school district per-pupil total revenue, total expenditures, current expenditures, and state aid, which we deflate into constant 2015 dollars using the consumer price index.

We begin by using the annual school district financial data to estimate event study specifications that examine the impact of school finance reforms on per-pupil revenues and expenditures while limiting the sample to districts contained in the NCES high school surveys that we utilize.[11] This allows us to examine whether school district revenues and expenditures were differentially trending among school districts exposed to a school finance reform prior to the reform, which would violate our identification strategy. It also allows us to examine how school finance reforms differentially affected per-pupil revenues and expenditures between districts in the first and fourth quartile of the within-state distribution of 1980 household income. Next, we use the F-33 financial data to construct per-pupil measures of the average level of revenue and spending that each student (cohort) in our sample was exposed to from the time they entered kindergarten until 12th grade. Specifically, for each student, we use the year they were in 12th grade to determine the year they would have started school. We then use the annual school district data on per-pupil revenue and spending to calculate each student's exposure to school resources by summing annual per-pupil revenues and expenditures in their school district over all the years a student was in primary and secondary school and then dividing by 12. This provides a measure of each student's school spending "dosage" that is similar to the one used by Jackson, Johnson, & Persico (2016).[12]

[10] The one exception is Michigan, where the reform was designed to level-up spending in previously low-spending districts and constrain spending in previously high-spending districts. Thus, we base the quartiles on the within-state distribution of spending per pupil for Michigan. Our core results are similar if we use the distribution of median household income to create quartiles for Michigan (see Table A5) though we naturally lose some predictive power in terms of the impact of school finance reforms on spending.

[11] See Figure 2 panel B for a visualization of the geographical distribution of school districts included in the three NCES surveys we employ.

[12] Given that school finance reforms only impacted public school districts, we limit the sample to students attending public schools and hence drop the roughly 20% of students in the NCES high school surveys that attend private schools.

Table 1 presents means and standard deviations for the variables used in our analysis. Columns 1 and 2 provide this information for the full sample of students and school districts in our three surveys, while columns 3-4 and 5-6 present the same information for students that attend districts located in either the first or fourth quartile of the within-state distribution of 1980 household income. The top panel of Table 1 provides summary statistics for our main outcomes of interest, while the middle panel provides the same information for the control variables we utilize. The bottom panel provides means and standard deviations for per-pupil revenues and expenditures among districts in our sample and information on exposure and years of exposure to a school finance reform for the various cohorts of students in our sample. Not surprisingly, the means of most of our outcomes of interest and student and parent control variables are larger for students attending districts located in the top quartile (quartile 4) of the within-state distribution of household income relative to the bottom quartile (quartile 1). Table 1 also reveals that roughly 30% of the students in our surveys were exposed to a school finance reform while attending a K-12 school.

III. Empirical Framework

To examine the effect of SFR-induced changes in school resources on our outcomes of interest, we follow Lafortune, Rothstein, & Schanzenbach (2018) and focus on the difference between students attending districts in the bottom and top quartiles of the within-state distribution of 1980 household income and thus drop observations pertaining to students that attend a district located in the middle two quartiles.[13] We further follow Lafortune, Rothstein, & Schanzenbach (2018) and Rothstein & Schanzenbach (2022) and allow school finance reforms to have two separate effects: an initial jump associated with the reform and a linear trend that allows the effect of the reform to vary with the length of exposure to a reform. For example, if a school finance reform occurred in 1996 and we observed a cohort of high school seniors in 2004, we would code the cohort as having been exposed to a school finance reform for nine years. This trend allows for both increases in in-school exposure to a school finance reform, as well as an upward trend in school spending that follows the adoption of a reform.[14]

We then estimate reduced-form models of the following form:

[13] Lafortune, Rothstein, & Schanzenbach (2018) focus on the gap between the top and bottom quintiles of district income, while we focus on the gap between the top and bottom quartiles mainly to improve power.
[14] Note that following Lafortune, Rothstein, & Schanzenbach (2018), when a SFR occurs more than 12 years prior to a cohort entering primary school, $Exposure_{is}$ will be greater than 12.

$$y_{isjq} = \beta_1(T^{q=1} \cdot SFR_s) + \beta_2(T^{q=1} \cdot SFR \cdot Exposure_{is}) + X_{isj}\alpha + \delta_{sj} + \lambda_{sq} + \Theta_{jq} + \mu_{isjq} \quad (1)$$

where y_{isjq} is an outcome of interest for student *i*, in state *s*, in base survey *j*, and income quartile *q*, $T^{q=1}$ is an indicator equal to unity if student *i* attends a school district located in the first quartile of the within-state distribution of 1980 median household income, SFR_s is an indicator equal to unity in all years after a state has a SFR (zero for states that never have a SFR) and $Exposure_{is}$ is a linear trend that counts the number of years since a SFR was enacted (equal to zero for states that never had a SFR) and varies based on the year each of the cohorts in our surveys were exposed to a SFR starting in the year the cohort entered kindergarten. Turning to the remaining variables in (1), X_{isj}, a vector of student characteristics (mother and father's education, family income, gender, race/ethnicity), δ_{sj} is a vector of state-by-survey fixed effects, λ_{sq} is a vector of state-by-quartile fixed effects, Θ_{jq} is a vector of survey-by-quartile fixed effects, and μ_{isjq} is a random disturbance term. In all our specifications we cluster the standard errors at the state-by-quartile level.

Note that given our fixed effect structure, the main effects of $T^{q=1}$, SFR_s and $Exposure_{is}$ are captured by the state-by-survey fixed effects, δ_{sj}, the state-by-quartile fixed effects, λ_{sq}, and survey-by-quartile, Θ_{jq} fixed effects. Thus, equation (1) represents a triple-difference (DDD) identification strategy where the first DiD is the difference in outcomes before and after a SFR for districts located in the 1st quartile of the within-state income distribution (more treated group) relative to the same difference for districts in the 1st quartile of the within-state income distribution located in states that never experienced a school finance reform, and the second DiD is the same as the first except for districts in the 4th quartile of the within-state income distribution (less treated group).

The coefficient of primary interest in (1) is β_2, which captures the effect of an additional year of exposure to a school finance reform on our outcomes of interest. β_1, which captures any initial jump in our outcomes of interest following a school finance reform, is also potentially of interest. However, as noted by Rothstein & Schanzenbach (2022), the effect of any school finance reform on our human capital accumulation and civic engagement outcomes will likely grow with the number of years of exposure to a reform and is unlikely to lead to an immediate jump in outcomes. Note that the state-by-survey fixed effects in (1) net out any state-specific trends in our outcomes of interest that are common to all students. Furthermore, the inclusion of state-by-survey fixed effects in (1) implies we are identifying the effect of SFRs based solely on within-state variation in exposure to a reform. Any direct effects of a SFR on our outcomes of interest are captured by the fixed effects, implying β_2 is identified based on the within-state variation in reform-induced exposure to school resources between districts in the top and bottom income quartile, where the bottom quartile districts are typically more heavily exposed to additional spending as part of a SFR. Further, the survey-by-quartile fixed effects capture any average differences in our outcomes of interest between the 1st and 4th quartiles for students that are not in a state where those students would have been exposed to school finance reform.

To provide evidence that SFRs induce exogenous variation in per-pupil revenue and expenditures among districts in our sample, we also estimate event study models of the following form:

$$y_{ist} = \sum_{k=-6}^{10} \gamma_k T_{k,st} + \delta_i + \lambda_t + \eta_{ist}, \qquad (2)$$

where y_{ist} is a measure of real per pupil revenues or expenditures in district i, located in state *s* in year *t*, $T_{k,st}$ represents a series of lead and lag indicator variables for when the state *s* implemented a SFR, δ_i and λ_t are district, and year fixed effects respectively, and η_{ist} is a random disturbance term. We re-center the year a SFR was enacted so that $T_{0,st}$ always equals one in the year in which the state *s* implemented a SFR. We include indicator variables for 2 to 6 or more years prior to implementation of a SFR ($T_{-6,st} \ - \ T_{-2,st}$), the year of implementation, $T_{0,st}$, and 1 to 10 or more years after implementation ($T_{1,st} - T_{10,st}$). Note that T_{-6st} equals one in all years that are 6 or more years prior to the implementation of a SFR, and $T_{10,st}$ equals one in all years that are 10 or more years after the implementation of a SFR. The omitted category is the year just prior to a state implementing a SFR, $T_{-1,st}$. We estimate equation (2) separately for districts in the 1st and the 4th quartiles of the within-state 1980 income distribution to highlight how SFRs differentially affect low versus high-income districts.

The coefficients of primary interest in equation (2) are the $\gamma_k{'}s$, which represent the DiD estimates of the impact of SFRs on per-pupil revenues and expenditures in each year from t_{-6} to t_{+10}. The estimated coefficients on the lead treatment indicators ($\gamma_{-6}, \ldots, \gamma_{-2}$) provide evidence on whether school spending was trending pre-reform. If reforms induce exogenous variation in spending per pupil, these lead treatment indicators should generally be small in magnitude and statistically insignificant. The lagged treatment indicators ($\gamma_{+1}, \ldots, \gamma_{+10}$) allow the effect of SFRs on school spending to evolve slowly and in a nonparametric way over time.

IV. Results

A. Effect of SFRs on District Revenues and Expenditures

We begin by illustrating the impact of school finance reforms on school district per-pupil revenues and expenditures among the districts included in the NCES longitudinal high school surveys by plotting the estimated $\gamma_k's$ and associated 95 percent confidence intervals from our event study specification given by equation (2). Figures 3a and 3b illustrate the impact of school finance reforms on per-pupil state aid among districts in the 1st and 4th income quartiles, respectively. For districts in the 1st quartile, there is clear evidence of an upward shift in state aid following the implementation of a reform, with state aid growing slowly over time and peaking at nearly $2,000 per-pupil approximately nine years after a reform was implemented. Districts in the 4th quartile also experience an increase in state aid, but it is more immediate and much smaller in magnitude, leveling out at slightly over $500 per-pupil. Importantly, in both figures, the estimated coefficients on the lead treatment indicators are all small in magnitude and statistically insignificant, providing evidence that the parallel trends assumption holds.

Figures 3c-3d and 3e-3f present the impact of school finance reforms on per-pupil total and current expenditures, respectively. Once again, for both total and current expenditures, the figures reveal a clear increase in spending following a school finance reform with spending increasing substantially more among districts located in the 1st quartile compared to districts located in the 4th quartile. For example, as seen in Figure 2e, current spending per-pupil increases by nearly $1,500 among districts in the 1st quartile approximately 5 years after a school finance reform is enacted. In contrast, districts located in the 4th quartile experience much smaller increases in spending after a school finance reform with spending increasing by less than $500 starting approximately three years after a reform is enacted. It is this differential impact of the effect of school finance reforms on school spending between districts located in the 1st or 4th quartile of the within-state income distribution that we exploit in our identification strategy.

Table 2 provides estimates based on equation (1) of the impact of exposure to a school finance reform on the average level of school spending that each cohort in our sample was exposed to during their time in primary and secondary school. Thus, the dependent variable in Table 2 provides a measure of the accumulated "dosage" of school spending each cohort of students received from kindergarten through 12th grade. Columns 1 and 2 report results when the outcome is per-pupil state aid and total revenue, while columns 3 and 4 report results when the outcome is per-pupil total or current expenditures, respectively. The top panel presents results from models that include district controls for the percent of Black students, the percent urban and the percent of the population age 18 or older with a college education in a school district All these control variables are measured in 1980 (rather than being time variant) to ensure that they are not affected by treatment (exposure to a SFR). Results based on specifications without any controls are presented in the bottom panel of Table 2.

In both the top and bottom panels of Table 2, the estimated coefficient on the SFR "jump" variable in column 1 is positive and statistically significant, implying an initial jump due to a SFR in average per-pupil state aid during a cohort's time in K-12 schools. Furthermore, the estimates of the jump variable in the specifications with and without controls are similar in magnitude and suggest that for districts located in the 1^{st} quartile of the within-state distribution of income, school finance reforms led to approximately a \$1,400 to \$1,500 increase in state aid relative to districts in the 4^{th} quartile. Thus, the results in column 1, tell a similar story to the event study estimates presented in Figure 3: SFRs result in a much larger increase in state aid among districts located in the 1^{st} quartile of the within-state income distribution relative to districts in the 4^{th} quartile.

The results for per-pupil total revenue and expenditures reported in columns 2 and 3 tell a similar story, except now the estimated coefficient on the linear trend variable measuring years of exposure to a SFR is also positive and statistically significant. For example, the estimated coefficient on the trend variable in column 3 of the top panel suggests that for districts in the 1^{st} quartile of the within-state income distribution, each additional year of exposure to a SFR, leads to approximately a \$100 increase in average total spending per-pupil, relative to districts in the 4^{th} quartile. As shown in column 4, for current expenditures, the estimated coefficient on the trend variable is once again positive and statistically significant in both the top and bottom panels and suggests that for districts in the 1^{st} quartile of the income distribution, each additional year of exposure to a SFR, leads to approximately an \$80 (top panel) to \$100 increase in average current spending per-pupil, relative to districts in the 4^{th} quartile. However, in contrast to the results for state aid, the estimated coefficient on the jump variable is small in magnitude and statistically insignificant, suggesting that SFRs have a slightly delayed effect on current expenditures. Across all models, the slope estimates are very stable between the panel 1 and panel 2 models, with and without district controls.

B. Balancing Tests for Pre-Treatment Covariates

Having shown that SFRs have a much larger and sustained effect on the level of resources devoted to schools located in 1^{st} quartile of the income distribution relative to the 4^{th} quartile, we now turn to examining the effect of SFRs on educational attainment and our measures of civic engagement. However, before we present our main results, we first report the results of a series of balancing tests designed to examine whether the observable characteristics of students and their parents are similar among respondents living in states that experienced a SFR, and those living in a state that did not experience a reform. Specifically, we regress measures of student and parent baseline characteristics from the NELS 1988 (which predates the SFRs in our sample), on an indicator for whether a state experienced a SFR. We focus on five observable student, and parent characteristics, namely: 1) parents educational attainment, measured as having a high school diploma or higher; 2) gender; 3) family income; and 4) a student's race/ethnicity. The idea behind these tests is that if the location and timing of SFRs are as good as randomly assigned, the characteristics of students and parents in states that were treated by a SFR should be similar to the characteristics of students and parents that were not treated (i.e., controls).

The results of the balancing tests are reported in Table 3, with standard errors clustered at the state level in parentheses. With the exception of the estimated coefficient in column 4, all of the estimated coefficients are relatively small in magnitude and statistically insignificant. Furthermore, in the one case where we do find a statistically significant estimate, the estimate is once again relatively small in magnitude and only marginally significant at the 10% level. Thus, the results reported in Table 3 are encouraging in the sense that they suggest the location and timing of SFRs appear relatively random. With that in mind, we now turn to examine the impact of SFRs on our outcomes of interest.

C. Effect of SFRs on Civic Engagement and Educational Attainment

Table 4 presents the effect of school finance reforms on our main outcomes of interest. The top panel presents estimates with controls consisting of baseline student and parental attributes, while the bottom panel presents estimates without controls. We begin by discussing the results in columns 1 and 2, which show the effect of SFRs on educational attainment as measured by whether a respondent graduated from high school on time (column 1) and whether a respondent ever attended a post-secondary institution (column 2). As shown in column 1, exposure to a school finance reform is associated with a small and statistically insignificant "jump" in high school graduation and a positive and statistically significant effect of exposure to a school finance reform that grows with the number of years a cohort is exposed to the reform. Specifically, the results in column 1 imply that each additional year of exposure to a SFR leads to approximately a 0.43 percentage point increase in the propensity of on-time high school graduation or a 5.16 percentage point increase in the propensity of on-time high school graduation for a student that was exposed to a school finance reform for 12 full years while in school. As shown in column 2, school finance reforms also appear to have a positive impact on college attendance: each additional year of exposure to a SFR leads to approximately a 0.36 percentage point increase in the likelihood that a student ever attends a post-secondary institution or a 4.3 percentage point increase for a student that was exposed to a school finance reform for 12 full years while in school. These results are consistent with the results of Jackson, Johnson, & Persico (2016) and Rothstein & Schanzenbach (2022) who both find that school finance reform-induced changes in resources improve high school graduation rates and the propensity to attend college.[15]

Columns 3-7 of Table 4 present estimates of the effect of school finance reforms on our four measures of civic engagement, namely: 1) whether a student ever volunteered; 2) the weekly amount of time spent volunteering; 3) whether a student was registered to vote; and 4) whether they voted in the last presidential election. As shown in columns 3 and 4, each additional year of exposure to a school finance reform increases the probability that a respondent ever volunteered by 0.47 percentage points and increases the hours spent volunteering per week by 0.072 hours, with the estimates being statistically significant at the 10 and 5 percent level, respectively. These estimates imply that for a student exposed to a school finance reform for 12 full years while in school, the probability of every volunteering would increase by 5.6 percentage points while the hours spent volunteering per week would increase by 0.87 hours. To put the latter estimate into perspective, the average time spent volunteering per week among students in our sample is 1.59 hours. Thus, exposure to a SFR for a full 12 years would increase the hours spent volunteering per week by approximately 55 percent. As shown in columns 5 and 6, we find much less evidence that school spending affects voting behavior. Specifically, being exposed to a

[15] Note that in terms of magnitude, our results are not directly comparable to the results of either Jackson, Johnson, & Persico (2016) or Rothstein & Schanzenbach (2022). Specifically, Jackson, Johnson, & Persico (2016) report 2SLS estimates rather than reduced form estimates and focus on years of completed education rather than college attendance. Similarly, Rothstein & Schanzenbach (2022) examine the impact of exposure to a school finance reform at the state-level rather than by comparing the impact on the 1st versus the 4th quartile of the within-state income distribution.

school finance reform leads to an initial jump in the probability of being registered to vote, but years of exposure to a reform appears to have little impact on the probability of being registered to vote. Similarly, exposure to a school finance reform appears to have little impact on the probability of voting in the most recent presidential election.

C. *Robustness and Falsification Tests*

In this section, we conduct a number of robustness and falsification tests to examine the internal validity of the results presented in Table 4. Our first robustness check examines whether our results are sensitive to the staggered timing of school finance reforms in our sample and the potential presence of heterogeneous treatment effects. Specifically, several recent studies have shown that estimates from standard event study and DD specifications relying on the staggered timing of treatment for identification may be biased in the presence of heterogeneous treatment effects (Callaway & Sant'Anna, 2021; Sun & Abraham, 2020; Goodman-Bacon, 2021). To address this concern, we follow Cengiz, Dube, Lindner, & Zipperer (2019) and Goodman & Bacon (2021) and estimate stacked event study and difference-in-differences models. Specifically, we first create a set of datasets that include observations from a cohort of states that experienced a school finance reform in the same year and other states that never had a school finance reform. We then append these cohort-specific datasets and estimate models similar to equation (1), except we replace the state-by-survey, quartile-by-survey and state-by-quartile fixed effects in (1) with state-by-survey-by-cohort, quartile-by-survey-by-cohort, and state-by-quartile-by-cohort fixed effects. Similarly, we replace the school district and year fixed effects in equation (2) with district-by-cohort, and year-by-cohort fixed effects.

Figure A1 presents event study estimates based on our stacked event study specification. The results are nearly identical to those reported in Figure 3, suggesting our basic event study estimates are robust. Table A4 presents DDD estimates for our outcomes of interest based on our stacked specification. Once again, the results are quite similar to those reported in Table 4.

Our second robustness check focuses on whether our results could potentially be driven by pre-existing trends in our outcomes of interest. Specifically, it is possible that high school graduation rates, college enrollment, and volunteering were all trending upward prior to the year a state enacted a school finance reform, which would violate the parallel trend assumption that underlies all causal claims based on DD models. The standard approach to testing for pre-trends is to estimate event study specifications for the outcomes of interest. However, in our case, that is not possible due to the nature of our data. Specifically, because we only observe three cohorts of students (specific to each of the three NCES surveys that we employ), we do not have enough observations prior to treatment to establish a trend.

In light of this constraint, we make two adjustments to our preferred analytic sample and empirical specification given by equation (1). First, we add data on one additional cohort of students who graduated high school prior to any of the school finance reforms in our sample to allow us to identify a pre-trend in our outcomes. Specifically, we add data from the High School & Beyond (HS&B) survey of 1980 to our sample, which includes a cohort of students who were sophomores in high school in 1980. Note, however, that unlike the other three NCES surveys we utilize, the HS&B survey does not contain an identifier for the school district in which a student resides and thus does not allow us to assign individual school districts to quartiles of 1980 household income.[16] To overcome that issue, we use information on each student's socioeconomic status contained in the HS&B survey to define within-state income quartiles. Specifically, we first, construct a school-level median socioeconomic status variable by taking the median of each student's socioeconomic status within a given school and state. We then divide schools in the HS&B survey into within-state quartiles based on each school's median socioeconomic status value.[17]

The second adjustment we make is to the empirical specification given by equation (1). Specifically, to examine pre-trends in our outcomes, we adjust equation (1) and follow Rothstein & Schanzenbach (2022) by estimating models of the following form:

$$y_{isjq} = \beta_1(T^{q=1} \cdot SFR_s) + \beta_2(T^{q=1} \cdot SFR \cdot Exposure_{is}) + \beta_3 Exposure_{is} + X_{isj}\alpha + \kappa_s + \rho_j + \lambda_{sq} + \Theta_{jq} + \mu_{isjq}, \quad (3)$$

[16] In fact, the survey does not include an identifier for the state in which a student resides. Fortunately, however, we were able to obtain information on the state in which each student resides from Josh Goodman who generously provided us with a crosswalk between students in the HS&B survey and states. See Goodman (2019) for details. The lack of any information on the geographic location of students in the HS&B survey is why we do not use data from the HS&B survey in our preferred specifications.

[17] Note that the socioeconomic status variable contained in the HS&B survey is an index based on several family attributes, including mother and father's education, father's occupation and other factors.

where κ_s and ρ_j are vectors of state and survey fixed effects, respectively, and all other terms are as defined in equation (1). Thus, equation (3) is nearly identical to equation (1), except it replaces the state-by-survey fixed effects with separate state and survey fixed effects and adds the direct effect of years of exposure to a school finance reform to the model, which can take on both negative (pre-SFR years) and positive (post-SFR years) values.[18] This essentially changes our identification strategy from a triple difference to a difference-in-differences. As noted by Rothstein & Schanzenbach (2022), β_3, the estimated coefficient on the linear trend variable capturing the number of years prior to and after a SFR, captures any pre-existing trends in our outcomes of interest between states that enacted a SFR in a given year and those that did not. Thus, β_3 allows us to test whether our outcomes of interest in states that enacted a SFR were trending higher prior to the enactment of the reform.

Before presenting results based on equation (3), we first show that we can replicate our core results when we add data from the HS&B survey but maintain the fixed effect structure given by equation (1). Specifically, Panel 1 of Table 5 presents estimates based on equation (1) that add data from the HS&B survey to our sample. With the exception of the estimates in column 3 (ever volunteered), which are smaller in magnitude when we add data from the HS&B survey, our core results for educational attainment and volunteering are quite robust. Having established that we can replicate most of our core results when we add the HS&B survey in Panel 2 of Table 5, we present estimates based on equation (3) that allows us to examine potential pre-trends in our outcomes of interest. Only one of the estimated coefficients on the linear trend variable (Exposure) is statistically significant, and even in that case, it is negative rather than positive. Thus, the results reported in Table 5 provide evidence that our core results are not being driven by positive pre-existing trends in our outcomes of interest.

Finally, in Table 6 we present estimates based on falsification tests where we move the actual date of a school finance reform back 10 years and drop all observations that occur after the actual date of the reform.[19] Relative to the results reported in Table 4, none of the estimates on either the “jump” variable or the years of exposure to a SFR variable are statistically significant

[18] Note that our Exposure variable is equivalent to the linear trend variable in Rothstein & Schanzenbach (2022).
[19] In order to implement these falsification tests, we once again augment our preferred sample with data from the HS&B survey because many of the school finance reforms in our sample occurred in between the NELS 1988 and the ELS 2002. Thus, if we move the actual date of a SFR back 10 years we would have no pre-treatment observations for many of the states in our sample if we did not include the HS&B survey.

and nearly all of the estimates are considerably smaller in magnitude than the estimates in Table 4. Thus, the results reported in Table 6 provide additional evidence that our core results have a causal interpretation.

D. Heterogeneity Analysis

In Table 7 we examine potential heterogeneity by gender and race/ethnicity in the impact of school finance reforms on our outcomes of interest. Specifically, Table 7 presents estimates based on specifications where we add interaction terms between the two treatment variables and an indicator for female students (Panel A) and an indicator for nonwhite students (Panel B) for outcomes where we find significant effects in Table 4. As shown in the top panel of Table 7, we find little evidence of any heterogeneous treatment effects based on gender: none of the estimated coefficients on the interaction terms are statistically significant, and nearly all of our main treatment effects are relatively stable and similar to those reported in Table 4. Turning to the results reported in Panel B, we do find some evidence of heterogeneity in the probability of ever volunteering based on race/ethnicity. Specifically, column 3 in Panel 2 suggests that effects for whites on volunteering happen relatively quickly after a school finance reform, while the trend in effect size over time is driven primarily by the minority population. However, given the number of tests with only one significant difference, these results may be driven simply by type 1 error.

V. Conclusion

One of the most frequently cited justifications for the public provision of K-12 education and the large subsidies provided by state governments to the financing of school spending is that education enhances civic engagement. Despite that fact, to date, very little evidence exists on whether and how K-12 school spending affects civic engagement. In this paper, we provide some of the first causal evidence on how exogenous increases in K-12 school spending impact civic engagement. We exploit the plausibly exogenous shocks to school spending induced by school finance reforms to estimate difference-in-difference-in-differences models designed to isolate the causal impact of K-12 school spending on educational attainment and civic engagement. Consistent with the results of Jackson, Johnson, & Persico (2016), Hyman (2017), and Rothstein & Schanzenbach (2022) we first show that SFRs led to an increase in high school graduation rates and college attendance using our data and model. Thus, our paper provides additional evidence from a unique and different data source that school spending has a positive impact on educational attainment.

After replicating earlier findings, we then re-examine the question of whether education influences voting behavior. Unlike the previous literature, however, we focus on the effect of school spending, which we believe ties more closely to a core justification for large public investments in education. We find that increases in school spending have no impact on voter registration or voting. These findings are consistent with Berinsky & Lenz (2011) and Kam & Palmer (2008) who find no impact of educational attainment on political participation, but are in contrast to the results in two well-known studies within economics by Dee (2004) and Milligan, Moretti, & Oreopoulos (2004) who find that political participation increases with educational attainment.

Turning to volunteerism, we find that the increases in school spending induced by school finance reforms led to statistically significant and economically meaningful increases in the propensity of students to volunteer and the hours spent volunteering per week. To our knowledge, this is the first study that directly examines the impact of education on volunteering. Our finding that increases in school spending lead to increases in volunteerism suggests that public investments in education can meaningfully improve civic engagement. In that sense, our results give credence to the long held (but largely untested in areas outside of political participation) belief that schooling enhances social capital and civic engagement.

`

References

Biasi, Barbara. School finance equalization increases intergenerational mobility, *Journal of Labor Economics,* forthcoming January 2023.

Berinsky, A. J., & Lenz, G. S. (2011). Education and political participation: Exploring the causal link. *Political Behavior*, 33(3), 357-373.

Brunner, E., Hyman, J., & Ju, A. (2020). School finance reforms, teachers' unions, and the allocation of school resources. *Review of Economics and Statistics*, 102(3), 473-489.

Callaway, B., & Sant'Anna, P. H. (2021). Difference-in-differences with multiple time periods. *Journal of Econometrics*, 225(2), 200-230.

Cengiz, D., Dube, A., Lindner, A., & Zipperer, B. (2019). The effect of minimum wages on low-wage jobs. *The Quarterly Journal of Economics*, *134*(3), 1405-1454.

Costa, D. L., & M. E. Kahn. (2004). Civic engagement and community heterogeneity: An Economist's Perspective. *Perspectives on Politics*, 1(1), 103-111.

Dee, T. S. (2020). Education and civic engagement. In *The Economics of Education (Second Edition): A Comprehensive Overview* (Eds. Bradley and Green), 103-108. London: Academic Press.

Dee, T. S. (2004). Are there civic returns to education? *Journal of Public Economics*, 88(9-10), 1697-1720.

Downes, T. A., & Figlio, D. N. (1997). School finance reforms, tax limits, and student performance: Do reforms level up or dumb down? (pp. 418-443). Institute for Research on Poverty, University of Wisconsin--Madison.

Goodman, J. (2019). The labor of division: Returns to compulsory high school math coursework. *Journal of Labor Economics*, 37(4), 1141-1182.

Goodman-Bacon, A. (2021). Difference-in-differences with variation in treatment timing. *Journal of Econometrics*, 225(2), 254-277.

Hyman, J. (2017). Does money matter in the long run? Effects of school spending on educational attainment. *American Economic Journal: Economic Policy*, 9(4), 256-80.

Jackson, C. K., Johnson, R. C., & Persico, C. (2016). The effects of school spending on educational and economic outcomes: Evidence from school finance reforms. *The Quarterly Journal of Economics*, 131(1), 157-218.

Kam, C. D., & Palmer, C. L. (2008). Reconsidering the effects of education on political participation. The *Journal of Politics*, 70(3), 612-631.

Lafortune, J., Rothstein, J., & Schanzenbach, D. W. (2018). School finance reform and the distribution of student achievement. *American Economic Journal: Applied Economics*, 10(2), 1-26.

Lochner, L. (2011). Nonproduction Benefits of Education: Crime, Health, and Good Citizenship. Handbook of the Economics of Education, 4, 183.

Milligan, K., Moretti, E., & Oreopoulos, P. (2004). Does education improve citizenship? Evidence from the United States and the United Kingdom. *Journal of Public Economics*, 88(9-10), 1667-1695.

Nie, N. H., Junn, J., & Stehlik-Barry, K. (1996). Education and democratic citizenship in America. University of Chicago Press.

Odden, A. (1993). School finance reform in Kentucky, New Jersey and Texas. *Journal of Education Finance*, 18(4), 293-317.

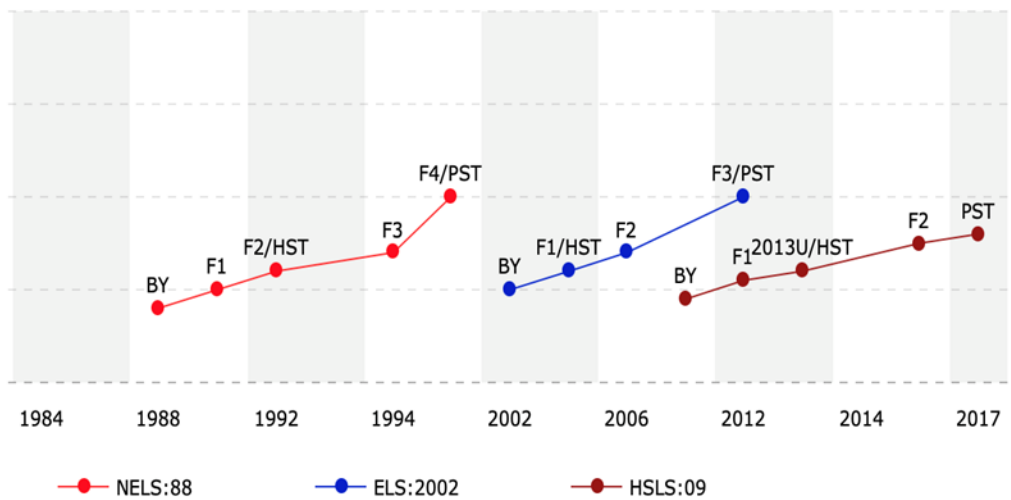
Putnam, R. D. (2001). Civic disengagement in contemporary America. *Government and Opposition*, *36*(2), 135-156.

Rothstein, J., & Schanzenbach, D. W. (2022). Does money still matter? Attainment and earnings effects of post-1990 school finance reforms. *Journal of Labor Economics*, 40(S1), S141-S178.

Sondheimer, R. M., & Green, D. P. (2010). Using experiments to estimate the effects of education on voter turnout. *American Journal of Political Science*, 54(1), 174-189.

Sun, L., & Abraham, S. (2021). Estimating dynamic treatment effects in event studies with heterogeneous treatment effects. *Journal of Econometrics*, 225(2), 175-199.

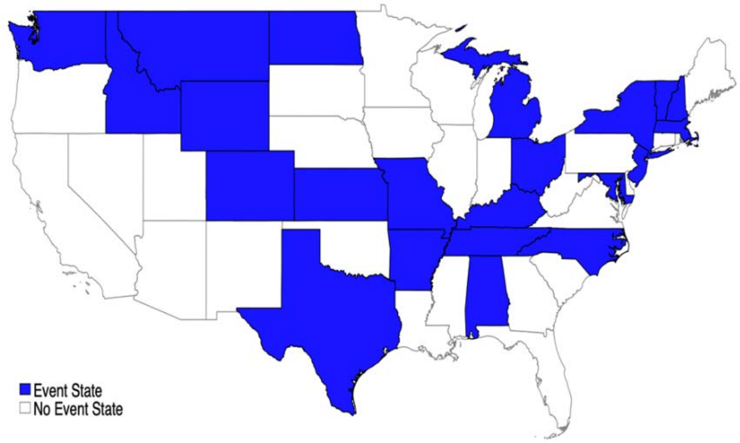
Figure 1
Research Design for the NCES High School Cohorts



Notes: NELS:88 denotes the National Education Longitudinal Study of 1988, ELS: 2002 denotes the Education Longitudinal Study of 2002, and HSLS:09 denotes the High School Longitudinal Study of 2009. BY denotes the base year of the survey, F1-F4 denotes 1st through 4th follow-up years, and PST and HST denote high school and post-secondary transcripts, respectively.

Figure 2

Panel A: Geographic Distribution of School Finance Reforms



Panel B: Geographic Distribution of School Districts Represented in NCES High School Surveys

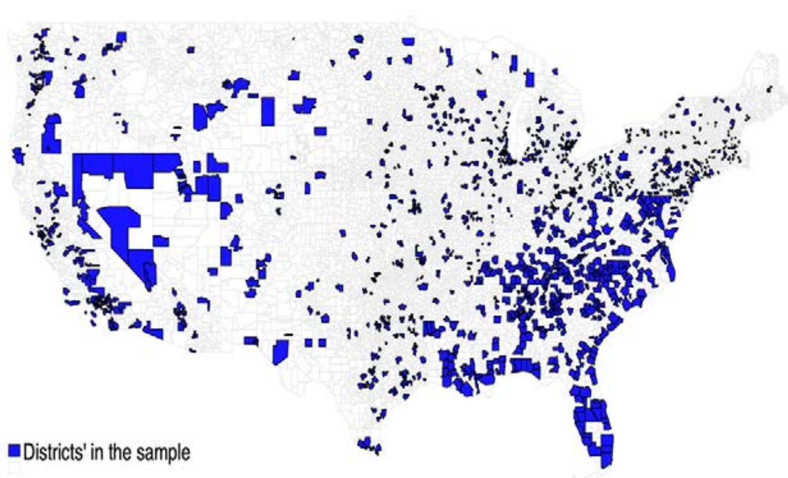
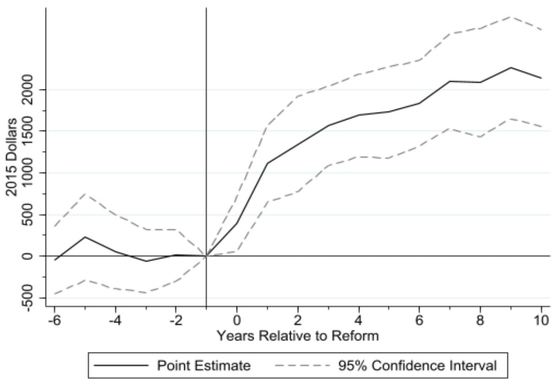
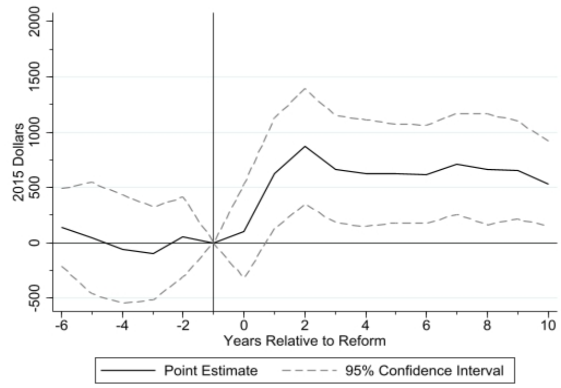


Figure 3
School Finance Reforms and District Resources: Event Study Estimates

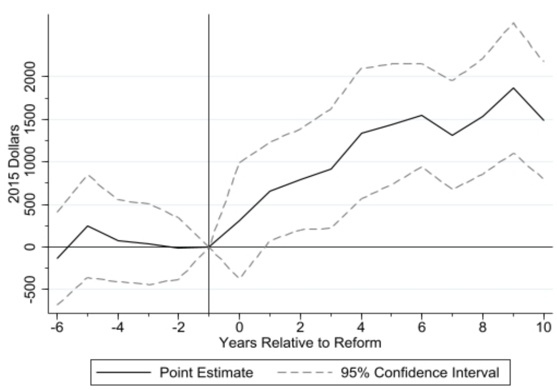
A: State Aid Bottom Income Quartile, Q1



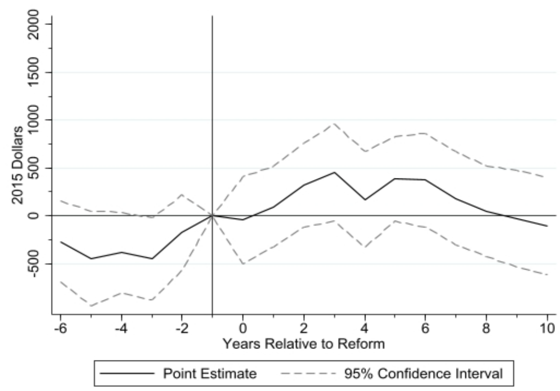
B: State Aid Top Income Quartile, Q4



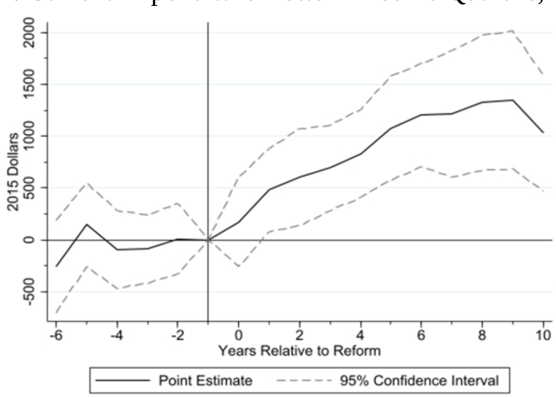
C: Total Expenditure Bottom Income Quartile, Q1



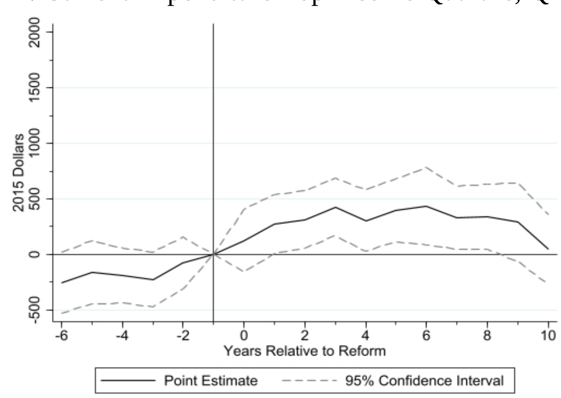
D: Total Expenditure Bottom Income Quartile, Q4



E: Current Expenditure Bottom Income Quartile, Q1



F: Current Expenditure Top Income Quartile, Q4



Notes: Figures show event study estimates of the effects of school finance reforms on per-pupil current expenditure, state aid, and total expenditure. Panels A, C, and E show the impact of school finance reforms on per-pupil state aid, total expenditure, and current expenditures for districts located in the 1st quartile of the within-state 1980 income distribution, while panels B, D, and F show the same information for districts located in the 4th quartile. All per-pupil spending variables are measured in 2015 dollars. Solid lines are point estimates, and dashed lines are 95% confidence intervals.

Table 1
Summary Statistics

	Full Sample		Quartile 1		Quartile 4	
	Mean	St. Dev	Mean	St. Dev	Mean	St. Dev
	(1)	(2)	(3)	(4)	(5)	(6)
Main Outcomes						
High School Graduate	0.887	(0.316)	0.858	(0.349)	0.903	(0.296)
Post Secondary Attendance	0.684	(0.465)	0.620	(0.485)	0.737	(0.440)
Voluntary Hours	1.587	(4.107)	1.564	(4.240)	1.613	(4.012)
Ever Volunteered	0.365	(0.481)	0.327	(0.469)	0.403	(0.490)
Register to Vote	0.628	(0.483)	0.613	(0.487)	0.645	(0.479)
Vote in Presidential Election	0.461	(0.500)	0.419	(0.493)	0.496	(0.500)
Baseline Characteristics						
Mother completed high school	0.845	(0.362)	0.794	(0.404)	0.891	(0.311)
Father completed high school	0.836	(0.370)	0.780	(0.414)	0.885	(0.319)
Gender: Female	0.498	(0.500)	0.500	(0.500)	0.501	(0.500)
Race: Non-White	0.421	(0.494)	0.491	(0.500)	0.393	(0.488)
Family Income	87,655	(75,817)	66,376	(58,610)	105,821	(87,109)
Per-Pupil Spending and SFR Indicators						
Total Spending	10,634	(3,295)	11,632	(3,647)	10,550	(3,163)
Current Spending	9,097	(2,737)	10,124	(3,049)	8,930	(2,643)
State Aid	4,991	(2,085)	6,191	(2,679)	4,262	(1,563)
Total Revenue	10,450	(3,227)	11,468	(3,523)	10,334	(3,082)
% Exposed to SFR	0.329	(0.470)	0.345	(0.475)	0.341	(0.474)
Avg years exposed to SFR	3.288	(6.023)	3.266	(5.953)	3.367	(6.053)
Years exposed to SFR if >0	10.004	(6.572)	9.459	(6.637)	9.884	(6.570)
Number of School Districts	1444		243		507	

Notes: Individual-level data are from a combination of the National Education Longitudinal Survey 1988 (NELS:88), Education Longitudinal Survey 2002 (ELS:02), and High School Longitudinal Survey 2009 (HLS:09). School district-level per-pupil spending data are from the 1987-2012 National Center for Education Statistics, Local Education Agency Finance Survey (F33). School district median household income in 1980 data are used to define districts' income quartiles within a state. Table presents summary statistics for primary outcomes, individuals' baseline characteristics, spending per student, and years of exposure to a school finance reform. Education and civic engagement outcomes are measured two years after high school graduation. Columns 1-2 present the means and standard deviations for the full sample, while columns 3-4 and 5-6 present separate summary statistics for the 1st quartile (lower-income quartile) and 4th quartile (upper-income quartile) sample.

Table 2
School Finance Reform and Spending Per-pupil

	State Aid	Total Revenue	Total Spending	Current Spending
	(1)	(4)	(2)	(3)
Panel A: Outcomes with Controls				
SFR*Quartile 1*Years Exposed to SFR	15	121***	100***	81***
	(32)	(29)	(32)	(29)
SFR*Quartile 1	1,509**	1,045**	1,078**	329
	(654)	(398)	(431)	(389)
District Black Percentage	3,195**	3,071***	3,385***	3,051***
	(1274)	(934)	(1034)	(801)
District Urban Percentage	139	561**	423	726***
	(176)	(269)	(286)	(179)
District College Percentage	-4,724***	6,542***	7,184***	5,182***
	(900)	(839)	(725)	(822)
Panel B: Outcomes with no Controls				
SFR*Quartile 1*Years Exposed to SFR	20	141***	122***	100***
	(31)	(28)	(32)	(25)
SFR*Quartile 1	1,412**	565	564	-118
	(587)	(394)	(441)	(394)
Observations	26,939	26,939	26,939	26,939
State by Survey FE	YES	YES	YES	YES
State by Quartile FE	YES	YES	YES	YES
Quartile by Survey FE	YES	YES	YES	YES
Number of School Districts	750	750	750	750

Notes: The sample includes students attending districts located in the 1st (lower income) and 4th (upper income) quartiles of the within-state 1980 income distribution. Table reports the impact of the state's SFR event interacted with years of exposure and an indicator if a district is in the first quartile of the within-state 1980 income distribution in the first row, and the state's SFR event interacted with the first quartile indicator in the second row. Columns 1 and 2 present results based on specifications where the dependent variable is the real mean state aid per-pupil and real mean total revenue per-pupil, respectively. Columns 3 and 4 present results where the dependent variable is real mean total expenditures per-pupil and real mean current expenditure per-pupil, respectively. All specifications include state-by-year, state-by-quartile, and quartile-by-survey fixed effects. Panel A adds additional controls for the 1980 district fraction of the population black, fraction of urban, and fraction with a BA or higher, while panel B does not include district-level controls. Robust standard errors clustered at the state-by-quartile level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 3
Balancing Tests

	Mother's high school	Father's high school	Ln(Family Income)	Female
	(1)	(2)	(3)	(4)
SFR	-0.0172	-0.0229	0.0328	-0.0110*
	(0.0220)	(0.0220)	(0.0700)	(0.0070)
Observations	8,215	7,753	8,721	9,311
	White	Black	Hispanic	Other Race
	(5)	(6)	(7)	(8)
SFR	0.0286	-0.0083	0.0015	-0.0217
	(0.0630)	(0.0400)	(0.0540)	(0.0220)
Observations	8,823	8,823	8,823	8,823

Notes: Data are from the National Education Longitudinal Survey 1988 (NELS:88) and include students attending districts located in the 1st (lower income) and 4th (upper income) quartiles of the within-state 1980 income distribution. Table presents balancing tests for the sample of NELS:88 students' baseline characteristics. SFR is an indicator variable equals to 1 if a state had s school finance reform event and 0 otherwise. Columns 1-4 present balancing tests for individual covariates, including parents' education level, family income, and gender, while columns 5-8 show balancing tests for individual racial categories. Robust standard errors clustered at the state level are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 4
Impact of SFRs on Educational Attainment and Civic Engagement

	High School	Postsecondary	Ever Volunteered	Voluntary Hours	Register to Vote	Vote in Election
	(1)	(2)	(4)	(3)	(5)	(6)
Panel A. Outcomes with Controls						
SFR*Quartile 1*Years Exposed to SFR	0.0043***	0.0036**	0.0047*	0.0722***	-0.0008	-0.0001
	(0.0010)	(0.0020)	(0.0030)	(0.0170)	(0.0030)	(0.0060)
SFR*Quartile 1	0.0063	0.0265	0.0606	-0.3122	0.1007**	0.0087
	(0.0180)	(0.0260)	(0.0380)	(0.3300)	(0.0450)	(0.0460)
Mother's high school	0.0818***	0.1353***	0.0690***	0.3597**	0.0885***	0.0985***
	(0.0110)	(0.0180)	(0.0110)	(0.1400)	(0.0190)	(0.0150)
Father's high school	0.0685***	0.1468***	0.0814***	0.2734*	0.0598***	0.0711***
	(0.0090)	(0.0150)	(0.0120)	(0.1540)	(0.0140)	(0.0130)
Ln(Family Income)	0.0305***	0.0670***	0.0460***	0.0085	0.0279***	0.0355***
	(0.0040)	(0.0070)	(0.0050)	(0.0440)	(0.0050)	(0.0050)
Non-White	-0.0042	0.0267***	0.0398***	0.3697***	-0.0423***	-0.0944
	(0.0060)	(0.0090)	(0.0090)	(0.0920)	(0.0140)	(0.0140)
Female	0.0315***	0.0797***	0.0463***	-0.1506*	-0.0341***	0.0286***
	(0.0040)	(0.0070)	(0.0070)	(0.0800)	(0.0080)	(0.0080)
Panel B. Outcomes with no Controls						
SFR*Quartile 1*Years Exposed to SFR	0.0051***	0.0046**	0.0054*	0.0732***	0.0002	0.0026
	(0.002)	(0.002)	(0.003)	(0.016)	(0.004)	(0.0070)
SFR*Quartile 1	0.0116	0.0296	0.0613	-0.3542	0.0946*	0.0191
	(0.0200)	(0.0290)	(0.0410)	(0.3240)	(0.0520)	(0.0470)
Observations	22,399	18,660	17,617	11,862	11,876	11,150
State by Survey FE	YES	YES	YES	YES	YES	YES
State by Quartile FE	YES	YES	YES	YES	YES	YES
Quartile by Survey FE	YES	YES	YES	YES	YES	YES

Notes: The sample includes students attending school districts in the 1st (lower income) and 4th (upper income) quartiles of the within-state distribution of 1980 median income. Table reports the impact of the state's SFR event interacted with years of exposure and an indicator if a district is in the first quartile of the within-state 1980 income distribution in the first row, and the state's SFR event interacted with the first quartile indicator in the second row. Outcomes are 1) an indicator for whether a respondent graduated high school as measured in the first follow-up survey after a cohort was scheduled to graduate from high school; 2) an indicator for whether a respondent ever was enrolled in a post-secondary institution; 3) an indicator for whether a respondent had ever volunteered; 4) hours spent volunteering per week; 5) an indicator for whether a respondent was registered to vote; and 6) and an indicator for whether a respondent voted in the last presidential election. All specifications include state-by-survey, state-by-quartile, and quartile-by-survey year fixed effects. Panel A adds additional controls for the individual baseline characteristics, including parents' high school graduation status, family income, race, and gender, while panel B does not include individual covariates. Robust standard errors clustered at the state-by-quartile level are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 5
Replication Adding the HS&B 1980 Survey and Tests for Trends

	High School	Postsecondary	Ever Volunteered	Voluntary Hours	Register to Vote	**Vote in Election**
	(1)	(2)	(3)	(4)	(5)	(6)
			Standard Estimates with HSB 1980			
SFR*Quartile 1*Years Exposed to SFR	0.0038***	0.0034**	0.0012	0.0722***	-0.0015	-0.0027
	(0.0010)	(0.0020)	(0.0030)	(0.0170)	(0.0030)	(0.0060)
SFR*Tercile 1	0.0167	0.0034	0.0223	-0.3122	0.1065**	0.0064
	(0.0150)	(0.0230)	(0.0420)	(0.3300)	(0.0460)	(0.0410)
			Trend Estimates			
Years of Exposure	-0.0009	-0.0013*	-0.0006	-0.0087	-0.0017	0.0005
	(0.0010)	(0.0010)	(0.0010)	(0.0080)	(0.0010)	(0.0010)
Observations	26,827	23,138	21,867	11,862	16,132	15,400
State FE	YES	YES	YES	YES	YES	YES
Survey FE	YES	YES	YES	YES	YES	YES
State by Quartile FE	YES	YES	YES	YES	YES	YES
Quartile by Survey FE	YES	YES	YES	YES	YES	YES

Notes: The sample includes HSB 1980 survey in addition to our main sample in Table 4. Panel 1 reports our main results with the addition of the HSB 1980 to the sample. The second panel shows estimates on years of exposure trend variable that ranges between -30 and 20. Regressions include state, survey, state-by-quartile, and quartile-by-survey as fixed effects and individual covariates, including parents' high school graduation status, family income, and race. Robust standard errors clustered at the state-by-quartile level are in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 6
Falsification Tests

	High School	Postsecondary	Ever Volunteered	Register to Vote	Vote in Election
	(1)	(2)	(4)	(6)	(7)
SFR*Quartile 1*Years Exposed to SFR	0.0038	0.0006	0.0002	0.0059	-0.0026
	(0.002)	(0.007)	(0.008)	(0.004)	(0.0050)
SFR*Tercile 1	-0.0239	0.0197	0.0496	-0.0174	0.003
	(0.0200)	(0.0490)	(0.0570)	(0.0400)	(0.0390)
Observations	20,349	17,758	16,953	12,991	13,639
State by Survey FE	YES	YES	YES	YES	YES
State by Quartile FE	YES	YES	YES	YES	YES
Quartile by Survey FE	YES	YES	YES	YES	YES

Notes: The sample includes the HS&B 1980 survey in addition to our main sample in Table 4. Table reports falsification test results based on moving the year of a SFR back 10 years prior to the actual reform year. All specifications include state-by-year, state-by-quartile, and quartile-by-survey fixed effects and individual covariates, including parents' high school graduation status, family income, and race. Robust standard errors clustered at the state-by-quartile level are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

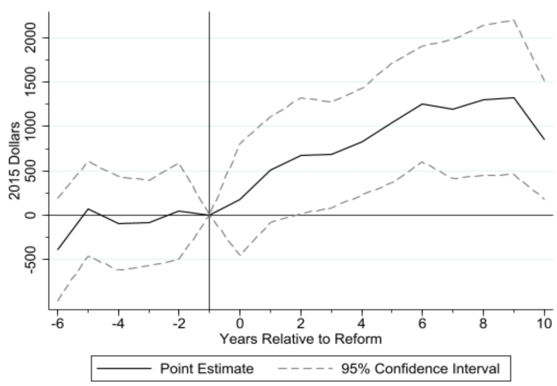
Table 7
Heterogeneity Analysis

	High School	Postsecondary	Ever Volunteered	Voluntary Hours	Register to Vote
	(1)	(2)	(3)	(4)	(5)
			Panel A: Gender		
SFR*Quartile 1*Years Exposed to SFR	0.0032*	0.0052**	0.0035	0.0709***	0.0007
	(0.0020)	(0.0020)	(0.0020)	(0.0230)	(0.0040)
SFR*Quartile 1	0.0186	-0.0163	0.0662**	-0.2020	0.0825*
	(0.0220)	(0.0260)	(0.0320)	(0.3250)	(0.0480)
SFR*Female	0.0023	-0.0031	0.0024	0.0032	-0.0027
	(0.0010)	(0.0030)	(0.0040)	(0.0240)	(0.0050)
SFR*Quartile 1*Female	-0.0240	0.0798***	-0.0102	-0.2186	0.0337
	(0.0160)	(0.0290)	(0.0450)	(0.2530)	(0.0500)
Observations	22,399	18,660	17,617	11,862	11,876
			Panel B: Race		
SFR*Quartile 1*Years Exposed to SFR	0.0037***	0.0043*	0.0013	0.0469**	-0.0022
	(0.0010)	(0.0030)	(0.0030)	(0.0230)	(0.0030)
SFR*Quartile 1	0.0204	0.0170	0.1028**	-0.0617	0.1038**
	(0.0150)	(0.0380)	(0.0400)	(0.4050)	(0.0450)
SFR*Nonwhite	0.0011	-0.0013	0.0067**	0.0550	0.0036
	(0.0020)	(0.0040)	(0.0030)	(0.0370)	(0.0030)
SFR*Quartile 1*Nonwhite	-0.0289	0.0187	-0.0842**	-0.5015	-0.0062
	(0.0280)	(0.0470)	(0.0380)	(0.5260)	(0.0390)
Observations	22,399	18,660	17,617	11,862	11,876
Controls	YES	YES	YES	YES	YES
State by Survey FE	YES	YES	YES	YES	YES
State by Quartile FE	YES	YES	YES	YES	YES
Quartile by Survey FE	YES	YES	YES	YES	YES

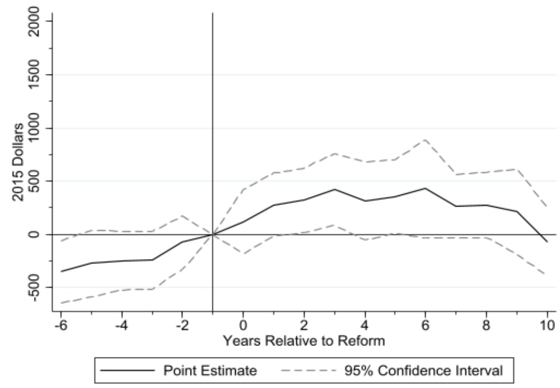
Notes: The sample includes students attending school districts in the 1st (lower income) and 4th (upper income) quartile of the within-state distribution of 1980 median income. Panel A presents estimates of the impact of SFRs on outcomes of interest, including treatment interactions with an indicator for female students, while panel B presents the same information, except treatment interactions are with an indicator for nonwhite students. All specifications include state-by-survey, state-by-quartile, and quartile-by-survey fixed effects and the controls listed in Table 4. Robust standard errors clustered at the state-by-quartile level are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Figure A1
School Finance Reforms and District Resources: Stacked DD Event Study Estimates

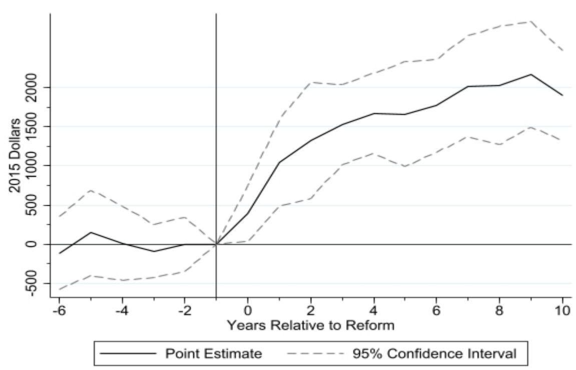
A: Current Expenditure Bottom Income Quartile, Q1



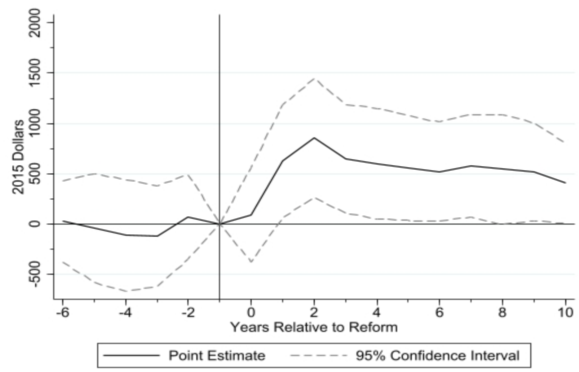
B: Current Expenditure Top Income Quartile, Q4



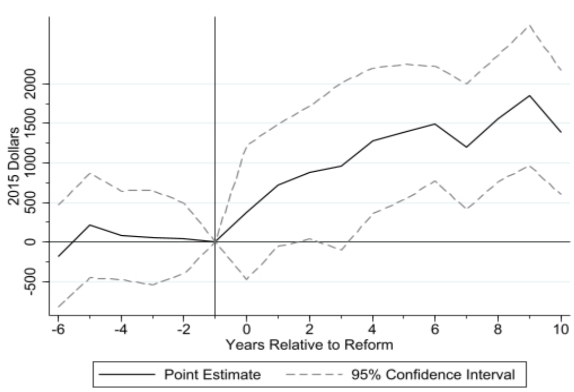
A: State Aid Bottom Income Quartile, Q1



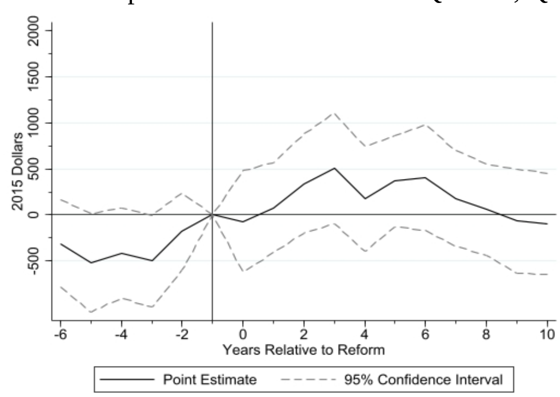
B: State Aid Top Income Quartile, Q4



A: Total Expenditure Bottom Income Quartile, Q1



B: Total Expenditure Bottom Income Quartile, Q4



Notes: Figures show event study estimates from a stacked difference-in-differences specification Panels A, C, and E present estimates of the effect of school finance reforms on current expenditure, state aid, and total expenditure among district located in the 1st quartile of the 1980 within-state income distribution, while panels B, D, and F present the same information for districts located in the 4th quartile of the 1980 within-state income distribution. All per-pupil revenue and spending variables are measured in 2015 dollars. Solid lines are point estimates, and dashed lines are 95% confidence intervals.

Table A1
Outcome Definitions

Outcome Variables	Definition
High School	Indicator equal to 1 if respondent had graduated high school as measured in the first follow-up survey after a cohort was scheduled to graduate from high school
Postsecondary	Indicator equal to 1 if a respondent ever was enrolled in a post-secondary institution at age 20
Voluntary Hours	Number of hours the respondent spent on voluntary activities in a week at ge 20
Ever Volunteered	Indicator equal 1 if the respondent ever attended a voluntary activity at age 20
Register to Vote	Indicator equal to 1 if the respondent registered to vote at age 20
Vote in Election	Indicator equal 1 if the respondent voted in the last presidential election at age 20

Table A2
Outcomes of Surveys and Timing of Measurement

	HS&B :1980	NELS: 1988	ELS: 2002	HSLS: 2009
High School	2 years after high school in 1984	2 years after high school in 1994	2 years after high school in 2006	3 years after high school in 2016
Postsecondary	2 years after high school in 1984	2 years after high school in 1994	2 years after high school in 2006	3 years after high school in 2016
Voluntary Hours		2 years after high school in 1994		3 years after high school in 2016
Ever Volunteered	2 years after high school in 1984	2 years after high school in 1994	2 years after high school in 2006	3 years after high school in 2016
Register to Vote	2 years after high school in 1984	2 years after high school in 1994		3 years after high school in 2016
Vote in Election	2 years after high school in 1984	2 years after high school in 1994	2 years after high school in 2006	

Notes: HS&B:1980 began tracking 10th graders in 1980; for example, students were surveyed two years after high school, in 1984, about their high school graduation status and other variables listed in the table. NELS:1988 consists of 8th graders first surveyed in 1988, while ELS:2002 and HSLS:2009 consist of a group of 10th graders first surveyed in 2002 and 9th graders first surveyed in 2009, respectively.

Table A3
School Finance Reforms

Fips Code	SFR Year	State
1	1995	Alabama
5	2005	Arkansas
8	1995	Colorado
16	1994	Idaho
20	2006	Kansas
21	1991	Kentucky
24	2003	Maryland
25	1993	Massachusetts
26	1994	Michigan
29	2007	Missouri
30	2006	Montana
33	1999	New Hampshire
34	1998	New Jersey
36	2006	New York
37	1997	North Carolina
38	2009	North Dakota
39	1998	Ohio
47	1997	Tennessee
48	1991	Texas
50	1999	Vermont
53	2011	Washington
56	1996	Wyoming

Notes: List includes all school finance reforms used in the analyses.

Table A4
Impact of SFRs on Educational Attainment and Civic Engagement: Stacked DD Estimates

	High School	Postsecondary	Ever Volunteered	Voluntary Hours	Register to Vote	Vote in Election
	(1)	(2)	(3)	(4)	(5)	(6)
SFR*Quartile 1*Years	0.0040***	0.0033*	0.0055**	0.0752***	-0.0007	-0.0018
Exposed to SFR	(0.001)	(0.002)	(0.002)	(0.016)	(0.003)	(0.0060)
SFR*Quartile 1	0.0061	0.0325	0.0694*	-0.2801	0.1015**	-0.0082
	(0.0180)	(0.0210)	(0.0380)	(0.3230)	(0.0440)	(0.0500)
Observations	161,564	135,088	127,649	84,077	84,169	83,105
State by Survey FE	YES	YES	YES	YES	YES	YES
State by Quartile FE	YES	YES	YES	YES	YES	YES
Quartile by Survey FE	YES	YES	YES	YES	YES	YES

Notes: The sample includes students attending school districts in the 1st (lower income) and 4th (upper income) quartile of the within-state distribution of 1980 median income. Table reports stacked DiD estimates of the impact of the state's SFR event interacted with years of exposure and an indicator if a district is in the first quartile of the within-state 1980 income distribution in the first row, and the state's SFR event interacted with the first quartile indicator in the second row. All specifications include state-by-survey, state-by-quartile, and quartile-by-survey year fixed effects along with controls for the individual baseline characteristics, including parents' high school graduation status, family income, race, and gender. Robust standard errors clustered at the state-by-quartile level are in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A5
Impact of SFRs on Main Outcomes: Michigan Income Quartiles

	High School	Postsecondary	Ever Volunteered	Voluntary Hours	Register to Vote	Vote in Election
	(1)	(2)	(4)	(3)	(5)	(6)
Panel A. Outcomes with Controls						
SFR*Quartile 1*Years	0.0036**	0.0040**	0.0049*	0.0724***	-0.0019	-0.0027
Exposed to SFR	(0.0010)	(0.0020)	(0.0030)	(0.0160)	(0.0030)	(0.0070)
SFR*Quartile 1	-0.0125	0.0081	0.0347	-0.3094	0.0874*	-0.0448
	(0.0210)	(0.0270)	(0.0400)	(0.3160)	(0.0490)	(0.0640)
Mother's high school	0.0808***	0.1379***	0.0699***	0.3912***	0.0881***	0.0994***
	(0.0110)	(0.0180)	(0.0110)	(0.1290)	(0.0190)	(0.0150)
Father's high school	0.0700***	0.1455***	0.0807***	0.2620*	0.0664***	0.0744***
	(0.0090)	(0.0140)	(0.0120)	(0.1510)	(0.0150)	(0.0140)
Ln(Family Income)	0.0296***	0.0670***	0.0467***	0.0018	0.0265***	0.0354***
	(0.0040)	(0.0060)	(0.0050)	(0.0430)	(0.0050)	(0.0050)
Non-White	-0.0053	0.0265***	0.0416***	0.3748***	-0.0417***	-0.0990
	(0.0060)	(0.0090)	(0.0090)	(0.0910)	(0.0140)	(0.0130)
Female	0.0325***	0.0786***	0.0476***	-0.1368*	-0.0305***	0.0308***
	(0.0040)	(0.0060)	(0.0070)	(0.0780)	(0.0070)	(0.0090)
Panel B. Outcomes with no Controls						
SFR*Quartile 1*Years	0.0042**	0.0046**	0.0053*	0.0719***	-0.0013	-0.0003
Exposed to SFR	(0.0020)	(0.0020)	(0.0030)	(0.0160)	(0.0040)	(0.0080)
SFR*Quartile 1	-0.0095	0.0072	0.0327	-0.3687	0.0779	-0.0405
	(0.0250)	(0.0320)	(0.0440)	(0.3150)	(0.0570)	(0.0710)
Observations	22,713	18,937	17,865	12,071	12,083	11,281
State by Survey FE	YES	YES	YES	YES	YES	YES
State by Quartile FE	YES	YES	YES	YES	YES	YES
Quartile by Survey FE	YES	YES	YES	YES	YES	YES

Notes: Sample includes students attending school districts in the 1st (lower income) and 4th (upper income) quartile of the within-state distribution of 1980 median income including Michigan. All specifications include state-by-survey, state-by-quartile, and quartile-by-survey fixed effects. Panel A adds additional controls for the individual baseline characteristics, including parents' high school graduation status, family income, race, and gender, while panel B does not include individual covariates. Robust standard errors clustered at the state-by-quartile level are in parentheses.
*** p<0.01, ** p<0.05, * p<0.1.