



Paying for School Finance Reforms: How States Raise Revenues to Fund Increases in Elementary-Secondary Education Expenditures

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Elementary-Secondary Education Expenditures**


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Abstract

This study investigates how individual states raise revenue to pay for elementary-secondary education spending after a school finance reform (SFR). We consider 24 states that implemented SFRs between 1989 and 2005. Using a synthetic control approach, we identify six case-study states (Arkansas, Kansas, Maryland, Michigan, New Hampshire, and Vermont) that increased and sustained education expenditures after reform. We then searched for legislative statutes that appropriated funding for increased education spending and identified how policymakers intended to fund the SFR. Five states—AK, KS, MI, NH, and VT—paid for increased education expenditures by altering tax rates and changing tax revenue sources. A common feature among these five states is that they increased their control over the management of property tax revenues.

Keywords: educational finance, state aid, school finance reforms

Paying for School Finance Reforms: How States Raise Revenues to Fund Increases in Elementary-Secondary Education Expenditures

School finance reforms (SFRs) are typically defined as state-level education reforms resulting from court rulings or legislative statutes. Many of these reforms have induced states to either revise school funding formulas to provide equitable funding to school districts, increase state funding for elementary-secondary public education, or both (Jackson, 2020; Lafortune et al., 2018; Shores et al., 2023).

As research demonstrates, SFRs, on average, have led to increased education spending in public school districts across the United States, particularly among districts serving economically disadvantaged students (Lafortune et al., 2018; Liscow, 2018; Shores et al., 2023; Sims, 2011). And this additional money matters for student outcomes (Jackson, 2020). Spending increases induced by SFRs have led to improvements in test scores (Lafortune et al., 2018), graduation rates (Candelaria & Shores, 2019), and adult outcomes, such as lower poverty incidence (Jackson et al., 2016) and upward intergenerational income mobility (Biasi, 2023).

The primary driver of increased district spending after an SFR is the expansion of state-level expenditures for elementary-secondary education (Card & Payne, 2002; Lafortune et al., 2018; Liscow, 2018; Shores et al., 2023). When state governments spend more on education, local school districts, which are beneficiaries of state elementary-secondary education expenditures, receive these funds as state aid revenues. And as a response to this increased state aid, school districts typically spend more on education, a phenomenon known as the flypaper effect (Card & Payne, 2002; Shores et al., 2023). Of course, other district-level revenue accounts—federal and local revenues—might also induce more district spending after an SFR;

however, there is no evidence that these funds increase, on average, after reform (Lafortune et al., 2018; Shores et al., 2023).

Although state governments play an important role in increasing district spending, existing research does not systematically document and evaluate how individual states pay for SFRs. National-level studies find that state governments fund SFRs by raising taxes (Liscow, 2018) and not by reducing non-education-related state expenditures (Baicker & Gordon, 2006; Lafortune et al., 2018; Liscow, 2018; Murray et al., 1998). However, because these national studies leverage the differential timing of SFRs across states, they do not address state-specific heterogeneity. For example, it is not clear what types of taxes states choose to increase. And among state-level case studies that evaluate SFRs, some descriptively document how states pay for SFRs in a background section, while others estimate how they pay in an analysis section. Different methodologies across these state-specific studies make it difficult to have a benchmark for comparison across the states. For policymakers, the lack of standardized reporting across state-specific studies, along with national studies that do not document unique state actions, masks a feasible set of funding options for SFRs.

In this paper, we seek to understand how state governments pay for increases in state expenditures for elementary-secondary education. Addressing this question, however, comes with a challenge: not all documented SFRs increase state expenditures for education (Shores et al., 2023). One explanation for this lack of state investment could be legislative postponement in the allocation of funds; some legislatures take years to adequately appropriate funds after an SFR (Candelaria et al., 2022; Sun et al., 2024). Alternatively, state legislatures might opt to redistribute funds from low-poverty to high-poverty districts without increasing funding. This scenario could happen if a state government assumes full funding control from local

governments but does not have the fiscal capacity to increase state expenditures on education (Manwaring & Sheffrin, 1997; Silva & Sonstelie, 1995).

To ensure we can address our core question, we first identify a purposive case-study analytic sample by selecting states that increased and sustained elementary-secondary education spending after a documented SFR. Next, we use document analysis and quantitative methods to examine how states pay for this increased spending. We summarize our two research questions below:

- RQ1. Which states increased and sustained higher levels of elementary-secondary education spending (i.e., state aid to school districts) after an SFR?
- RQ2. How did state legislatures pay for increased and sustained education spending after an SFR?

To address our first question (RQ1), we select a sample of states that increased and sustained elementary-secondary education spending—on a per pupil basis—in the years following their first documented reform in our sample period. To do this, we use U.S. Census Bureau state-level finance data from fiscal years (FYs) 1987-2007, a list of court-ordered and legislative SFRs that occurred in 24 states between FYs 1989-2005, and the ridge augmented synthetic control method (Ben-Michael et al., 2021), which we refer to as ridge ASCM. We find that six out of 24 state SFRs increased per pupil state education expenditures at the 5 percent significance level in the years following reform. Our selected analytic sample includes the following six states: Arkansas (1995), Kansas (1992), Maryland (1997), Michigan (1994), New Hampshire (1994), and Vermont (1997)—parentheses denote the FY of the SFR.

To address our second question (RQ2), we begin by searching for the legislative acts that were passed around the same time as—and often because of—the documented SFR in each of

our six states. As we discuss further in our methods section, we identified the legislative acts for our case study states by referencing a set journal articles, books, and websites commonly used in the school finance literature. We ultimately found that four of our six state SFRs were the direct result of a documented legislative statute, requiring no additional search. We used our defined search process to find statutes for the two remaining states, all of which had court-ordered reforms.

We then conduct a document analysis and use the ridge ASCM to assess how state legislatures paid for the reforms. First, we analyzed each legislative statute to record what changes to state finances were enacted (e.g., increasing state sales tax rates) to fund proposed increases in education spending. We then assessed our legislative evidence by quantitatively examining the effect of a state's SFR on the revenues and/or expenditures that were specified in legislation as being adjusted to fund increased education spending. The idea is simple: if a state, for example, funds increased education spending by increasing sales tax revenues, then there should be both legislative evidence that an increased sales tax rate was enacted and financial evidence that the state collected increased sales tax revenues. Like the analysis conducted to identify the selected analytic sample of state SFRs, we use U.S. Census Bureau finance data from 1982-2007 and the ridge ASCM to examine the effect of a state's SFR on various state revenues and expenditures.

In our case study of six state SFRs, we discovered that five were funded through adjustments to tax rates and revenue sources. Although these five states varied in the sources of revenue where rate adjustments occurred, they all adopted a similar strategy: increasing their control over property tax revenue management. Among all the states, we also found no

conclusive evidence that non-education state expenses were reduced or that state debt was increased to finance increased elementary-secondary education spending after a reform.

Overall, this study informs policymakers about different approaches for supporting education spending in their states, especially since increased spending can promote equity goals. We also highlight complementary policy considerations beyond funding an SFR—that is, distributing state aid equitably and accounting for the preferences of local taxpayers—in our discussion section. Our study proceeds as follows: we first briefly review the history of SFRs and what is currently known about the effects of SFRs on education spending and state finances to frame the study. Next, we describe the analytic approaches employed, followed by the results of our analysis. We close with a discussion section and the conclusion.

School Finance Reforms and their Fiscal Impacts

In this section, we provide an overview of SFRs and discuss their impacts on both education spending and state finances. Although our study privileges reforms between 1989 and 2005, which is part of the so-called adequacy era, we provide the broader context of SFRs, beginning with the equity era in the 1970s. Next, we summarize the literature on how SFRs affect education spending and discuss the heterogeneity of spending effects across states—some states increased spending while others did not. Finally, we discuss what is known about the impact of SFRs on state finances—whether SFRs affect non-education expenditures and how states pay for reforms, on average.

School Finance Reforms

The history of state-level SFRs is often divided into two periods: the “equity” era and the “adequacy” era. Across both eras, court-ordered and legislative reforms have induced many states to revise their funding formulas; however, not every SFR has been successful.

The equity era began in the 1970s when unequal education funding based on property taxes led citizens to file litigation against state governments in efforts to *equalize* funding. Equity cases were built on the belief that states were responsible for funding per-pupil education spending equally across districts so that all children had an equal opportunity to succeed in the education system. Reforms during the equity era often resulted in increased state funding of education in districts with low property tax bases. However, by the mid to late 1980s, equity arguments were becoming less successful in state courthouses and statehouses (Candelaria & Shores, 2019; Corcoran & Evans, 2014; Hanushek & Lindseth, 2009).

Since the late 1980s, SFRs have shifted to focusing on the *adequacy* of school funding to provide students, especially those from disadvantaged backgrounds, the ability to achieve some minimum threshold of academic proficiency (Odden & Picus, 2020). Reforms based on adequacy grounds can require increased education funding for all districts in a state since all schools could be inadequately funded. In addition, adequacy reforms can target larger spending increases to districts serving economically disadvantaged students in order to compensate for the out-of-school inequalities that disadvantaged students face. The first SFR of the adequacy era was in response to the 1989 Kentucky case *Rose v. Council for Better Education*; the majority of SFRs that occurred post-1989 were also based on adequacy grounds. Between 1989 and 2011, 74 legislative and court-ordered SFRs occurred in 27 states. Appendix Table A1 lists all documented SFRs that occurred between 1989 and 2011.

While SFRs typically result in state education finance systems being overturned and school funding formulas being rewritten (Corcoran & Evans, 2014; Hanushek & Lindseth, 2009; Hoxby, 2001), most of these reforms do not specify how much school funding should be increased. And one SFR does not guarantee success—some states have experienced more than

one SFR (Corcoran & Evans, 2014). Eighteen states have had multiple SFRs during the adequacy era alone. New Hampshire has had six SFRs, and New Jersey has had seven SFRs between 1989 and 2011 (Shores et al., 2023).

Effects of SFRs on Education Spending

Multiple studies find that SFRs, on average, increase education spending at the state and district levels (Candelaria & Shores, 2019; Jackson et al., 2016; Lafortune et al., 2018; Liscow, 2018; Shores et al., 2023). At the same time, there is considerable heterogeneity underlying these estimated average effects—some states spend a lot more in the aftermath of the SFR, and others spend little to nothing (Hoxby, 2001; Shores et al., 2023). Thus, whether spending increases and is sustained after reform is an empirical question.

At the state level, education spending increases after an SFR, and these state-level expenditures are progressively distributed to districts. Liscow (2018) and Lafortune et al. (2018) estimate that state education expenditures per pupil increased by just over \$900 (real \$2015 USD) following an SFR. And when the funds are distributed to districts, lower-income districts benefit more. For example, Lafortune et al. (2018) estimate that state education expenditures increased by approximately \$500 per student in high-income districts and \$1,200 per student in low-income districts after the reform. Shores et al. (2023) similarly show a progressive distribution of state expenditures in percentage terms. The authors find that state revenues increased by 9 percent in high-income districts and 12 percent in low-income districts.

The increases in state-level education expenditures after an SFR translate to additional district-level spending, on average. After state-level expenditures are distributed to districts, the funds become state aid revenues in district-level accounts. Importantly, rather than substituting state dollars for local dollars to maintain pre-reform spending levels, local districts spend more.

Leveraging SFRs from the late 1970s to the early 1990s, Card & Payne (2002) find that a \$10 increase in state aid is associated with about a \$6 increase in district spending. Shores et al. (2023) find a similar pattern using more recent reforms from the adequacy era. Among lower-income districts, for example, a 10 percent increase in state aid is associated with a 3.5 percent increase in district spending. Other revenue accounts are not affected after an SFR occurs. Federal funding remains the same after reform (Shores et al., 2023). Local funding declines, but it is not enough to decrease district spending (Lafortune et al., 2018; Shores et al., 2023).

District-level spending increases after an SFR reflect a progressive funding distribution within states. Jackson et al. (2016) examined the impact of court-ordered SFRs that occurred during the equity era between 1972 and 1990. The authors found that students in high-income districts who attended all twelve grades of primary and secondary schooling in the years following an SFR were exposed to a 6 percent increase in funding, while students in low-income districts were exposed to a 12 percent increase in funding. Lafortune et al. (2018) and Candelaria & Shores (2019) examined the effects of SFRs that occurred during the post-1990 adequacy era; the former study included both legislative and court-ordered reforms, and the latter study examined only court-ordered. Both studies found that SFRs lead to sustained increases in school spending, especially in low-income school districts. Seven years after reform, the highest poverty districts in reform states experienced an 11.5 to 12.1 percent increase in per-pupil spending (Candelaria & Shores, 2019).

And while SFRs increased state and district expenditures, on average, there is substantial heterogeneity in the effects of SFRs across states (Hoxby, 2001; Shores et al., 2023). For example, Shores et al. (2023) estimated the state-specific effects of court- and legislative-ordered SFRs that occurred between 1993 and 2012 using the ridge ASCM. Their study found that, while

SFRs increased spending in low-income districts on average, more than half of the states experienced a null or negative effect on education spending after an SFR.

The presence of null and negative effects after an SFR highlight the importance of determining whether state education funding increased. In this paper, we require evidence of increased and sustained funding as necessary conditions for investigating how SFRs were funded. Imposing these conditions facilitated the search for legislation after reform.

How do states pay for SFRs, on average?

As described above, SFRs, on average, lead states to increase their education expenditures, which then induces local districts to follow suit. But how do states fund these new expenditures? In what follows, we summarize how states pay by highlighting national-level studies that leverage cross-state variation over time.

One way to pay is to cut expenditures on non-education programs. Four studies examine the effects of SFRs on non-education state expenditures (Baicker & Gordon, 2006; Lafortune et al., 2018; Liscow, 2018; Murray et al., 1998). All four studies use data from the U.S. Census Bureau's Annual Survey of State Government Finances and an event study design that exploits the exogeneity of reform timing. However, the time frames and types of SFR events included differ across studies. Murray et al. (1998) examine court-ordered SFRs that occurred in 11 states between 1971 and 1992; Baicker and Gordon (2006) examine court-ordered SFRs that occurred in 22 states between 1971 and 1997; Liscow (2018) examines court-ordered SFRs that occurred in 25 states between 1971 and 2013; and Lafortune et al. (2018) examine legislative and court-ordered SFRs that occurred in 26 states between 1990 and 2011.

Across the four studies, SFRs have no effect on non-education expenditures at the aggregate state level (Baicker & Gordon, 2006, p. 1533; Lafortune et al., 2018, p. xxvii of

appendix; Liscow, 2018 pp. 20-21; Murray et al., 1998, pp. 805-807). This result is promising, especially since state-level expenditures on social welfare and health programs may complement education spending. An analysis at the county level (i.e., within states), however, presents a different story. Baicker and Gordon (2006) find that increases in intergovernmental state education funding to counties were offset by reduced state aid to counties for health and hospitals, highways, and public welfare.

So, if states are not reducing non-education expenditures, where does the money come from? Liscow (2018) finds that increases in education expenditures resulting from SFRs were primarily paid for through tax increases. The author estimates that SFRs lead to a \$152 per capita increase in state education expenditures and a nearly equivalent increase of \$150 per capita in taxes (real \$2015 USD). While this result provides some insight into the way states fund education expenditures, we still lack state-specific details about which taxes increase and by how much. Our study contributes a granular understanding of state-level policy implementation.

Data

We use five sources of data to inform our understanding of how states pay for SFRs. First, we use the Annual Survey of State Government Finances to obtain data on state revenues, expenditures, and debt. The survey is collected by the U.S. Census Bureau and has been compiled into a panel dataset called the Government Finance Database by Pierson et al. (2015). Revenues and expenditure data are generally available for fiscal years (FYs) 1972 and 1977-2017. Our analytic sample draws upon data from FYs 1982 (the first year that measures of elementary-secondary education expenditures are available for all 50 states) through 2007. Because the Great Recession, which began in December 2007, substantially altered (and in some cases, continues to alter) state revenues and expenditures (Rosewicz, 2019), we elect to examine

the effects of SFRs on state finances only through FY 2007, the last fiscal year prior to the Great Recession.

Second, we use a dataset on local property tax revenues to understand how shifts in local property tax collections were related to funding SFRs. When reviewing the text of state legislative acts to determine how SFR-induced increases in education spending were funded, it became apparent that changes to state and local property tax revenues played a key role in funding SFRs in many of the states included in our analytic sample; thus, we incorporate these data into the study. These property tax data come from the Local Education Agency Finance Survey (F-33), which is collected by the U.S. Census Bureau and audited and distributed by the National Center for Education Statistics (NCES). These data were also compiled into a panel dataset spanning FYs 1987 through 2007.¹ We aggregate the local property tax revenue data, which is collected at the district level, up to the state level to examine the effect of a given SFR on statewide local property collections.

Third, we merge state population data collected by the U.S. Census Bureau to create annual measures of each state's population between 1982 and 2007. The annual state population data estimates the population on July 1 of that calendar year (e.g., population data in 1989 refers to the estimated population on July 1, 1989). However, the year of the state revenue and expenditure data corresponds to fiscal years typically starting on the preceding July 1 and ending on June 30 (e.g., the fiscal year 1989 typically starts on July 1, 1988, and ends on June 30, 1989). To align the two types of data, we convert the state population data to fiscal years by adding a value of 1 to the calendar year (e.g., so after conversion, population data in fiscal year 1989 refers to the estimated population on July 1, 1988).

Fourth, we also use data on state student enrollments—pre-K through grade 12—from an annual panel spanning the years 1982 to 2007. For years post-1986, enrollment data were obtained from the National Public Education Financial Survey (NPEFS), which is distributed by NCES. Enrollment data prior to 1987 were obtained from the U.S. Department of Education (DOE) and NCES series reports, including *Digest of Education Statistics* and *Statistics of State School Systems*, by Paglayan (2019).

For analyses that address our second research question, most fiscal variables are presented in per-capita terms by dividing by the total state population in the relevant year. We measure in per-capita terms because we are examining state expenditures and revenues, which are financed by the general population, not pupils. Local property tax revenues and total property tax revenues (i.e., the sum of local and state property tax revenues), however, are scaled by student enrollment because the US Census Bureau data not including a measure of the population associated with local (i.e., school district) boundaries. All nominal dollar values are transformed into 2007 USD using the *cpiget* Stata command, which converts nominal to real dollars using an annual average of monthly CPI data over the state-specific fiscal-year time span (Shores & Candelaria, 2019).

Fifth, to identify states that have experienced an SFR, we draw from a list of court-ordered and legislative SFRs that was compiled by Shores et al. (2023). Their list includes SFRs that occurred during the adequacy era between academic years 1989 and 2011, which aligns well with the years in which state finance data is available (FYs 1982-2007). For our analysis, we include SFRs that occurred from FY 1989 up to FY 2005 to have at least two years of state finance data following an SFR.

We also include Michigan's 1994 passage of Proposal A as a legislative SFR. Shores et al. (2021) did not consider Proposal A to be an SFR because it was a constitutional amendment approved by state voters. However, Proposal A was originally drafted and referred to voters by the state legislature (and thus is considered a legislatively-referred constitutional amendment). Because the legislature was responsible for initiating Proposal A, we consider Proposal A to fall within the definition of a legislative SFR and thus include it in our list of court-ordered and legislative SFRs that occurred between FYs 1989-2005.

We convert the dates associated with the SFRs, which Shores et al. (2023) provide in both calendar and academic school years, to fiscal years aligned with the reporting of the revenue and expenditure data. For more detailed information on the conversion process, see Appendix A1. Appendix Table A1 lists the SFRs under consideration. Sixty-three SFR events occurred in 24 states between FYs 1989 and 2005. Twenty-six states did not have a reform during this period. These never-treated states serve as a consistent comparison group for all the treated states. Kentucky was the first state to have an SFR reform in the adequacy era in FY 1990, whereas New York did not have its first SFR reform of the adequacy era until FY 2004.

For states with multiple SFRs, we leverage the first reform that overturned the state's funding system during the adequacy era. These events are bolded in Appendix Table A1.

Research Methods

In this section, we discuss the methodologies we use to address each of our research questions. To answer RQ1, we motivate our process for identifying increased and sustained education spending and discuss why we use the ridge ASCM to estimate the impact of SFRs in each state. For RQ2, we describe our archival search process for finding legislative statutes, our document analysis, and our use of ridge ASCM to validate the results of our document analysis.

RQ1: Which states increased and sustained higher levels of elementary-secondary education spending?

In this study, we are interested in assessing how states fund increases in education spending after an SFR. As such, we select an analytic sample of states for which their first SFR increased per pupil state expenditures on public elementary-secondary education in the years after reform at the 5 percent significance level.² This selection was purposeful and made for the following two reasons. First, requiring that per pupil education expenditures increase following an SFR demonstrates that state elementary-secondary education spending was impactful for students, since not all SFRs result in increased state education funding (Shores et al., 2021). Second, we examine average effects up through ten years post-reform to allow sufficient time for state legislatures to alter both education expenditures and other state revenues and/or expenditures in response to an SFR.

To determine which state SFRs meet the criteria discussed above, we separately estimate the effect of the state SFR on per capita state education expenditures for each of the 24 states that experienced an SFR between FYs 1989 and 2005. To estimate these models, we use the ridge augmented synthetic control method (ridge ASCM), which is an approach similar to difference-in-differences (Ben-Michael et al., 2021). For states with multiple SFRs, we leverage the first year a state's finance system was overturned; see bolded events in Appendix Table A1. To compute standard errors that allow for auto-correlation within states, we use a row-based jackknife (Shores et al., 2023). These jackknife standard errors are implemented in the *augsynth* package that we use to estimate the ridge ASCM (Ben-Michael, 2022).

Intuition and justification of the ridge ASCM

In what follows, we provide an intuitive overview of the ridge augmented synthetic control method (ridge ASCM). Our overview, modeled after Shores et al. (2023), begins by describing various approaches to estimating state-specific individual effect sizes. A technical description of the ridge ASCM using formal notation can be found in Appendix B. Additionally, the results of robustness checks used to evaluate the quality and validity of the ridge ASCM estimates are available in Appendix C.

To build intuition regarding ridge ASCM, consider estimating the effect of one state SFR, New Hampshire's 1994 SFR, on per pupil state education spending. Using the potential outcomes framework (Rubin, 1974), we can define individual effect sizes as $\gamma_t = Y_t(1) - Y_t(0)$, where γ_t represents the treatment effect estimate for New Hampshire in year t . $Y_t(1)$ represents the potential education expenditures under the assumption of having an SFR in New Hampshire in year t and $Y_t(0)$ represents the potential education expenditures under the assumption of not having an SFR in New Hampshire in year t . However, we cannot observe $Y_t(0)$; meaning, we cannot observe what education expenditures would have been in New Hampshire if it had not experienced an SFR.

Although $Y_t(0)$ is not observed, we can estimate $Y_t(0)$ using the outcomes of non-treated states. Specifically, we can leverage the education expenditures of states that did not experience an SFR to construct a comparison group for New Hampshire in three ways: (1) compute the mean education expenditures of non-treated states, weighting each non-treated state equally (referred to as "equal-weight average"); (2) compute a synthetic control mean of education expenditures in non-treated states through assigning time-invariant weights of differing values to non-treated states based on their pre-treatment match (referred to as "traditional SCM"), where

pre-treatment match is based on the outcome variable; (3) compute a de-biased synthetic control mean of education expenditures in non-treated states through adding a bias correction term to the synthetic control mean calculated in (2) above (referred to as “ridge ASCM”).

The left panel of Figure 1 displays per pupil education expenditures over time in real 2007 USD for New Hampshire and all non-treated states. The purple line represents New Hampshire’s per capita education expenditures over time, and the grey lines represent per capita education expenditures over time in non-treated control states. The vertical dashed line denotes the timing of New Hampshire’s 1994 SFR. When we compare New Hampshire’s per capita education expenditures before and after its SFR, we see that per capita education expenditures were higher on average after the SFR. However, per capita education expenditures were also generally higher in non-treated states in the years following 1994 compared to the years prior, suggesting that all states may have been experiencing an upward trend in per capita education expenditures regardless of whether they had an SFR.

[Insert Figure 1 about here]

The right panel of Figure 1 displays dynamic effect estimates of New Hampshire’s 1994 SFR on per capita education expenditures using three different comparison groups. The short-dashed orange line represents differences between New Hampshire’s per capita education expenditures and average education expenditures of non-treated states, where each non-treated state is weighted equally (referred to as “equal-weight average”). The long-dashed green line represents differences between New Hampshire’s per capita education expenditures and a synthetic control mean of education expenditures, where the synthetic control mean is calculated by assigning time-invariant weights of differing values to non-treated states based on their pre-treatment match (referred to as “traditional SCM”). The solid blue line represents differences

between New Hampshire's per pupil education expenditures and a de-biased synthetic control mean of education expenditures, where the de-biased synthetic control mean is calculated by adding a bias correction term to the traditional synthetic control mean (referred to as "ridge ASCM"). In Appendix B, we include a subsection called "Bias Correction Term in the Ridge ASCM" to provide further details about the de-biased synthetic control approach.

If the comparison group adequately mirrors the pre-SFR spending levels and trend of New Hampshire, then the difference in per pupil education expenditures between the comparison group and New Hampshire should be approximately zero. As shown in Figure 1, only the ridge ASCM comparison group (blue line) consistently results in a difference of zero in the pre-treatment period, suggesting that the equal-weight average and traditional SCM comparison groups do not serve as good counterfactuals to New Hampshire. In turn, the use of the ridge ASCM comparison group provides differing estimates of the effect of New Hampshire's SFR on per capita education expenditures compared to the other two approaches.

RQ2: How did each state's legislature fund additional education revenues?

To identify the legislative statutes that increased state education expenditures, we used a systematic process. We began by reviewing relevant school finance history in each state using the first five sources listed in Table 1. These sources included four websites (edlawcenter.org, n.d.; schoolfunding.info, n.d.; U.S. Department of Education, National Center for Education Statistics, 2001; Verstegen, 2018) and a book on school finance history (Thompson et al., 2019). Collectively, these sources provided a comprehensive overview of litigation and legislative actions related to school finance in our sample of case study states. Each of these sources either provided the name of the statute that enacted the SFR or the year in which legislation altered school funding.

Based on the legislative details we obtained, we conducted our search using two more steps, both of which were facilitated using the sixth source in Table 1 (congress.gov, n.d.). First, if the name of a statute was not provided, we visited the state legislature’s website. We then searched through histories of bills and acts to find the relevant statute, guided by the details we obtained from the five sources in Table 1. Second, we prioritized finding the original text of each statute. We were able to successfully find all but one original document, Kansas’ School District and Quality Performance Act (1992). In lieu of the actual text, we obtained a thorough summary of the act compiled by the Kansas Legislative Research Department. We include in-text citations of statutes we examined when discussing our results and provide the full reference with weblinks in our works cited list.

After identifying the legislative activity that was associated with each SFR, we obtained and reviewed the text of these legislative statutes and amendments for evidence of what changes to state finances were made to fund increases in education spending. As needed, we also reviewed any secondary sources (i.e., academic journal articles, news articles, published briefs, or reports) we found with relevant information on how state legislatures funded SFRs. We cite all these additional sources when reporting results. Each author conducted their document analysis separately and maintained analytic memos to ensure any discrepancies were addressed.

Finally, using the ridge augmented synthetic control method, we quantitatively examine the effect of a given state’s SFR on the state revenues or expenditures that were specified in legislation as being adjusted to fund increased education spending. We use the same approach to estimate the Ridge ASCM as we did with RQ 1, leveraging the same comparison group of states that did not have a documented SFR during our period of analysis.³ This second step provides an important check on whether the state legislature “followed through” and adjusted state (and, in

some cases, local) finances in the ways specified in the referenced legislation to fund SFRs. Ultimately, this approach combines evidence regarding how SFRs are funded from multiple sources (both legislative acts and finance data), leading to further confidence in our results.

Results

RQ1: Which states increased and sustained higher levels of elementary-secondary education spending (i.e., state aid to school districts) after an SFR?

We find that six out of 24 state SFRs significantly increased per capita state education expenditures at the $p < 0.05$ level in the years following their first reform and thus are included in our selected analytic sample. We display the ridge ASCM effect estimates of SFRs on per pupil, as well as per capita, base elementary-secondary education expenditures for these six state SFRs graphically in Figure 2. The six state SFRs include Vermont's *Brigham v. State* (FY 1997); Michigan's Proposal A (FY 1994); New Hampshire's *Claremont School District v. Governor* [*Claremont I*] (FY 1994); Kansas' School District Finance and Quality Performance Act (FY 1992); Arkansas' *Lake View v. Huckabee* and the Equitable School Finance System Act [Act 917] (FY 1995); Maryland's *Bradford v. Maryland State Board of Education* (FY 1997); and New Jersey's *Abbott v. Burke II* & the Quality Education Act (FY 1991).

[Insert Figure 2 about here]

As Table 2 shows, there is variation in the timing of SFRs and in their effects on state education spending. The fiscal year of the six SFR reforms ranged from 1992 (Kansas) to 1997 (Maryland and Vermont). The average effect of a state SFR on state education spending ranges from a high of \$5,553 per pupil (\$914 per capita) in Vermont to a low of \$479 per pupil (\$77 per capita) in Maryland.

[Insert Table 2 about here]

Appendix Figure C1 displays ridge ASCM effect estimates of SFRs on per capita and per pupil elementary-secondary education expenditures for all 24 states that experienced an SFR between FY 1989 and 2005. For reference, Appendix Figure C2 shows the percent improvement in pre-treatment model fit obtained from the per capita and per pupil elementary-secondary expenditures ridge ASCM models relative to models that use equal or traditional SCM weights.

RQ 2: How did state legislatures pay for increased and sustained education spending after an SFR?

For each of the six state SFRs included in our analytic sample, we provide a brief description of the SFR and the extent to which state education expenditures increased following the SFR. We then detail how each state funded these increases in state expenditures. We begin with Vermont's 1997 SFR, which had the largest average effect on state education funding of the seven state SFRs, and we continue in descending order based on the SFR's average effect on education funding. When reporting averages of point estimates, we compute the averages using precision weighting, where the weight is the inverse of the square of the standard error. The full list in the order of presentation is as follows: Vermont, Michigan, Arkansas, New Hampshire, Kansas, and Maryland.

Vermont: Brigham v. State (FY 1997)

In February 1997, the Vermont Supreme Court ruled in *Brigham v. State* (1997) that the state's funding system was unconstitutional and directed the Legislature to create a system that would enable "substantially equal opportunity." In June of that same year, lawmakers enacted the Equal Educational Opportunity Act, also known as Act 60 (VT Bill H.527, 1997). Under Act 60, which went into effect in FY 1999, the legislature enabled greater equality of education funding by distributing education funds using a foundation grant program.⁴ Foundation programs

guarantee a base level of funding per pupil. The state also presumed the primary responsibility for funding the foundation grant program, and state contributions to elementary-secondary education increased as a result. Per capita state education expenditures were approximately \$962 higher on average in the nine years following Act 60's enactment (FYs 1999-2007) compared to the state's synthetic counterfactual (see Figure 3a).

[Insert Figure 3 about here]

The Vermont legislature increased state education funding under Act 60 primarily by exerting increased control over property taxes. Specifically, the state replaced district-controlled local property taxes with a state-controlled state property tax. As a result, per capita state property tax revenues increased by approximately \$763 on average—per pupil property tax revenues increased by \$5,348 on average—relative to Vermont's synthetic control group in the years following Act 60's passage (FYs 1999-2007; see Figure 3b).

In contrast, local property tax revenues decreased over the same time period; per pupil property tax revenues decreased by \$4,180 on average.⁵ Act 60 also raised additional state education funding by mandating small increases to a variety of taxes, including the rooms and meals sales tax (2 percentage point increase), motor fuels sales tax (4 cent increase), purchase and use tax on motor vehicles (1 percentage point increase), sales tax on telecommunication services (imposing a 4.36% tax), corporate income tax (1.5 percentage point increase), and bank franchise tax and brokerage fees. Corporate income tax revenues and selective sales tax revenues (which encompass many of the tax increases mentioned above) increased post-1997, though these effects are not consistently significant (see Figure 3a). Act 60 also specified that lottery revenues be used solely for funding elementary-secondary public education. Lottery revenues were to be placed in the education fund, rather than the general fund, as a result.⁶

Michigan: Proposal A (FY 1994)

In 1993, the Michigan state legislature and Governor eliminated local property taxes as a source of funding for education, which state politicians and citizens had been attempting to do since the early 1970s (Courant & Loeb, 1997). To fund education, the legislature proposed—and voters subsequently approved—a constitutional amendment, Proposal A, in FY 1994 (MI Senate Joint Resolution S, 1993). As depicted in Figure 4a, per capita education spending was \$532 higher, on average, in Michigan post-1994 compared to its synthetic counterfactual.

[Insert Figure 4 about here]

Proposal A led to increased state education expenditures by altering Article 9 (Finance and Taxation) of the Michigan state constitution and changing the tax systems that raised revenues for schools. Specifically, Proposal A required that the sales and use tax be raised by two percentage points and that a new state property tax of 6 mills be enacted. As a result, Michigan's state general sales tax revenues were about \$201 higher per capita in Michigan post-1994, while state property tax revenues were approximately \$165 higher per capita (\$843 higher per pupil; see Figure 4b). Proposal A also sets limits on the minimum and maximum property tax rates that could be imposed locally. Consequently, local property tax rates and revenues declined on average by \$3,605 per pupil, as well as total (state + local) property tax rates and revenues by \$2,610 per pupil; see Figure 4b. Thus, Proposal A ultimately resulted in property tax relief for Michigan citizens.

Proposal A also increased the cigarette tax by 50 cents per pack, and the real estate transfer tax increased by 0.45 percentage points, leading to small increases in state tobacco tax and documentary/stock tax revenues (\$27 and \$4 per capita on average, respectively; see Figure 4a).⁷ All revenues generated from the Proposal A tax increases were earmarked for the

state School Aid Fund and then distributed by the state to school districts based on a foundation grant formula (Courant & Loeb, 1997).⁸

Arkansas: Lake View v. Huckabee and the Equitable School Finance System Act (FY 1995)

In December 1994, the court declared in *Lake View v. Huckabee* (1994) that Arkansas' state education funding system was unconstitutional and that the state had two years to enact a new funding scheme. In February 1995, the Arkansas General Assembly approved the Equitable School Finance System Act, more commonly known as Act 917 (AR Act 917, 1995).⁹ Under Act 917, which went into effect in FY 1997, the General Assembly implemented an equalization school funding formula. Equalization plans typically supplement local funding by providing a block grant to districts based on certain characteristics, such as local tax base or revenues (Shores et al., 2023). In the eleven years following Act 917's passage (FYs 1997-2007), Arkansas' per capita state education funding increased by \$208, on average, relative to its synthetic counterfactual (see Figure 5a).

[Insert Figure 5 about here]

Act 917 enabled increased state education funding by replacing local property tax rates that varied by district with a state-controlled uniform property tax rate or "base millage rate."¹⁰ The state both collected and redistributed the revenues which were raised from the uniform property tax. As shown in Figure 5b, per capita state property tax revenues increased by approximately \$117 on average (per pupil property tax revenues increased by \$555 on average) relative to Arkansas' synthetic control group after Act 917's enactment (FYs 1997-2007). In contrast, local property tax revenues decreased over the same time period (per pupil property tax revenues decreased by \$245 on average). Total (local plus state) property tax revenues increased by \$655 per pupil on average following HB Act 917's passage (FYs 1997-2007).

Based on our legislative document search, we also found that the General Assembly further increased state education funding in February 2004 by passing Act 107 during the Second Extraordinary Session of 2003 (AR Act 107, 2004). Effective in March of 2004, Act 107 increased the state sales and use tax rate by 0.875 percent, expanded the services that the state general sales tax applies to, and increased the wholesale vending tax to provide additional revenues for elementary-secondary education. Descriptively, we find that per capita general sales tax revenues increased by \$324, on average, between 2005 and 2007 compared to Arkansas' synthetic counterfactual (see Figure 5a).¹¹ And between 2005 and 2007, Arkansas' per capita state education funding had risen by \$419 relative to the synthetic control group.

New Hampshire: Claremont School District v. Governor [Claremont I] (FY 1994)

In FY 1994, the New Hampshire Trial Court ruled in *Claremont School District v. Governor [Claremont I]* (1994) that the state has a constitutional duty to provide each child with an adequate education. Three years later, in December of 1997, the New Hampshire Supreme Court further declared in *Claremont II (Claremont v. Governor, 1997)* that the state education finance system was unconstitutional because it enabled inequitable local property tax rates and fostered inadequate educational opportunities. The court ordered that the state needed to define and fund what constituted an adequate education. In response to the *Claremont* rulings, the state legislature eventually passed House Bill (HB) 117 in April 1999 after several failed reform plans (NH H.B. 117, 1999). HB 117 established a foundation grant program for distributing education funding. In the eight years following HB 117's passage (FYs 2000-2007), New Hampshire's per capita state education funding increased by \$420, on average, relative to its synthetic counterfactual (see Figure 6a).

[Insert Figure 6 about here]

HB 117 enabled increased state education funding by replacing district-controlled local property taxes with a state-controlled state property tax.¹² As shown in Figure 6b, per capita state property tax revenues increased by approximately \$424 on average (per pupil property tax revenues increased by \$2,438 on average) relative to New Hampshire's synthetic control group after HB 117's enactment (FYs 2000-2007). In contrast, local property tax revenues decreased over the same time (per pupil property tax revenues decreased by \$3,180 on average).¹³ HB 117 also increased funding for education by mandating smaller increases to a variety of state taxes, including raising the business enterprise and business profits tax, raising the real estate transfer tax, expanding the rooms and meals sales tax, and instituting a statewide property tax on utility properties. In turn, corporate income tax revenues (which includes business enterprise and business profits tax revenues) and documentary/stock transfer tax revenues (which includes real estate transfer tax revenues) increased in New Hampshire after HB 117's enactment relative to the state's synthetic control (see Figure 6a).¹⁴ The bill also dedicated revenues from future increases in the tobacco tax to the education trust fund and designated certain tobacco settlement funds received by the state for education funding.

Kansas: The School District Finance and Quality Performance Act (FY 1992)

Throughout the late 1980s and early 1990s, 42 Kansas districts filed four separate legal challenges regarding the constitutionality of the current state school finance system under the School District Equalization Act (SDEA). These legal challenges were consolidated into one case, and in 1991, a district court judge indicated in a pre-trial ruling that if the case went to trial, he would likely declare that the SDEA violated the state constitution's requirement that the legislature "make suitable provision for finance of the educational interests of the state" (*Mock v. State*, 1991). Subsequently, the Kansas Governor (Joan Finney) established a task force for

determining a new state school funding formula. And in 1992, the Kansas Legislature passed the School District Finance and Quality Performance Act (SDFQPA) (KS H.B. 2892, 1992). The SDFQPA, which went into effect in FY 1994, established a foundation grant program.¹⁵ The funding scheme also included a variety of special weightings or cost adjustments to accommodate differences in district characteristics and differences in the student populations served by districts. In the 14 years following the SDFQPA's enactment (FYs 1994-2007), Kansas' per capita state education expenditures were approximately \$249 higher on average compared to the state's synthetic counterfactual (see Figure 7a).

[Insert Figure 7 about here]

The SDFQPA increased state education expenditures by raising state tax revenues while simultaneously reducing local property tax burdens. State tax increases included raising the state general sales tax rate (from 4.25% to 4.9%), enacting higher individual and corporate income tax rates, and eliminating various sales tax exemptions (Johnston & Duncombe, 1998; Thompson & Clark, 2001). As shown in Figure 7a, increased general sales and individual income tax revenues enabled the bulk of new education funding, with general sales tax revenues increasing by \$157 on average and individual income tax revenues increased by \$144 on average in the years post-SDFQPA (FYs 1994-2007).¹⁶ The SDFQPA also required districts to impose a uniform local property tax rate,¹⁷ which ultimately resulted in lower local property tax revenues—a decrease of \$888 per pupil on average; see Figure 7b—and property tax relief for citizens.¹⁸

Maryland: Bradford v. Maryland State Board of Education (FY 1997)

In FY 1997, the court ruled in *Bradford v. Maryland State Board of Education* (1996) that students in Baltimore City public schools were not receiving a constitutionally adequate education; thereafter, Maryland's General Assembly and Governor established a commission to

study the school finance system of Maryland and calculate the cost of providing an adequate education.¹⁹ In January 2002, the commission issued its final recommendations, which included revising the state school funding formula and increasing state education expenditures by \$1.1 billion (Commission on Education Finance, Equity, and Excellence, 2002).

Based on the commission's recommendations, the Maryland General Assembly passed the Bridge to Excellence in Public Schools Act (Senate Bill 856) in FY 2002 (MD S.B. 856, 2002). The Bridge to Excellence (BTE) Act implemented a foundation program funding scheme, where districts were provided with a uniform per-pupil base amount of funding that the state estimated to be the minimum amount required to provide an adequate education. BTE also called for an increase in state education funding, especially in districts with large populations of children from educationally disadvantaged backgrounds, which was to be phased in over six years. As shown in Figure 8a, per capita education spending subsequently increased in Maryland in the five years following the passage of BTE.²⁰ Per capita state education expenditures were approximately \$132 higher on average in FYs 2003-2007 compared to Maryland's synthetic counterfactual.

[Insert Figure 8 about here]

BTE required taxes on cigarettes to be raised from 66 cents to \$1 per pack in FY 2003 to fund the first year of increased state education expenditures. Tobacco tax revenues and education expenditures subsequently increased in FY 2003; see Figure 8a.²¹ However, the Act did not specify a revenue source to fund the remaining five years (FYs 2004-2008) of mandated education spending increases.²² Additional searches by the authors through state finance documents and news articles on BTE and Maryland state education funding during this time did not provide any additional details regarding revenue sources. However, as we show descriptive

in Figure 8b, the elevated tax revenues from the cigarette tax increase likely continued to fund education. We also examined the effects of Maryland's SFR on other state revenue and expenditure categories using ridge ASCM but did not find any evidence of funding sources that could be corroborated with legislative documents.

Discussion

We find that five of the six state SFRs examined in this study—Vermont (1997), Michigan (1994), Arkansas (1995), New Hampshire (1994), and Kansas (1992)—were funded by altering tax rates and changing tax revenue sources. These five states paid for increased education expenditures by increasing a variety of state tax revenues, such as general and selective sales taxes and individual and corporate income taxes. And while none of these five state legislatures altered the exact same set of tax revenues, suggesting there is no single “recipe” for funding SFRs, they did share a common approach with respect to property taxes: all five states increased state education spending by increasing state control over the imposition, collection, or distribution of property tax revenues. Consequently, local property tax revenues decreased in all five states. State property tax revenues, however, only increased in four states: Vermont, Michigan, Arkansas, and New Hampshire.

Maryland funded increased state education spending for one year by raising tobacco tax revenues, but we were unable to determine how the state funded the remaining five years of mandated education spending increases using the Ridge ASCM approach. Our descriptive time series of Maryland's tobacco tax revenues (Figure 8b), however, does suggest that the increased revenue from the cigarette tax may have been able to sustain funding through the end of our sample period. Thus, in the case of Maryland, the combination of legislative document analysis,

ridge ASCM, and descriptive time series graphs suggests multiple approaches are needed to understand the ways in which SFR-induced increases in state education spending are funded.

The five states that increased control over property tax revenues and raised a variety of other state tax revenues experienced larger average increases in state education funding compared to Maryland, which only increased one, non-property tax revenue. Specifically, as shown in Table 2, the average estimated effect of a state SFR reform on state education spending was \$77 per capita in Maryland. In contrast, the average estimated effect of an SFR on state education funding ranged from \$222-\$914 per capita in Arkansas, Kansas, Michigan, New Hampshire, & Vermont. These findings suggest that larger increases in state education funding are achieved when (a) multiple tax revenue streams are increased and when (b) states exercise increased control over property tax revenues.

At the same, the goal of an SFR should not only be increasing state funding—an equitable distribution mechanism for state aid and the total amount of aid allocated to students (state spending plus local spending) should also be part of policy conversation. Researchers and policymakers should consider the extent to which state aid is equitably distributed to districts across the state. For example, one mechanism to distribute state aid equitably is through a weighted student funding formula (WSFF), which gives each student a base funding amount and provides additional funding according to selected demographic characteristics as shares of the base amount—the so-called weights. But while a policy mechanism such as a WSFF provides more funding to students who are in need of more resources, there are tradeoffs associated with setting the weights, since state aid is ultimately constrained by the revenue sources that fund it (Candelaria et al., 2023).

The total amount of state and local aid is also an important policy consideration for further study. As shown in Figure 9, the extent to which local funding changes after an SFR is quite heterogenous. Even though state spending per pupil increases—a mechanical artifact based on our case study selection approach—local funding decreased significantly in Kansas, Michigan, New Hampshire, and Vermont. Indeed, this is not completely surprising as these states absorbed control of property taxes for the purpose of redistribution. But from a total resource perspective, the extent to which state and local spending per pupil increase will matter, especially if the goal is to provide adequate funding to students. Thus, policy designs for funding SFRs will need to account for the preferences of local voters and whether they are likely to support education reform or engage in a tax revolt, which occurred in New Hampshire (Dandurant, 2000), if property taxes substantially increase.

Finally, we think it is important for policymakers to assess budgetary tradeoffs—reducing non-education expenditures or increased state debt—associated with funding SFRs. We find no legislative evidence, based on our document analysis, that non-education state expenditures were reduced or that state debt was increased to fund increased elementary-secondary education spending following an SFR. However, one may wonder if we find no evidence of reduced non-education expenditures or increased debt because such things are not mentioned in legislative statutes related to increasing education funding—either because they are not required to be mentioned like raising revenues are or because doing so might have negative ramifications.

To assess whether non-education expenditures decreased or whether state debt increased, we estimated the effects of the seven state SFRs on per capita non-education state expenditures and total state debt outstanding using ridge ASCM. As described in detail in Appendix D, while we do find some evidence that non-education state expenditures were reduced and state debt

increased in the years following an SFR in a few states, we were unable to verify or corroborate such findings using other sources of information (e.g., academic journal articles, news articles, published briefs or reports). Without additional corroborating evidence, we are not confident that the effects on non-education expenditures and state debt should be attributed to SFRs (versus other events or policy changes that occurred at the same time). As such, we recommend further examination into whether SFR-induced increases in state education spending impact the funding of other state priorities or state deficits.

Overall, we acknowledge that these patterns we observe in this study are based on a small sample of seven state SFRs and thus should be interpreted with caution. Future research should examine the relationship between how SFRs are funded and the magnitude of the increase in state education spending using a larger sample of state SFRs.

Conclusion

Prior research suggests that potential avenues for funding SFRs include increasing revenues by raising taxes, reducing expenditures on other state priorities such as welfare, corrections, and higher education, or increasing state debt (Baicker & Gordon, 2006; Liscow, 2018). However, because states have different political environments, funding priorities, and tax systems, the ways in which SFR-induced increases in education expenditures are funded likely vary across states. Our study is the first to examine how individual states funded these increased expenditures using synthetic control methods and document analysis.

We use a novel, multi-step approach to determine how SFRs are funded, drawing on evidence from multiple sources (both legislative acts and quantitative finance data). Referencing relevant legislative statutes provided a very detailed accounting of (a) which specific state revenue categories were changed and (b) to what degree (e.g., how many percentage points a

given tax rate was raised). We were also able to verify the impact of this legislative change by quantitatively examining the effect of a state's SFR on the given revenue categories using state finance data and ridge ASCM (Ben-Michael et al., 2021). In comparison, research designs that rely solely on quantitative analysis of state finance data are unable to provide the same kind or level of detail as can be obtained from reviewing the legislation.

The search for funding is likely a barrier that policymakers face and is one possible explanation for why some SFRs did not result in increased education funding (Shores et al., 2021). Although our analysis focuses on the adequacy era before the Great Recession, SFRs continue to be prominent in education policy. There have been at least 10 court-ordered finance reforms that have resulted in a state high court requiring the legislature to either change their finance system or add additional funding (Hanushek & Joyce-Wirtz (2023). This trend highlights the need for additional research that examines the granular details of SFRs and their implementation.

Understanding the breadth of options available for increasing education expenditures will inform state and education policymakers about different approaches for supporting education spending in their own states. From a budgetary perspective, our analyses find that our case study states did not increase state debt or require reducing non-education expenditures. However, as we caution in our discussion, funding an SFR is only one part of the process. Policymakers must also consider the extent to which state aid is equitably distributed across districts as well as the extent to which local funding may change based on the preferences of taxpayers residing in the state, especially if the goal is to increase the sum of local and state aid to reach a funding target threshold.

End Notes

¹ During fiscal years 1987, 1988, 1989, 1991, 1993, and 1994, the full universe of school districts was not surveyed, and these years are not included in the NCES release of data; however, we were able to obtain district-level data from sampled districts directly from the U.S. Census Bureau.

² State expenditures on public elementary-secondary education is operationalized as the sum of direct and intergovernmental expenditures on elementary and secondary education (pre-K through grade 12). Direct expenditures include current expenditures (e.g., salaries, supplies, etc.), purchase of capital improvements, and construction expenditures. Intergovernmental expenditures are amounts paid to other governments for performance of specific functions or for general financial support. State elementary-secondary education expenditures also includes all expenditures for public charter schools offering elementary-secondary education.

³ An anonymous reviewer noted that states which did have an SFR might differ in unobservable ways from those that never had an SFR. While they acknowledged it was appropriate to have the donor pool of control states be only the never-treated states for RQ 1, they were concerned about leveraging this donor pool for RQ 2. Based on the reviewer's suggestion, we re-estimated our ridge ASCM model for our six case study states using a comparison group that consisted of the states that had an SFR but were unable to increase and sustain expenditures after their SFR. In Appendix E, we found that the revenue and expenditure results were very similar for each case study state. This result is not surprising. As noted by Shores et al. (2023), states without documented SFRs increased revenues in ways similar to states with reforms: "This result suggests that non-SFR states are able to increase revenues in ways not previously documented" (p. 418).

⁴ Districts could also opt to provide additional funding above the foundation grant by levying a local-share property tax. Funds raised from the local-share property tax were distributed using a power equalization formula. Power equalization plans guarantee that districts that approve the same tax rate receive the same amount of funding per pupil, regardless of local district property wealth, through redistribution.

⁵ Total (local + state) property tax revenues remained largely unchanged following Act 60's passage.

⁶ Vermont's lottery revenues were only about \$50 per capita on average between 1999-2007. Thus, diverting lottery funds from the general fund to the education fund likely had negligible effects on funding for other state priorities (ex. welfare, corrections, higher education, etc). we also do not find quantitative evidence that non-education state expenditures decreased post-1997 in Vermont.

⁷ Documentary/stock tax revenues includes real estate transfer tax revenues. The Census Bureau does not provide a separate measure of real estate transfer tax revenues.

⁸ Districts that raised more than the base foundation grant with the required local property tax rate were not required to return the excess revenue to the state, though the state did cap revenues in previously high-spending districts based on 1994 revenue levels (Cullen & Loeb, 2004). Certain high-wealth districts were also able to raise additional funding above the foundation grant via hold harmless mills.

⁹ Act 1307, which was passed by the General Assembly in April 1997, amended various sections of Act 917.

¹⁰ In November 1996, Amendment 74 to the Arkansas Constitution was passed. This amendment enacted a base millage rate of twenty-five mills for all school districts. It further allowed school districts to levy additional property taxes above the uniform property tax rate if approved by local voters (see Article 14, Section 3, of the Arkansas State Constitution).

¹¹ We do not find that selective sales tax revenues (which includes wholesale vending tax revenues) significantly increased in Arkansas after Act 107's enactment (FYs 2004-2007) relative to the state's synthetic control. This is likely due to wholesale vending tax revenues making up a small proportion of selective sales tax revenues.

¹² School districts were required to give excess property revenue to the state government if their state property tax revenue was greater than the base foundation grant. School districts were also allowed to spend more than the base foundation grant by levying additional local property taxes (Lutz, 2010).

¹³ Total (local + state) property tax revenues decreased following HB 117's passage.

¹⁴ We do not find that selective sales tax revenues (which includes rooms and meals sales tax revenues) significantly increased in New Hampshire after HB 117's enactment (FYs 2000-2007) relative to the state's synthetic control. This is likely due to the rooms and meals sales tax revenues making up a small proportion of selective sales tax revenues.

¹⁵ A provision of SDFQPA known as the Local Option Budget (LOB) also permitted districts to raise additional funding 25% above the base foundation grant if approved by local citizens (Johnston & Duncombe, 1998; Thompson & Clark, 2001).

¹⁶ As also shown in Figure 7, selective sales tax and corporate income tax revenues were not statistically different from those of Kansas' synthetic counterfactual.

¹⁷ The uniform tax rate was 32 mills in 1992; 33 mills in 1993; 35 mills in 1994 through 1996; 27 mills in 1997; and 20 mills in 1998 (Thompson & Clark, 2001).

¹⁸ If local property tax revenues were greater than a district's base foundation grant, the excess local revenues were remitted to the state and then redistributed to other "property poor" districts (Johnston & Duncombe, 1998; Thompson & Clark, 2001).

¹⁹ The commission's formal name was the Commission on Education Finance, Equity, and Excellence. The commission was also referred to as the Thornton Commission after its chairman, Alvin Thornton.

²⁰ We do not examine state education expenditures in the sixth year (FY 2008) following BTE as we only examine the effects of SFRs on state finances through FY 2007 (the last FY prior to the Great Recession).

²¹ While tobacco tax revenues did not significantly increase in FY 2003, the confidence intervals associated with the FY 2003 tobacco tax effect estimate and elementary-secondary education effect estimate overlap. Thus, we cannot reject the possibility that tobacco tax revenues and education expenditures in New Jersey rose by similar amounts in FY 2003.

²² In Maryland, an act does *not* have to identify a revenue source for future proposed spending, per a 1978 amendment to the state constitution (Norcross, 2010).

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Tables

Table 1: Reference sources for finding legislative statutes

Reference	Type of source	General information provided
1. <i>SchoolFunding.Info: A project of the Center for Educational Equity at Teachers College:</i> http://www.schoolfunding.info/ (schoolfunding.info, n.d.)	Website	Overview of school-funding court decisions in each state
2. <i>50 State Survey of School Finance Policies:</i> https://schoolfinancesdav.wordpress.com/ (Verstegen, 2018)	Website	Overview of the history of school finance in each state
3. <i>Public School Finance Programs of the United States and Canada (1998-99):</i> https://nces.ed.gov/edfin/state_financing.asp (U.S. Department of Education, National Center for Education Statistics, 2001)	Website	Overview of the history of school finance in each state
4. <i>Education Law Center Litigation State Profiles:</i> https://edlawcenter.org/litigation/states/ (edlawcenter.org, n.d.)	Website	Overview of school-funding court decisions in each state
5. <i>Funding public schools in the United States and Indian country.</i> (Thompson et al., 2019)	Book	Overview of the history of school finance in each state
6. <i>State Legislature Websites:</i> https://www.congress.gov/state-legislature-websites (congress.gov, n.d.)	Website	Provides a link to each state legislature's website to enable retrieval of statutes

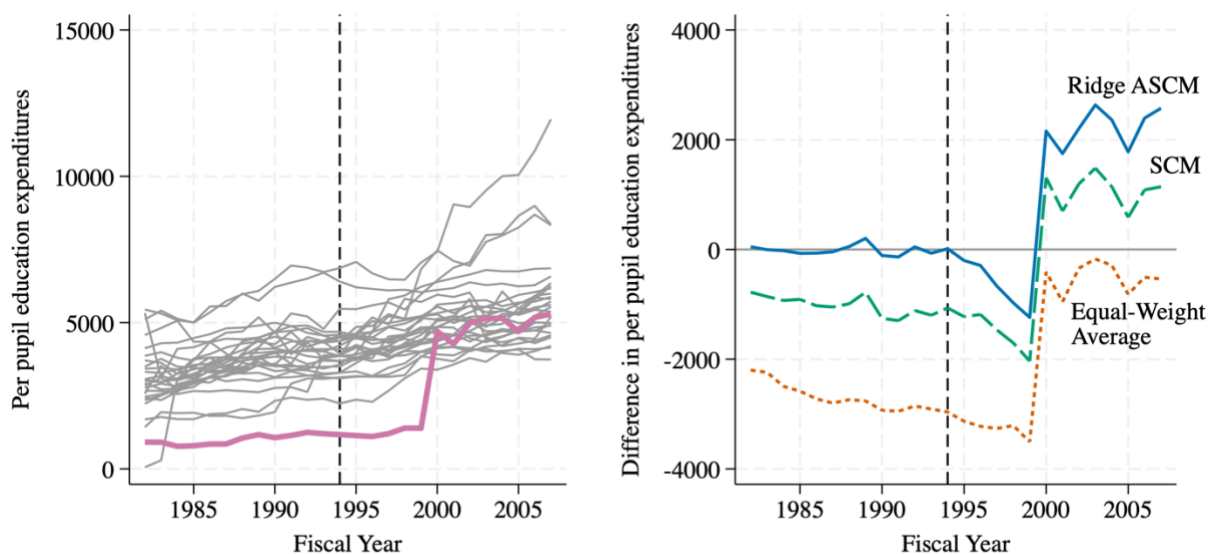
Table 2. Selected analytic sample of state SFRs

State SFR	Fiscal year of SFR	Average effect of SFR on state education spending	
		Per pupil	Per capita
Vermont: Brigham v. State	1997	\$5,553	\$914
Michigan: Proposal A	1994	\$3,031	\$518
Arkansas: <i>Lake View v. Huckabee</i> and the Equitable School Finance System Act (Act 917)	1995	\$1,555	\$248
New Hampshire: Claremont School District v. Governor [Claremont I]	1994	\$1,115	\$222
Kansas: School District Finance and Quality Performance Act	1992	\$1,086	\$234
Maryland: <i>Bradford v. Maryland State Board of Education</i>	1997	\$479	\$77

Notes: State SFRs entered the case study analytic sample if the average treatment effect on the treated (ATT) of per pupil state expenditures on elementary-secondary education was statistically significant at the 5 percent level across all years of treatment: the year after the SFR through 2007. We report the ATT of per pupil expenditures in column 3 and the ATT of per capita expenditures in column 4. The table is sorted based on per pupil amounts in column 3. Real dollar values are presented in 2007 USD.

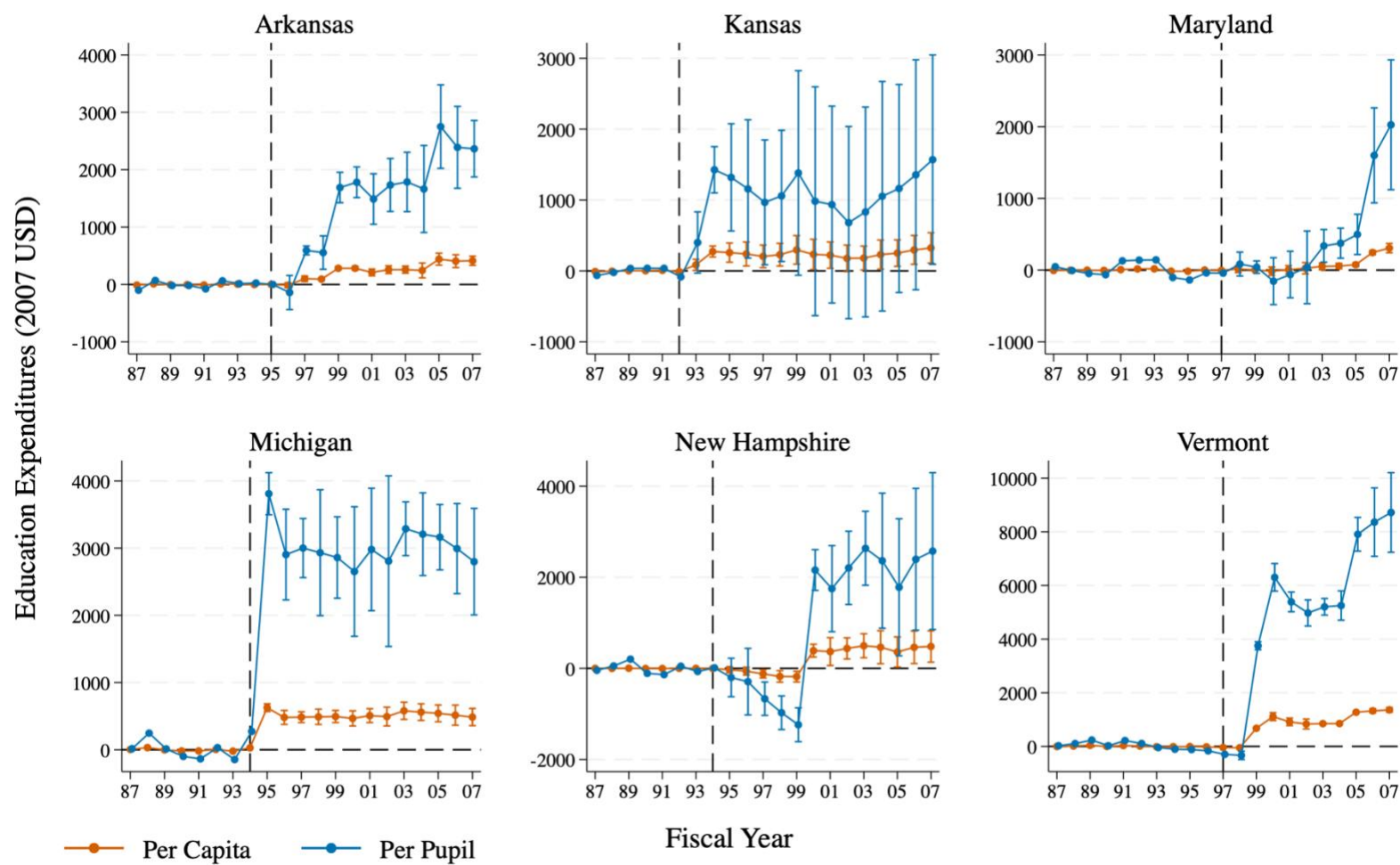
Figures

Figure 1. Estimating state-specific effects using Ridge ASCM and other estimation strategies



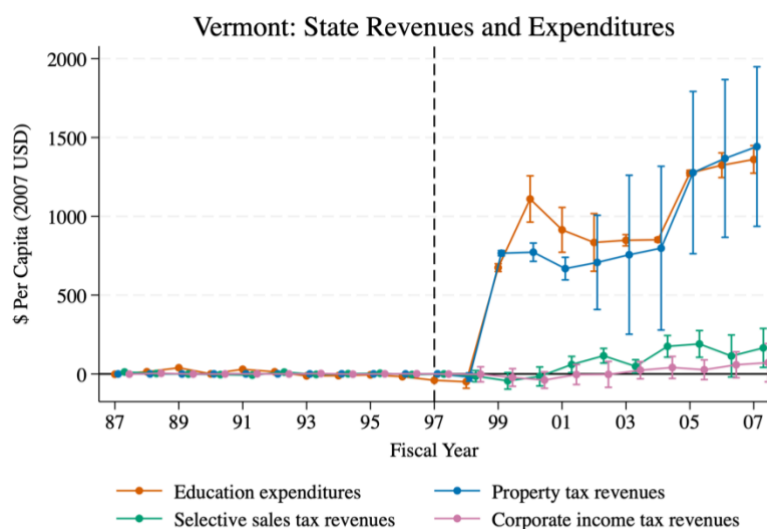
Notes: The left panel displays per pupil state elementary-secondary education expenditures in real 2007 USD over time. The purple line represents New Hampshire's per pupil state education expenditures over time, and the grey lines represent per pupil state education expenditures over time in non-treated control states. The vertical dashed line denotes the timing of New Hampshire's 1994 SFR. The right panel displays dynamic treatment effect estimates of New Hampshire's SFR on per pupil education expenditures using three different comparison groups. The short-dashed orange line represents differences between New Hampshire's per pupil education expenditures and average education expenditures of non-treated states, where each non-treated state is weighted equally (referred to as "equal-weight average"). The long-dashed green line represents differences between New Hampshire's per pupil education expenditures and a synthetic control mean of education expenditures, where the synthetic control mean is calculated by assigning time-invariant weights of differing values to non-treated states based on their pre-treatment match (referred to as "SCM"). The solid blue line represents differences between New Hampshire's per pupil education expenditures and a de-biased synthetic control mean of education expenditures, where the de-biased synthetic control mean is calculated by adding a bias correction term to the traditional synthetic control mean (referred to as "Ridge ASCM").

Figure 2. Ridge ASCM effect estimates of SFRs on per capita and per pupil elementary-secondary education expenditures

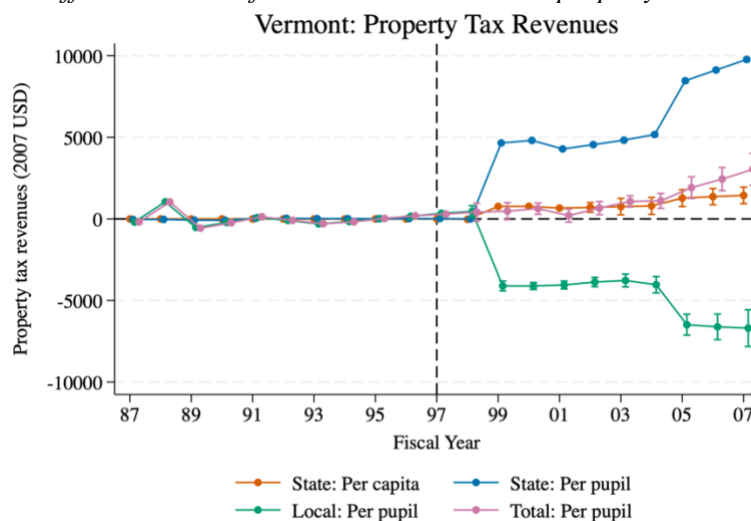


Notes: Ridge ASCM effect estimates for each state-year relative to its counterfactual are shown. Circular markers represent effect estimates, and spikes represent 90% confidence intervals. The vertical dashed line denotes the fiscal year of the SFR reform.

Figure 3. Ridge ASCM effect estimates of Vermont's 1997 SFR

Figure 3a. Ridge ASCM effect estimates of Vermont's 1997 SFR on state revenues and expenditures


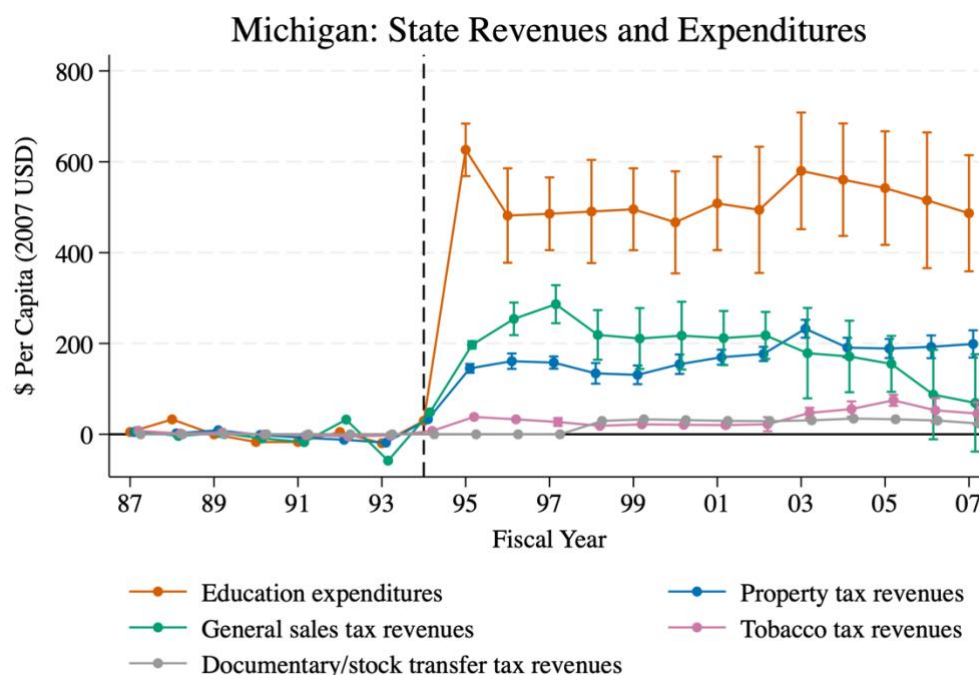
Notes: Ridge ASCM effect estimates for each state-year relative to its counterfactual are shown. Circular markers represent effect estimates, and spikes represent 90% confidence intervals (spikes may not be visible if the standard errors on which the spikes are based are relatively small in magnitude). The vertical dashed line denotes the fiscal year of the SFR reform.

Figure 3b. Ridge ASCM effect estimates of Vermont's 1997 SFR on property tax revenues


Notes: Ridge ASCM effect estimates for each state-year relative to its counterfactual are shown. Circular markers represent effect estimates, and spikes represent 90% confidence intervals (spikes may not be visible if the standard errors on which the spikes are based are relatively small in magnitude). The vertical dashed line denotes the fiscal year of the SFR reform. Total per pupil property tax revenues is the sum of per pupil local and state property tax revenues. Local and total property tax revenues are scaled by student enrollment, not population, due to the US Census Bureau data not including a measure of the population associated with local (i.e., school district) boundaries.

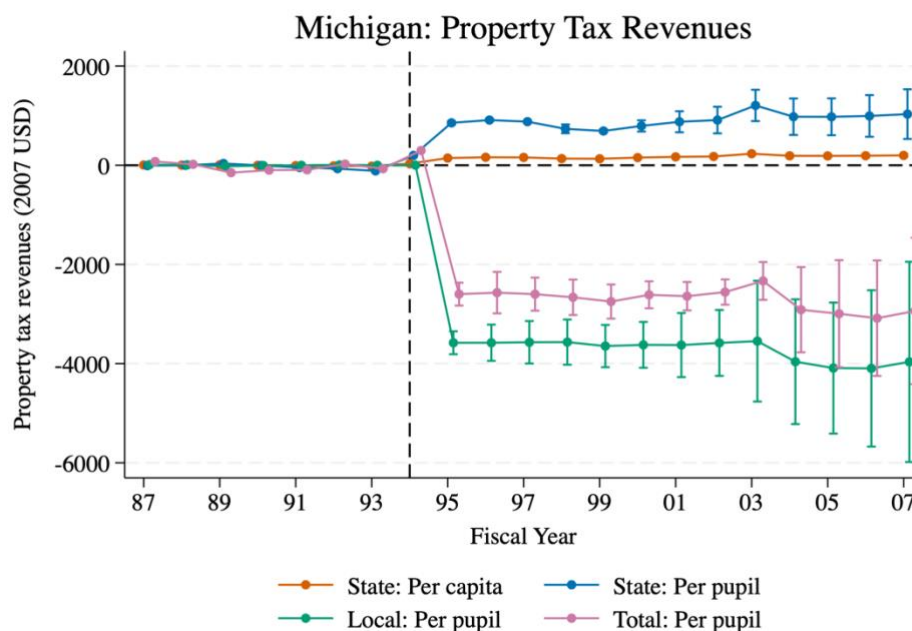
Figure 4. Ridge ASCM effect estimate of Michigan's 1994 SFR

Figure 4a. Ridge ASCM effect estimates of Michigan's 1994 SFR on state revenues and expenditures



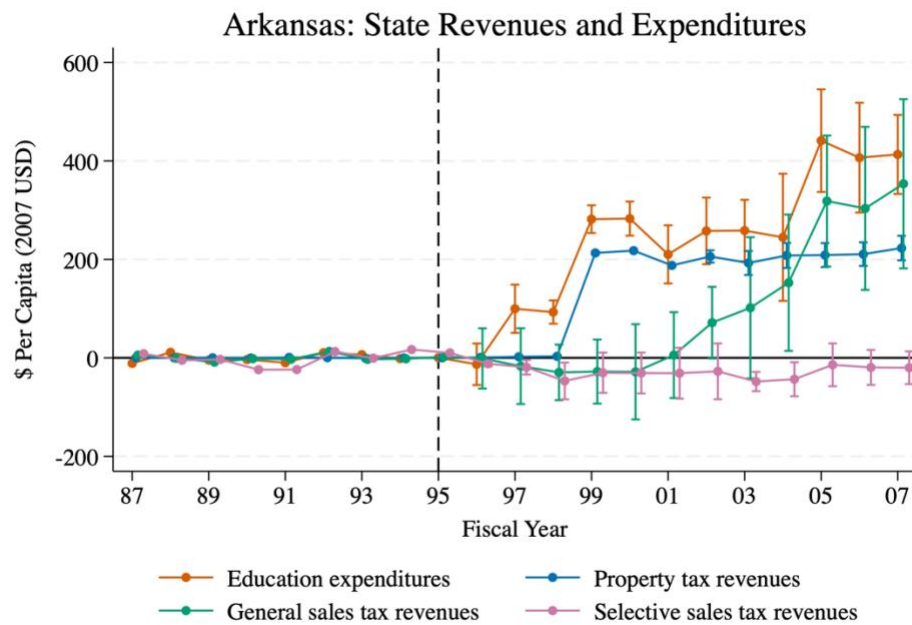
Notes: Because Figure 4 through Figure 9 are similar in appearance, detailed notes regarding how to interpret figures are only provided after Figure 3 for parsimony.

Figure 4b. Ridge ASCM effect estimates of Michigan's 1994 SFR on property tax revenues

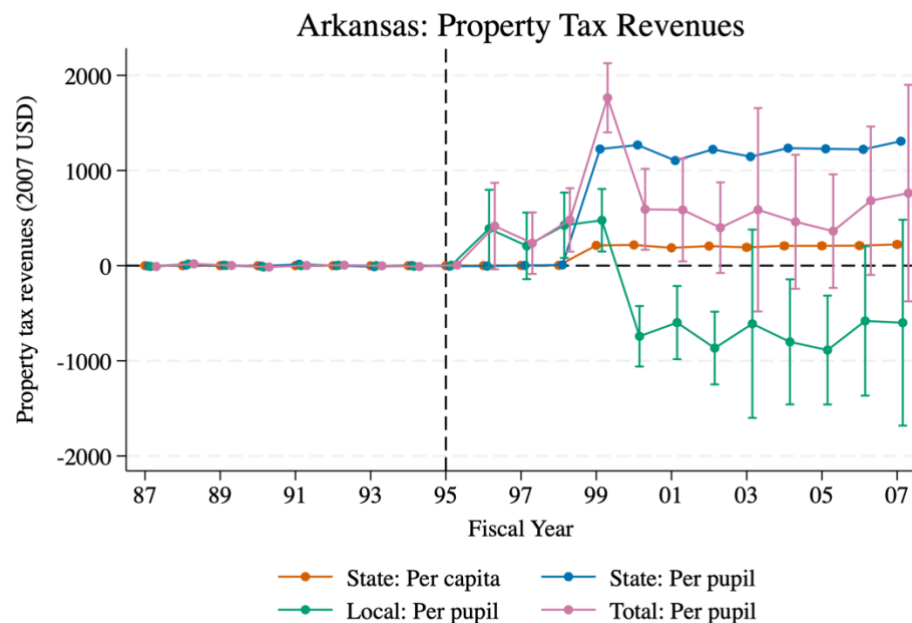


Notes: Because Figure 4 through Figure 9 are similar in appearance, detailed notes regarding how to interpret figures are only provided after Figure 3 for parsimony.

Figure 5. Ridge ASCM effect estimates of Arkansas 1995 SFR

Figure 5a. Ridge ASCM effect estimates of Arkansas 1995 SFR on state revenues and expenditures


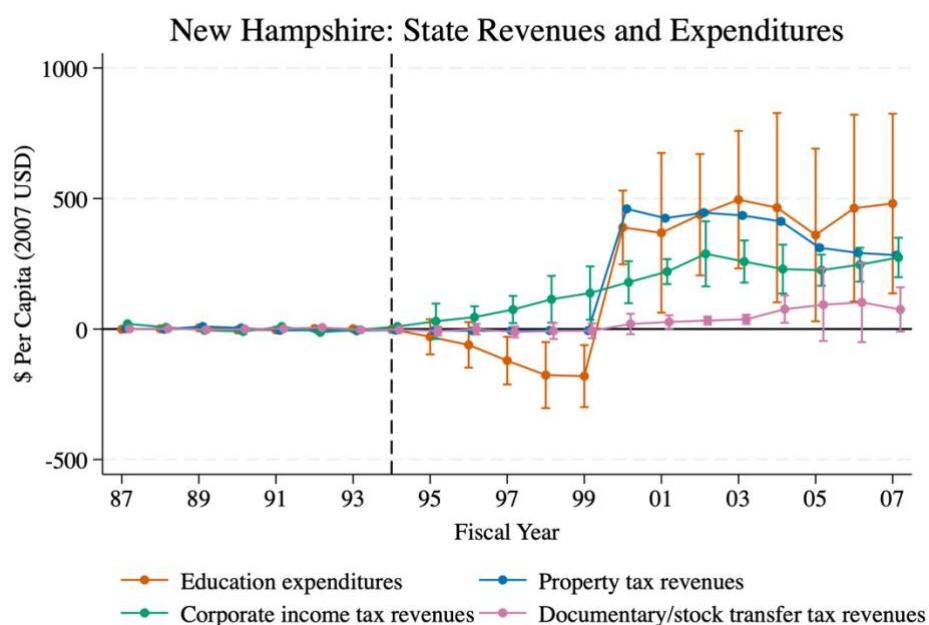
Notes: Because Figure 4 through Figure 9 are similar in appearance, detailed notes regarding how to interpret figures are only provided after Figure 3 for parsimony.

Figure 5b. Ridge ASCM effect estimates of Arkansas' 1995 SFR on property tax revenues


Notes: Because Figure 4 through Figure 9 are similar in appearance, detailed notes regarding how to interpret figures are only provided after Figure 3 for parsimony.

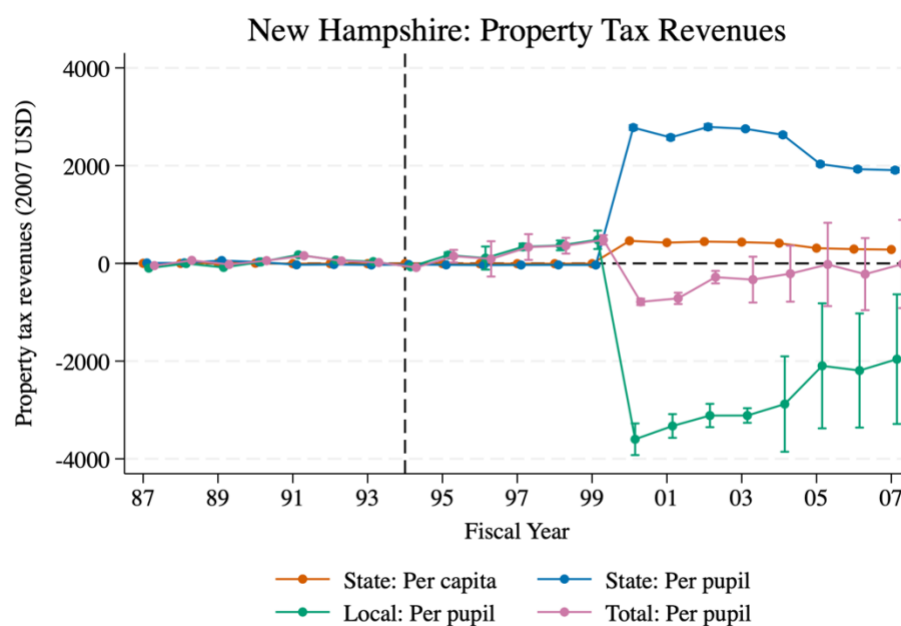
Figure 6. Ridge ASCM effect estimates of New Hampshire's 1994 SFR

Figure 6a. Ridge ASCM effect estimates of New Hampshire's 1994 SFR on state revenues & expenditures



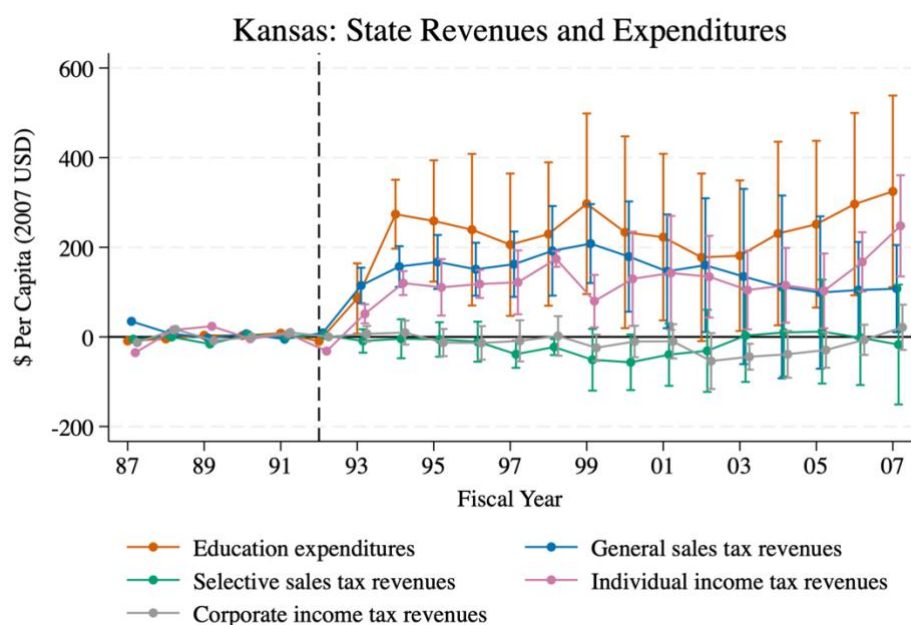
Notes: Because Figure 4 through Figure 9 are similar in appearance, detailed notes regarding how to interpret figures are only provided after Figure 3 for parsimony.

Figure 6b. Ridge ASCM effect estimates of New Hampshire's 1994 SFR on property tax revenues

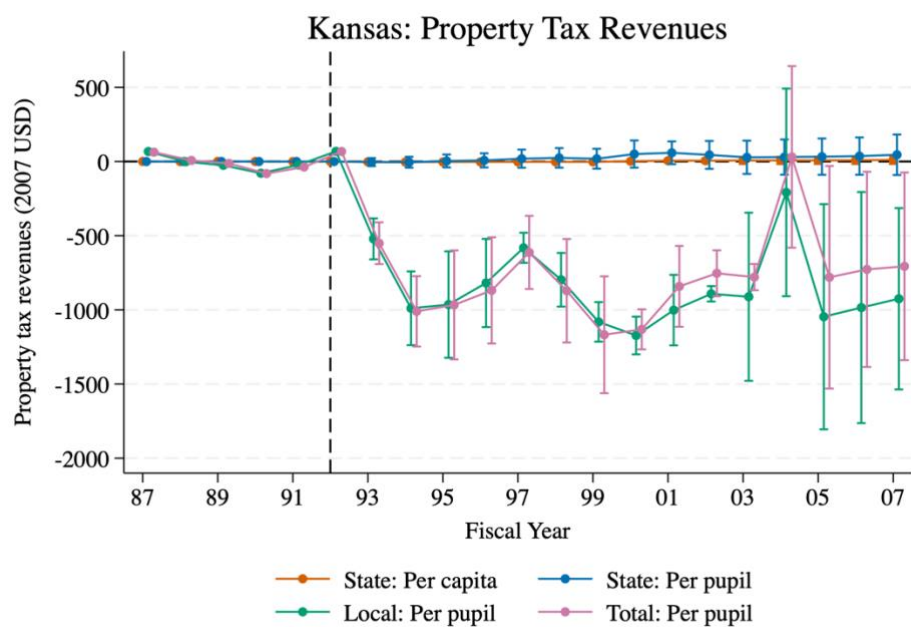


Notes: Because Figure 4 through Figure 9 are similar in appearance, detailed notes regarding how to interpret figures are only provided after Figure 3 for parsimony.

Figure 7. Ridge ASCM effect estimates of Kansas' 1992 SFR

Figure 7a. Ridge ASCM effect estimates of Kansas' 1992 SFR on state revenues and expenditures


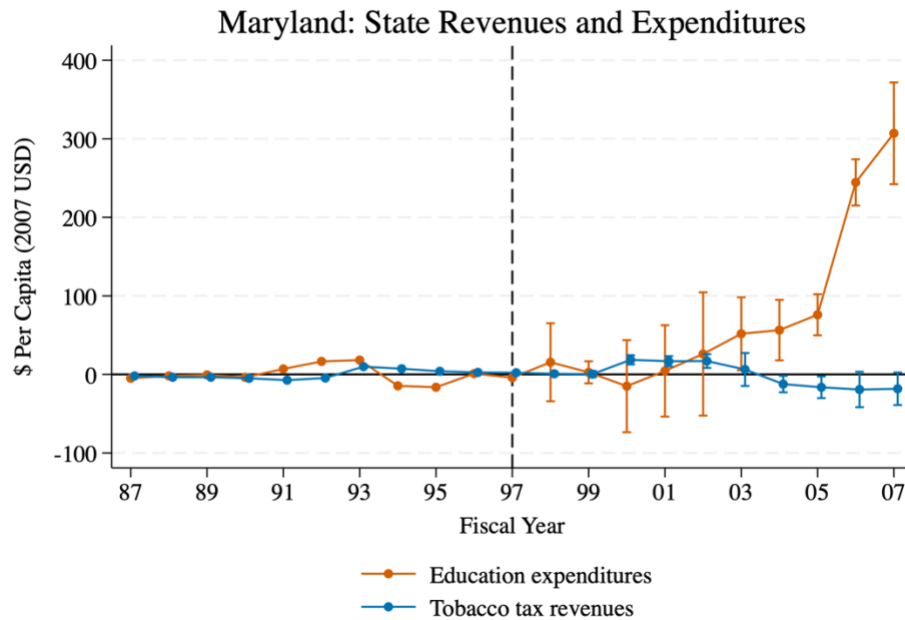
Notes: Because Figure 4 through Figure 9 are similar in appearance, detailed notes regarding how to interpret figures are only provided after Figure 3 for parsimony.

Figure 7b. Ridge ASCM effect estimates of Kansas' 1992 SFR on property tax revenues


Notes: Because Figure 4 through Figure 9 are similar in appearance, detailed notes regarding how to interpret figures are only provided after Figure 3 for parsimony.

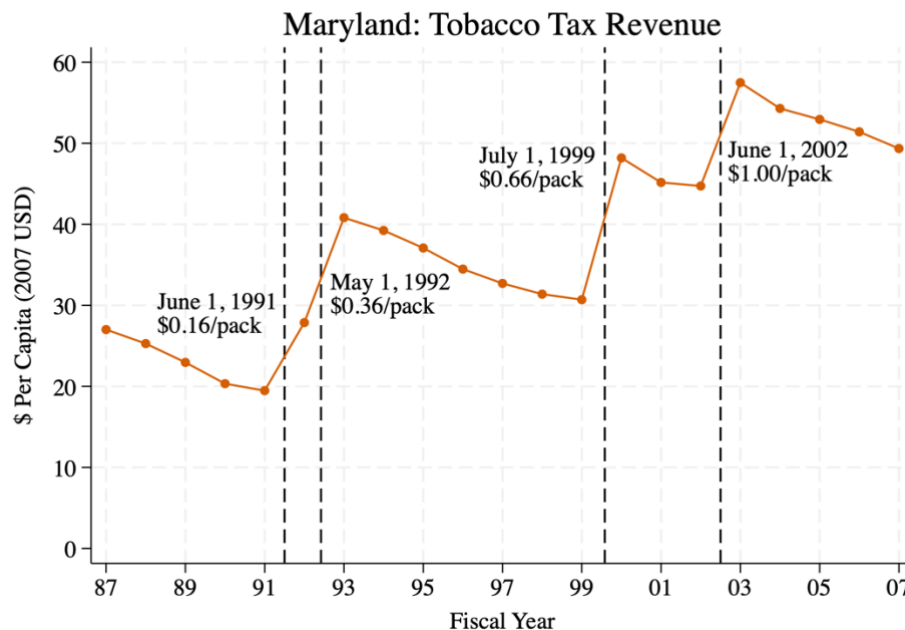
Figure 8. Ridge ASCM effect estimates of Maryland's 1997 SFR and time series of tobacco tax revenues

Figure 8a. Ridge ASCM effect estimates of Maryland's 1997 SFR on state revenues and expenditures

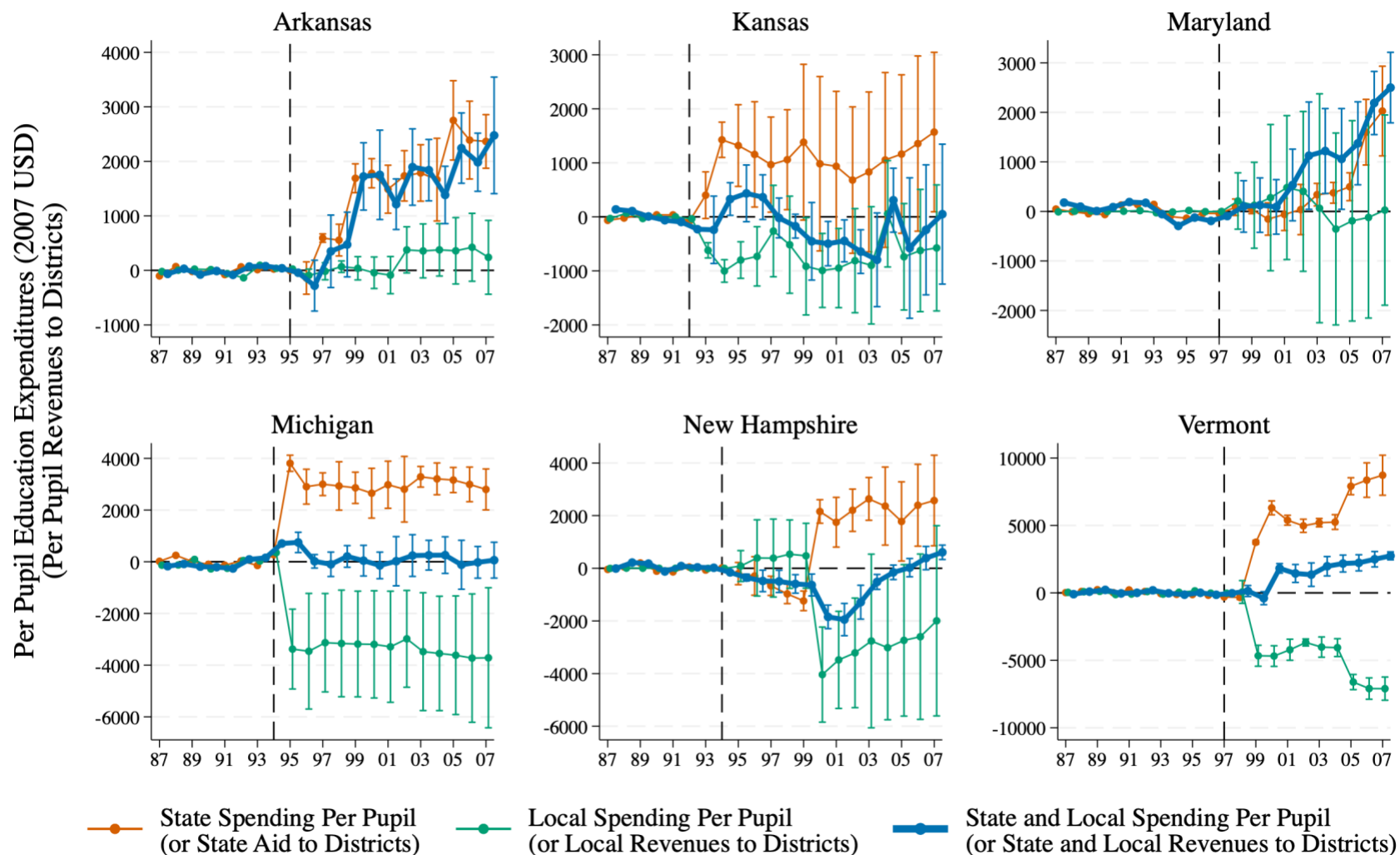


Notes: Because Figure 4 through Figure 9 are similar in appearance, detailed notes regarding how to interpret figures are only provided after Figure 3 for parsimony.

Figure 8b. Time series of tobacco tax revenues



Notes: Vertical dashed lines reflect tax increases on packs of cigarettes during the sample period. Linear declines in revenues after a tax increase reflect a consumer behavioral response to the tax increases.

Figure 9. Ridge ASCM effect estimates of SFRs on state and local expenditures per pupil


ONLINE APPENDIX

Appendix A. Creation of SFR Event List

Appendix Table A1 lists all of the SFRs that occurred between fiscal years [FYs] 1989 and 2005, based on tabulations by Shores et al. (2021).¹ Both legislative- and court-ordered reforms are included. Minor revisions to names of court orders or legislative bills, as well as historical decision dates, have also been made based on our own research. Historical decision dates of the SFRs, reported in calendar years, were converted into fiscal years that align with the reporting of the expenditure data collected in the Annual Survey of State Government Finances by the U.S. Census Bureau.

The Census Bureau collects financial data so that it covers each state government's own fiscal period rather than a standard calendar-based reporting period. Most state governments have a fiscal calendar that runs from July 1 to the following June 30, although a few states are exceptions (see state fiscal calendars in the fourth column of Table A1). New York's fiscal calendar runs from April 1 to the following March 31; Texas' fiscal calendar runs from September 1 to the following August 31; and the fiscal calendar for Alabama and Michigan runs from October 1 to the following September 30. We convert historical decision dates of SFRs into fiscal years based on the state and its associated fiscal calendar in which the reform occurred. For example, an SFR that occurred on May 1, 1995, in New York would be coded as occurring in FY 1996 based on New York's April 1 – March 31 fiscal calendar. In contrast, an SFR that occurred on May 1, 1995, in Tennessee would be coded as occurring in FY1995 based on Tennessee's

¹ In addition to the SFRs identified by Shores et al. (2021), we also include Michigan's 1994 passage of Proposal A as a legislative SFR. Shores et al. (2021) did not consider Proposal A to be an SFR because it was a constitutional amendment approved of by state voters (ref. footnote 8). However, Proposal A was originally drafted and referred to voters by the state legislature (and thus is considered a *legislatively-referred* constitutional amendment). Because the legislature was responsible for initiating Proposal A, we consider Proposal A to fall within the definition of a legislative SFR and thus include it in our list of court-ordered and legislative SFRs that occurred between FYs 1989-2005.

July 1 – June 30 fiscal calendar. For more information on the calendar to fiscal year conversion process, see Section 3.2 Fiscal Years as Statistical Reporting Tools of the U.S. Census Bureau's 2006 Government Finance and Employment Manual

(<https://www.census.gov/govs/classification/>).

Appendix Table A1. List of School Finance Reforms, Fiscal Years 1989-2005

State	Court Case or Legislative Bill	Historical Decision Date	Fiscal Calendar	Converted Fiscal Year
Alaska	Kasayulie v. State of Alaska	1-Sep-99	Jul 1 - Jun 30	2000
Arizona	Roosevelt v. Bishop	21-Jul-94	Jul 1 - Jun 30	1995
Arizona	Hull v. Albrecht	23-Dec-97	Jul 1 - Jun 30	1998
Arizona	Hull v. Albrecht	18-Feb-98	Jul 1 - Jun 30	1998
Arkansas	Lake View v. Huckabee I	1-Dec-94	Jul 1 - Jun 30	1995
Arkansas	Equitable School Finance System Act (Act 917)	1-Feb-95	Jul 1 - Jun 30	1995
Arkansas	Lake View v. Huckabee II	21-Nov-02	Jul 1 - Jun 30	2003
Arkansas	Lake View v. Huckabee III	5-May-05	Jul 1 - Jun 30	2005
California	Leroy F. Greene School Facilities Act of 1998	27-Aug-98	Jul 1 - Jun 30	1999
California	Senate Bill 6, Senate Bill 550, Assembly Bill 1550, Assembly Bill 2727, and Assembly Bill 3001	1-Aug-04	Jul 1 - Jun 30	2005
Colorado	Senate Bill 181; Various Other Acts	1-Jul-00	Jul 1 - Jun 30	2001
Idaho	Idaho Schools for Equal Educational Opportunity v. State I	18-Mar-93	Jul 1 - Jun 30	1993
Idaho	Senate Bill 1560	1-Mar-94	Jul 1 - Jun 30	1994
Kansas	The School District Finance and Quality Performance Act	20-May-92	Jul 1 - Jun 30	1992
Kansas	Montoy v. State; Montoy v. State funding increases	3-Jan-05	Jul 1 - Jun 30	2005
Kentucky	Rose v. Council for Better Education, Inc.	28-Sep-89	Jul 1 - Jun 30	1990
Kentucky	Kentucky Education Reform Act (House Bill 940)	24-Mar-90	Jul 1 - Jun 30	1990

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Appendix Table A1 - continued from the previous page.

State	Court Case or Legislative Bill	Historical Decision Date	Fiscal Calendar	Converted Fiscal Year
Maryland	Bradford v. Maryland State Board of Education	18-Oct-96	Jul 1 - Jun 30	1997
Maryland	Bridge to Excellence in Public Schools Act	6-May-02	Jul 1 - Jun 30	2002
Massachusetts	McDuffy v. Secretary of the Executive Office of Education	15-Jun-93	Jul 1 - Jun 30	1993
Massachusetts	Massachusetts Education Reform Act	18-Jun-93	Jul 1 - Jun 30	1993
Michigan	Proposal A	15-Mar-94	Oct 1 – Sep 30	1994
Missouri	Committee for Educational Equality v. State of Missouri	1-Jan-93	Jul 1 - Jun 30	1993
Missouri	Outstanding Schools Act (Senate Bill 380)	1-Aug-93	Jul 1 - Jun 30	1994
Missouri	Senate Bill 287	29-Jun-05	Jul 1 - Jun 30	2005
Montana	House Bill 667	1-Apr-93	Jul 1 - Jun 30	1993
Montana	Columbia Falls Elementary School v. State	22-Mar-05	Jul 1 - Jun 30	2005
New Hampshire	Claremont v. Governor I	30-Dec-93	Jul 1 - Jun 30	1994
New Hampshire	Claremont v. Governor II	17-Dec-97	Jul 1 - Jun 30	1998
New Hampshire	Claremont v. Governor III	15-Oct-99	Jul 1 - Jun 30	2000
New Hampshire	Opinion of the Justices–School Financing (Claremont VI)	7-Dec-00	Jul 1 - Jun 30	2001
New Hampshire	Claremont v. Governor IV	11-Apr-02	Jul 1 - Jun 30	2002

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Appendix Table A1 - continued from the previous page.

Court Case or Legislative Bill		Historical Decision Date	Fiscal Calendar	Converted Fiscal Year
New Jersey	Abbott v. Burke II	5-Jun-90	Jul 1 - Jun 30	1990
New Jersey	The Quality Education Act	3-Jul-90	Jul 1 - Jun 30	1991
New Jersey	Abbott v. Burke III	12-Jul-94	Jul 1 - Jun 30	1995
New Jersey	Comprehensive Educational Improvement and Financing Act of 1996	20-Dec-96	Jul 1 - Jun 30	1997
New Jersey	Abbott v. Burke IV	14-May-97	Jul 1 - Jun 30	1997
New Jersey	Abbott v. Burke V	21-May-98	Jul 1 - Jun 30	1998
New Jersey	Abbott v. Burke VI	7-Mar-00	Jul 1 - Jun 30	2000
New Mexico	Zuni School District v. State	14-Oct-99	Jul 1 - Jun 30	2000
New Mexico	Deficiencies Corrections Program; Public School Capital Outlay	5-Apr-01	Jul 1 - Jun 30	2001
New York	Campaign for Fiscal Equity, Inc. v. State	26-Jun-03	Apr 1 - Mar 31	2004
North Carolina	Leandro v. State	24-Jul-97	Jul 1 - Jun 30	1998
North Carolina	Hoke County Board of Education v. State	30-Jul-04	Jul 1 - Jun 30	2005
Ohio	DeRolph v. State I	25-Apr-97	Jul 1 - Jun 30	1997
Ohio	DeRolph v. State II	11-May-00	Jul 1 - Jun 30	2000
Ohio	DeRolph v. Ohio III	6-Sep-2001	Jul 1 - Jun 30	2002
Ohio	DeRolph v. Ohio IV	11-Dec-02	Jul 1 - Jun 30	2003
Tennessee	The Education Improvement Act	11-Mar-92	Jul 1 - Jun 30	1992
Tennessee	Tennessee Small School Systems v. McWheter I	22-Mar-93	Jul 1 - Jun 30	1993
Tennessee	Tennessee Small School Systems v. McWheter II	16-Feb-95	Jul 1 - Jun 30	1995

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Appendix Table A1 - continued from the previous page.

State	Court Case or Legislative Bill	Historical Decision Date	Fiscal Calendar	Converted Fiscal Year
Tennessee	Tennessee Small School Systems v. McWheter III	8-Oct-02	Jul 1 - Jun 30	2003
Texas	Edgewood ISD v. Kirby	22-Jan-91	Sept 1 - Aug 31	1991
Texas	Carrollton-Farmers Branch ISD v. Edgewood ISD	30-Jan-92	Sept 1 - Aug 31	1992
Texas	Senate Bill 7	31-May-93	Sept 1 - Aug 31	1993
Vermont	Brigham v. State	5-Feb-97	Jul 1 - Jun 30	1997
Vermont	Revisions to Act 68; H.480	18-Jun-03	Jul 1 - Jun 30	2003
West Virginia	Tomblin v. Gainer	1-Aug-00	Jul 1 - Jun 30	2001
Wyoming	Campbell County School District v. State I	8-Nov-95	Jul 1 - Jun 30	1996
Wyoming	The Education Resource Block Grant Model	Apr-97	Jul 1 - Jun 30	1997
Wyoming	Wyoming Comprehensive Assessment System	Jun-97	Jul 1 - Jun 30	1997
Wyoming	Campbell County School District v. State II; Recalibration of the MAP model	23-Feb-01	Jul 1 - Jun 30	2001

Notes: Bolded survey years indicate the first year that a state's funding system was altered in the adequacy era by a legislative or court-ordered SFR reform and is considered the start of treatment.

Appendix B. SCM and Ridge ASCM

Traditional SCM Notation

We discuss the traditional synthetic control method (SCM), which serves as the basis for ridge ASCM, within the context of this study. We use a similar notation as Abadie, Diamond, and Hainmueller (2010) and Ben-Michael et al. (2020). We first describe general notation regarding the sample and time frame. During the analysis period of this study (fiscal years 1988-89 to 2006-07), 24 states experienced an SFR and thus are considered treated, while 26 states did not experience an SFR and thus are considered control. In order to compute state-specific treatment effect estimates, we run separate synthetic control models for each treated state. Thus, our analytic sample, s , includes one treated state + 26 non-treated control states = 27 states for any given analysis. We designate the first unit of the analytic sample (i.e., $s = 1$) to be the treated state.

Our dataset includes 26 years of fiscal data on state revenues and expenditures from fiscal years 1981-82 to 2006-07. Fiscal years are denoted as t , where $t \in \{1, 2, \dots, 26\}$. Although we observe all 26 fiscal years of data for each state, the number of years before an SFR (i.e., pre-treatment years) and the number of years after an SFR (i.e., post-treatment years) will vary by treated state depending on when their SFR occurred. We denote the number of pre-treatment years as T_0 . Because we assume treatment begins in the *following* fiscal year after an SFR occurs, we operationalize T_0 to include the fiscal year in which an SFR occurs. The first SFR that occurred during this study's analytic period was in fiscal year 1990 (Kentucky), and the most recent was in fiscal year 2004 (New York). Thus, T_0 ranges from 9 to 23, depending on the treated state.

SCM formally estimates dynamic treatment effects as follows:

$$\hat{\gamma}_{1t}^{SCM} = Y_{1t} - \hat{Y}_{1t}^{SCM}(0) = Y_{1t} - \sum_{s=2}^{27} \hat{w}_s^{SCM} Y_{st} \text{ for } t > T_0, \quad (1)$$

where $\hat{\gamma}_{1t}^{SCM}$ represents the estimated treatment effect for treated state $s = 1$ at time t , Y represents the expenditure or revenue outcome of interest, and \hat{w}_s^{SCM} is a time-invariant, optimally chosen weight for each state s in the non-treated control group. T_0 represents the number of pre-treatment years. $\hat{w}_s^{SCM} Y_{st}$ can be interpreted as a weighted average of control outcomes, and it characterizes the counterfactual outcome of the treated state in year t if it had not undergone reform.

The state-specific weights w_s^{SCM} are estimated using a minimization procedure that attempts to set all differences between the pre-treatment outcomes of the treated state and the pre-treatment outcomes of the non-treated control states to zero. Pre-treated outcomes are denoted as X_{st} . Specifically, the minimization procedure attempts to satisfy the following conditions for $t \leq T_0$:

$$\begin{aligned} \sum_{s=2}^{27} w_s^{SCM} X_{s1} &= X_{11} \\ \sum_{s=2}^{27} w_s^{SCM} X_{s2} &= X_{12} \\ &\vdots \\ \sum_{s=2}^{27} w_s^{SCM} X_{sT_0} &= X_{1T_0}, \end{aligned} \quad (2)$$

where $w_2^{SCM} + w_3^{SCM} + \dots + w_s^{SCM} = 1$. In other words, the sum of all weights must equal one.

The system of equations above demonstrates that the weighted average of the 26 non-treated control states attempts to balance the treated state's pre-treatment outcomes as closely as

possible. Non-treated control states that more closely resemble the pre-treatment outcome level and trend of the treated state receive larger weights than non-treated control states that less closely resemble the pre-treatment outcome level and trend of the treated state. Abadie et al. (2015) note that if the number of pre-treatment periods in the data is “large,” then matching on pre-treatment outcomes can allow one to control for heterogenous responses to multiple unobserved factors. The idea here is that only units that are alike on unobservable and observable characteristics would follow a similar trajectory pre-treatment.

Ridge ASCM Notation

Ben-Michael et al. (2020) note that SCM should only be used when the level and trend of the synthetic control’s pre-treatment outcomes closely match the level and trend of the treated unit’s pre-treatment outcomes. If the SCM weights do not achieve a good pre-treatment fit, then SCM estimates can be biased. Ben-Michael et al. (2020) propose the ridge augmented synthetic control method [ridge ASCM] as the preferred alternative when a good pre-treatment fit using SCM is not feasible. In short, ridge augmentation provides a way to avoid overfitting to noisy or poor pre-treatment outcome data by taking the traditional SCM estimate, estimating the bias due to poor pre-treatment fit, and then augmenting the traditional SCM estimate by adding a bias correction term. In formal terms, ridge SCM estimates dynamic treatment effects as:

$$\hat{\gamma}_{1t}^{AUG} = Y_{1t} - \hat{Y}_{1t}^{AUG}(0) = Y_{1t} - \left[\underbrace{\sum_{s=2}^{27} \hat{w}_s^{SCM} Y_{st}}_{(1) \text{ SCM estimate}} + \underbrace{\left(X_{1\cdot} - \sum_{s=2}^{27} \hat{w}_s^{SCM} X_{s\cdot} \right)}_{(a) \text{ SCM pre-treatment match quality vector}} \cdot \underbrace{\hat{\eta}_t^r}_{(b) \text{ Ridge coefficient vector}} \right]_{(2) \text{ Bias correction}} \quad \text{for } t > T_0. \quad (3)$$

The (1) *SCM estimate* term can be interpreted as the weighted average of non-treated control outcomes, $\hat{Y}_{1t}^{SCM}(0)$, and was previously shown and described in the traditional SCM Equation (1).

Bias Correction Term in the Ridge ASCM

We now turn to describe the (2) *Bias correction* term from Equation (3). The bias correction term can be broken down into two parts: (a) the SCM pre-treatment match quality vector and (b) the ridge coefficient vector. The SCM pre-treatment match quality vector is a 1-by- T_0 row vector that accounts for the differences between the treated unit and the SCM counterfactual unit for each pre-treatment year t . This match quality vector is calculated using X_1 , which represents a 1-by- T_0 row vector of pre-treatment outcomes for treated state $s = 1$. X_s is also a 1-by- T_0 row vector of pre-treatment outcomes but for each non-treated control state s , where $s \in \{2, 3, \dots, 27\}$. Each X_s row vector is multiplied by the state-specific weight \hat{w}_s^{SCM} . If the pre-treatment differences between the treated unit and the SCM counterfactual unit are small, then SCM pre-treatment match quality is good; if the pre-treatment differences are large, then SCM pre-treatment match quality is poor.

The ridge coefficient vector, $\hat{\eta}_t^r$, is a T_0 -by-1 column vector of coefficients that account for the estimated relationship between pre-treatment outcomes and post-treatment outcomes at post-treatment year t for the *non-treated control* states. The ridge coefficients contained in $\hat{\eta}_t^r$ are estimated using multivariate ridge regression. In simple terms, ridge regression will reduce the $\hat{\eta}_t^r$ coefficient estimates of the relationship between pre-treatment and post-treatment outcomes for the non-treated control states by adding a penalty term. This penalty term leads to a more parsimonious model that will likely perform better at predicting the relationship between pre-treatment and post-treatment outcomes for the *treatment* state. Specifically, multivariate ridge

regression involves regressing the centered, post-treatment outcomes for non-treated control states for post-treatment year t , \tilde{Y}_{st} , on the centered, pre-treatment outcomes for non-treated control states, \tilde{X}'_s , plus the penalty term. The following minimization problem provides the full ridge coefficient matrix, $\hat{\eta}^r$:

$$\min_{\eta^r} \frac{1}{2} \sum_{s=2}^{27} \sum_{t=T_0+1}^T \underbrace{(\tilde{Y}_{st} - \tilde{X}'_s \eta^r)^2}_{\text{Sum of squared residuals}} + \underbrace{\lambda^r \|\eta^r\|_2^2}_{\text{Penalty term}} \quad (4)$$

The penalty term is comprised of a penalty parameter, λ^r , and an L2 penalty, $\|\eta^r\|_2^2$. The L2 penalty is equal to the sum of the squared ridge coefficients contained in η^r . The penalty parameter, λ^r , controls the strength of the penalty term by influencing the magnitude of the η^r coefficient estimates. As $\lambda^r \rightarrow \infty$, the η^r coefficient estimates shrink towards zero. When the η^r coefficient estimates are smaller, the bias correction term in Equation 3 is also smaller, leading the ridge ASCM estimate of $Y_{1t}(0)$ to converge to the SCM estimate of $Y_{1t}(0)$. As $\lambda^r \rightarrow 0$, the η^r coefficient estimates converge towards the coefficient estimates obtained from least squares regression. When the η^r coefficient estimates are larger, the bias correction term in Equation 3 is also larger, leading the ridge ASCM estimate of $Y_{1t}(0)$ to diverge from (and become larger than) the SCM estimate of $Y_{1t}(0)$.

As shown above, the value of the penalty parameter, λ^r , influences the magnitude of the η^r coefficient estimates, which in turn influences the magnitude of the $\hat{Y}_{1t}^{AUG}(0)$ counterfactual estimate. So how does one determine the value of λ^r ? Following Ben-Michael et al. (2020), we use a leave-one-out cross-validation procedure to choose the value of λ^r . Specifically, for a given value of λ^r , we estimate $\hat{X}_{1t}^{AUG}(0)$ across pre-treatment periods (i.e., $t \leq T_0$) using the following ridge ASCM model and data that *excludes* time period t :

$$\hat{X}_{1t}^{AUG} = \underbrace{\sum_{s=2}^{27} \hat{w}_s^{SCM} X_{s(-t)}}_{(1) \text{ SCM estimate}} + \underbrace{\left(X_{1 \cdot (-t)} - \sum_{s=2}^{27} \hat{w}_s^{SCM} X_{s \cdot (-t)} \right)}_{\substack{(a) \text{ SCM pre-treatment} \\ \text{match quality vector}}} \cdot \underbrace{\hat{\eta}_{(-t)}^r}_{\substack{(b) \text{ Ridge} \\ \text{coefficient} \\ \text{vector}}} , \quad \text{for } t < T_0. \quad (5)$$

(2) Bias correction

We then compute the mean squared error (MSE) across pre-treatment periods as follows:

$$CV(\lambda^r) = \sum_{t=1}^{T_0} (X_{1t} - \hat{X}_{1t}^{AUG})^2 \quad (6)$$

We select λ^r as the maximum λ^r with MSE within one standard deviation of the minimum MSE computed from Equation 6.

All ridge ASCM models are estimated using R's *augsynth* command (Ben-Michael, 2020). Standard errors are based on a row-based jackknife to allow for autocorrelation within states (Ben-Michael et al., 2019).

Appendix C. Evaluating Education Expenditure Ridge ASCM Estimates

We evaluate the quality and validity of the per capita and per pupil elementary-secondary education expenditure ridge ASCM estimates using the following three tests. First, we examine whether good pre-treatment fit is achieved by the ridge ASCM models. To assess pre-treatment fit, we plot the dynamic effect estimates for each of the 24 states that experienced an SFR between FY 1989 and 2005. These plots visually illustrate the quality of the pre-treatment match. As shown in Figure C1, effect estimates prior to the fiscal year of the SFR reform (indicated by the vertical dashed line) are close to zero for most states. Thus, the level and trend of each treated state's pre-treatment outcomes appear to closely match the level and trend of its synthetic control's pre-treatment outcomes, suggesting that good pre-treatment fit is achieved.

Second, we assess the percent improvement in the L^2 -norm model fit statistic obtained from the per capita and per pupil elementary-secondary expenditures ridge ASCM models relative to the L^2 -norm statistic obtained from models that use equal or SCM weights. The L^2 -norm statistic indicates the cumulative pre-treatment effect size deviation from zero. A percent improvement value that is closer to 0% indicates that a ridge ASCM model has a similar pre-treatment fit as models that use equal or SCM weights. A percent improvement value that is closer to 100% indicates that a ridge ASCM model has a better pre-treatment fit than models that use equal or SCM weights.

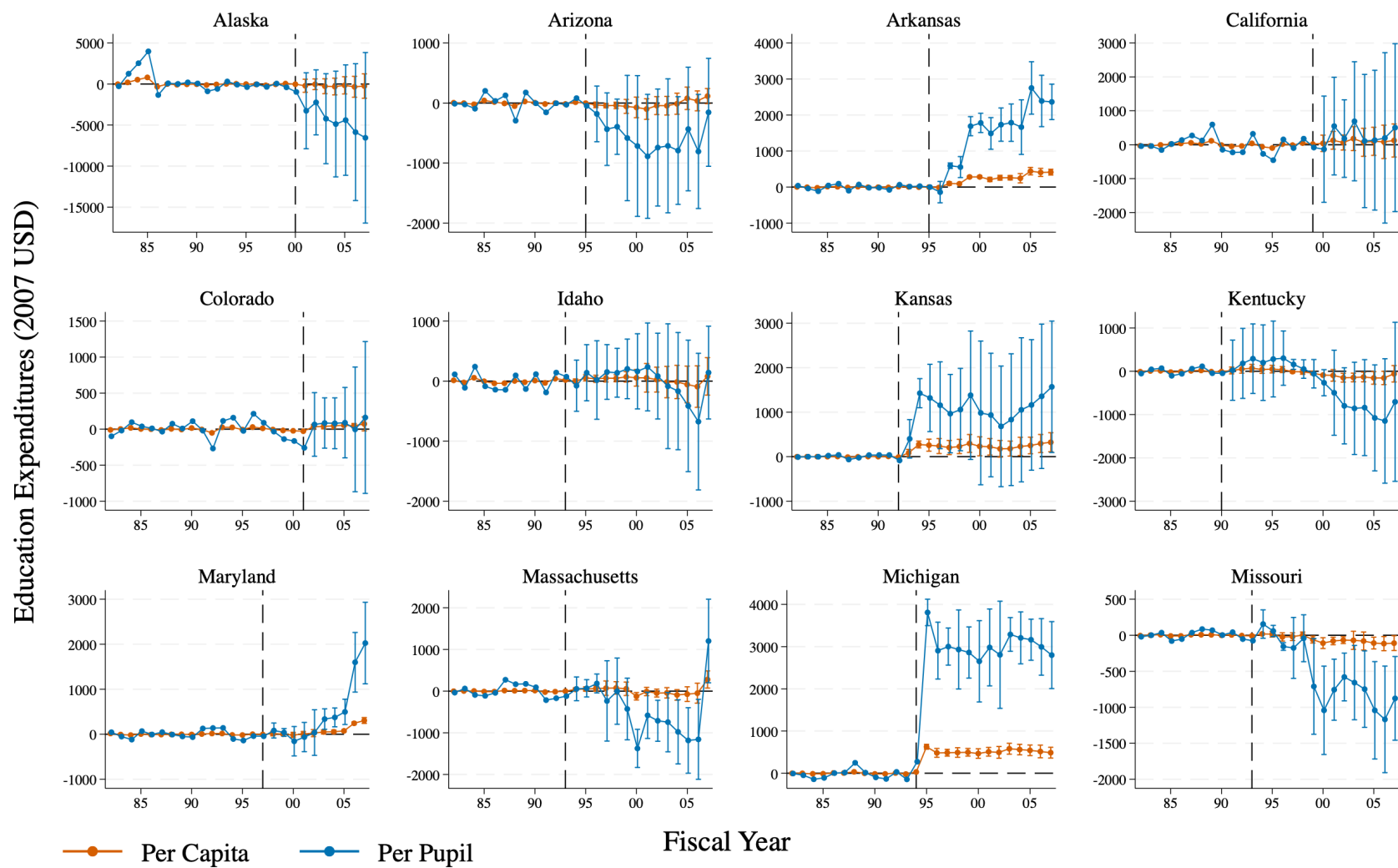
As shown in Figure C2, ridge ASCM models always have better pre-treatment fit than models that use equal weights. For the majority of states, using ridge ASCM weights does not improve pre-treatment fit relative to SCM weights, which suggests that augmenting the traditional SCM estimate by adding a bias correction term (as is done when using ridge ASCM) is not really beneficial. However, for a few states (Alaska, California, Massachusetts, New

Hampshire, New Mexico, Tennessee, and West Virginia), model fit is improved by using ridge ASCM relative to traditional SCM.

Third, we test the sensitivity of the average (i.e., across the treated years) effect estimates, $\bar{\gamma}_1^{AUG}$, obtained from the per capita and per pupil elementary-secondary expenditures ridge ASCM models to different values of the penalty parameter, λ^r (the penalty parameter is discussed in detail in Appendix B). Specifically, we estimate ~ 100 alternative specifications of the ridge ASCM models with imposed values of λ^r between 1×10^{-7} and 9×10^5 . In order to compare the average effect estimate from a model with a specified λ^r to the average effect estimate from our preferred model with a cross-validated λ^r , we calculate an \widehat{effect}^{ratio} statistic equal to $\widehat{effect}^{L(\lambda)} / \widehat{effect}^{CV(\lambda)}$. $\widehat{effect}^{L(\lambda)}$ is the average effect estimate for specification L given penalty parameter λ^r , and $\widehat{effect}^{CV(\lambda)}$ is the average effect estimate from our preferred model with a cross-validated λ^r . To present the \widehat{effect}^{ratio} statistics in a digestible manner, we calculate and plot the median \widehat{effect}^{ratio} for each state and outcome (per capita and per pupil elementary-secondary expenditures).

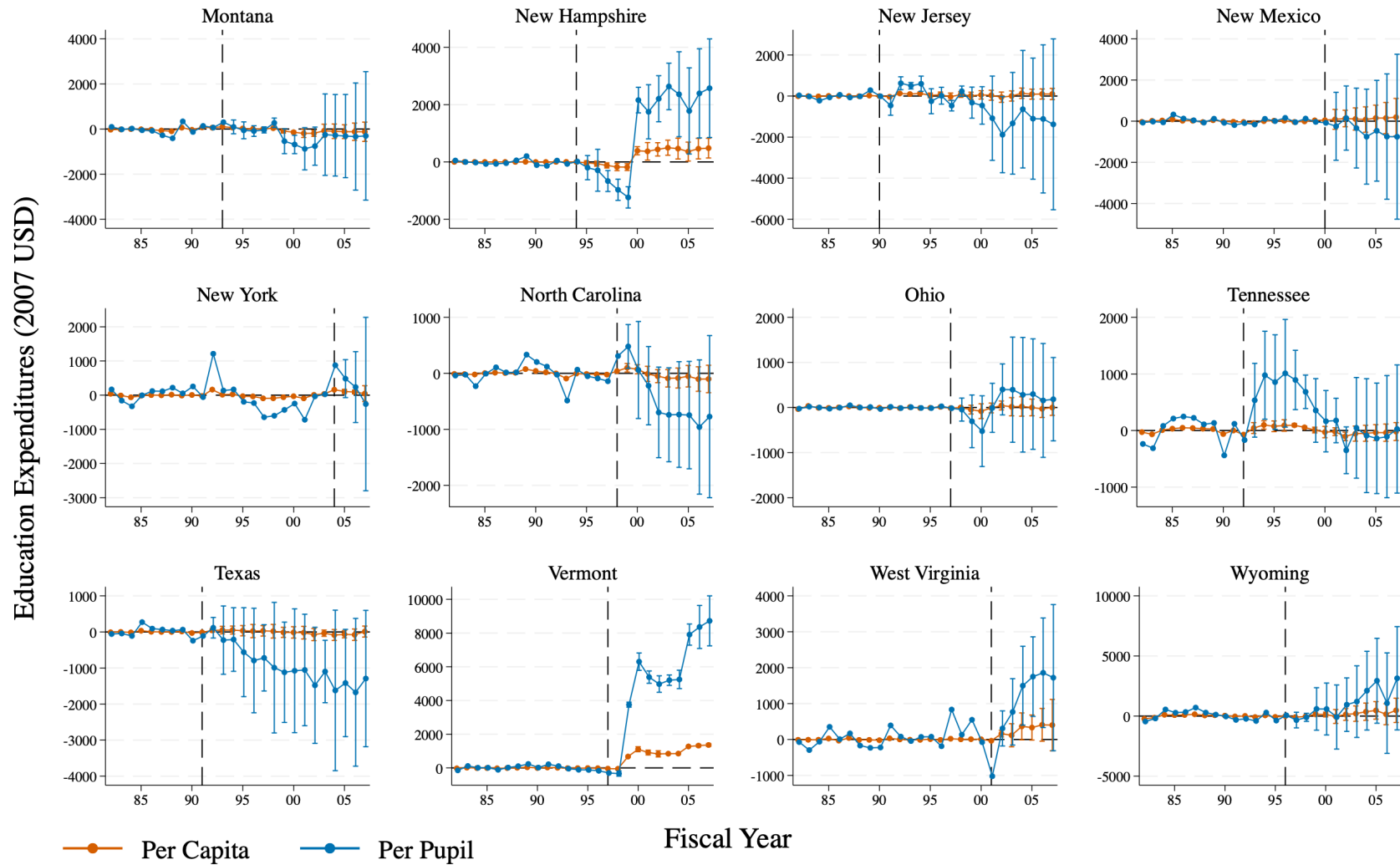
As shown in Figure C3, we find that the median \widehat{effect}^{ratio} for per capita and per pupil education expenditures are centered around one for most states. This implies that the value of λ^r is largely inconsequential in determining the average effect estimate. However, for a few states (California, Colorado, Idaho, and New York), the median \widehat{effect}^{ratio} for per-pupil education expenditures is more widely dispersed. This implies using the cross-validation procedure to select the value of λ^r is useful in that it prevents one from subjectively choosing the λ^r value.

Figure C1. Ridge ASCM effect estimates of SFRs on per capita and per pupil elementary-secondary education expenditures

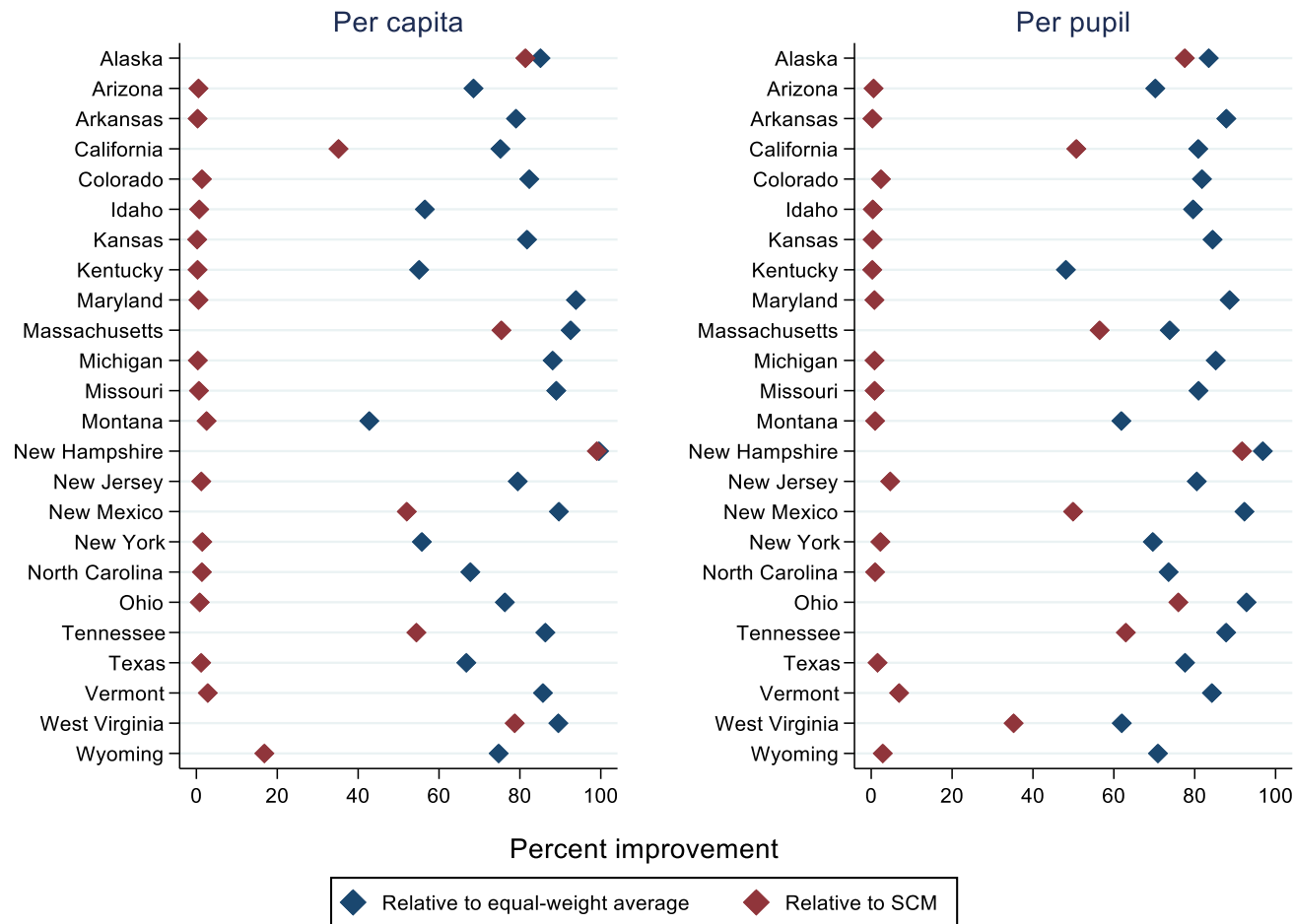


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Figure C1. Ridge ASCM effect estimates – continued from the previous page.

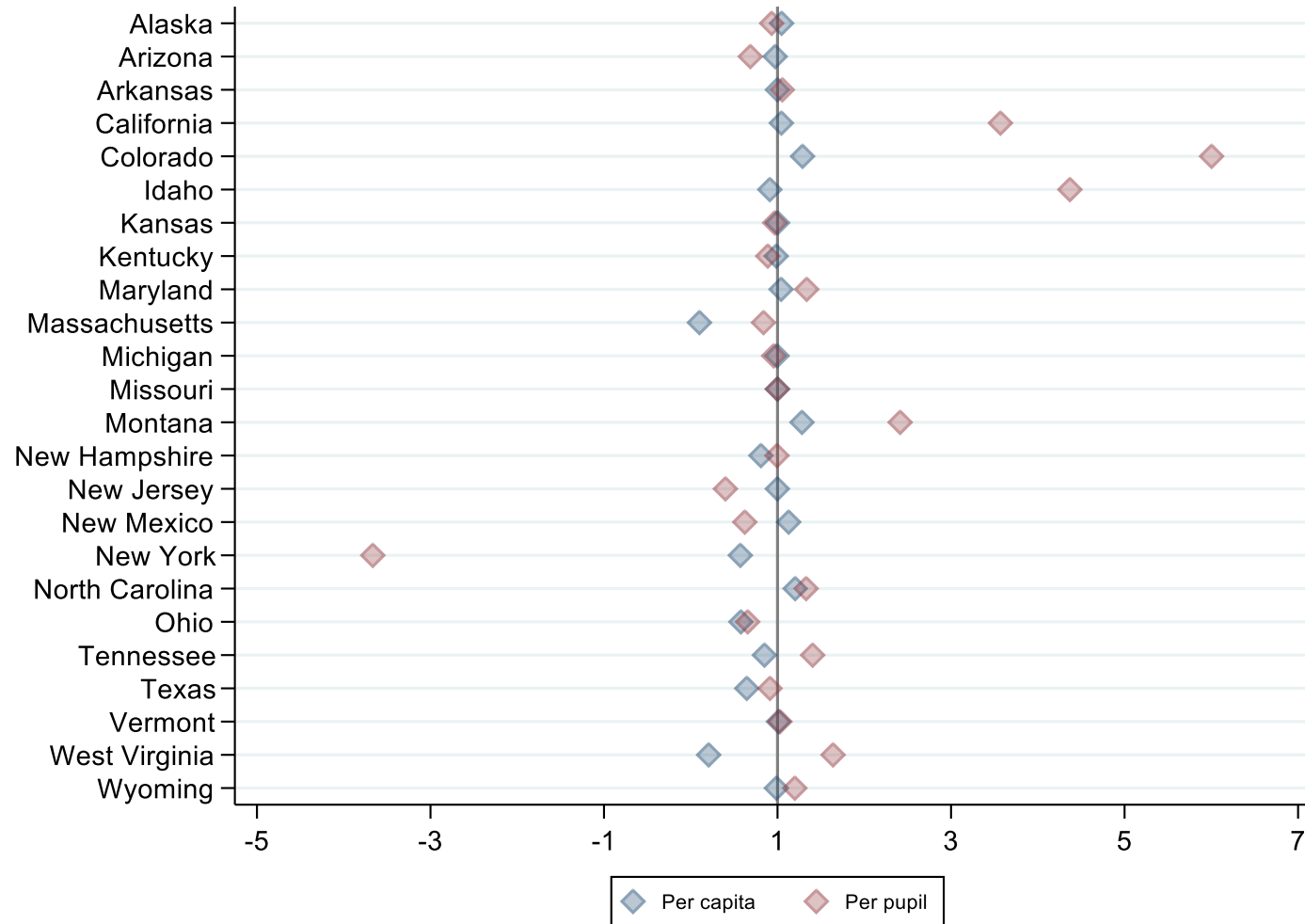


Notes: Ridge ASCM effect estimates for each state-year relative to its counterfactual are shown. Circular markers represent effect estimates, and spikes represent 90% confidence intervals. The vertical dashed line denotes the fiscal year of the SFR reform.

Figure C2. Ridge ASCM percent improvement in pre-treatment model fit, elementary-secondary education expenditures

Notes: The percent improvement in the L^2 -norm model fit statistic obtained from the per capita and per pupil elementary-secondary expenditures ridge ASCM models relative to the L^2 -norm statistic obtained from models that use equal or SCM weights are shown. The L^2 -norm statistic indicates the cumulative pre-treatment effect size deviation from zero. A percent improvement value that is closer to 0% indicates that a ridge ASCM model has a similar pre-treatment fit as models that use equal or SCM weights. A percent improvement value that is closer to 100% indicates that a ridge ASCM model has a better pre-treatment fit than models that use equal or SCM weights.

Figure C3. Sensitivity of average elementary-secondary expenditure effect estimates to different values of the penalty parameter, λ^r



Notes: Each diamond represents the median \widehat{effect}^{ratio} for a state and outcome (per capita or per pupil elementary-secondary education expenditures). \widehat{effect}^{ratio} is equal to $\widehat{effect}^{L(\lambda)} / \widehat{effect}^{CV(\lambda)}$, where $\widehat{effect}^{L(\lambda)}$ is the average effect estimate for specification L given penalty parameter λ^r , and $\widehat{effect}^{CV(\lambda)}$ is the average effect estimate from our preferred model with a cross-validated λ^r .

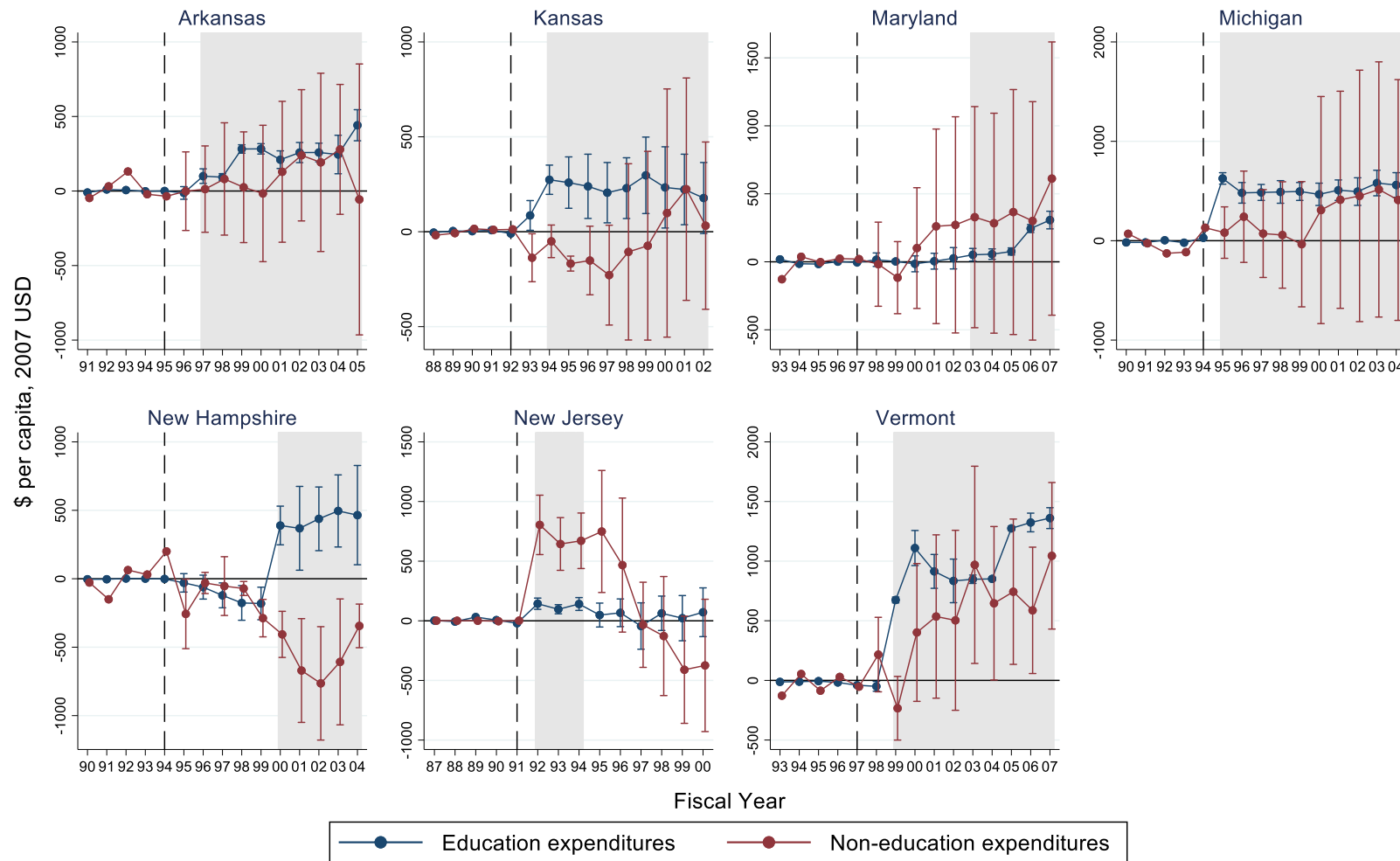
Appendix D. Effect Estimates of SFRs on Non-Education Expenditures and Debt

As mentioned in the “Discussion” section of the main text, we find no legislative evidence that non-education state expenditures were reduced or that state debt was increased to fund increased elementary-secondary education spending following an SFR. However, one may wonder if we find no evidence of reduced non-education expenditures or increased debt because such things just aren’t mentioned in legislative statutes related to increasing education funding (either because they’re not required to be mentioned like raising revenues are or because doing so might have negative ramifications). Because this may certainly be the case, we sought out further evidence by estimating the effects of the seven state SFRs included in the selected analytic sample on per capita non-education state expenditures and total state debt outstanding using ridge ASCM. Non-education state expenditures include all state expenditures besides expenditures on elementary-secondary education (e.g., expenditures on health and hospitals, higher education, highways, corrections, and welfare are included in non-education state expenditures).

As shown in Figures D1 and D2, we do find some quantitative evidence that non-education state expenditures were reduced and that state debt increased in the years following an SFR in a few states. Namely, New Hampshire experienced decreases in non-education state expenditures relative to its synthetic counterfactual in the same years (2000-2004) that education expenditures increased post-SFR. Kansas, Michigan, New Jersey, and Vermont also experienced imprecise increases in total state debt outstanding in similar years as education spending increased following their SFRs. However, it is difficult to determine whether such changes in non-education expenditures and debt were actually the result of SFRs or some other event or policy change that occurred at the same time. Given that a multitude of events and policy

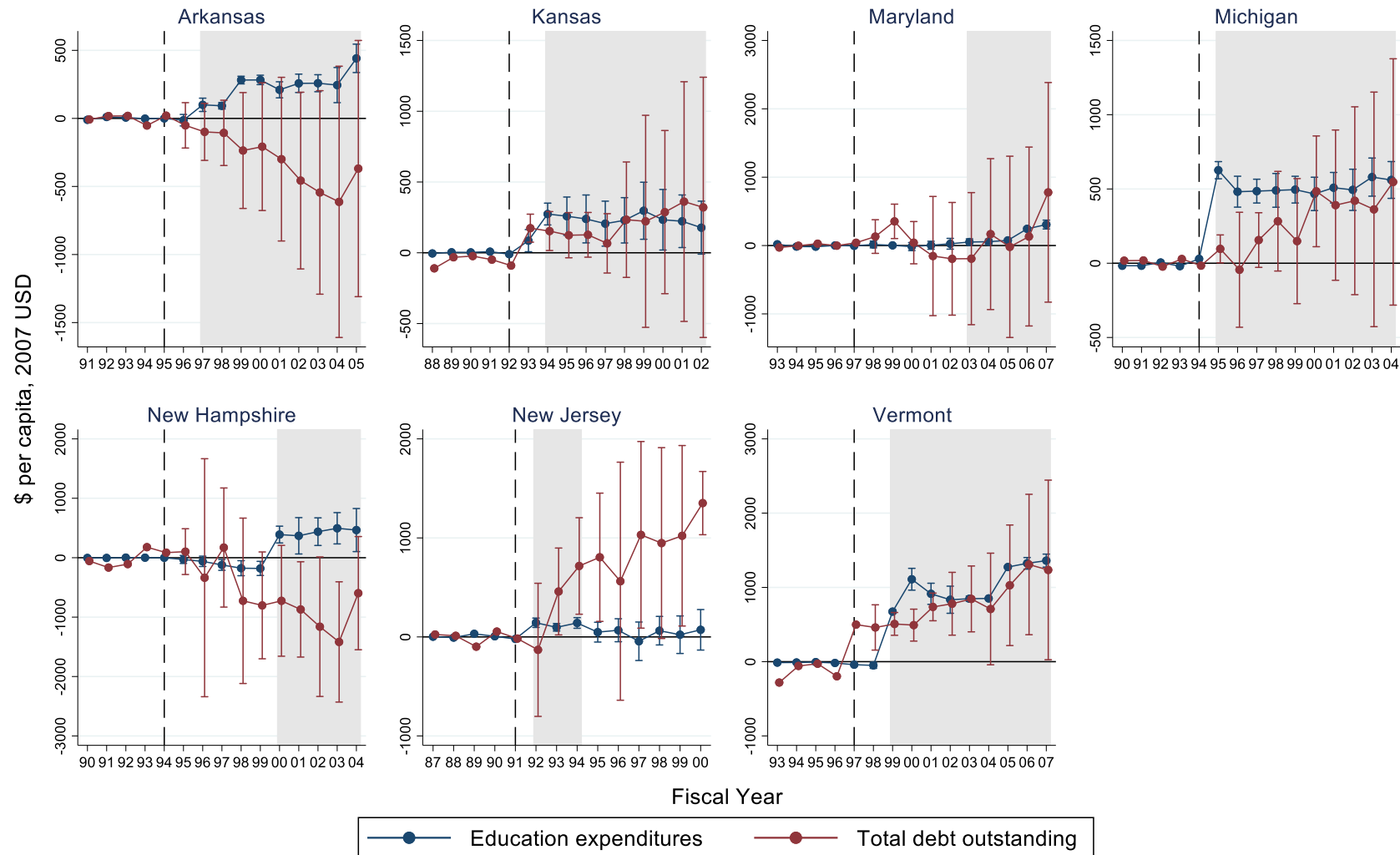
changes related to state revenues and expenditures occur every fiscal year, we sought to verify or corroborate these quantitative findings using other sources of information (e.g., academic journal articles, news articles, published briefs, or reports). We read through all accessible information sources we could find associated with each state SFR and state finances in New Hampshire, Kansas, Michigan, New Jersey, and Vermont. In short, we never found credible evidence of any kind indicating that non-education state expenditures were reduced or that state debt was increased to fund SFRs. Without additional corroborating evidence, we are not confident that the effects on non-education expenditures and state debt (shown in Figures D1 and D2) should rightly be attributed to SFRs. As such, we recommend further examination into whether SFR-induced increases in state education spending impact the funding of other state priorities or state deficits.

Figure D1. Ridge ASCM effect estimates of SFRs on per capita education and non-education expenditures



Notes: Ridge ASCM effect estimates for each state-year relative to its counterfactual are shown. Non-education state expenditures include all state expenditures besides expenditures on elementary-secondary education (e.g., expenditures on health and hospitals, higher education, highways, corrections, and welfare are included in non-education state expenditures). Circular markers represent effect estimates, and spikes represent 90% confidence intervals. The vertical dashed line denotes the fiscal year of the SFR reform. Grey background shading indicates years in which per capita education expenditures significantly increased at the $p < .10$ level.

Figure D2. Ridge ASCM effect estimates of SFRs on per capita education expenditures and total debt outstanding



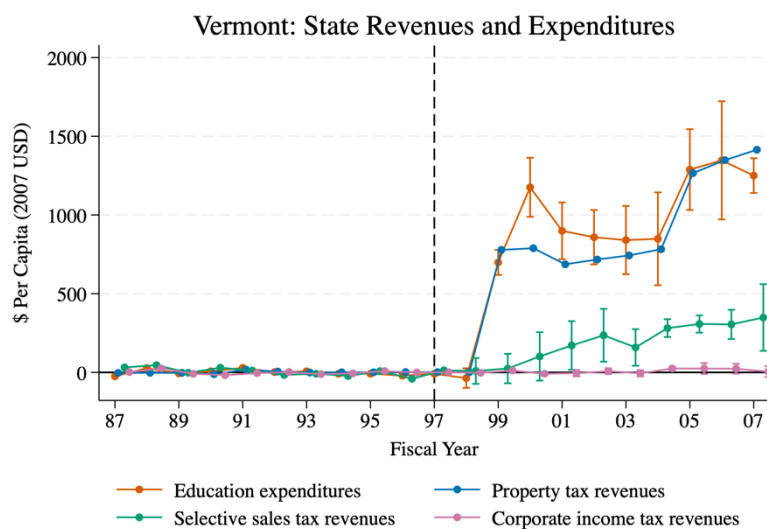
Notes: Ridge ASCM effect estimates for each state-year relative to its counterfactual are shown. Circular markers represent effect estimates, and spikes represent 90% confidence intervals. The vertical dashed line denotes the fiscal year of the SFR reform. Grey background shading indicates years in which per capita education expenditures significantly increased at the $p < .10$ level.

Appendix E. Ridge ASCM with Alternative Comparison Group

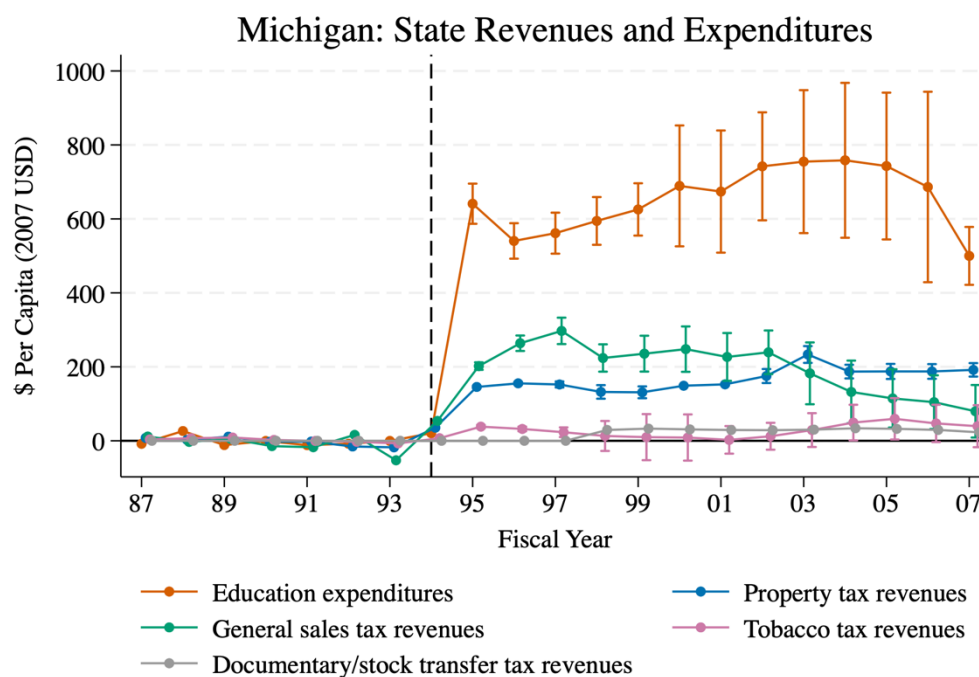
As we mentioned in endnote 3, an anonymous reviewer commented that states which did have an SFR might differ in unobservable ways from those that never had an SFR. While they acknowledged it was appropriate to have the donor pool of control states be only the never-treated states for RQ 1, they were concerned about leveraging this donor pool for RQ 2. Based on the reviewer's suggestion, we re-estimated our ridge ASCM model for our six case study states using a comparison group that consisted of the states that had an SFR but were unable to increase and sustain expenditures after their SFR. We present the results across Figures E1 to E6.

Figure E1. Ridge ASCM effect estimates of Vermont's 1997 SFR

Ridge ASCM effect estimates of Vermont's 1997 SFR on state revenues and expenditures



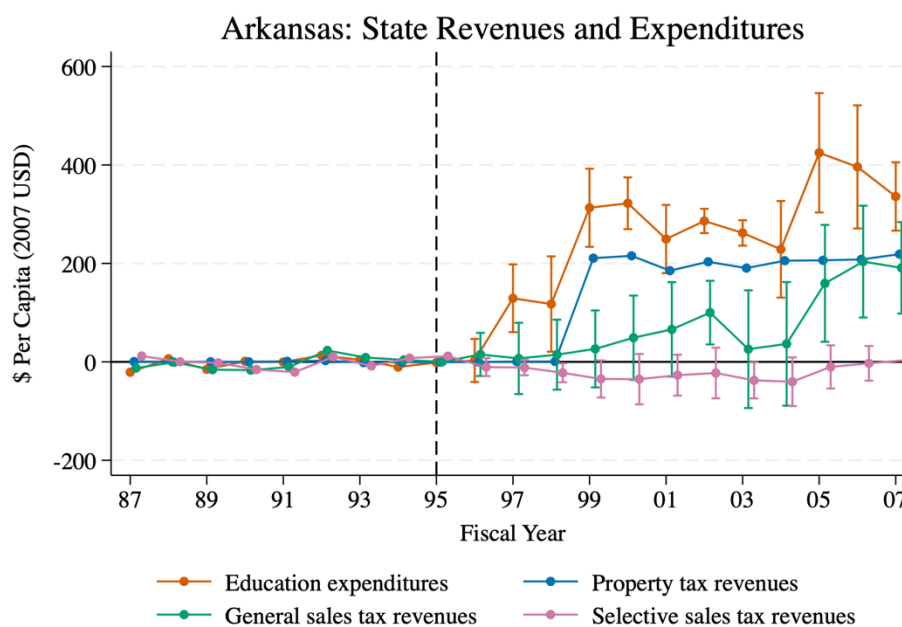
Notes: Ridge ASCM effect estimates for each state-year relative to its counterfactual are shown. Circular markers represent effect estimates, and spikes represent 90% confidence intervals (spikes may not be visible if the standard errors on which the spikes are based are relatively small in magnitude). The vertical dashed line denotes the fiscal year of the SFR reform.

Figure E2. Ridge ASCM effect estimate of Michigan's 1994 SFR*Ridge ASCM effect estimates of Michigan's 1994 SFR on state revenues and expenditures*

Notes: Because Figure E1 through Figure E6 are similar in appearance, detailed notes regarding how to interpret figures are only provided after Figure E1 for parsimony.

Figure E3. Ridge ASCM effect estimates of Arkansas 1995 SFR

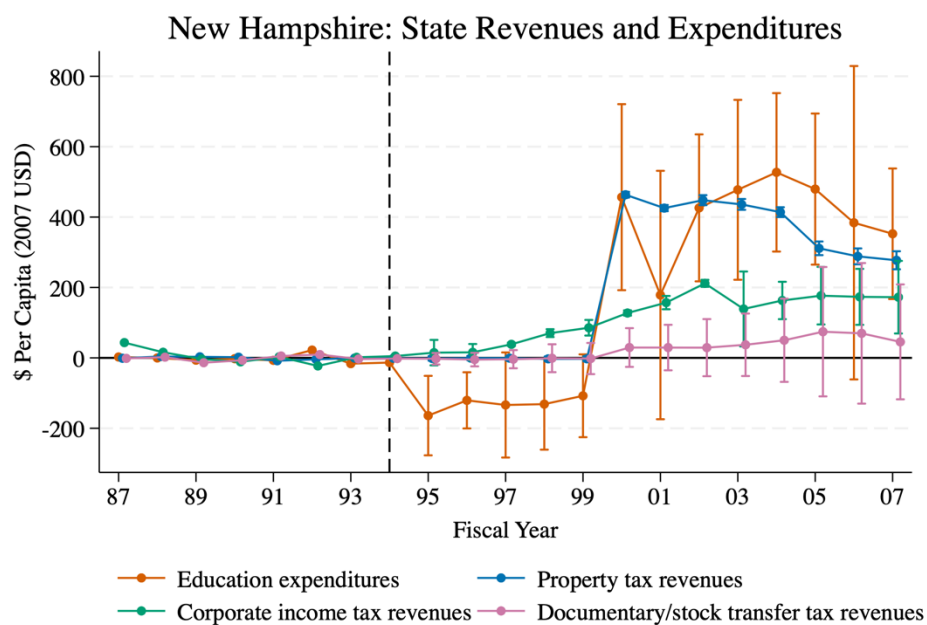
Ridge ASCM effect estimates of Arkansas 1995 SFR on state revenues and expenditures



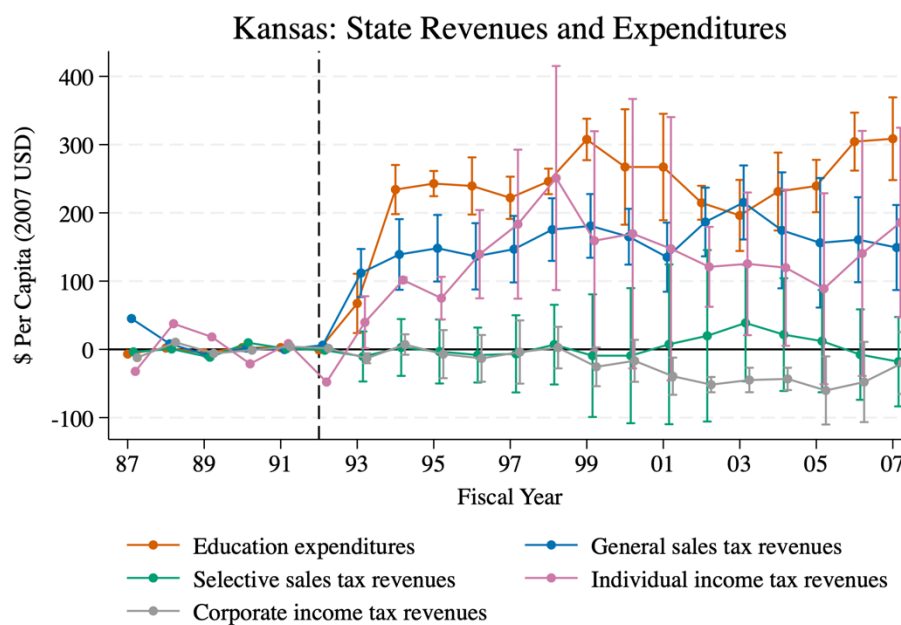
Notes: Because Figure E1 through Figure E6 are similar in appearance, detailed notes regarding how to interpret figures are only provided after Figure E1 for parsimony.

Figure E4. Ridge ASCM effect estimates of New Hampshire's 1994 SFR

Ridge ASCM effect estimates of New Hampshire's 1994 SFR on state revenues & expenditures



Notes: Because Figure E1 through Figure E6 are similar in appearance, detailed notes regarding how to interpret figures are only provided after Figure E1 for parsimony.

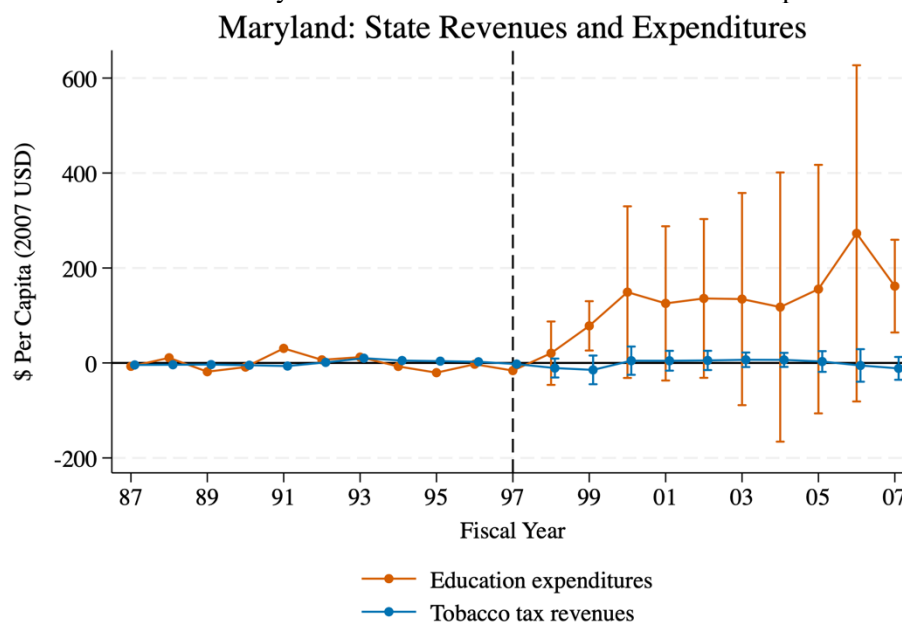
Figure E5. Ridge ASCM effect estimates of Kansas' 1992 SFR*Ridge ASCM effect estimates of Kansas' 1992 SFR on state revenues and expenditures*

Notes: Because Figure E1 through Figure E6 are similar in appearance, detailed notes regarding how to interpret figures are only provided after Figure E1 for parsimony.

Figure E6. Ridge ASCM effect estimates of Maryland's 1997 SFR and time series of tobacco tax revenues

Ridge ASCM effect estimates of Maryland's 1997 SFR on state revenues and expenditures

Ridge ASCM effect estimates of Maryland's 1997 SFR on state revenues and expenditures



Notes: Because Figure E1 through Figure E6 are similar in appearance, detailed notes regarding how to interpret figures are only provided after Figure E1 for parsimony.