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

Shelby M. McNeill[1] and Christopher A. Candelaria[2]

[1]Independent Researcher

[2]Vanderbilt University

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Abstract

This study investigates how individual states raise revenue to pay for elementary-secondary education spending following school finance reforms (SFRs). We identify states that increased and sustained education expenditures after reform, search for legislative statutes that appropriated more education spending, and assess how policymakers funded the SFRs. Our results show that state legislatures increase investments in education by increasing tax revenue streams, such as sales and excise taxes, and by taking over property tax collections. Considering these results, we discuss that increased state investment in education should be accompanied by a policy mechanism to distribute state aid equitably to districts. Moreover, policymakers should consider local voters' preferences when implementing SFR policies, as tax increases may reduce local fiscal effort for education.

Keywords: educational finance, state aid, school finance reforms

Paying for School Finance Reforms: How States Raise Revenues to Fund Increases in

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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). 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—the beneficiaries of state elementary-secondary education expenditures—receive these funds as state aid revenues. 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. 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 increased 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 combine U.S. Census Bureau state-level finance data from fiscal years (FYs) 1987-2007 with a list of court-ordered and legislative SFRs that occurred between FYs 1989-2005, a period which is often referred to as the adequacy era (Candelaria & Shores, 2019). In our data, there were 24 states that had at least one SFR during our sample period. Because some of these states had multiple reforms, we focus our analyses on the first documented reform in each state. We then estimate the impact of each state's reform on elementary-secondary education spending use the ridge augmented synthetic control method (Ben-Michael et al., 2021), which we refer to as ridge ASCM. States that increased spending across all years for which we have data at the 5 percent significance level are included in our case study sample.

To address our second question (RQ2), we first search for the legislative acts that were passed around the same time as—and often because of—the documented SFRs in each of our case study states. Second, we analyze 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. Third, we assess our legislative evidence by quantitatively examining the effect of a state's SFR on the revenues or expenditures that were specified in legislation as being adjusted to fund increased education spending. Similar to RQ 1, we continue using 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. 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 quantitative financial evidence that the state collected increased sales tax revenues.

While this study examines SFRs prior to the Great Recession, our findings continue to be relevant for ongoing policy discussions about education finance and state investment. Following the Great Recession, several states have had their education finance systems declared unconstitutional by high courts (Hanushek & Joyce-Wirtz, 2023), requiring legislatures to enact SFRs. This study informs policymakers about different approaches for financing increased education spending in their states, whether they are required to do so by court order or whether state legislators want to independently reform their education finance system. Moreover, this study highlights additional policy considerations beyond increasing education expenditures. Specifically, we suggest policymakers consider mechanisms for distributing state aid equitably and consider the fiscal responses of local taxpayers after a reform, given that district revenues are largely composed of state aid and local revenue.

School Finance Reforms and their Fiscal Impacts

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 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. 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. 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).

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, increased and sustained funding are necessary conditions for investigating how SFRs were financed. Imposing these conditions facilitates the search for legislation following 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. Yet, 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 approach for state legislatures to pay for SFRs is by reducing 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.

If states can maintain spending on non-education programs after implementing an SFR,

policymakers would benefit from understanding how to increase investments in education

without affecting these expenditures. 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 the relationship

between shifts in the collection of local property taxes and the funding of SFRs. When reviewing

the text of state legislative acts, we found that changes to state and local property tax revenues

played a key role in funding SFRs within 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. 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. 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—for example, after conversion, population data in fiscal year 1989 refers to the estimated population on July 1, 1988.

Fourth, we 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 do not include 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. (2023) 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; thus, it 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 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. In Appendix Table A1, we list the SFRs under consideration in our study. 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

For 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 state legislatures 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. 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 and supplies), 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.

Our selection of case study states 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., 2023). 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 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 autocorrelation within states, we use a row-based jackknife, following 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 thick 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, on average, of approximately 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. The use of the ridge ASCM method, therefore, serves as our preferred approach for estimating the effect of SFRs on per capita education expenditures.

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 documenting 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 statue 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 the changes

legislatures made to state finances 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 states funded SFRs. We cite all these

additional sources when reporting results. Each author of this paper 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. 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.

As a robustness check for RQ 2, we estimate the ridge ASCM models using a comparison

group of states that had an SFR but were unable to increase and sustain expenditures after their

reform. We perform this check because states with SFRs might differ in unobservable ways from

those that never had a reform. As we show in Appendix E, the revenue and expenditure results

are qualitatively similar to our main results, which appear in the results section. The congruence

of these results is not surprising; as noted by Shores et al. (2023), states without documented

SFRs increased revenues in ways similar to states with reforms. Thus, we have suggestive

evidence that our results are not driven by unobservable factors such as the propensity or

motivation to engage in a finance reform.

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; therefore, they are

included in our selected analytic sample. We display the ridge ASCM effect estimates of SFRs

on per pupil, as well as per capita, elementary-secondary education expenditures for these six

state SFRs graphically in Figure 2. For presentation purposes, we plot point estimates using 90

percent confidence intervals in Figure 2, given that annual dynamic effects are less precise that

the average of these effects. 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

six state SFRs, and we continue in descending order—Vermont, Michigan, Arkansas, New

Hampshire, Kansas, and Maryland—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.

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, which guarantees a base level of funding per pupil. The state also presumed the primary responsibility for funding the foundation grant program; as a result, state contributions to elementary-secondary education increased.

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, which guaranteed that districts approving the same tax rate would receive the same amount of funding per pupil, regardless of local district property wealth, through redistribution. As shown in Figure 3a, 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.

[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 period; per pupil property tax revenues decreased by $4,180 on average. Total property tax revenues—the sum of state and local property tax revenues—remained largely unchanged following Act 60's enactment.

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).

Finally, Act 60 also specified that lottery revenues be used solely for funding elementary-secondary public education. Consequently, lottery revenues were to be placed in the education fund, rather than the general fund. Because Vermont's lottery revenues were only about $50 per capita on average between 1999-2007, diverting lottery funds from the general fund to the education fund likely had negligible effects on funding for other state priorities, including welfare, corrections, and higher education. We also do not find quantitative evidence that non-education state expenditures decreased post-1997 in Vermont.

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 set 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—the sum of state and 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. As shown in Figure 4a, these tax changes led to small increases in the state tobacco tax—$27 per capita, on average—and in documentary and stock tax revenues—$4 per capita, on average. We note that our measure of documentary and stock tax revenues includes real estate transfer tax revenues, as the Census Bureau does not provide a separate measure of them.

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). 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.

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). 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).

<div align="center">[Insert Figure 5 about here]</div>

   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."

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). However, 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. As shown in Figure 5a, 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. 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; however, districts were also allowed to spend more than the base foundation grant by levying additional local property taxes (Lutz, 2010). 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. Together, the sum of local and state property taxes decreased following HB 117's enactment.

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 and 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). 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. 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.

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. In addition, a provision of SDFQPA, known as the Local Option Budget (LOB), permitted districts to raise additional funding 25% above the base foundation grant if approved by local citizens (Johnston & Duncombe, 1998; Thompson & Clark, 2001). 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). As also shown in Figure 7a, selective sales tax and corporate income

tax revenues were not statistically different from those of Kansas' synthetic counterfactual.

The SDFQPA also required districts to impose a uniform local property tax rate, which

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). The imposition of the uniform local property tax rate

resulted in lower local property tax revenues—a decrease of $888 per pupil on average (see

Figure 7b)—and property tax relief for citizens. 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).

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. The commission's formal name was the Commission on Education Finance, Equity,

and Excellence, but it was also referred to as the Thornton Commission after its chairman, Alvin

Thornton. 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. We do not examine state education expenditures

in the sixth year (FY 2008) because our fiscal data set ends FY 2007, the last FY prior to the

Great Recession. 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. 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. However, the Act did not specify a revenue source to

fund the remaining five years (FYs 2004-2008) of mandated education spending increases. 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).

  Additional searches based on state finance documents and news articles on BTE and

Maryland state education funding during this time did not yield 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. 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. Legislative documents
were not helpful because Maryland's state constitution does not require the legislature to identify
a revenue source for proposed spending in the future (Norcross, 2010). Our descriptive time
series of Maryland's tobacco tax revenues (Figure 8b), however, does suggest that the increased
revenue from the cigarette tax sustained funding through the end of our sample period. Thus, in
the case of Maryland, multiple approaches—the combination of legislative document analysis,
ridge ASCM, and descriptive time series graphs—were needed to understand how the legislature
likely funded increases in state education spending.

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, and Vermont. These findings suggest that larger increases in state education funding
are achieved when legislatures (a) increase multiple tax revenue streams, including sales and
excise taxes, and (b) exercise more control over property taxes. Our results align with prior
research, indicating that reforms are funded by tax revenues (Liscow, 2018); however, our study
documents the specific sources of these tax revenues and identifies their contribution to funding.

Policy Considerations

Distribute State Aid Equitably

Although a primary objective of many SFRs is to increase state education expenditures

for elementary-secondary education, policymakers should also consider how to equitably

distribute these funds to school districts (Duncombe & Yinger 1998, Downes & Pogue, 1994).

Districts receive state expenditures for education as state aid in their revenue accounts via an

intergovernmental transfer, so the allocation of these funds matters for their district-level

budgeting and planning (Odden & Picus, 2020). If policymakers were to allocate a

disproportionate amount of state aid to wealthy districts, they might exacerbate the inequality of

education funding driven by local property wealth (Chetty & Friedman, 2010), undermining any

desired equity goals of an SFR. Alternatively, allocating additional state aid to districts with

lower property wealth or more economic disadvantage could work to increase funding in these

districts, which research shows translates to improved student outcomes (Candelaria & Shores,

2019; Jackson et al., 2016; Lafortune et al., 2018).

Several approaches exist for distributing state aid, ranging from distributing funds

according to enrollment to targeting aid based on need (Morgan & Pelissero, 1989; Odden &

Picus, 2020). One concrete example of a distribution mechanism, which was recently adopted in

Tennessee because of their legislative SFR in 2022, is a weighted student funding formula

(WSFF). The state's WSFF 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 (Odden & Picus, 2020; Tennessee Department of Education, 2023). However,

given that state aid is ultimately constrained by the revenue sources that fund it, any distribution

mechanism, including WSFF, will involve tradeoffs about which student demographic groups

should receive additional funding (e.g., economically disadvantaged students, English language

learners, students with unique learning needs) and what proportion of funding goes to each group

(Candelaria et al., 2023).

Account for the Preferences of Local Taxpayers

The preferences of local taxpayers matter because these taxpayers fund the local share of

revenue that school districts receive (Barlow, 1970; Loeb, 2001). For school districts, the two

largest revenue sources for their budget are state aid and local funding, as federal aid typically

comprises a small share (Howell & Miller, 1997). The response of local taxpayers to an SFR

warrants consideration from policymakers, as it affects the total funding districts receive. As

shown in Figure 9, the effects of SFRs on local funding are quite heterogenous. Even though per

pupil state spending 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. However, New Hampshire's local funding decrease is

notable given that the sum of local and state revenues decreased in the years after reform. One

explanation for the sharp decrease in local funding is that local taxpayers engaged in a tax revolt

in response to their SFR (Dandurant, 2000).

When local taxpayers face significant tax increases, there might be potential resistance to

support education through fiscal effort (Hoxby, 2001; Rockoff, 2004; Steinberg et al., 2016).

Therefore, assessing the impact of SFRs requires a comprehensive view of total revenues,

particularly if the aim is ensuring adequate funding for students, which requires examining total

revenue. State aid alone may not suffice if local funding substantially drops. SFR funding

policies will need to account for the preferences of local voters to gauge whether they are likely

to support education reform through additional fiscal effort, seek property tax relief, or engage in a tax revolt.

Assess Budgetary Tradeoffs

Finally, we think it is important for policymakers to assess budgetary tradeoffs—reducing non-education expenditures or increased state debt—associated with funding SFRs (Baicker & Gordon, 2006; Liscow, 2018). 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 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 six 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 six 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). Research designs focusing exclusively on quantitative analyses of state finance data lack the detailed insights that can be gained from legislative review.

Although our analysis focuses on the adequacy era before the Great Recession, SFRs continue to be relevant in education policy discussions. 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). In one

prominent case, *McCleary, et al. v. State of Washington*, the state supreme court declared

Washington's education funding system unconstitutional in 2012. Although the ruling occurred

in 2012, the state did not satisfy the court order until the 2018-19 school year when lawmakers

enacted a bill substantially increasing state aid (Sun et al., 2024). Independent of a court order,

states may also choose to pursue finance reforms to overhaul their education funding systems.

For example, Tennessee policymakers recently enacted the Tennessee Investment in Student

Achievement Act into law in 2022, introducing a weighted student funding formula for

distributing state aid to districts. These examples reveal that SFRs are not policy relics of the

past; rather, they underscore the need for further research that examines the granular details of

these reforms, including how they are funded.

      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.,

2023). 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.

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Tables

Table 1: Reference sources for finding legislative statutes

Reference	Type of source	General information provided
1. *SchoolFunding.Info: A project of the Center for Educational Equity at Teachers College:* http://www.schoolfunding.info/ (schoolfunding.info, n.d.)	Website	Overview of school-funding court decisions in each state
2. *50 State Survey of School Finance Policies:* https://schoolfinancesdav.wordpress.com/ (Verstegen, 2018)	Website	Overview of the history of school finance in each state
3. *Public School Finance Programs of the United States and Canada (1998-99):* 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. *Education Law Center Litigation State Profiles:* https://edlawcenter.org/litigation/states/ (edlawcenter.org, n.d.)	Website	Overview of school-funding court decisions in each state
5. *Funding public schools in the United States and Indian country.* (Thompson et al., 2019)	Book	Overview of the history of school finance in each state
6. *State Legislature Websites:* 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: *Lake View v. Huckabee* 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: *Bradford v. Maryland State Board of Education*	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 though 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
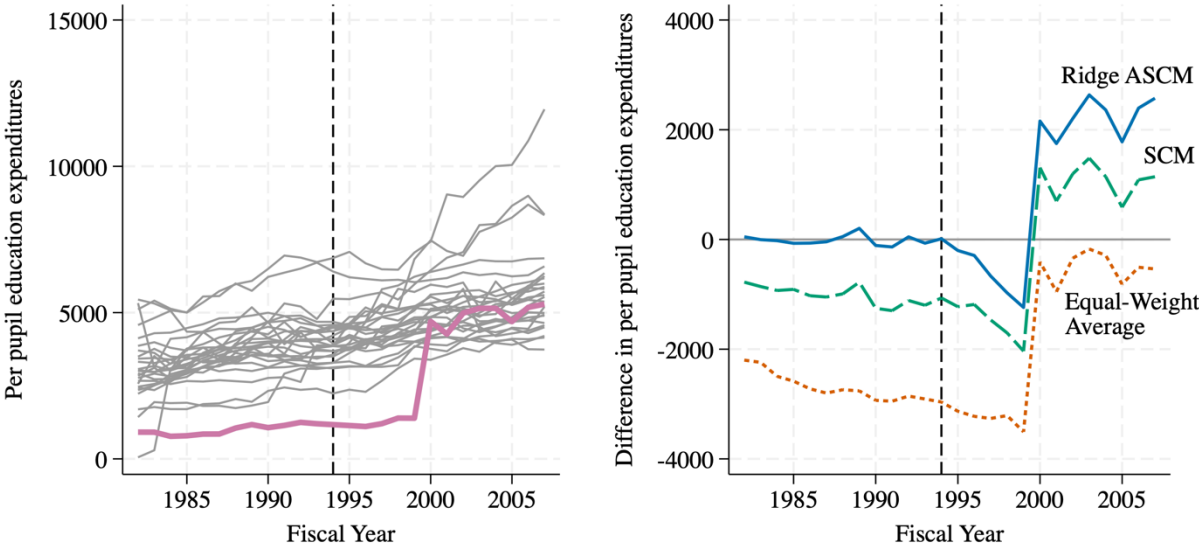
Notes: The left panel displays per pupil state elementary-secondary education expenditures in real 2007 USD over time. The purple line represents New Hampshire's per pupil state education expenditures over time, and the grey lines represent per pupil state education expenditures over time in non-treated 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
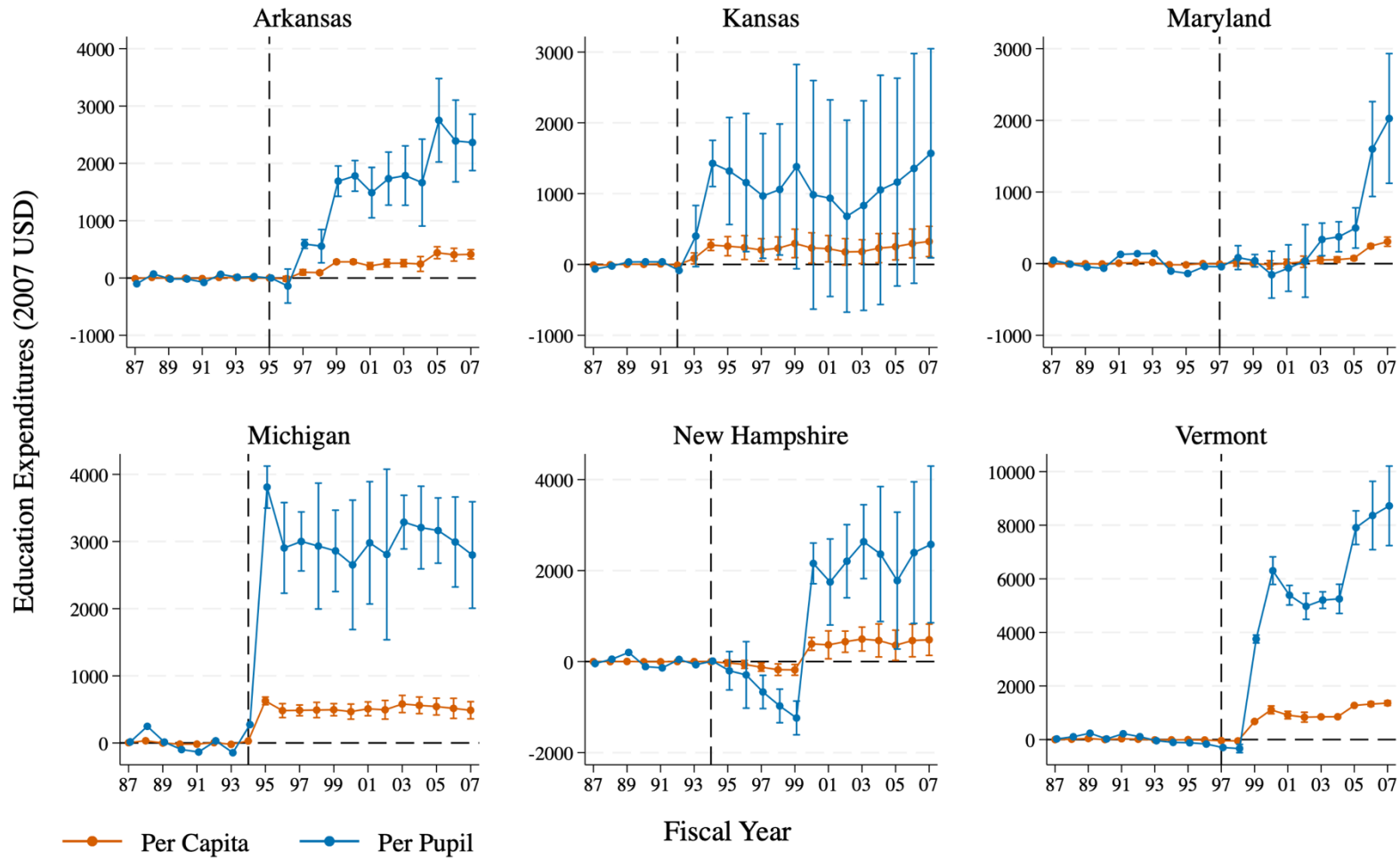
Notes: Ridge ASCM effect estimates for each state-year relative to its counterfactual are shown. Circular markers represent effect estimates, and spikes represent 90% confidence intervals. The vertical dashed line denotes the fiscal year of the SFR reform.
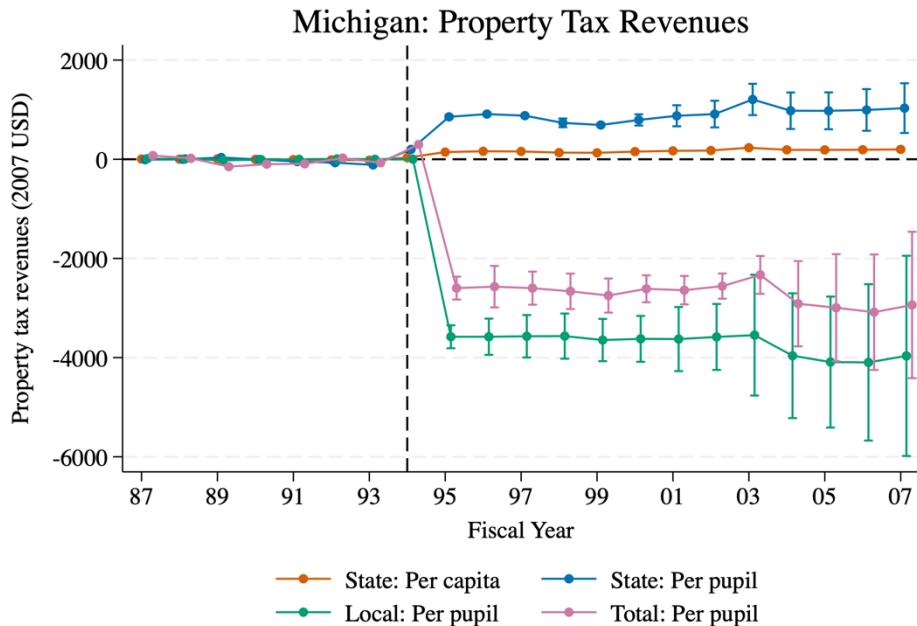
Figure 3. Ridge ASCM effect estimates of Vermont's 1997 SFR

Figure 3a. Ridge ASCM effect estimates of Vermont's 1997 SFR on state revenues and expenditures
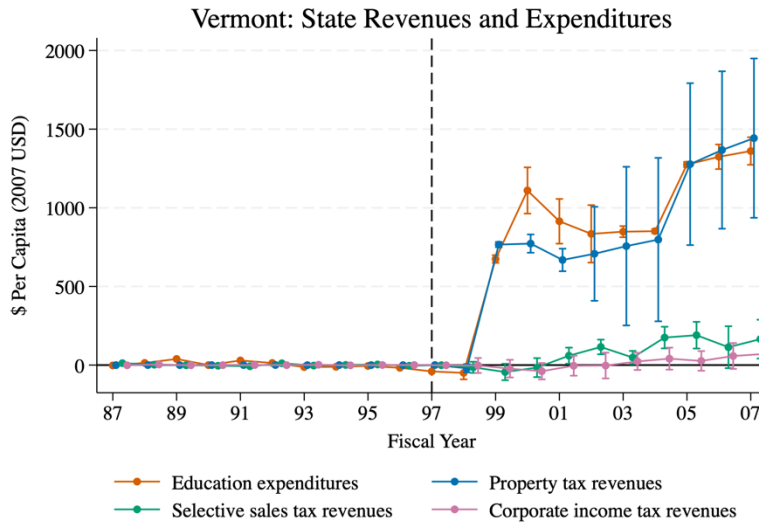
Notes: Ridge ASCM effect estimates for each state-year relative to its counterfactual are shown. Circular markers represent effect estimates, and spikes represent 90% confidence intervals (spikes may not be visible if the standard errors on which the spikes are based are relatively small in magnitude). 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
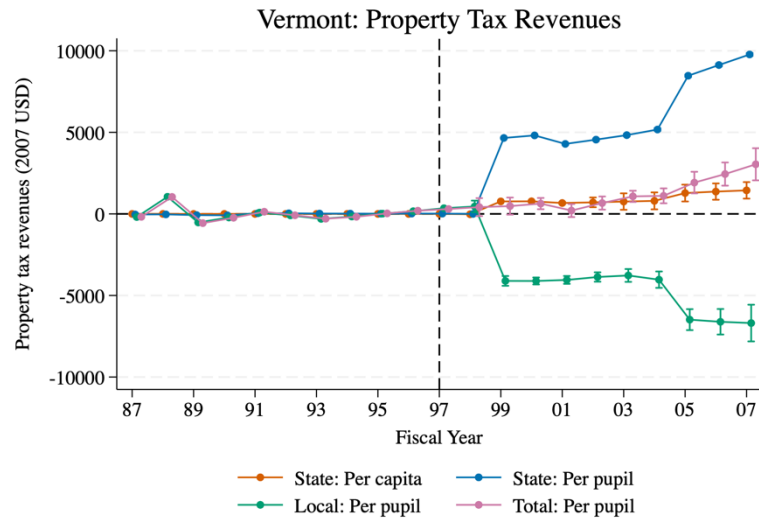
Notes: Ridge ASCM effect estimates for each state-year relative to its counterfactual are shown. Circular markers represent effect estimates, and spikes represent 90% confidence intervals (spikes may not be visible if the standard errors on which the spikes are based are relatively small in magnitude). 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
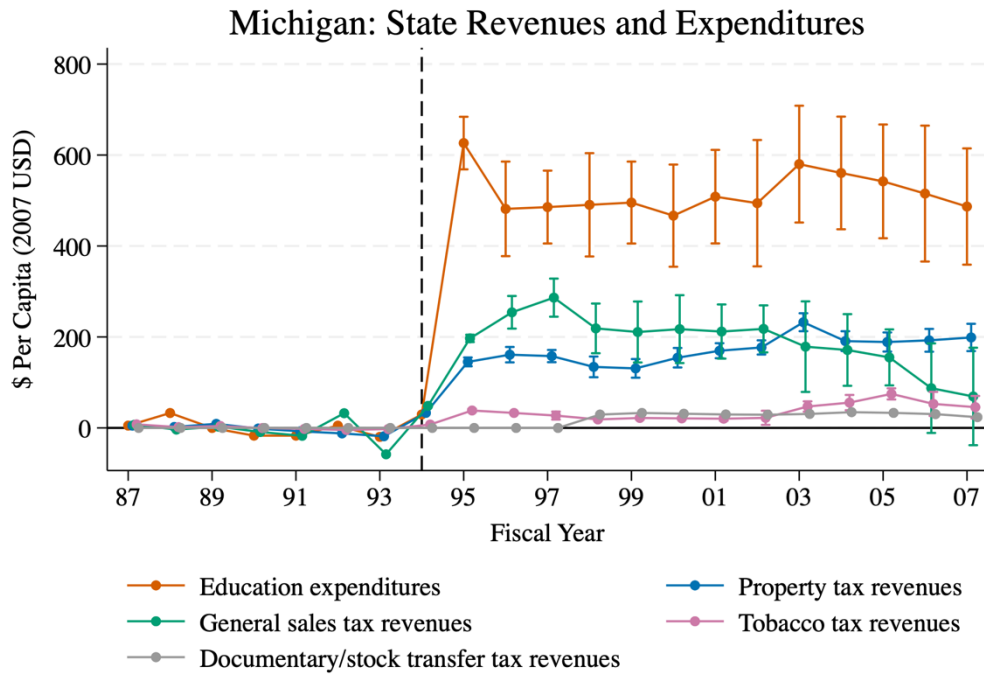
Notes: Because Figure 4 through Figure 9 are similar in appearance, detailed notes regarding how to interpret figures are only provided after Figure 3 for parsimony.
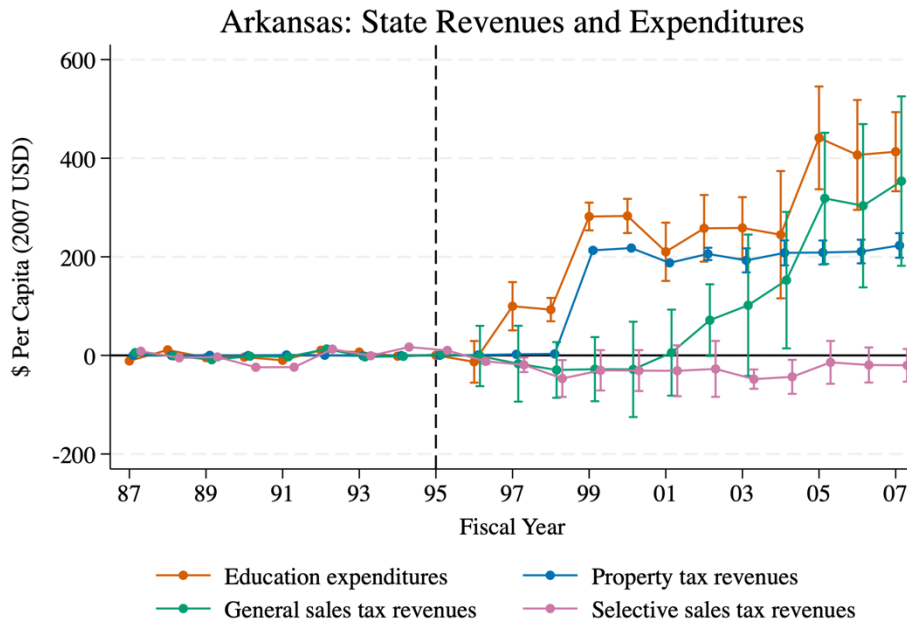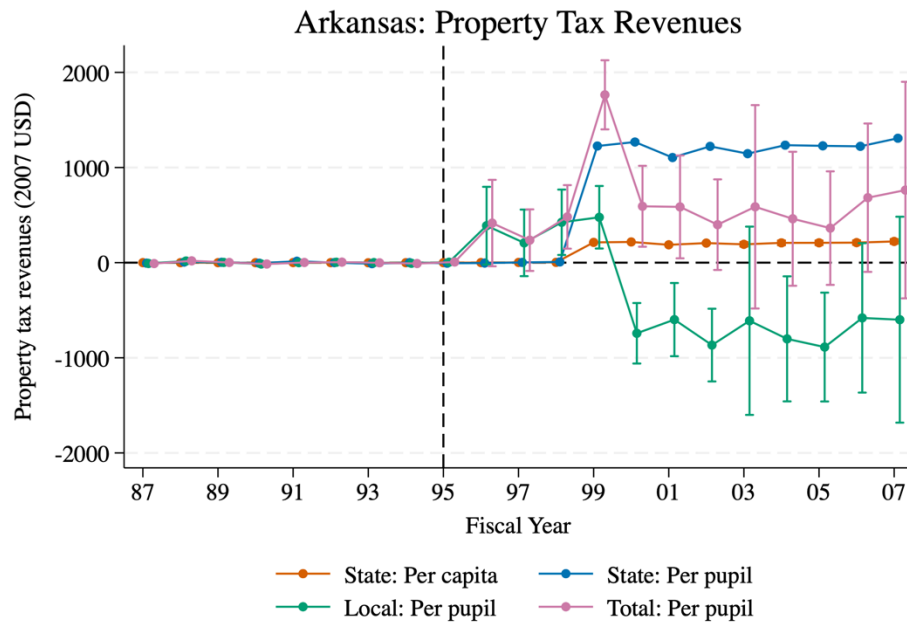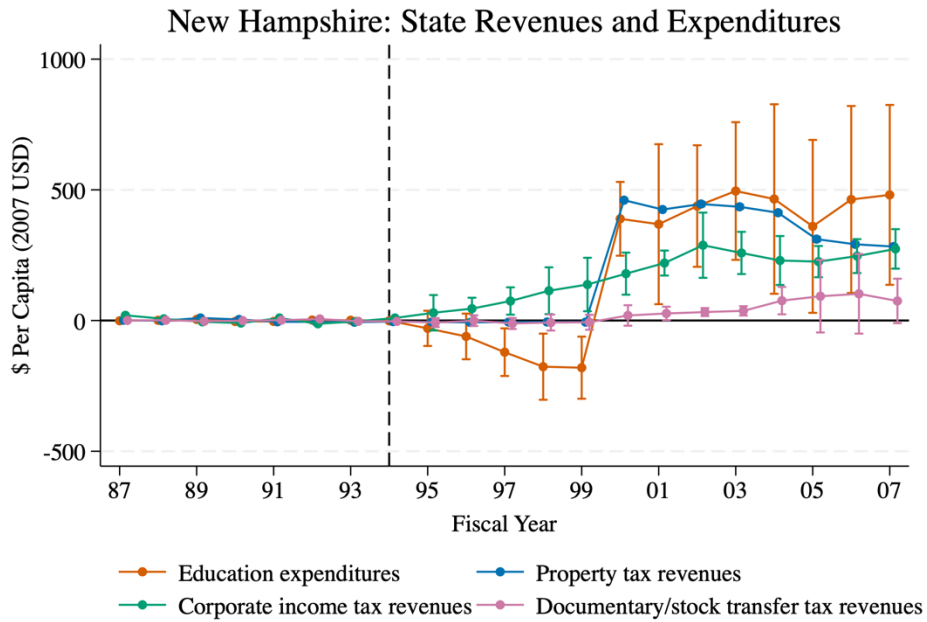
Figure 4b. Ridge ASCM effect estimates of Michigan's 1994 SFR on property tax revenues

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.
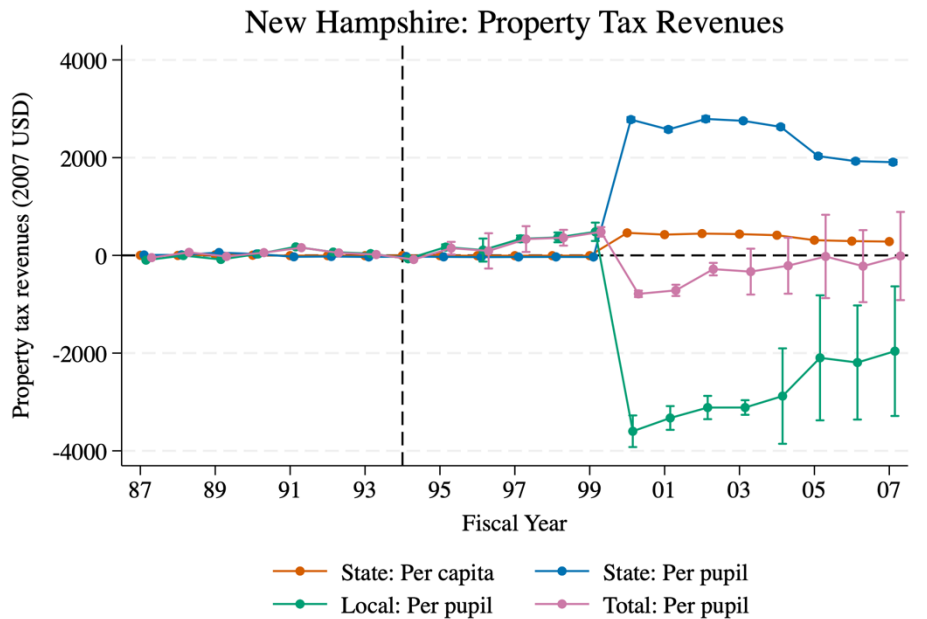
Figure 5. Ridge ASCM effect estimates of Arkansas 1995 SFR

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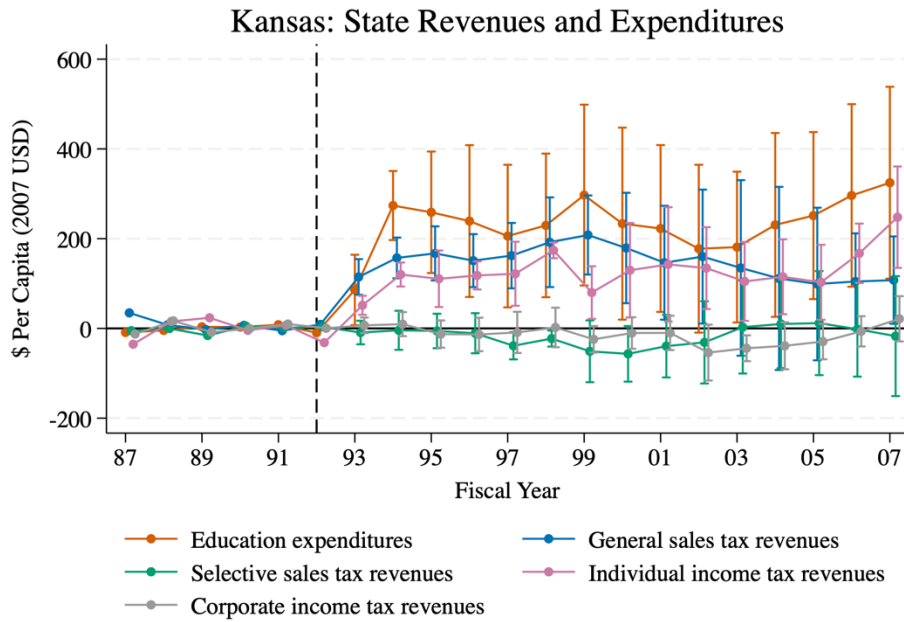
Figure 6. Ridge ASCM effect estimates of New Hampshire's 1994 SFR

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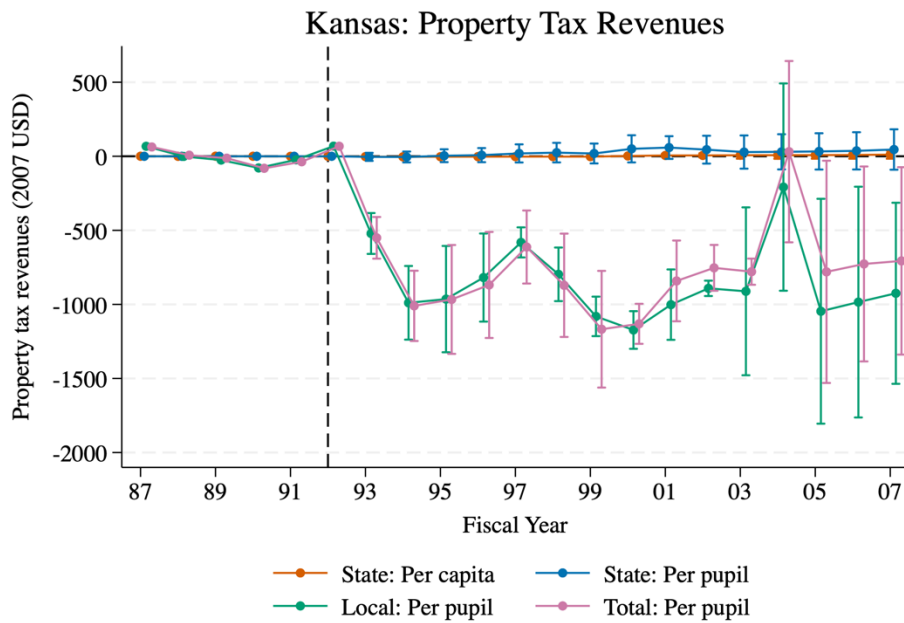
Figure 6b. Ridge ASCM effect estimates of New Hampshire's 1994 SFR on property tax revenues

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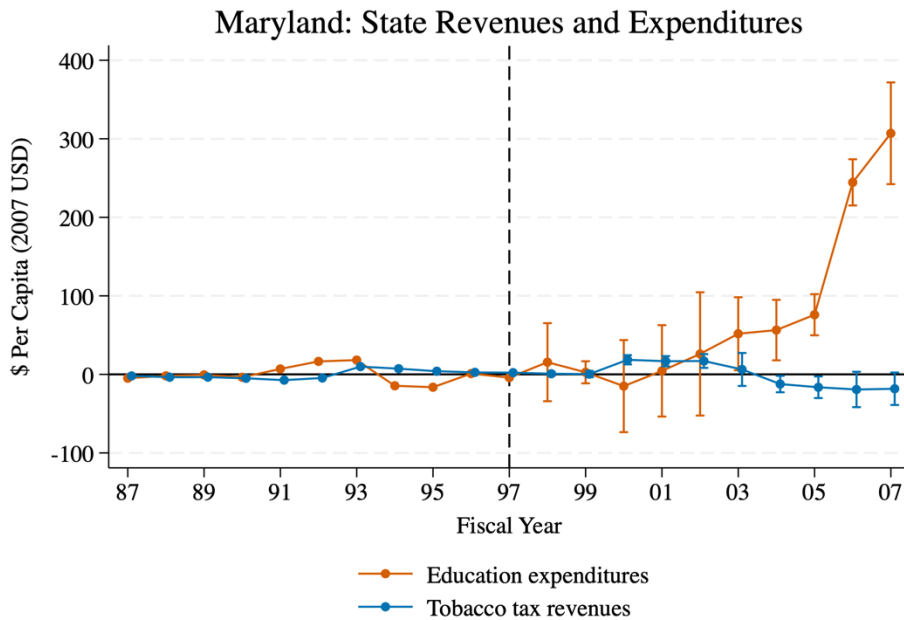
Figure 7b. Ridge ASCM effect estimates of Kansas' 1992 SFR on property tax revenues

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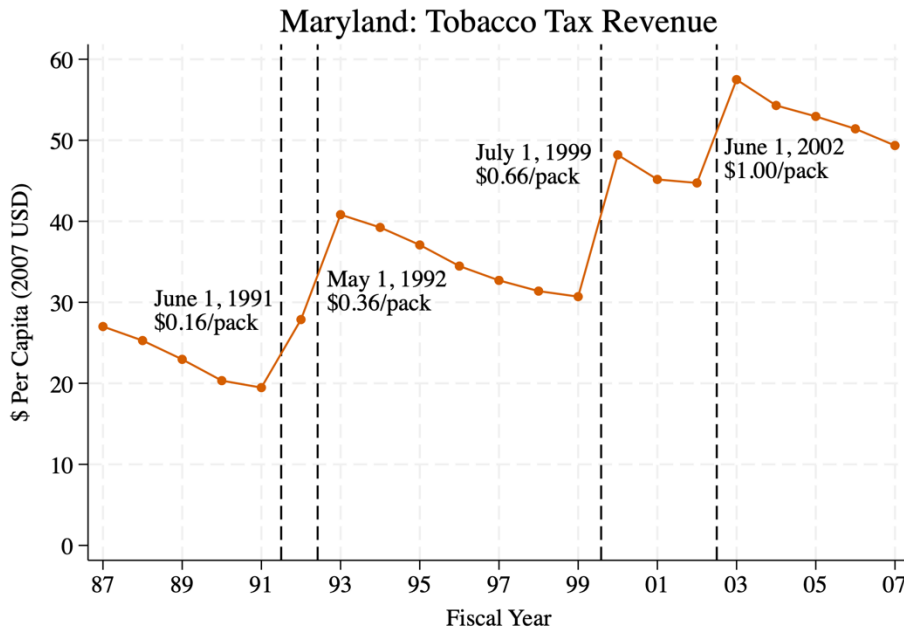
Notes: Vertical dashed lines reflect tax increases on packs of cigarettes during the sample period. Linear declines in revenues after a tax increase reflect a consumer behavioral response to the tax increases.

Figure 9. Ridge ASCM effect estimates of SFRs on state and local expenditures per pupil
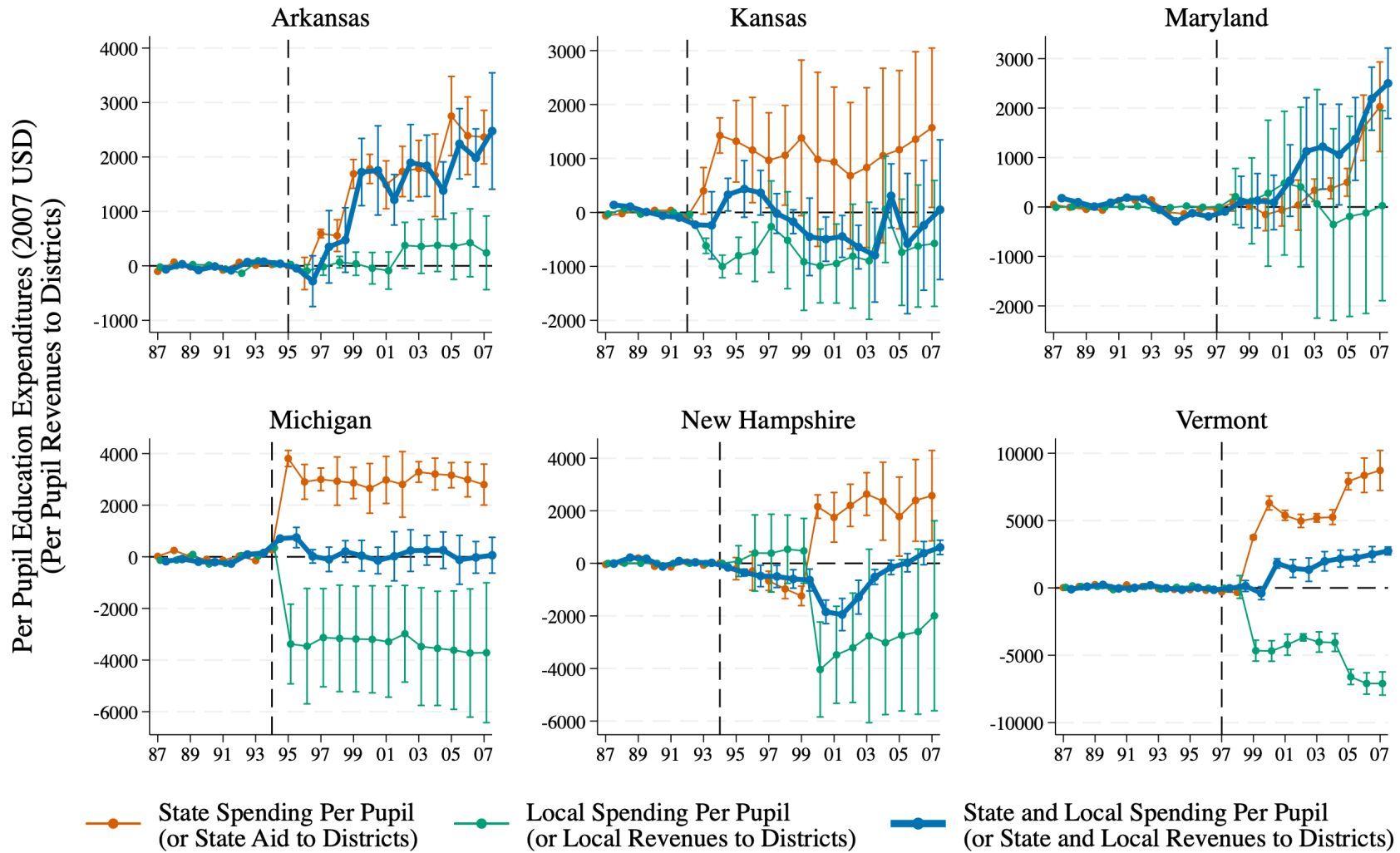
Notes: Ridge ASCM effect estimates for each state-year relative to its counterfactual are shown. Circular markers represent effect estimates, and spikes represent 90% confidence intervals. The vertical dashed line denotes the fiscal year of the SFR reform.

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

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[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

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

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

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	Carrolton-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 $t \in \{1, 2, \ldots, 26\}$. 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:

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

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 $t \leq T_0$:

$$\sum_{s=2}^{27} w_s^{SCM} X_{s1} = X_{11}$$

$$\sum_{s=2}^{27} w_s^{SCM} X_{s2} = X_{12} \tag{2}$$

$$\vdots$$

$$\sum_{s=2}^{27} w_s^{SCM} X_{sT_0} = X_{1T_0},$$

where $w_2^{SCM} + w_3^{SCM} + \cdots w_s^{SCM} = 1$. 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:

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

$$\text{for } t > T_0. \tag{3}$$

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 $s \in \{2, 3, \ldots, 27\}$. 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, $\hat{\eta}^r$:

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

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 $\lambda^r \to \infty$, 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 $\lambda^r \to 0$, 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:

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

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

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

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,

$\bar{\hat{\gamma}}_1^{AUG}$, 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*
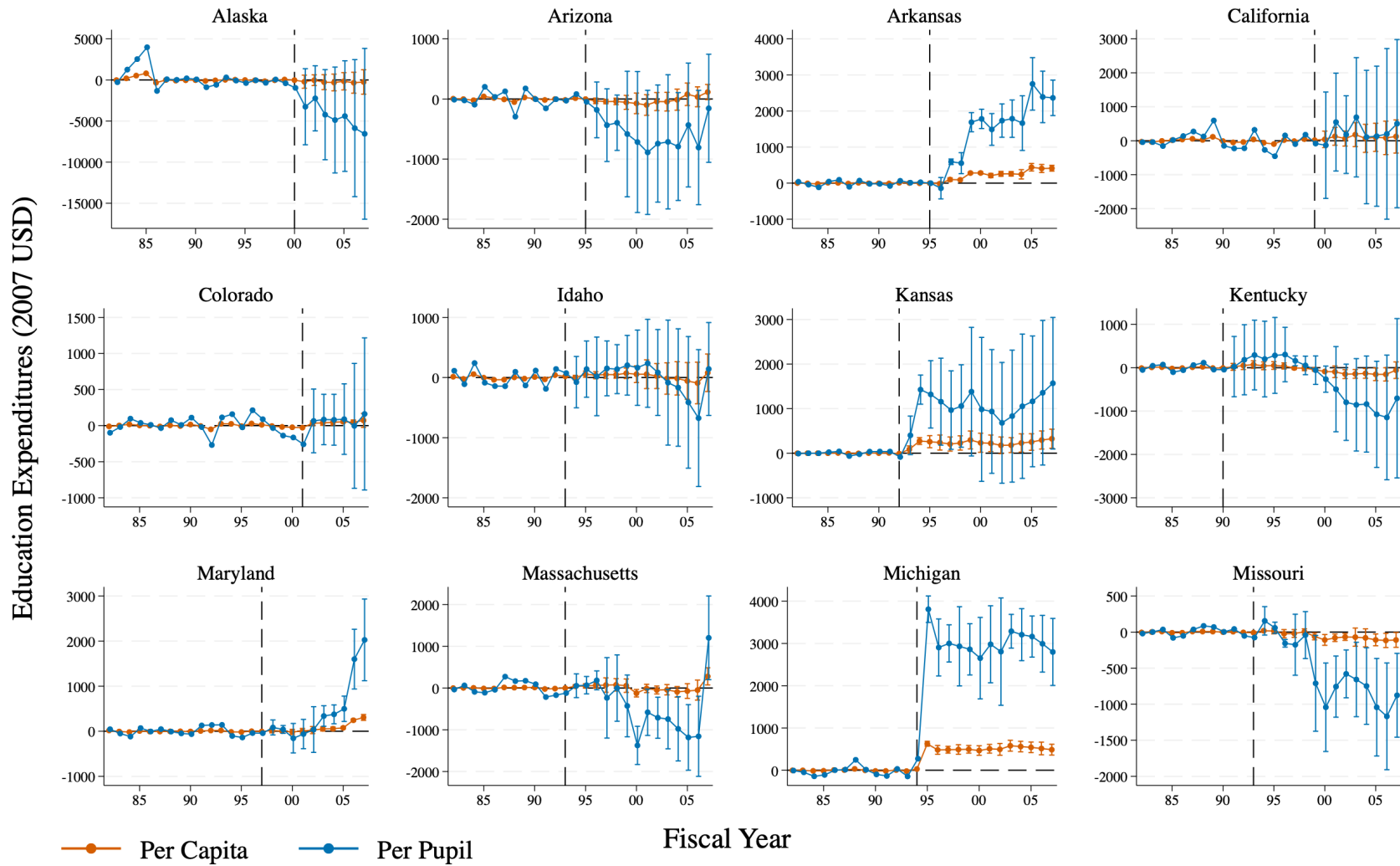
*Figure C1. Ridge ASCM effect estimates – continued from the previous page.*
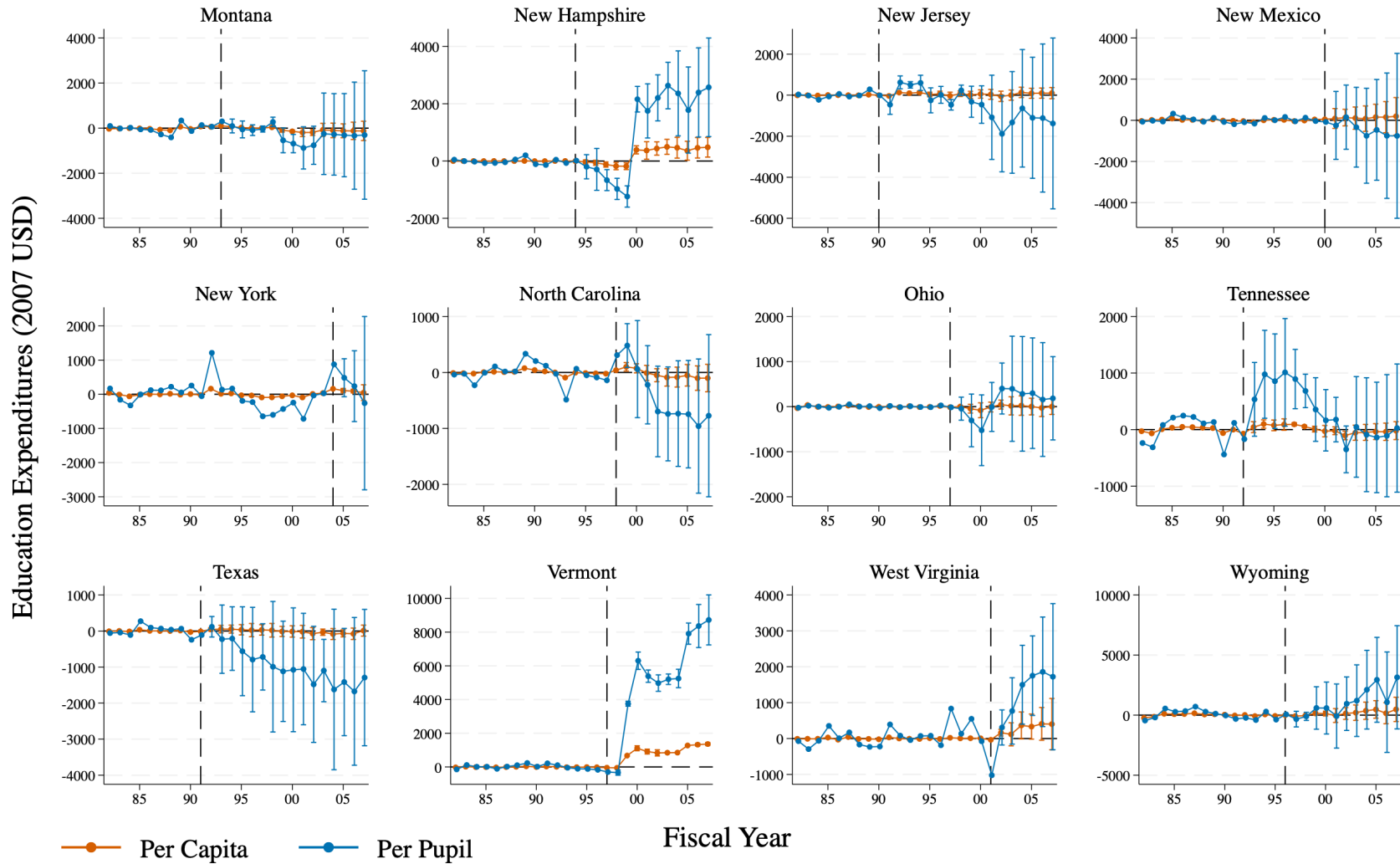
Notes: Ridge ASCM effect estimates for each state-year relative to its counterfactual are shown. Circular markers represent effect estimates, and spikes represent 90% confidence intervals. The vertical dashed line denotes the fiscal year of the SFR reform.

Figure C2. Ridge ASCM percent improvement in pre-treatment model fit, elementary-secondary education expenditures
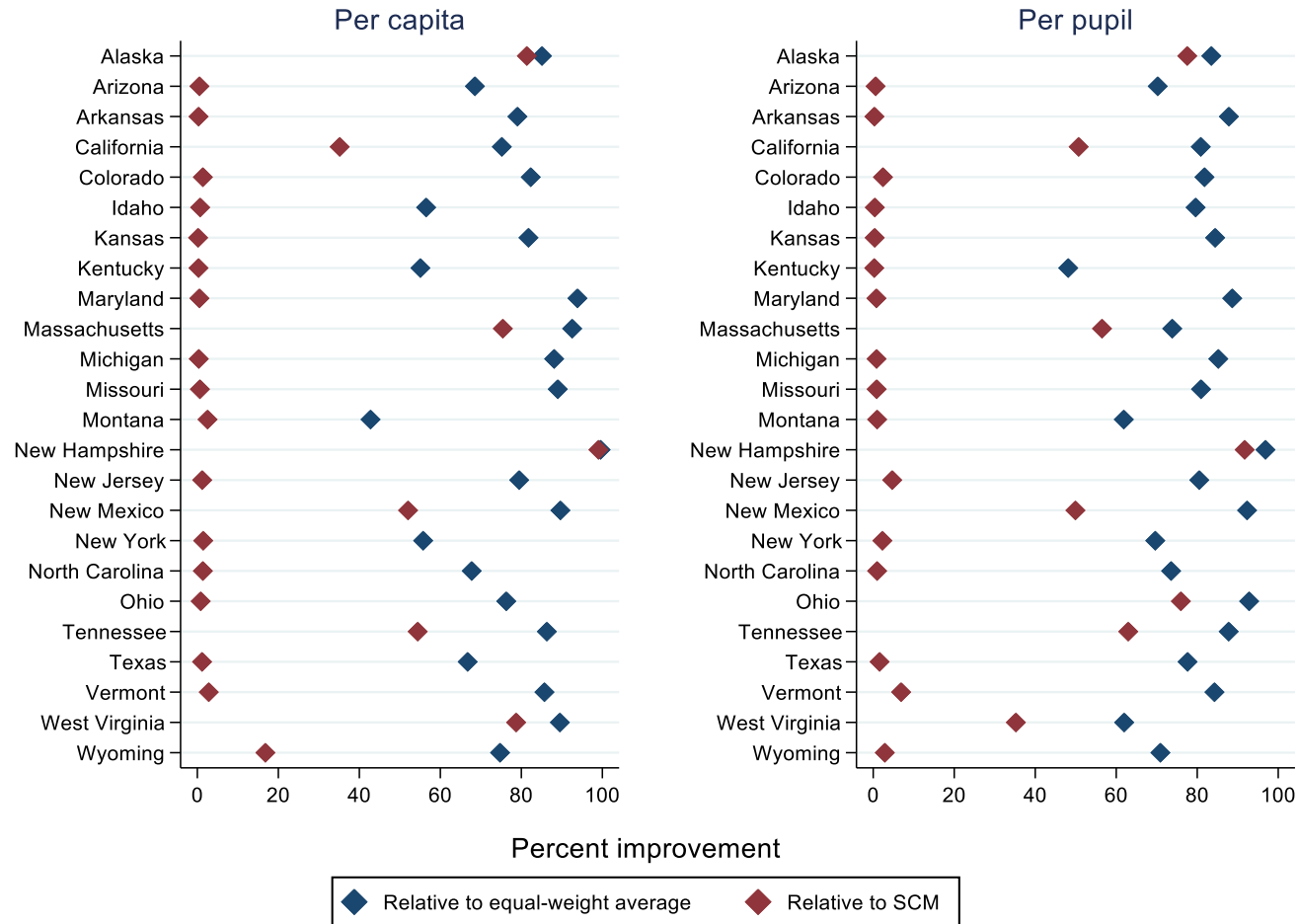
Notes: The percent improvement in the L^2-norm model fit statistic obtained from the per capita and per pupil elementary-secondary expenditures ridge ASCM models relative to the L^2-norm statistic obtained from models that use equal or SCM weights are shown. The L^2-norm statistic indicates the cumulative 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
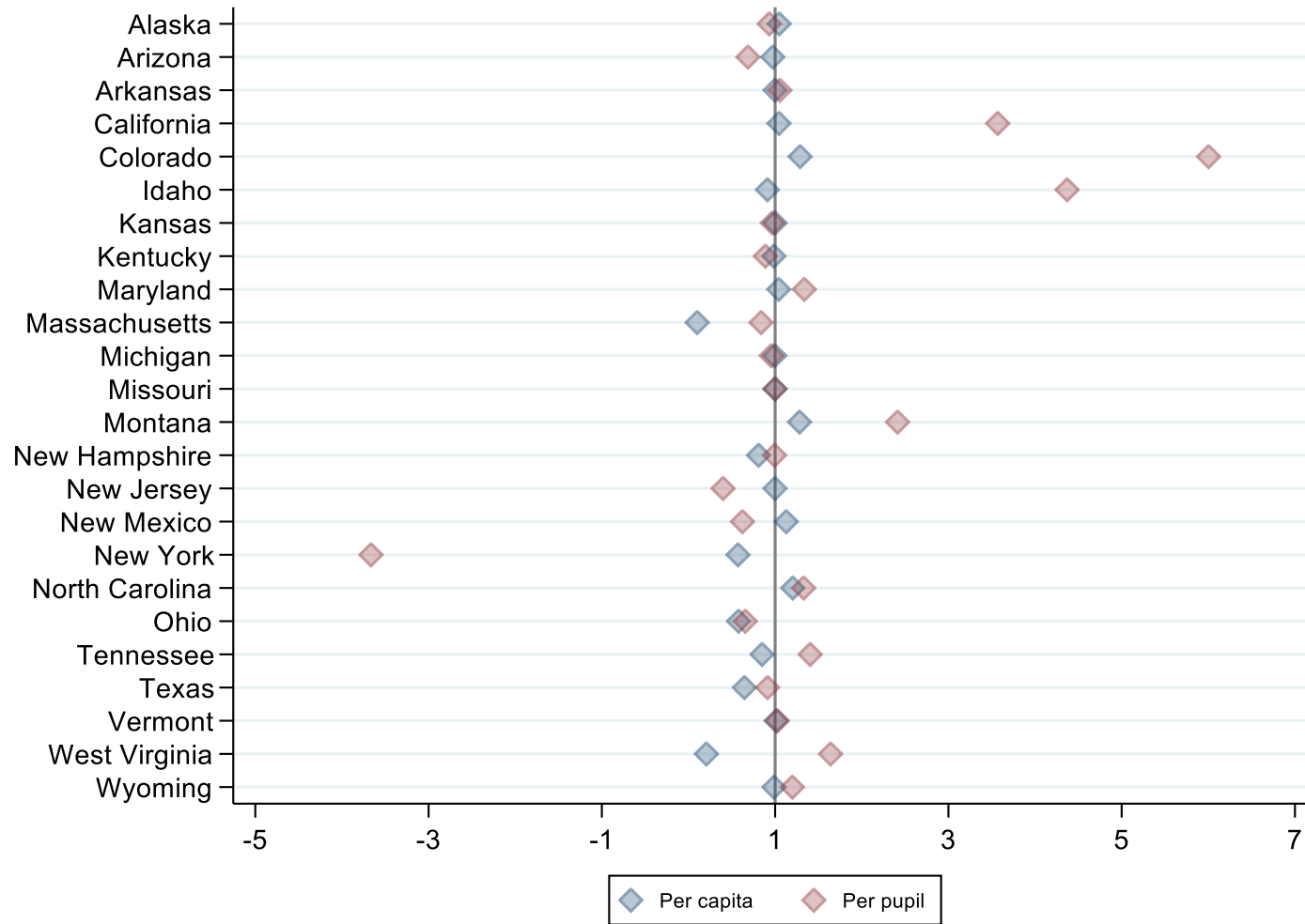
Notes: Each diamond represents the median $\widehat{effect}^{ratio}$ for a state and outcome (per capita or per pupil elementary-secondary education expenditures). $\widehat{effect}^{ratio}$ is equal to $\widehat{effect}^{L(\lambda)} / \widehat{effect}^{CV(\lambda)}$, where $\widehat{effect}^{L(\lambda)}$ 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
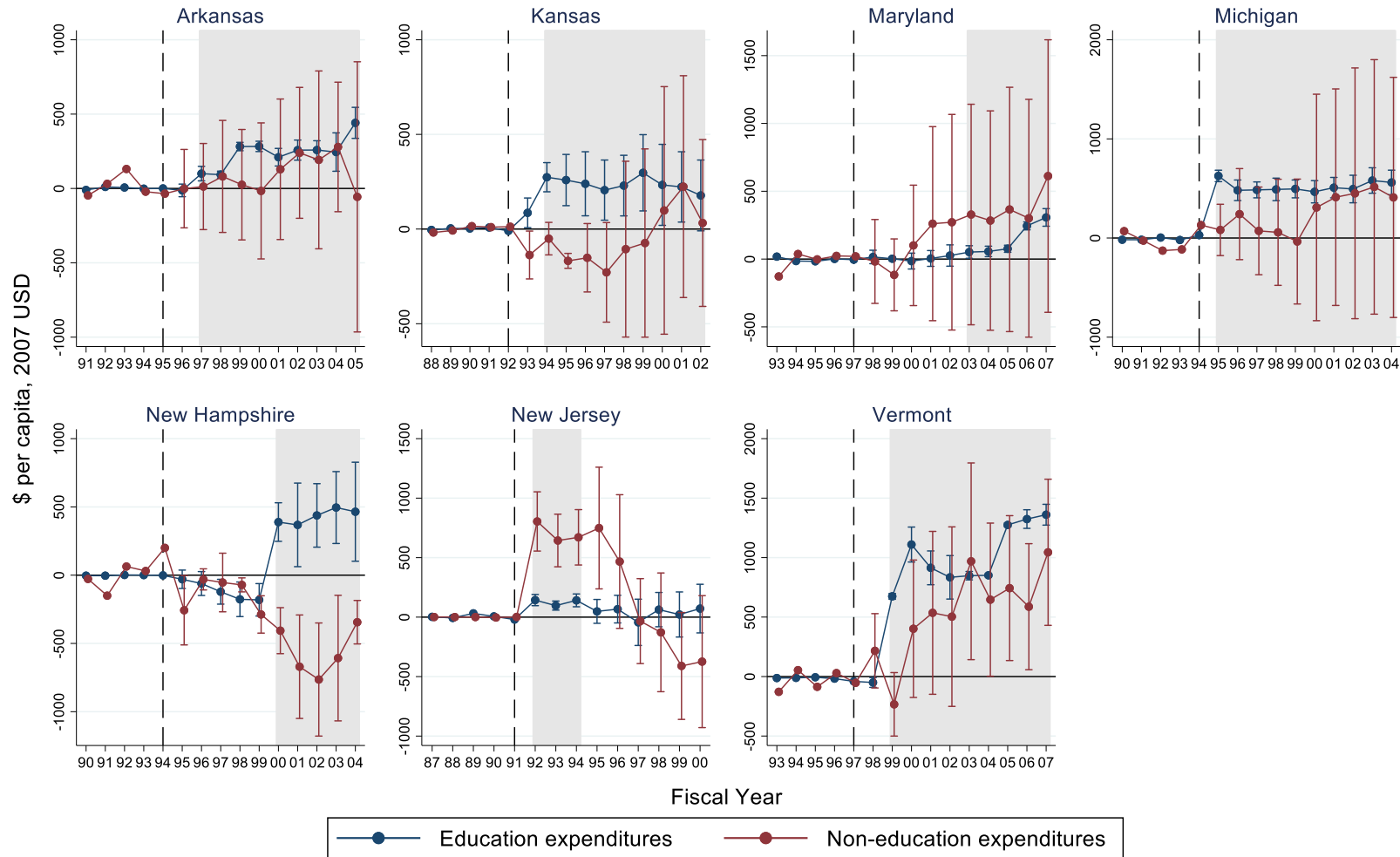
Notes: Ridge ASCM effect estimates for each state-year relative to its counterfactual are shown. Non-education state expenditures include all state expenditures besides expenditures on elementary-secondary education (e.g., expenditures on health and hospitals, higher education, highways, corrections, 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
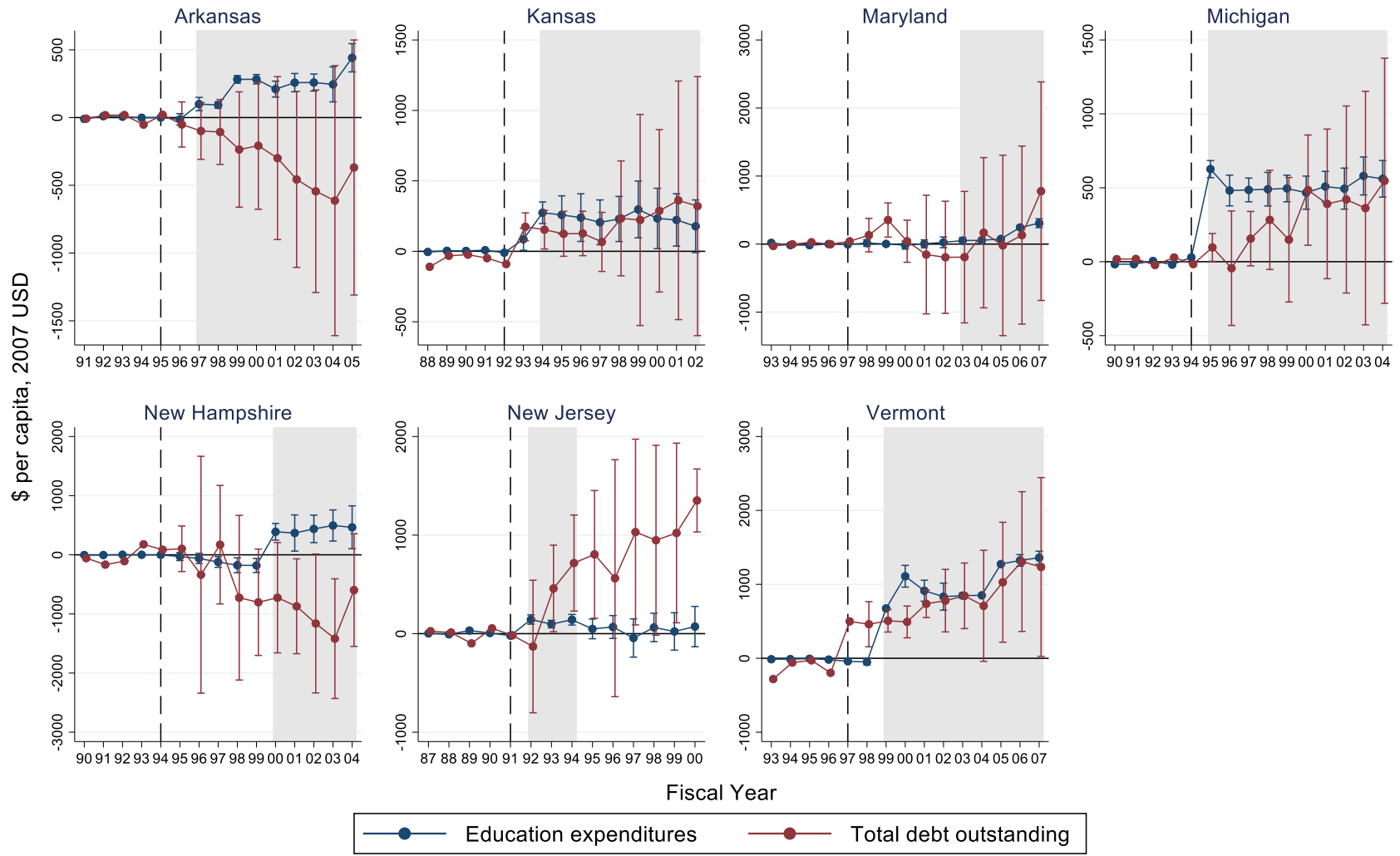
Notes: Ridge ASCM effect estimates for each state-year relative to its counterfactual are shown. Circular markers represent effect estimates, and spikes represent 90% confidence intervals. The vertical dashed line denotes the fiscal year of the SFR reform. Grey background shading indicates years in which per capita education expenditures significantly increased at the $p < .10$ level.

Appendix E. Ridge ASCM with Alternative Comparison Group

In this appendix, we re-estimate our ridge ASCM model for our six case study states using a

comparison group that consists of the states that had an SFR but were unable to increase and

sustain expenditures after their SFR. We perform this check because states which did have an

SFR might differ in unobservable ways from those that never had an SFR. We present the results

across Figures E1 to E6. Results are qualitatively similar to what we present in the main text of

the paper.

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
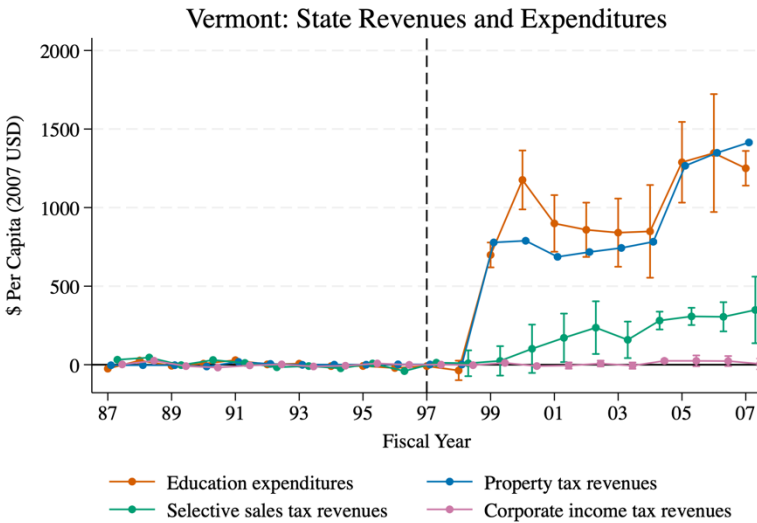
Notes: Ridge ASCM effect estimates for each state-year relative to its counterfactual are shown. Circular markers represent effect estimates, and spikes represent 90% confidence intervals (spikes may not be visible if the standard errors on which the spikes are based are relatively small in magnitude). 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
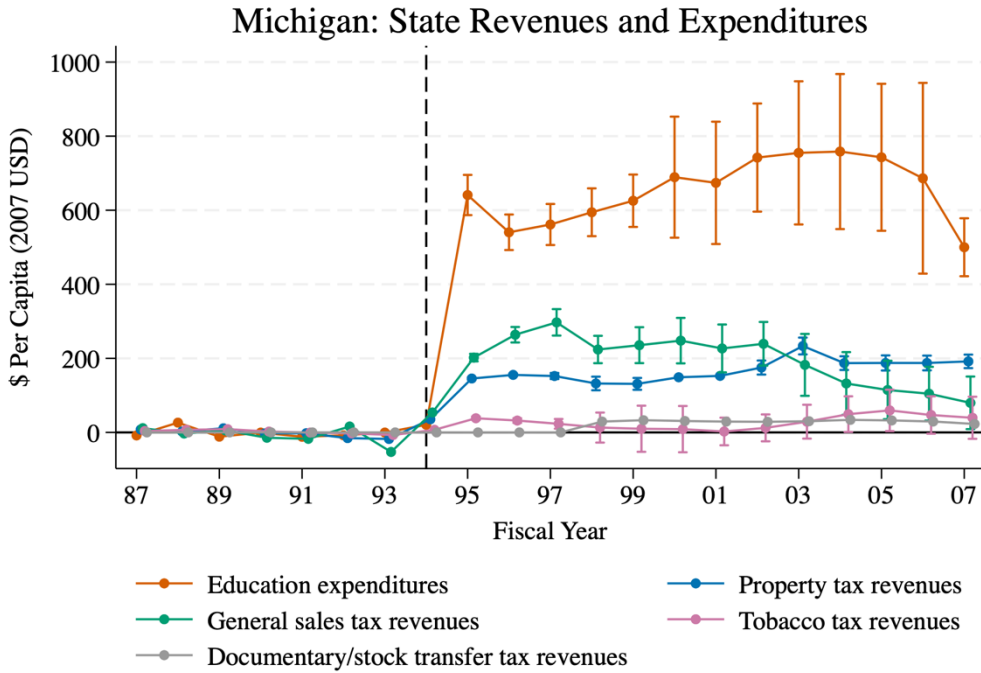
Notes: Because Figure E1 through Figure E6 are similar in appearance, detailed notes regarding how to interpret figures are only provided after Figure E1 for parsimony.

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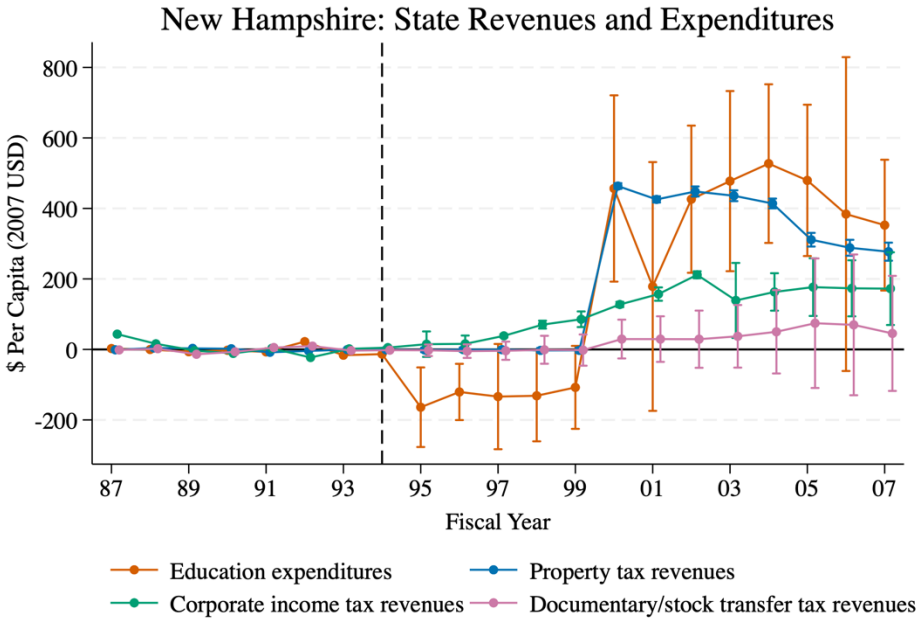**Figure E3.** Ridge ASCM effect estimates of Arkansas 1995 SFR

Ridge ASCM effect estimates of Arkansas 1995 SFR on state revenues and expenditures
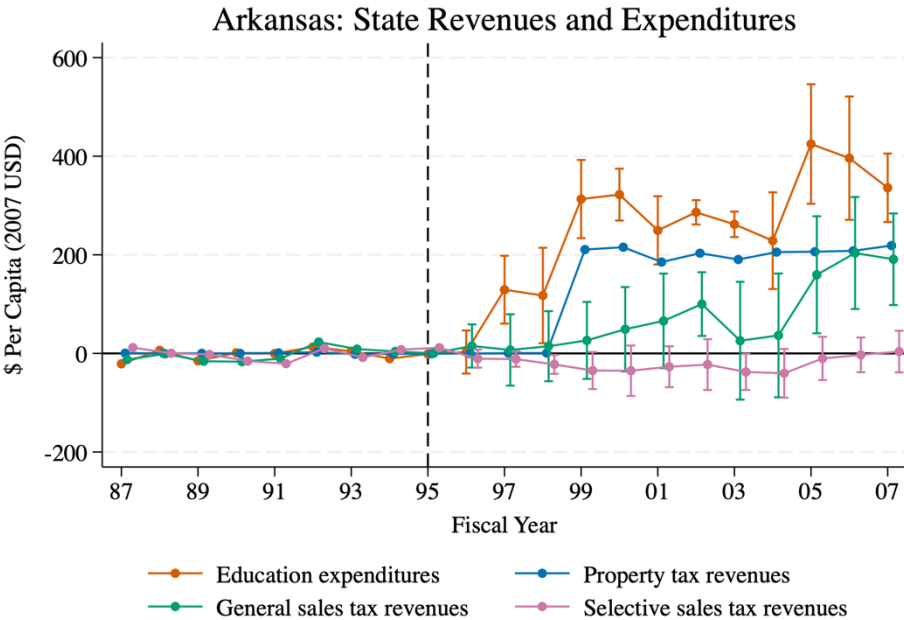
Notes: Because Figure E1 through Figure E6 are similar in appearance, detailed notes regarding how to interpret figures are only provided after Figure E1 for parsimony.

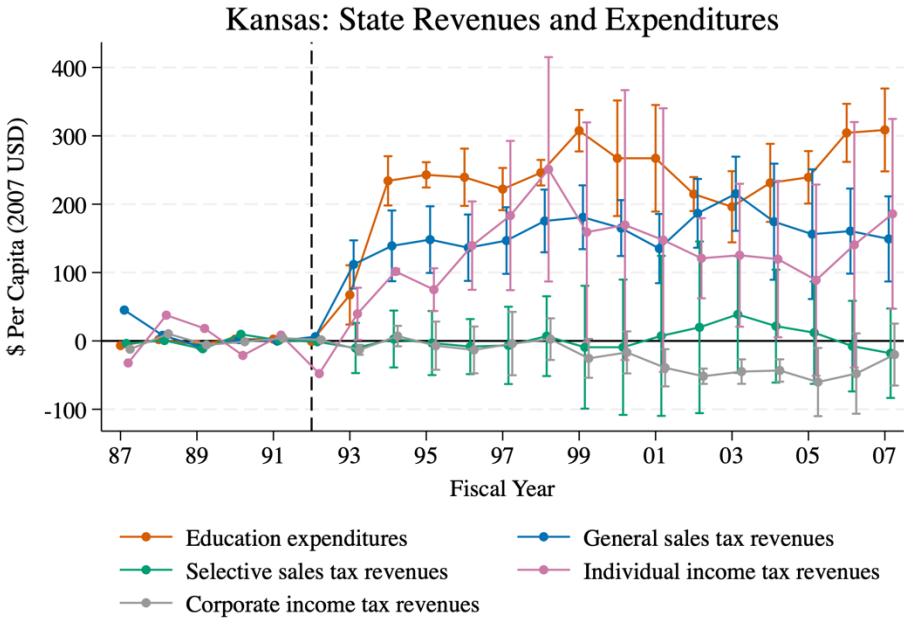**Figure E4.** Ridge ASCM effect estimates of New Hampshire's 1994 SFR

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
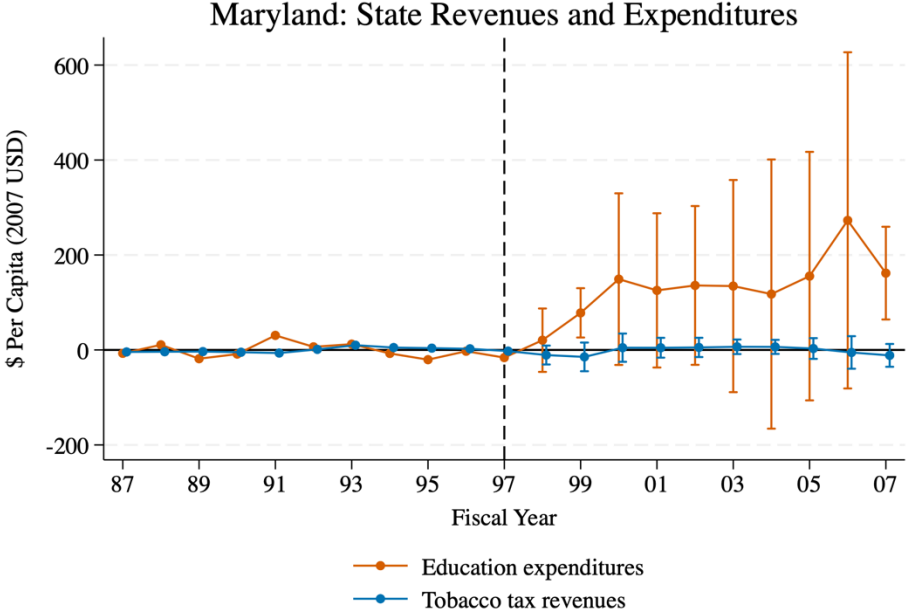

Notes: Because Figure E1 through Figure E6 are similar in appearance, detailed notes regarding how to interpret figures are only provided after Figure E1 for parsimony.

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.