Do Court Mandates Change the Distribution of Taxes and Spending?

Evidence from School Finance Litigation

Zachary Liscow*

January 2017

Abstract: Little is known about whether court mandates ultimately affect the distribution of taxes and spending or whether the legislature offsets the distributional consequences of those court orders with other changes. To offer insight into this question, I use an event-study methodology to show how state revenues and expenditures respond to court orders to increase funding for schools. The court orders appear to be financed almost entirely through increases in taxes, and there is little evidence of offsetting behavior by the legislature. State income tax changes appear to be broad-based across the income distribution and do not appear to target tax filers with children. The results suggest that welfare analysis of these legal rules should take into account not only efficiency but also distribution.

* Yale Law School, Associate Professor. Contact: zachary.liscow@yale.edu. Thanks to Conor Clarke, Ed Fox, Jacob Goldin, Louis Kaplow, Jonatahn Katz, Al Klevorick, Mitch Polinsky, William Woolston, and participants at the Georgetown Law and Economics Workshop, Conference on Empirical Legal Studies, National Tax Association Annual Meetings, and Hebrew University for helpful comments. Michael Loughlin provided excellent research assistance.

Since the early 1970s, state supreme courts have ordered increased state aid for schools in poor areas, on the basis of state constitutional clauses on equal protection and access to education. These cases began in California in 1971 with *Serrano v. Priest*[1] and continue through today. Various authors have studied how much state funding for education increased as a result of the court orders (though none for the full range of years of school finance decisions) (Evans et al. 1997, Corcoran and Evans 2015, Lafortune et al. 2016).[2] However, none have studied how state legislatures have paid for it.[3] This paper uses an event study methodology to measure how legislatures pay for these mandates, and to evaluate the distributional consequences of the decisions to finance increased expenditures on education.

There are at least two reasons why it is important to develop an understanding of these implications of education finance decisions. First, how legislatures choose to pay for increased funding on education matters intrinsically, for understanding the ultimate distributive impacts of the significant decisions. States now spend approximately $300 billion per year on K-12 education, much of it driven by school finance court decisions. These decisions have been viewed as progressive distributionally. However, how states have paid for the decisions has a significant impact on how progressive the decisions are. If they are financed with progressive income taxes, they are even more progressive than it would seem from looking at the spending alone. If they are financed by regressive income tax changes or reductions in other forms of spending that benefit the poor, then the opposite is true. Indeed, education finance litigation

[1] 487 P.2d 1241 (Cal. 1971) (finding that the Equal Protection Clause of the U.S. and California constitutions guarantee more equal funding across school districts, leading to more centralized funding).

[2] Lafortune et al. (2016) use data through recent years, but they do not consider court decisions prior to 1990. Additionally, others have studied the impact on local educational expenditures, but not state expenditures. See especially Jackson et al. (2016).

[3] Others have studied the impact on state government expenditures. Murray et al. (1998) find no significant impacts (p. 804-807, especially Table 5). Baicker and Gordon (2006) also do not find any significant impacts (p. 1533). However, no other paper has studied the impact on the structure of state government revenues.

continues in many states,[4] and one factor that could affect the value of the suits in the eyes of their advocates—and whether it is worth putting thousands of hours into them—is whether they ultimately impact the distribution of taxes and spending.

Second, this paper provides insight into a fundamental question in the empirical study of law: when a court orders changes that have certain distributional consequences, do those distributional consequences stick or do legislatures enact policies that offset the distributional consequences of the court order? The answer to this question, on which this paper is the first rigorous analysis of which I am aware, has significant implications for the welfare analysis of legal rules: if policies have distributive impacts that stick, those distributive impacts affect the welfare impacts of the law, in addition to the efficiency impacts that are the typical focus of economic analysis of legal rules (Posner 2014, p. 15-20, Cooter and Ulen 2012, p. 2-3, Shavell 2004, p. 7-8). Economic analysis of legal rules typically focuses on efficiency and proceeds as if the distributional consequences are offset. However, if distributional consequences of changes in legal rules are not offset, then those distributional consequences, and not just the efficiency consequences, matter for welfare. Therefore, if welfare is the ultimate normative goal, evidence that the legislature does not offset the distributive consequences of court orders would be evidence in favor of making different assumptions about political economy than those that are implicit in typical economic analysis of legal rules.

There are five reasons why the school finance natural experiment is a good one to provide insight into the distributive impacts that result from the interplay of courts and legislatures in the aftermath of court orders. In particular, the school finance natural experiment

[4] Cases are ongoing in Connecticut (Connecticut Coalition for Justice in Education, Inc. v. Rell, No. X07HHDCV145037565S, (Sup. Ct. Conn., Sept. 7, 2016)), Florida (Citizens for Strong Schools Inc. v. Florida State Board of Education, Case No. 1D16-2862 (First Dist. Ct. App. Fl., October 6, 2016)), and New York (Maisto v. State, No. 8997-08 (N.Y., September 19, 2016)), among many others.

provides fertile grounds for finding evidence of offsetting legislative behavior. First, the change in legal rules is sufficiently large that an empirically detectable offsetting change in taxes incentivizes legislatures to overcome inertia and enact offsetting taxes. These changes are big—very big. Consistent with others in the literature (Evans et al. 1997, Corcoran and Evans 2015, Lafortune et al. 2016), I find that spending increases by $910 per student in the aftermath of a school finance decision, a huge increase for one program and a promising area to look for offsetting legislative behavior.

Second, the economic incidence of the change in legal rule is relatively clear. Families with children benefit more from spending on schools than those without children, and poorer families benefit more—as a fraction of income—than richer families do from a given amount of per-student spending. So, to offset the distributional consequences of this progressive spending change that especially benefits households with children, the legislature would need to enact regressive tax increases or to cut spending on programs that benefit lower-income households and have similar measures that benefit families without children. A key part of what makes this analysis feasible is that the change is to a significant extent measurable since it comes in the form of government outlays rather than other more difficult-to-measure types of government entitlements (e.g., procedural rights in court or a change in liability rules). As a result, measuring whether the change in taxes matches with the change in spending is easier than it would be in other areas of law.

Third, legislatures may be particularly likely to respond via taxes to offset the distributional consequences of a change in legal rules brought about by education finance court decisions because state income tax codes have the ability to target the groups that disproportionately gain and lose from the rule changes. State tax codes are typically progressive

(targeting the well-off) and often tax differentially depending upon the number of children that a family has.[5] Since state legislatures already use taxes to target groups that disproportionately gain and lose, it presumably would be easier to tweak these tools to offset the distributional consequences of a court order.

Fourth, this natural experiment has a long time horizon for adjustment, which is consistent with the goal of finding a natural experiment with a high likelihood of showing a legislative response. Because school finance decisions have been occurring for many years, I can look 25 years after a decision to whether—even after a long period of time—state governments act to offset the distributional consequences of court orders.

Finally, and perhaps most importantly, there is plausibly exogenous variation in the legal rule. I conduct an event study, which takes advantage of the specific timing of court decisions. Any given state may be on a trend toward both greater state spending on poor schools and a changing distribution of taxes, but an event study takes advantage of the particular—and likely somewhat random—timing of the court decisions. In any case, a benefit of the methodology is that any overall trends are visible in an event study figure by looking at pre-trends. I also address the concern through robustness checks.

The results of the paper are as follows. I find little evidence of tax offset. To the contrary, I find that school finance reform does indeed lead to increases in education expenditure, which states pay for through tax increases. There is no evidence that the tax increases target the beneficiaries of the school spending and little evidence that other forms of spending decline in response to the decisions. At the end of the paper, I discuss possible implications for the welfare analysis of changes in legal rules, including considering the

[5] See infra Section II.

distributive impacts in addition to efficiency impacts, in light of the evidence that the distributive impacts "stick" in this context.

I. School Finance Decisions

School finance was historically largely done at the local level (McGuinn 2006). Starting with 1971 with the landmark *Serrano v. Priest*[6] decision in California, state supreme courts began requiring state legislatures to contribute more toward schools.[7] For a time, it appeared possible that the U.S. Supreme Court would also make a similar holding on the basis of the U.S. Constitution, but in *San Antonio Intermediate School District v. Rodriguez*,[8] it held that the U.S. Constitution contained no such right.

In the absence of federal intervention, courts in many but not all states stepped in, thereby leaving a patchwork of state decisions. State supreme court cases have come in roughly two waves: first, the "equity" cases and later the "adequacy" cases. Typically, the "equity" cases are based on equal protection clauses in state constitutions, and focus on achieving greater equality in funding between rich and poor school districts. One prototypical case of this sort is the 1973 New Jersey case *Robinson v. Cahill*,[9] which required equalization on the basis of New Jersey's state equal protection clause.

More recently, "adequacy" cases have been more prominent (Heise 1995). These cases are based on clauses in all state constitutions requiring the provision of education (Briffault and Reynolds 2009, Enrich 1995). State courts have interpreted these clauses as requiring a certain level of adequacy in the quality of education. As a result, these decisions can require more

[6] 487 P.2d 1241 (Cal. 1971)
[7] For more context, see Liscow (2016).
[8] 411 U.S. 1 (1973)
[9] 303 A.2d 273 (N.J. 1973), cert. denied, 414 U.S. 976 (1973).

funding in the entire state, since all schools in principle could be inadequate. One prototypical case of this sort is the 1989 Kentucky case *Rose v. Council for Better Education*,[10] which required that, on the basis of "adequate national standards,"[11] all Kentucky schools including those in well-off areas receive more funding. There is not a firm division between the two types of cases, as cases can reference both provisions in state constitutions and use both lines of argument, in addition to others.

The decisions do not generally require specific remedies from the legislature, and rarely require specific additional amounts of spending. Perhaps partly due to the typically vague language of the court mandates, many states have had multiple state court opinions. Instances of multiple opinions may suggest that the legislature does not know what the court requires or is trying to reduce its costs of compliance. In the case of New Hampshire, for example, there were five state supreme court decisions in the *Claremont v. Governor* series[12] between 1993 and 2002 (Jackson et al. 2016). By 2009, the New Jersey supreme court had issued 23 opinions.[13]

Based on textual analysis of cases, with the assistance of previous scholars who have studied cases, I generate a list of potential events from the entire history of state-level school finance decisions back to the early 1970s. In this list, I attempt to identify all major state court decisions that have called for state legislatures to significantly reform their of education finance. I use similar lists developed in Jackson et al. (2016) and Lafortune et al. (2016) as initial references, and I supplement their lists with my own research, generally including on the basis of my own research their cases but sometimes disagreeing and sometimes adding others. Appendix Table 1 lists the 65 cases that I code as important decisions requiring changes in school finance;

[10] 790 S.W.2d 186 (Ky. 1989).
[11] 790 S.W.2d at 198.
[12] These cases culminated with Claremont School District v. Governor, 794 A.2d 744 (N.H. 2002).
[13] Briffault and Reynolds (2009), p. 515.

it also includes columns listing how Jackson et al. (2016) and Lafortune et al. (2016) coded the cases.

Of the 65 potential decision years in my list, 56 reflect court decisions that were made by a state's highest court. As both Jackson et al. (2016) and Lafortune et al. (2016) did in their respective lists, I also include certain lower-court decisions as potential events. Of the 65 potential decision years in my list, 9 are lower-court decisions. I only include lower-court decisions if they were important decisions and their appeal was not heard by a higher court. The Online Appendix includes a detailed explanation for the inclusion of every case, as well as explanations for cases that were included in Jackson et al. (2016) or Lafortune et al. (2016) but that I do not include.

A key point about these decisions as they relate to my identification strategy is that they involve a lot of chance. It would be concerning if say, after Democrats retake power from Republicans in a state legislature, litigants could file papers in a state's highest court and quickly get a decision. However, that is far from how things work. Rather, litigants must typically file in the lowest court in a state and go through an often lengthy trial, replete with all of the procedural hurdles and delays that tie up many cases—in particular ones as complicated as those involving entire school systems. Then litigants typically appeals to an intermediate court and then the highest court, often years later. The outcomes are unpredictable. Which judge litigants receive in lower courts is not predictable. Membership on courts changes. And, outcomes are uncertain; states have given different interpretations to similar text, depending a host of contingent factors including membership on the court, the quality of the litigating, the facts of the case, and precedent in that jurisdiction. And that is when precedent matters; often courts appear to make out of whole cloth holdings; after all, before the early 1970s courts had not

gotten involved. Though of course influenced by its context and other states, each court that got involved decided in its own way whether to issue a decision contrary to the government, how strongly to word it, and how persistent to be if the state was ungenerous with additional school funding.

II. Data

I use three sources of data. The first, as just described, is a dataset of years of major state court holdings that I constructed. These cases constitute the "event" in the event study. Second, I use the Annual Survey of Governments from the Census Bureau, which has annual data from 1972[14] and 1977 to 2014 for breakdowns by type all state government revenues and expenditures.[15] These data are for the state government, not all governments in the state; that is, they do not include local expenditures and revenues. All revenue and expenditure measures are presented in per capita terms by dividing by the state population in the relevant year[16] and are inflated to 2015 dollars using the CPI-U Index from the Bureau of Labor Statistics.[17]

Third, I construct an annual distribution of average state income tax rates across time.[18] To do so, I use the National Bureau of Economic Research's online TAXSIM tax calculator[19] and the U.S. Census data on income distribution to create a dataset of yearly state income tax

[14] For revenue variables, 1972 data are often missing, so I exclude 1972 for those years.
[15] *Annual Survey of Governments*. Pierson K., Hand M., and Thompson F. (2015). The Government Finance Database: A Common Resource for Quantitative Research in Public Financial Analysis. PLoS ONE doi: 10.1371/journal.pone.0130119.
[16] The state population data is retrieved from U.S. Census, Population Estimates, Population Change, and Components of Change, http://www.census.gov/data/tables/2016/demo/popest/state-total.html
[17] The CPI-U data is retrieved from https://www.minneapolisfed.org/community/teaching-aids/cpi-calculator-information/consumer-price-index-and-inflation-rates-1913.
[18] Income percentiles come from U.S. CENSUS, HISTORICAL INCOME TABLES: INCOME INEQUALITY, tbl. H-1, http://www.census.gov/data/tables/time-series/demo/income-poverty/historical-income-households.html.
Income percentile data come from the Current Population Survey. In 2014, the Census Bureau used a "probability split panel design to test a redesigned set of income questions," and subsequently released two sets of income estimates for 2013. I average these two income estimates to generate the income distribution for 2013.
[19] NBER, TAXSIM, http://users.nber.org/~taxsim/.

rates. I produce these rates from 1977 to 2015, which is the range of years that TAXSIM can produce state income tax rates. I produce these state average income tax rates for households at the 20th, 80th, and 95th percentiles of the annual national income distribution. At these percentiles, I consider both a representative unmarried individual without children and with two children. The Appendix has parallel sets of results for married filers. I assume that the individuals take the standard deduction and receive income only in the form of wages. I produce the average (not the marginal) tax rate, which is the relevant statistic of distributional concerns.[21]

Table 1 presents summary statistics. Table 1A shows the summary statistics for annual total educational expenditure.[22] States spent an average of $1,552 per year per resident on K-12 education in 1972 and 1977 to 2014. (In these summary statistics, as in the regressions, I exclude data on Alaska prior to 1986 due to highly anomalous measurements.) The Appendix shows some illustrative figures showing per capita educational spending across time in specific states, with a line indicating a major state supreme court decision; these five examples are not random, but rather chosen to show states that do appear to respond to court orders. Table 1A also shows that a quarter of observations are after a decision; the rest are before a decision or in a state that did not have a decision.

Table 1B describes the revenue structure of states, with an average revenue of $4,377 per person, roughly half of which ($2,321) comes from taxes (and most of the rest of which comes from intergovernmental transfers from the federal government). Nearly two fifths of state taxes

[21] What matters for people's after-tax income is how much the government taxes overall—that is, for the average dollar. In contrast, what matters for the behavioral response to taxation is the marginal tax rate, since individual deciding whether to earn another $100 pre-tax will look primarily at the tax rate on that last, marginal, dollar.

[22] I use total educational expenditure, rather than K-12 educational expenditure, because the K-12 data only goes back to 1981, and I want to use as much data as possible. Results are similar when using only K-12 with the reduced number of years.

come from income taxes ($922 per capita), and roughly one third of state taxes comes from sales taxes ($753 per capita). A small amount ($45 per capita) comes from property taxes.

Table 1 C details the portion of state revenue generated by license fees. These fees total $150 per capita, roughly half of which is motor vehicle license fees ($79). A small amount of revenue ($29 per capita) comes from corporation license fees.

Table 1D describes the expenditure structure of states. States have average expenditure roughly equal to total taxes. Of that expenditure, $3,390 is on items other than K-12 education. States spend $1,086 per capita on "welfare," which means Medicaid, Temporary Assistance for Needy Families and its predecessor, and Supplemental Security Income; of that $416 is the state contribution. States also spend $331 on health care other than Medicaid, including state-run hospitals and community health centers. States also spend $562 per person on higher education, $330 on employee retirement, $176 on unemployment benefits, and $325 on highways. Across all categories of expenditures, states spend an average of $273 per capita on construction. States on average have $2,577 of debt outstanding.

Tables 1E and 1F present summary statistics on the structure of state income taxes. The summary statistics show fairly substantial rates, especially for those at the top of the income distribution. Table 1E has the average state income tax rates for single tax filers without children. The average rate is 2.04% at the 20th income percentile, 3.97% at the 80th percentile, and 4.34% at the 95th percentile. Table 1F has analogous rates for those with children, with an average tax rate of 0.70% at the 20th percentile, 3.80% at the 80th percentile, and 4.24% at 95th percentile.

These rates create a structure that is progressive. For households with children, taxes are 1.93 percentage points lower at the at the 20th percentile of the income distribution than at the

80th percentile and 2.30 percentage points lower than at the 95th percentile, as Table 1E shows. The progressivity of state income taxes varies widely, with 20th percentile income earners paying anywhere from 8.28 percentage points less than 95th percentile income earners (California in 1978), to 0.11 percentage points more than their 95th percentile counterparts (Alabama in 2015). The rate structure is even more progressive for families without children. As table 1F shows, for these households, taxes are 3.10 percentage points lower at the at the 20th percentile of the national income distribution than at the 80th percentile and 3.55 percentage points lower than at the 95th percentile.

Table 1G shows that state income taxes have lower rates for families with children than those without children, especially at the bottom of the income distribution. At the 20th, 80th, and 95th percentiles, those with children have a 1.34, 0.17, and 0.10 percentage point lower average income tax rate, respectively.

Finally, Table 1G presents summary statistics for "differences in differences" estimates: that is, the difference between those of the 20th percentile and higher-income percentile of the difference in average tax rates between those with and without children. These summary statistics reflect the greater progressivity for households without children than with children. The difference in difference is 1.17 percentage points lower at the 80th percentile than the 20th percentile and 1.25 percentage points lower at the 95th percentile than the 20th percentile.

III. Methodology

Using this data, I then conduct an event study with various outcome variables, to identify how much the outcome changes after a decision. I control for state and year fixed effects and, in the preferred specification, allow for differential pre and post trends. Effectively I compare

changes in outcome variables in states with decisions to changes in those variables in states without decisions. I use three specifications. The first and simplest just measures the jump in the outcome variable after a school finance decision:

$$(1) \quad \theta_{it} = 1(t > t_i^*)\beta^{jump} + I_t + I_i + \varepsilon_{it},$$

where θ_{it} is the outcome in state i in year t—typically either per capita spending or revenue or a function of state average income tax rates. The main coefficient of interest is β^{jump}, which measures how much the outcome changes after a school finance decision compared to its pre-decision average (indicated by being in a year greater than decision year t_i^*). I also include fixed effects for each year (I_t) and each state (I_i). I refer to this specification as specification (1) or the "jump" specification.

I also include specifications that allow for not only a jump in the level of the outcome variable, but also a shift in the trend:

$$(2) \quad \theta_{it} = 1(t > t_i^*)\beta^{jump} + 1(t > t_i^*)(t - t_i^*)\beta^{phasein} + (t - t_i^*)\beta^{trend} + I_t + I_i + \varepsilon_{it}.$$

With specification (2), I build on specification (1) and measure not only the jump coefficient but also whether the outcome increases after a decision ($\beta^{phasein}$) when compared to the underlying trend (β^{trend}). I refer to this specification as specification (2) or the "trend" specification. This specification is my preferred one, and the results section will focus on it.

With increases in school funding after court orders, we should expect to see a positive β^{jump} for the regressions with school funding as an outcome variable in the jump and trend

specifications. If the distributive consequences of these progressive expenditure changes are offset through tax changes, we should expect to see taxes go up on the poor relative to the rich because the poor are benefitting disproportionately (as a percent of income) from the state aid for schools, resulting in a positive β^{jump} where the outcome is the average tax rate of poorer groups minus the average tax rate of richer groups. Similarly, a positive β^{jump} for the revenue variables would indicate that the increase in school finance is funded through that form of revenue, and a negative β^{jump} for expenditure variables would indicate that a decrease in that form of spending funds the increase in education spending.

Third, I produce nonparametric specifications that measure how outcome variables change year-by-year before and after decisions:

$$(3)\quad \theta_{it} = \sum_{r=k_{min}}^{k_{max}} 1(t = t^* + r)\beta_r + I_t + I_i + \varepsilon_{it}.$$

In this specification, in addition to the state and year fixed effects, I have a measure of how much the outcome variable varies in each year. So, for example for $r = k_{min} = -15$, the coefficient β_{-15} would measure the average difference in spending 15 years before a decision relative to the year of a decision (conditioning on year and state fixed effects). With increases in school spending and taxes to fund the spending, we expect a jump in the β_r coefficients after a decision.

Several states have multiple important decisions. For these states, I follow Lafortune et al. (2016) and Card et al. (2008), and use the data itself to identify the most pivotal event. In particular, for each state, I run the regression:

$$(4)\quad E_{it} = \alpha + 1(t > t_l^*)k + \epsilon_{it}$$

In the regression, E_{it} is state education spending in state i in year t (after subtracting average spending in all other states in year t to adjust for country-wide changes)[23], and k is the coefficient on an indicator variable for years past the decision year. For each state with more than one decision, I then choose the year with the largest k, indicating that it has the largest difference in average spending, following Bai (1997). Figure 1 shows the number of states in the dataset that have data a given number of years from a decision based on this methodology. Twenty-five states have decisions that appear to require significant increases in school finance. The number of states is non-monotonic because I have data from 1972, but not 1973-1976.[24]

As a result of this methodology, I will of course be measuring the impact on spending for the most impactful year in every state that has a decision requiring more school spending. If the goal of the paper were to measure the effect of the average state supreme court holding on state-level school spending, this methodology would be problematic, since it by definition only captures the most impactful decision. However, the goal of the paper is to measure, *conditional on the increase in school funding*, how that increase in funding has been financed. And the year is not selected on the basis of a particular funding mechanism. So long as the specific times of the most impactful decisions to not relate to an increased likelihood of particular kind of funding, choosing the most impactful decision will not bias the results. I will return to an assessment of this assumption below.

All regressions, unless stated otherwise (as in a robustness check) are weighted by population, to capture the effect for the average person rather than the average state. In particular, I weight by the state's population divided by US population in that year, so that earlier

[23] There are substantial country-wide trends. In particular, per capita K-12 education spending is basically flat through the mid-1980s, then increases until the Great Recession, when it faces a steep decline.

[24] The increase in the number of states with data after a decision is the result of decisions in New Jersey in 1973 and California in 1976, which are during the gap of four years when I do not have state finance data.

years do not have less weight than later years. Robust standard errors are clustered at the state level for all regressions. In all specifications outside of robustness checks (which use more years of data), I include data from 15 years before a decision through 25 years after a decision. I do so in order to include as much data as possible without suffering too much from having an unbalanced panel.

Finally, figures of tax filers with children include back-of-the-envelope estimates of what offset of the distributive consequences of the change in legal rules might constitute; these estimates are presented in the figures as green dotted lines. As the discussion section explains at greater length, the estimates measure what fraction of household income the increased student spending is as a fraction of family income. The estimates also adjust for the likelihood that not all state spending on schools will actually reach students, using the canonical compilation of estimates of the "flypaper effect," giving 63.7% (Hines and Thaler 1995).[25] So, for example, a $1,000 per student increase in education spending[26] for a family earning $100,000 with two children would have a back-of-the-envelope estimate of 1.27% (= $1,000 * 2 * 63.7% / $100,000). Figures for tax rates of households without children do not have analogous lines, since households without children are not direct beneficiaries of the increases in school finance. I defer discussion of the results relative to the back-of-the-envelope estimates, as well as qualifications of these estimates, to the discussion section.

[25] The paper lists ten studies with an average result of 63.7% of spending being used for the intended purpose.

[26] I measure the per capita change in spending. To convert the per capita measure to a per student measure, I perform the following calculation: There were 53.66 million children in K-12 school in 2015. Table 1 at https://www.census.gov/hhes/school/index.html. (The calculation assumes that assuming that half of those in preschool or kindergarten were in kindergarten.) With a total US population of 321.42 million, this means that the ratio of total population to school-attending population is 5.99.

IV. Results

A. Main results

Table 2 answers the baseline question of the effect of the school finance decisions on educational expenditure, showing that decisions increase expenditures by $144 per capita in the "jump" specification, which is significant at the 5% level, and $152 in the "trend" specification, which is significant at the 1% level. (The richer trend specification is my preferred specification, so I will focus discussion on that throughout.) There are not statistically significant pre- or post-trends. Figure 2 shows the nonparametric regression in the solid blue line with dots representing the point estimates and gray dashed lines representing the 90% confidence interval; the red solid line without dots represents the estimates for the trend specification. In the 15 years prior to a decision, per capita K-12 spending is flat, with the jump up coming in the few years after a decision. The flat pre-trend, in both the non-parametric specification and the "trend" specification, is reassuring for the exogeneity of the natural experiment; I will return to this issue below. Spending then stays at roughly the same level (with the exception of a dip from 18 to 22 years past the decision) in the subsequent 25 years. Most individual years are greater than zero with 95% confidence. These results are in line with the existing literature. Converting the per capita increase to a per student increase gives a $910 increase per student, which is very similar to the $912 that Lafortune et al. (2015) find, the $786 that Corcoran and Evans (2015) find, and the $744 that Evans et al. (1997) find[27] on a much earlier and smaller sample of school finance decisions.

Table 3 turns to the financing of this increase in education spending. In this table, and those that follow, part A of the table has the jump specification and part B has the trend specification. In the wake of a decision, government revenue increased by $221, which is

[27] This number inflates to 2015 dollars from $437 in 1992 dollars.

significant at the 1% level, in the trend regression (and by $277 in the jump regression, which is also significant at the 1% level). That $221 increase mostly comes from increased taxes—with an increase of $176 in taxes per capita, which is significant at the 5% level. I cannot reject the null that all of the increased revenue comes from increased taxes. While I do not have the power to know with precision the entire composition of that increase in tax revenue, the regressions show that $91 come from increases in income tax revenue,[28] $47 come from increases in sales tax revenue (significant at the 10% level),[29] and $9.33 from increases in property tax revenue, though none is significant at the 5% level. The jump regression shows a larger share for income tax, with a $179 increase in total taxes (significant at the 5% level), of which $133 comes from increases in the income tax (significant at the 10% level).

Table 4 shows how license fees changed in response to court decisions. Total license fees increased by $17 following school finance decisions (significant at the 5% level), which represents an 11% increase in license fees. While increasing license fees is typically thought of as a regressive policy, breaking down this increase shows that most of the increase comes from a $12 increase in corporation license fees (significant at the 1% level). This increase is even larger in the "jump" specification ($15) and is significant at the 5% level.

Tables 5 and 6 turn to how expenditures changed in response to the school finance decisions. Column (1) of Table 5B shows that total expenditure increased by $155, roughly the same amount as K-12 education spending increased (recall the $152 increase in K-12 education spending). I then assess the significant categories of expenditure—construction, healthcare,

[28] Income tax revenue includes not only the personal income tax but also the corporate income tax. I also looked at the impacts on a breakdown of different variables, including the individual income tax, corporate income tax, and death tax, as well as lottery revenue. None had significant effects at the 5% level, although the corporate tax did have significant effects at the 10% level (coefficient = 25.43, SE = 14.58).

[29] The regressions include general sales tax revenue, but not sales tax revenue from the sales of specific items, like alcohol. There are no significant impacts on other forms of sales tax revenue.

welfare (including Medicaid, Temporary Assistance for Needy Families and its predecessor, and Supplemental Security Income), state-level welfare, higher education, financial administration, employee retirement, unemployment benefits, and highways. I also include total debt outstanding. None of these categories has a statistically significant decrease. (State-level welfare spending has a small decrease that is far from statistically significant and in any case has a post-trend that is large enough to make up for the decline in about two years.) The only significant coefficient is on higher education (Table 6B, column (1)), which is actually positive. One interpretation of that result is that, given the large number of categories, it is likely that one category will by chance come out as a significant increase.

Having shown that the increases in educational spending appear to be financed by increases in taxes, I now show how the structure of state income taxes change in response to education finance court decisions. While the post decision coefficient from the "trend" specification for income taxes is not significant in Table 3B, the magnitude of this coefficient ($91, most of the increase in taxes), and the large and significant increase in income taxes shown in the "jump" specification, suggest that income tax increases account for an important portion of the overall increase in state taxes. Tables 7, 8, and 9 show the changes in the structure of state income taxes for single filers; the Appendix has results for married filers, which have similar point estimates, but are often less precisely estimated than for single filers.

I focus on measuring whether those who directly benefit most (as a percent of income) from the school finance decisions – those of lower incomes and those with children – face disproportionate income tax increases to pay for the increases in spending on education. I find that these groups do not disproportionately benefit from tax changes in the aftermath of decisions. Tables 7 and 8, columns (1) – (3), describe the changes in tax rates at three points in

the income distribution, 20th (column (1)), 80th (column (2)), and 95th (column (3)), for filers without children (Table 7) and filers with children (Table 8). The estimates in Table 7B show statistically significant increases in average tax rates at the 20th percentile (an increase of 0.15 percentage points), but also shows a statistically significant negative post-trend of -0.023. The table also shows a statistically significant increase in tax rates at the 80th percentile (0.28 percentage points), and a comparable but statistically insignificant increase at the 95th percentile (0.28 percentage points). The left-hand-side panel of Figure 3 shows graphically how income tax rates changed at these percentiles for single filers without children. (I defer discussion of the green dotted "tax offset" line to the discussion section.)

Table 8B contains an analogous set of regressions for filers with two children. Again, there are larger tax increases at the 80th and 95th percentiles than at the 20th percentile, but only the increase at the 80th percentile is statistically significant. The right-hand-side panel of Figure 3 show graphically how average income tax rates changed at these percentiles for filers with children.

Columns (4) and (5) of Tables 7 and 8 calculate the effect of a decision on the difference in tax rates between higher and lower incomes. None of the estimates show a statistically significant difference from zero; that is, I cannot reject the claim that income tax rates increased at the same rate for lower-income and higher-income taxpayers. But, there is a difference between an imprecisely estimated zero and a precisely estimated zero. Figure 4, which displays the nonparametric estimates for the difference between the 20th and higher income percentiles, shows that, particularly for those without children (on the left-hand side) the estimates are quite precise. As shown in Figure 4A, for single filers without children, we can say with 95% confidence that taxes did not increase by more than 0.33 percentage points for the 20th percentile

than the 95th percentile, even in the year that has the highest upper bound on its confidence interval.[31] And, as shown in Figure 4C, also on single filers without children, we can say with 95% confidence that taxes did not increase by more than 0.51 percentage points more for 20th percentile earners than 95th percentile earners in any year within 25 years of a decision.[32] In other words, these zeros (and the point estimates in both these cases are almost exactly zero) are fairly precise zeros. For filers with children, the point estimates are about the same as those without children, but the estimates are noisier. The discussion section further considers the degree of precision in these estimates.

Columns (1) – (3) in Table 9 compare the average income tax rates for filers with children to those without children, since the largest direct beneficiaries of the school finance decisions are those with children. The results for higher income earners show very small, statistically insignificant jumps of 0.04 percentage points at the 80th percentile in column (2) (very precisely estimated with a standard error of 0.04) and 0.02 at the 95th percentile in column (3) (very precisely estimated with a standard error of 0.02). Column (1) of Table 9B has a point estimate suggesting that tax rates for filers with children at the 20th percentile actually decrease relative to filers without children after school finance decisions, although this estimate is imprecisely estimated. At the 80th and 95th percentiles, the results also show small, but statistically significant, downward trends after the decisions: -0.013 at the 80th percentile and -0.006 at the 95th percentile (both significant at the 5% level), meaning that with time tax policy actually becomes more favorable to those *without* children than those without children after a school finance decision. Figure 5 shows the nonparametric estimates for these same outcome

[31] After generating the non-parametric regression results, I identify the largest point estimate within 25 years of a decision to be 0.106. Using this point estimate and the its standard error of 0.135, I construct a one-sided confidence interval as follows: (0.106)+(1.645)*(0.135)=0.33.

[32] The largest point estimate is 0.144 with a standard error of 0.220, giving: (0.144)+(1.645)*(0.220)=0.5059.

variables.[33] The estimates for 20th percentile earners are rather noisy, but for the 80th and 95th percentile, I can say with 95% confidence that the tax rates did not increase for those with children more than those without children.[34] I defer discussion of these results relative to the tax offset estimates, presented in the green dotted lines, to the discussion section.

Table 9, columns (4) – (5), shows the difference in difference estimates: that is, they see whether there was a disproportionate increase in taxes for the most directly-benefited group: low-income households with children, relative to higher-income households and those without children. This difference in difference removes differential baseline trends affecting those with different numbers of children or different income levels. Column (4) measures the difference between column (1) (the difference in tax rates between 20th percentile filers with and without children) column (2) (the difference in tax rates between 80th percentile filers with and without children). Column (5) repeats the exercise for the 95th percentile, and takes the difference between column (1) and column (3). The resulting point estimates on the post decision coefficient are small (-0.15 for the 20th versus 80th percentiles and -0.13 for the 20th versus the 95th percentiles), and there are no statistically significant trends. The standard errors in these estimates are quite large, which is unsurprising given the magnitude of the standard errors in column (1) (0.25), for the difference in tax rates at the 20th percentile for those with and without children. Figure 6 shows the nonparametric estimates corresponding to columns (4) and (5) of Table 9. Point estimates are roughly zero, but the confidence intervals are large, and I cannot

[33] The large drops at 22 years in Figure 5B and Figure 5C is driven by California; excluding California eliminates the drop. In 1998, 22 years after California's 1976 decision, the difference in tax rates for single filing 80th percentile income earners between those with and without children dropped from roughly -0.2 to -0.65. Similarly, in 1998 this difference for 95th percentile income earners also fell from roughly -0.06 to roughly -0.31.

[34] The highest post-decision point estimate at the 80th percentile is -0.001 with a standard error of 0.017. (-0.001)+(1.645)*(0.017)=0.018. The highest post-decision point estimate at the 95th percentile is 0.001 with a standard error of 0.011. (0.001)+(1.645)*(0.011)=0.019

rule out tax increases on 20th percentile earners with children versus those without children that are one percentage point more than the difference between the same difference for 80th and 95th percentile earners.[35]

B. Robustness Checks

Tables 10 and 11 include robustness checks. A concern that one might have is that states are trending in a particular direction politically and that, as a result, the distribution of spending and taxation may be out of equilibrium in a state before a decision. For example, if a state is trending more Democratic, but has not yet had a chance to make its spending and taxation more progressive (assuming that that is what Democrats want), it may be that a decision triggers the change that the state's voters had been asking for, consistent with pent-up demand. If that is true, we would not expect offset of the distributional consequences of the change in legal rules, since courts are just acting in accord with changing distributional preferences of the population. To address this concern, I control for presidential vote share and state government control. The presidential vote share measures the percent share that the Democratic presidential candidate captured in the election that year or the most recent presidential election. I construct the variable on the control of state government two ways. The first way is as follows: if a Democrat is the governor, I assign the variable 50, and otherwise 0; and if Democrats lead a chamber of the legislature, I assign the variable 25, and otherwise 0. So, if Democrats are completely in charge of state government, the variable has a value of 100.[36] Columns (1) through (3) of Table 10 show that controlling for each of these variables separately and together has little effect on the

[35] The highest post-decision point estimate at the 80th percentile is 0.672 with a standard error of 0.527.
(0.672)+(1.645)*(0.527)=1.54
The highest post-decision point estimate at the 95th percentile is 0.509 with a standard error of 0.535.
(0.509)+(1.645)*(0.535)=1.39

[36] The data come from Carl Klarner's datset at Harvard's Institute for Quantitative Social Sciences. Klarner, Carl, 2013, "State Partisan Balance Data, 1937 - 2011", hdl:1902.1/20403, Harvard Dataverse, V1 https://dataverse.harvard.edu/dataverse.xhtml?alias=cklarner

results (although the result is modestly smaller when both controls are included).[37] I also construct the legislative control variable another way: for each state I construct three dummy variables for whether or not they have a Democratic Governor (column (4)), a state legislature in which Democrats control both chambers (column (5)), and both a Democratic Governor and an entirely Democratically controlled legislature (column (6)). As with the results in columns (1) through (3), when controlling for each of these factors individually and together (column (7)), the post decision coefficient for education expenditure remains greater than $130 per capita, and significant at the 1% level. The fact that the coefficient stays stable with these controls suggests that there was not pent-up political demand for a change in the distribution of taxes and spending to which state courts were responding. These results provide little evidence that political preferences are changing in a way that would make an absence of distributional offset consistent with voters' policy preferences.

Table 11 includes a variety of other specifications, including not weighting the results (column (1)), expanding the sample to 20 years before and 30 years after a decision instead of 15 years before and 25 years after (column (2)), and including all years of data (column (3)). None of these specifications substantially changes the estimate of the increase in per capita spending. I also include an estimate of the effect on log K-12 education expenditure (column (4)). That estimate is also highly statistically significant (at the 1% level in either the jump or trend specification), and suggests that a decision leads to approximately a 10% increase in spending. This coefficient is consistent with the estimate where education spending without the log is the outcome variable. The "jump" coefficient in Table 2 of $152 is 9.8% of average K-12 education expenditure ($1552).

[37] One might be interested in whether legislatures respond differently depending on who controls them or the governorship or the political leanings of the state. However, there are no statistically significant interactions between the jump variable and the political controls.

V. Discussion

The main results are straightforward: school finance litigation increases state funding for schools, and there is little evidence that these increases are offset by regressive changes in spending or taxes. This section first discusses identification concerns and then turns to interpreting the results in light of the tax changes would offset the court's distributional impacts.

A. Identification

A natural first question is whether the result is well-identified—that is, whether, in fact, taxes did not go up on the rich relative to the poor after the state supreme court opinions. For example, if one were analyzing how taxes responded over a decade in which a legislature gradually increased funding for schools, one might be concerned about long-term trends in preferences changing (for example, becoming more liberal) in ways that would lead to both more education spending and lower taxes on the poor relative to the rich, biasing the results against a showing of tax offset. However, the timing of school finance decisions is driven by courts, not by legislatures; as such, the timing is at least less likely to be driven by rapid changes in preferences. More importantly, the event study methodology takes advantage of the precise timing of the rulings, with an expected quick reaction afterwards as the new state education spending needs to be funded, and it is unlikely that changes or political circumstances would change as quickly as the new funding formulae need to be implemented. In any case, a benefit of the event study methodology is the ability to easily see the granular trends before and after a change, and the non-parametric estimates of both educational expenditure and state income tax rates evince no worrying pre-trends. Figure 2 shows flat pre-trends. And the political controls in Table 10 lead to little difference in the results, providing reassurance that there are not significant political confounds.

B. Do Taxes Offset Distributional Consequences?

I now turn to a discussion of the meaning of the results with respect to the broader question of whether legislatures respond to offset the distributional consequences of court orders. Asking that question first requires an estimate of these distributional consequences. As briefly discussed in the methodology section, the figures on changes in tax rates include in green dotted lines back-of-the-envelope estimates of the distributional consequences of the court orders. I call these the "tax offset estimates." This section discusses the plausibility of these back-of-the-envelope estimates relative to the magnitude of the state income tax results.

The tax offset estimates of how much a household benefits from the decision is calculated as follows: It takes the average increase in education spending per student, multiplies by the number of children in the household, and also multiplies by an estimate of the fraction of that spending that is actually spent on schools—and then divides by a household's income. This calculation provides an estimate of how much state income taxes would have to increase on a household in order to offset the increase in spending on education for that household. Given my estimate that per capita education spending jumps up by $152 (in inflation-adjusted 2015 dollars) because of a state court opinion (with little pre-trend before the decision), a student-to-population ratio of 5.99,[38] 2-child households, and a flypaper effect of 63.7%, the question becomes what fraction of income $1,157 is (= $152 * 5.99 * 2 * 0.637). These estimates will of course be regressive, since a given amount of spending on education is a larger fraction of income for poor families than for rich ones.

I focus on the state income tax primarily because it is the only tax tool that has the potential to be regressive enough to offset the very progressive distributional consequences of

[38] In 2015 there were 53.66 million students and a population of 321.42 million, giving a population-to-student ratio of 5.99. See supra note 26.

the increases in school finance. The sales tax and property tax, while regressive (since lower-income households spend rather than save a larger fraction of their income and also spend a larger share of their income on housing than higher-income households), are not nearly as regressive as spending on education is progressive. The reason is that higher-income households do in fact consume more and buy more housing as their income increases in absolute quantities; however, higher-income families typically do not get any more direct benefit from public school finance (and often get less). Another reason to focus on income tax rates is that, in any case, the state income tax is the largest source of state tax revenue and, though the estimates are noisy, the state income tax has by far the largest estimated increase in the aftermath of a school finance decision.

Before turning to a discussion of the results relative to the tax offset estimates, it is worth discussing the ways in which the estimates of income tax changes needed to offset the distributional consequences may deviate from what would actually be needed to offset their consequences. First, I assume that households value the increase in spending on their education at the amount spent. They could value it more or less (Cellini et al. 2010),[39] but I take this to be a reasonable baseline assumption.[40] Similarly, the estimate of the flypaper effect could be too high or too low. For example, I have estimated a lower flypaper effect in the state of Connecticut in other work (Liscow 2016, p. 43-48).

While the effects of these two factors are generally ambiguous, a more important factor suggests that the tax offset estimate is not too big but rather *too small*, which works against my

[39] For example, in this paper, the authors use evidence from changes in housing prices after close referenda to show that that people value school spending more than dollar-for-dollar.

[40] Of course, state aid for schools has many effects. Indeed, elsewhere I argue that it has important benefits in improving efficiency in where people live. See Liscow (2016), *Return to the Central City*. Nevertheless, for the purposes of estimating the distributive consequences, the main effect (as I show in my other work) results from the direct distributive consequences of the spending.

finding a failure of tax offset. In particular, the biggest set of assumptions concern how state spending is allocated. I assume that poor and rich households receive the same amount of school spending, which other work shows is not the case, since school finance redistribution sends far more money to poor school districts as to rich ones, and more poor households live in poor school districts than in rich ones (Jackson et al. 2016). A more sophisticated analysis would take account of the differential spending across school districts of different average incomes and the dispersion of household income in these different school districts. Furthermore, richer households tend to have fewer children and therefore benefit less from the spending.[41] Additionally, richer families are far more likely to send their children to private schools and therefore not benefit from the school spending.[42] So, incorporating how lower-income households likely benefit more than higher-income households in absolute spending levels means that I am biased toward finding that the distributional consequences of changes in legal rules are offset.

With these limitations in mind, consider how the results on changes to tax rates compare to the tax offset estimate. Given an estimated per student spending impact of $1,157, with a 2015 20th percentile income of $22,800, an 80th percentile income of $117,002, and a 95th percentile income of $214,462, school spending for two children amounts to 5.08% of income for 20th percentile filers, 0.99% for 80th percentile filers, and 0.54% for 95th percentile filers.[43] Thus, for 2-child filers, we expect a 4.09 percentage point increase in taxes for the 20th percentile

[41] U.S. CENSUS BUREAU, HISTORICAL INCOME TABLES: HOUSEHOLDS, Table H-1, http://www.census.gov/data/tables/time-series/demo/income-poverty/historical-income-households.html.

[42] U.S. CENSUS BUREAU, CURRENT POPULATION SURVEY: OCTOBER 2015, Table 8, http://www.census.gov/hhes/school/data/cps/index.html.

[43] See U.S. CENSUS BUREAU Income and Poverty in the United States: 2015, Table A2, http://www.census.gov/library/publications/2016/demo/p60-256.html

over the 80th percentile, and a 4.54 percentage point increase in taxes for the 20th percentile over the 95th percentile.

The analysis of tax rates strongly rejects these changes. As can readily be seen from the figures on the right-hand side of Figure 4, even with the relatively large standard errors for filers with two children, the upper bounds of the confidence interval are around 1, for comparisons of the tax rate at the 20th percentile versus both the 80th and 95th percentiles—far less than the "expected" tax offset estimates of 4.09 and 4.54, respectively, which are shown in dotted green lines. Similarly, Figure 5 shows that the differential impact of a school finance decision on filers with and without children at a given income percentile is far less than the "expected" changes of 5.08 percentage points at the 20th percentile, 0.99 percentage points for 80th percentile filers, and 0.54 percentage points for 95th percentile filers (just the value of the spending on two school-age children divided by income, minus the value of the spending on non-school-age children, estimated at 0). Again the tax offset estimates are shown in the green dotted lines.

Taking the tax offset estimates on their face, the state income tax regressions reject them by a long shot; there is no evidence for tax offset here. For example, taking the jump and trend estimates from Column 4 of Table 8B, and asking how far from a "null hypothesis" tax offset the estimate of spending 10 years after a decision would be yields an F-value of the test of the null that the coefficient is 4.09 equal to 43.63 (p-value < 0.0000).[44] So, depending on how one looks at it, the result either is a precisely-measured zero or a great rejection of a null hypothesis of tax-offset.

Although the estimated response is nowhere close to the back-of-the-envelope tax offset estimate and, indeed, there is even directionally little evidence of tax offset, one may still remain

[44] That is, I test the jump coefficient + 10 * the post-jump trend coefficient against the null of 4.09. As noted in the previous paragraph, the expected change in tax rates would be an increase of 4.09 percentage points more for the 20th percentile than the 80th percentile.

concerned that the tax offset estimates are missing something important, so here I address several potential concerns. But before doing so it is worth emphasizing how far off the results are from the tax offset estimates, which could be way off in many ways, and the results would still reject the hypothesis that policymakers responded in a way that offset the distributional consequences of the change in school spending. Nevertheless, it is worth considering potential objections, though in the interest of parsimony, I do not incorporate these factors into the estimates themselves with one exception.

First, one might be concerned that types of state taxes other than income taxes did increase. Sales taxes, in particular, form a sizable portion of state budgets. If sales taxes respond, then in fact the overall taxes may be becoming more regressive as the tax offset assumption predicts, because sales taxes are regressive owing to the larger fraction of income that poorer household spend rather than save. However, sales taxes are not nearly regressive enough to offset distributional consequences of school spending. The Tax Foundation estimates that the bottom quintile pays 2.56% of its income in the general sales tax, while the top quintile pays 1.94% (Chamberlain and Prante (2007) ,Table 16 at p. 42), a difference of 0.62 percentage points. Recall that, in contrast, the difference in estimated school finance spending as a fraction of income at the 20th and 80th percentiles is 4.09 percentage points, far greater than the 0.62 percentage point difference for the sales tax, since—though sales taxes are regressive—well-off families do spend more and pay more sales tax, while they do not receive any more school funding. (And, indeed, as noted, though it is not included in the tax offset estimates, the school finance spending targeted poor areas over rich ones, making lower-income families even greater beneficiaries.)

A second potential reason that one might expect less of a tax increase on the rich than the tax offset estimate is the uncertain economic incidence of school spending. Part of the concern could result from the possibility that some of the state spending on schools actually results in tax reductions to local residents; that is, when the state transfers money to a school district, the school district may reduce taxes instead of increasing spending by the full amount of the transfer.[45] Indeed, in earlier work, I have produced estimates that a significant share of state spending on schools in Connecticut may have gone to tax reductions (Liscow 2016, p. 43-48). However, I base the null hypothesis on my best guess of the actual increase in spending on education in school districts, so I should not be overestimating the change in school spending.

That said, one might be concerned that the distributional effects of the local tax reductions partly offset the distributional effects of school spending. However, those tax reductions actually *reinforce* the distributional effects of the school spending rather than counteracting those effects. The main tax used by local governments is the property tax, a regressive tax (at least in partial equilibrium (Zelinsky 2002))[46] since poorer people spend a higher fraction of their income on housing. So reducing local property taxes is a progressive policy, since tax reductions are generally proportional to property values.[47] Recall that my main outcome variable is a rate, a fraction of income, not an absolute dollar amount—so, even though richer people will benefit more in absolute dollars, they will benefit less proportionally. Furthermore, those tax reductions are disproportionately in poor areas, since state school aid goes disproportionately to the poor areas; the disproportionate number of poor people in poor

[45] A related concern is that some of the measured increase in spending on schools could come from the local governments themselves. As explained above, precisely the opposite is likely to be the case—there are local tax and spending reductions, not increases.

[46] As a tax on capital, the property tax can change the overall return to capital. However, these local property tax changes are small and do not have a bearing on the overall return to capital.

[47] Tax changes are not always proportional to property values. See the example of Proposition 13 in California, for example.

areas reinforces the extent to which the tax reductions further and do not counteract the progressive distributional effects of state aid for schools. If anything, on the basis of this factor, the tax offset estimates should be even higher—and even further from observed effects—than I estimate.

Third, one may be concerned that the decisions of judges themselves may reflect the electorate's preference for redistribution—that is, there should not be any offset because the electorate itself is deciding to have more redistribution and is choosing school finance as the means. If the electorate wished to use state aid to redistribute to the poor, then we would not expect tax offset, since there is no change from the distributional ideal to offset. In fact, most states do have some sort of election for their state supreme courts.[48] So the electorate's preferences for redistribution could be expressed through the election or retention of judges. Elsewhere, the electorate's preferences for redistribution could also enter through the decision to appoint and confirm state supreme court justices.

While such a concern is reasonable, it is difficult to maintain in light of the evidence. State judges would have to be responding to a change in demand for school finance—potentially even a slow, gradual change—to be responding to the population's desire to increase school spending. Otherwise, there would be no change in school finance desired. However, controlling for changes in the political leanings of the state do not substantially change the results. Nor is there any evidence of a trend in spending prior to a decision. So, to the best of my ability to detect such trends, there is no evidence that courts are responding to a changing desire for school spending or redistribution of taxes and spending toward lower-income groups.

---

[48] *See* AMERICAN BAR ASSOCIATION, FACT SHEET ON JUDICIAL SELECTION METHODS IN THE STATES http://www.americanbar.org/content/dam/aba/migrated/leadership/fact_sheet.authcheckdam.pdf.

In any case, a similar critique could be used against any state supreme court ruling. The question of tax offset is an extremely important one and the goal is to get the best evidence that we can, and I see little reason that—on this score—there are better policies than school finance decisions at state supreme courts to analyze. While court-mandated state aid for schools might be more likely to have a goal of redistribution than a decision aimed at enhancing efficiency, there is little reason that the influence of the electorate would be especially strong for school finance relative to other legal issues.[49] As well, even where there are elections, the intrusion of politics—and therefore the preferences of the electorate for redistribution—into state supreme court opinions is limited by the infrequency of elections, and the tendency to reelect judges, especially in states where the elections are only retention elections so that there is no opponent. And, as described above, the narrow window of timing around these supreme court decisions, which are themselves not apparently driven by any changes in the states (at least in the short run), increases the credibility of the results.

A fourth concern is that one may think that legislatures may not be able to target families with and without families. In the figures and thus far in the discussion, I treat legislatures as if they target households with and without children differentially. I do so because families with children benefit directly from increases in school finance while those without children do not and because, as described in the summary statistics in Tables 1E-1G, state tax codes do in fact tax households with children less than those without children (for example by altering the size of depending exemptions). Of course, if true, that fact alone suggests that changes in the distribution of income through courts do "stick" and are not undone by legislatures, at least as between families with and without children. One could argue that most filers do have children at some point and that, across their lifetimes, it would not matter if the tax code differentiated

[49] I am unaware of any judicial elections that focused on court-ordered state aid for schools.

between those who had more or fewer children. However, such an argument would be ignoring both the variance in the number of children that families have and also differentiation across cohorts: yes, elderly people may have had children at some point, but they will not be benefitting (at least directly) from that spending.

Nevertheless, one might still be curious about results relative to a tax offset estimate that instead assumes that policymakers cannot target households with children. To address this concern, I make the following calculation. To transform per capita spending to per student spending, I multiply the per capita spending amount by 5.99. Next, to transform per student to per household spending, I multiply by the average number of children per household (0.43).[50] So with spending of $911 per student (5.99 * $152) and 0.43 students per household, that means that we expect $391.51 in spending per household and, after a flypaper effect of 63.7%, $249.39 per household. This amount is 1.09% of income for the 20th percentile, 0.21% for the 80th percentile, and 0.12% for the 95th percentile. So the expected change in tax rates would be an increase of 0.88 percentage points more for the 20th percentile than the 80th and an increase of 0.97 percentage points more for the 20th than the 95th percentile. The left-hand-side figures in Figure 4, for those without children, strongly reject these predictions as well, with upper bounds of the confidence intervals well below relative increases of 0.5. And, for filers with children, with noisier estimates, these increases roughly coincide with the upper bound of the confidence interval.

Of course, other complexities of measuring the incidence of the state aid remain. For example, capitalization into housing prices complicates the analysis (Barrow and Rouse 2004, Dee 2000, Wyckoff 1995). I do not assess changes in federal funding (which would, of course be

[50] There were 124.59 million households in 2015. https://www.statista.com/statistics/183635/number-of-households-in-the-us/. And, as noted before, there are 53.66 million students. And 53.66 / 124.59 = 0.43.

difficult to target across states). Nor does the methodology address within-expenditure changes (e.g., redirecting funds away from low-income areas or programs). Additionally, incidence is generally more complicated when funds are directed to local governments, rather than individuals, since even the poorest city has some better-off people. In my estimate of the differential impact among the rich and the poor, I address this concern by assuming that the same amount is spent in the entire state, so almost certainly I am underestimating the expected tax offset.

VI. Implications and Conclusion

This paper documents—for the first time—how states pay for the increases in education expenditure brought about by education finance reform court cases. Using an event study methodology, I identify the substantial increase in state K-12 education spending that results, and show that the court orders are financed using income tax changes that do not target lower-income households and not cuts in spending. I also show that corporate licenses, likely a progressive tax instrument, fund part of the increase in school spending. The results imply that, in this setting, legal changes are effective at changing the distribution of taxes and spending.

More importantly, this Article is the first to offer evidence on whether legislatures offset the distributional consequences of court orders or whether those consequences stick. I offer this evidence in a setting promising for identifying legislative offset for five reasons: (1) the large size of the legal rule change, (2) the estimability of the incidence of the rule change, (3) the ability of legislatures to use income tax code to differentially tax those that most directly benefit from the change, (4) the long time horizon to assess after the rule change, and (5) the plausibly exogenous variation in changes across states and across time. Despite this advantageous setting

for finding offset, I find little evidence of any. I thereby provide empirical evidence that legislatures sometimes fail to offset the distributional consequences of court ordered legal reforms.

Of course, showing that across the states legislatures do not offset the distributional consequences of court orders on school finance does not prove that legislatures never offset distributional consequences. For example, one may think that taxes respond differently to progressive than to regressive legal rule changes. I cannot rule that out, and testing outcomes in other circumstances is a useful item for future research. What can be said now on this foundational question in law and economics is that there is some significant evidence against the notion that taxes offset the distributional consequences of changes in legal rules.

While we need much more evidence on this question of how other policies respond to a court-driven change in the distribution of taxes and spending, the finding is evidence against assuming a kind of "optimal" political economy when conducting economic analysis of of changes in legal rules. When conducting normative analysis to derive optimal legal rules, economic analysis of legal rules tends to focus on the efficiency of rules, as if the distributive consequences of those rules will be offset through changes elsewhere. If those changes are not offset, those distributive consequences matter for welfare analysis. However, these results provide evidence against the assumption that legislatures arrive at an "optimal" distribution of taxes and spending, even over a 25-year time horizon, in the aftermath of a court decision. That is, they suggest that institutions matter when assessing distributive consequences. And they suggest that normative analysis of legal rules, either when assessing the welfare consequences of actual or potential rule changes, might want to consider more complex models of political economy such as those developed by political scientists (Ferejohn and Weingast (1992), Calvert,

McCubbins, and Weingast (1989). McCubbins, Noll, and Weingast (2006)), which could provide guidance on when distributive consequences of court rulings are likely to "stick" and therefore matter for welfare analysis

References

Baicker, Katherine, and Nora Gordon. 2006. "The effect of state education finance reform on total local resources," *Journal of Public Economics*, 90: 1519-1535.

Bai, Jushan. 1997. "Estimation of a Change Point in Multiple Regression Models." *Review of Economics and Statistics* 79(4): 551–63.

Barrow, Lisa, and Cecilia Elena Rouse. 2004. "Using Market Valuation to Assess Public School Spending." *Journal of Public Economics* 88 (9-10): 1747–69.

Briffault, Richard, and Laurie Reynolds. 2009. *Cases and Materials on State and Local Government Law*. West.

Calvert, Randall, Mathew McCubbins, and Barry Weingast. 1989. "A Theory of Political Control and Agency Discretion." *American Journal of Political Science*, January, 588–611.

Card, David, Alexandre Mas, and Jesse Rothstein. 2008. "Tipping and the Dynamics of Segregation." *Quarterly Journal of Economics* 123 (1): 177–218.

Card, David, and A. Abigail Payne. 2002. "School Finance Reform, the Distribution of School Spending, and the Distribution of Student Test Scores." *Journal of Public Economics* 83 (1): 49–82.

Cellini, Stephanie Riegg, Fernando Ferreira, and Jesse Rothstein. 2010. "The Value of School Facility Investments: Evidence from a Dynamic Regression Discontinuity Design." *Quarterly Journal of Economics* 125 (1): 215–61.

Chamberlain, Andrew, and Gerald Prante. 2007. "Who Pays Taxes and Who Receives Government Spending? An Analysis of Federal, State and Local Tax and Spending Distributions, 1991-2004." *Tax Foundation*. https://taxfoundation.org/who-pays-taxes-and-who-receives-government-spending-analysis-federal-state-and-local-tax-and/.

Cooter, Robert, and Thomas Ulen. 2012. *Law & Economics*. Prentice Hall.

Corcoran, Sean, and William Evans. 2015. "Equity, Adequacy, and the Evolving State Role in Education Finance." In *Handbook of Research in Education Finance and Policy*, 2nd Edition. New York: Routledge.

Dee, Thomas S. 2000. "The Capitalization of Education Finance Reforms." *Journal of Law and*

Economics 43 (1): 185–214.

Enrich, Peter. 1995. "Leaving Equality behind: New Directions in School Finance Reform." *Vanderbilt Law Review*, 48:100-194.

Evans, William N., Sheila E. Murray, and Robert M. Schwab. 1997. "Schoolhouses, Courthouses, and Statehouses after Serrano." *Journal of Policy Analysis and Management* 16 (1): 10–31.

Ferejohn, John A., and Barry Weingast. 1992. "A Positive Theory of Statutory Interpretation." *International Review of Law and Economics* 12 (2): 263–79.

Heise, Michael. 1995. "State Constitutions, School Finance Litigation, and the 'Third Wave': From Equity to Adequacy." *Temple Law Review* 68: 1151–75.

Hines, James, and Richard Thaler. 1995. "The Flypaper Effect." *Journal of Economic Perspectives* 9 (4): 217–26.

Hoxby, Caroline M. 2001. "All School Finance Equalizations Are Not Created Equal." *Quarterly Journal of Economics* 116 (4): 1189–1231.

Jackson, C. Kirabo, Rucker C. Johnson, and Claudia Persico. 2016. "The Effects of School Spending on Educational and Economic Outcomes: Evidence from School Finance Reforms." *Quarterly Journal of Economics* 131 (1): 157–218.

Lafortune, Julien, Jesse Rothstein, and Diane Shanzenbach. 2016. "Can School Finance Reforms Improve Student Achievement?" *Institute For Research on Labor and Employment Working Paper* 100-16. http://irle.berkeley.edu/can-school-finance-reforms-improve-student-achievement/.

Liscow, Zachary. 2016. "The Efficiency of Equity in School Finance," Yale Law School Working Paper, https://papers.ssrn.com/sol3/papers.cfm?abstract_id=2551082.

McCubbins, Mathew, Roger Noll, and Barry Weingast. 2006. "Conditions for Judicial Independence." *Journal of Contemporary Legal Issues*, January, 105–28.

McGuinn, Patrick J. 2006. *No Child Left Behind and the Transformation of Federal Education Policy, 1965-2005*. Lawrence, Kan: University Press of Kansas.

Murray, Sheila E., William N. Evans, and Robert M. Schwab. 1998. "Education-Finance Reform and the Distribution of Education Resources." *American Economic Review* 88 (4): 789–812.

O'Sullivan, Joseph. 2014. "Contempt Ruling Ups Ante in Fight to Fund Public Schools." News. *The Seattle Times*. September 12. http://www.seattletimes.com/seattle-news/contempt-ruling-ups-ante-in-fight-to-fund-public-schools/.

Posner, Richard A. 2014. *Economic Analysis of Law*, 9th Edition. New York: Wolters Kluwer Law & Business.

Shavell, Steven. 2004. *Foundations of Economic Analysis of Law*. Cambridge, Mass: Belknap Press.

Wyckoff, Paul. 1995. "Capitalization, Equalization, and Intergovernmental Aid," *Public Finance Quarterly,* 23: 484-508.

Zelinsky, Edward A. 2002. "The Once and Future Property Tax: A Dialogue with My Younger Self." *Cardozo Law Review*, 23:2199.

Table 1A: Summary Statistics of Per-Capita K-12 Education Expenditure and Decision Years

	Mean	S.D.	Min	Max
Education Expenditure	1,551.60	427.44	452.80	4,038.48
Post Decision	0.29	0.45	0.00	1.00
Post Decision * Yrs. Elapsed	3.11	6.12	0.00	25.00
Observations	1716			

Note: Per-Capita K-12 Education Expenditure is shown in 2015 dollars.
Years: 1972, 1977-2014

Table 1B: Summary Statistics of Per-Capita Revenue

	Mean	S.D.	Min	Max
General Revenue	4,377	1,316	2,000	26,297
Total Taxes	2,321	603	979	14,190
Income Taxes	922	585	0	13,384
General Sales Taxes	753	280	0	2,293
Property Taxes	45	90	0	1,649
Observations	1686			

Note: 2015 Dollars. Years: 1977-2014.

Table 1C: Summary Statistics of Per-Capita License Fees

	Mean	S.D.	Min	Max
Total License Fees	150	84	23	1,577
Alcohol License Fees	2	1	0	18
Corporation License Fees	29	57	0	1,118
Hunting License Fees	6	6	0	92
Motor Vehicle License Fees	79	30	0	313
Occupational, Business, and Nontraditional License Fees	28	25	5	431
Observations	1686			

Note: 2015 Dollars. Years: 1977-2014.

Table 1D: Summary Statistics of Per-Capita Expenditure

	Mean	S.D.	Min	Max
Total Expenditure	4,942	1,595	2,180	19,314
Non-Education Total Expenditure	3,390	1,289	1,201	16,232
Construction	273	119	53	1,717
Healthcare	331	135	92	1,097
Total Welfare	1,086	523	198	3,057
State-Level Welfare	416	233	-336	1,388
Higher Education	562	189	168	1,484
Employee Retirement	330	207	0	1,666
Unemployment Benefits	176	115	15	872
Highways	352	125	122	2,310
Total Debt Outstanding	2,577	1,874	110	15,177
Observations	1716			

Note: 2015 Dollars. Unlike other categories, "Total Debt Outstanding" is not an expenditure. Years: 1972, 1977-2014.

Table 1E: Summary Statistics of Tax Rate Variables (Single Filer, Without Children)

	Mean	S.D.	Min	Max
Tax Rate at the 20th Percentile	2.04	1.36	0.00	6.12
Tax Rate at the 80th Percentile	3.97	2.31	0.00	8.78
Tax Rate at the 95th Percentile	4.34	2.62	0.00	10.52
20th Minus 80th Percentile	-1.93	1.80	-6.66	0.13
20th Minus 95th Percentile	-2.30	2.19	-8.28	0.11
Observations	1729			

Years: 1977-2015.

Table 1F: Summary Statistics of Tax Rate Variables (Single Filer, With Children)

	Mean	S.D.	Min	Max
Tax Rate at the 20th Percentile	0.70	1.67	-8.39	4.68
Tax Rate at the 80th Percentile	3.80	2.22	0.00	8.47
Tax Rate at the 95th Percentile	4.24	2.56	0.00	10.16
20th Minus 80th Percentile	-3.10	2.71	-14.03	0.15
20th Minus 95th Percentile	-3.55	3.04	-15.38	0.16
Observations	1729			

Years: 1977-2015.

Table 1G: Summary Statistics of Tax Rate Variables (Single Filer)

	Mean	S.D.	Min	Max
20th Percentile: With Children - Without Children	-1.34	1.78	-11.21	0.52
80th Percentile: With Children - Without Children	-0.17	0.18	-1.15	0.18
95th Percentile: With Children - Without Children	-0.10	0.11	-0.59	0.20
Diff. in Diff. 20th - 80th	-1.17	1.72	-10.66	0.38
Diff. in Diff. 20th - 95th	-1.25	1.75	-10.83	0.43
Observations	1729			

Years: 1977-2015.

Table 1H: Summary Statistics of Political Controls Used in Robustness Checks

	Mean	S.D.	Min	Max
State Presidential Vote	46.03	7.30	19.60	71.80
Composition of State Government	53.29	35.91	0.00	100.00
Entirely Democratic State Government	26.54	44.17	0.00	100.00
Democratic Governor	44.22	49.68	0.00	100.00
Democratic Legislature	50.03	50.01	0.00	100.00
Observations	1716			

Years: 1972, 1977-2014.

Table 2: Effect of Decision on Per-Capita K-12 Education Expenditure

	(1)	(2)
Post Decision	144.10**	151.64***
	(55.11)	(38.85)
Post Decision * Yrs. Elapsed		3.07
		(4.11)
Trend		-2.15
		(4.50)
Observations	1716	1716
R^2	0.884	0.884
Adjusted R^2	0.878	0.878

Note: Variables in real 2015 dollars. Robust standard errors clustered at the state level are displayed in parenthesis. Regressions include state and year fixed effects and are weighted by population. Significance levels: * p<0.10; ** p<0.05; *** p<0.01. Years: 1972, 1977-2014.

Table 3A: Effect of Decision on Per-Capita Revenue

	(1) General Revenue	(2) Total Taxes	(3) Income Taxes	(4) General Sales Taxes	(5) Property Taxes
Post Decision	277.24***	179.50**	133.30*	23.05	17.20
	(88.13)	(81.44)	(78.82)	(27.22)	(15.71)
Observations	1686	1686	1686	1686	1686
R^2	0.950	0.864	0.874	0.913	0.730
Adjusted R^2	0.947	0.856	0.867	0.908	0.716

Note: Variables in real 2015 dollars. Robust standard errors clustered at the state level are displayed in parenthesis. Regressions include state and year fixed effects and are weighted by population. Significance levels: * p<0.10; ** p<0.05; *** p<0.01. Years: 1977-2014.

Table 3B: Effect of Decision on Per-Capita Revenue

	(1) General Revenue	(2) Total Taxes	(3) Income Taxes	(4) General Sales Taxes	(5) Property Taxes
Post Decision	221.40***	176.21**	91.32	46.63*	9.33
	(56.70)	(71.95)	(59.25)	(27.11)	(11.96)
Post Decision * Yrs. Elapsed	-4.92	-0.49	-4.54	2.12	1.12
	(8.74)	(6.39)	(7.34)	(3.01)	(1.13)
Trend	6.75	0.49	5.48	-2.87	0.07
	(8.18)	(5.06)	(5.36)	(2.20)	(1.09)
Observations	1686	1686	1686	1686	1686
R^2	0.950	0.864	0.874	0.913	0.732
Adjusted R^2	0.947	0.856	0.867	0.909	0.717

Note: Variables in real 2015 dollars. Robust standard errors clustered at the state level are displayed in parenthesis. Regressions include state and year fixed effects and are weighted by population. Significance levels: * p<0.10; ** p<0.05; *** p<0.01. Years: 1977-2014.

Table 4A: Effect of Decision on License Fees

	(1) Total	(2) Alcohol	(3) Corporation	(4) Hunting	(5) Motor Vehicle	(6) Occupational, Business, and Nontraditional License Fees
Post Decision	14.52	0.68***	14.57**	0.33	-1.16	-0.62
	(9.46)	(0.23)	(6.26)	(0.30)	(3.54)	(4.82)
Observations	1686	1686	1686	1686	1686	1686
R^2	0.889	0.748	0.876	0.945	0.818	0.806
Adjusted R^2	0.883	0.735	0.870	0.942	0.808	0.795

Note: Variables in real 2015 dollars. Robust standard errors clustered at the state level are displayed in parenthesis. Regressions include state and year fixed effects and are weighted by population. Significance levels: * p<0.10; ** p<0.05; *** p<0.01. Years: 1977-2014.

Table 4B: Effect of Decision on License Fees

	(1) Total	(2) Alcohol	(3) Corporation	(4) Hunting	(5) Motor Vehicle	(6) Occupational, Business, and Nontraditional License Fees
Post Decision	16.68**	0.61***	11.79***	0.49*	3.75	-1.65
	(7.88)	(0.19)	(4.22)	(0.27)	(4.26)	(4.15)
Post Decision * Yrs. Elapsed	-0.39	-0.04**	-1.06	-0.07*	-0.13	0.84*
	(1.09)	(0.02)	(0.65)	(0.04)	(0.54)	(0.48)
Trend	0.02	0.03	0.73	0.02	-0.32	-0.33
	(0.76)	(0.02)	(0.46)	(0.03)	(0.39)	(0.36)
Observations	1686	1686	1686	1686	1686	1686
R^2	0.890	0.752	0.877	0.946	0.821	0.810
Adjusted R^2	0.884	0.738	0.871	0.943	0.811	0.800

Note: Variables in real 2015 dollars. Robust standard errors clustered at the state level are displayed in parenthesis. Regressions include state and year fixed effects and are weighted by population. Significance levels: * p<0.10; ** p<0.05; *** p<0.01. Years: 1977-2014.

Table 5A: Effect of Decision on Per-Capita Expenditure

	(1) Total Expenditure	(2) Non-Education Total Expenditure	(3) Construction	(4) Healthcare	(5) Total Welfare	(6) State-Level Welfare
Post Decision	271.84**	127.74	9.80	35.31	72.79*	-10.48
	(123.42)	(104.81)	(22.59)	(26.68)	(40.28)	(27.42)
Observations	1716	1716	1716	1716	1716	1716
R^2	0.959	0.956	0.706	0.785	0.945	0.854
Adjusted R^2	0.957	0.953	0.690	0.773	0.942	0.846

Note: Variables in real 2015 dollars. Robust standard errors clustered at the state level are displayed in parenthesis. Regressions include state and year fixed effects and are weighted by population. Significance levels: * p<0.10; ** p<0.05; *** p<0.01. Years: 1972, 1977-2014.

Table 5B: Effect of Decision on Per-Capita Expenditure

	(1) Total Expenditure	(2) Non-Education Total Expenditure	(3) Construction	(4) Healthcare	(5) Total Welfare	(6) State-Level Welfare
Post Decision	154.77**	3.14	20.59	7.45	18.08	-6.72
	(68.25)	(56.83)	(19.15)	(20.46)	(43.88)	(17.71)
Post Decision * Yrs. Elapsed	0.54	-2.53	-0.85	-0.23	-4.35	2.91
	(13.36)	(11.64)	(2.35)	(3.01)	(5.03)	(2.51)
Trend	8.64	10.78	-0.39	2.24	6.39	-1.77
	(12.73)	(11.67)	(1.85)	(1.64)	(5.09)	(2.93)
Observations	1716	1716	1716	1716	1716	1716
R^2	0.959	0.956	0.707	0.788	0.946	0.854
Adjusted R^2	0.957	0.954	0.691	0.776	0.943	0.846

Note: Variables in real 2015 dollars. Robust standard errors clustered at the state level are displayed in parenthesis. Regressions include state and year fixed effects and are weighted by population. Significance levels: * p<0.10; ** p<0.05; *** p<0.01. Years: 1972, 1977-2014.

Table 6A: Effect of Decision on Per-Capita Expenditure

	(1) Higher Education	(2) Employee Retirement	(3) Unemployment Benefits	(4) Highways	(5) Total Debt Outstanding
Post Decision	40.10*	-3.05	-2.80	-2.44	219.58
	(21.73)	(29.52)	(11.71)	(22.32)	(371.69)
Observations	1716	1716	1716	1716	1716
R^2	0.906	0.926	0.829	0.762	0.878
Adjusted R^2	0.901	0.922	0.820	0.749	0.871

Note: Variables in real 2015 dollars. Robust standard errors clustered at the state level are displayed in parenthesis. Regressions include state and year fixed effects and are weighted by population. Significance levels: * p<0.10; ** p<0.05; *** p<0.01. Years: 1972, 1977-2014.

Table 6B: Effect of Decision on Per-Capita Expenditure

	(1) Higher Education	(2) Employee Retirement	(3) Unemployment Benefits	(4) Highways	(5) Total Debt Outstanding
Post Decision	41.35**	-11.14	-1.55	14.81	5.91
	(17.33)	(15.93)	(12.88)	(22.21)	(131.22)
Post Decision * Yrs. Elapsed	1.36	-0.52	2.00	0.38	16.75
	(2.35)	(2.19)	(1.48)	(1.73)	(14.97)
Trend	-0.79	0.88	-1.12	-1.51	7.71
	(2.31)	(2.10)	(1.56)	(1.50)	(20.55)
Observations	1716	1716	1716	1716	1716
R^2	0.906	0.926	0.830	0.763	0.879
Adjusted R^2	0.901	0.922	0.821	0.750	0.873

Note: Variables in real 2015 dollars. Robust standard errors clustered at the state level are displayed in parenthesis. Regressions include state and year fixed effects and are weighted by population. Significance levels: * p<0.10; ** p<0.05; *** p<0.01. Years: 1972, 1977-2014.

Table 7A: Effect of Decision on Tax Rates (Single Filer, Without Children)

	(1) Tax Rate at the 20th Percentile	(2) Tax Rate at the 80th Percentile	(3) Tax Rate at the 95th Percentile	(4) 20th Minus 80th Percentile	(5) 20th Minus 95th Percentile
Post Decision	0.114	0.153	0.204	-0.039	-0.090
	(0.123)	(0.143)	(0.177)	(0.112)	(0.158)
Observations	1729	1729	1729	1729	1729
R^2	0.930	0.970	0.967	0.971	0.963
Adjusted R^2	0.927	0.969	0.965	0.969	0.961

Note: Robust standard errors clustered at the state level are displayed in parenthesis. Regressions include state and year fixed effects and are weighted by population. Significance levels: * p<0.10; ** p<0.05; *** p<0.01. Years: 1977-2015.

Table 7B: Effect of Decision on Tax Rates (Single Filer, Without Children)

	(1) Tax Rate at the 20th Percentile	(2) Tax Rate at the 80th Percentile	(3) Tax Rate at the 95th Percentile	(4) 20th Minus 80th Percentile	(5) 20th Minus 95th Percentile
Post Decision	0.146**	0.277**	0.276	-0.131	-0.130
	(0.069)	(0.128)	(0.184)	(0.128)	(0.188)
Post Decision * Yrs. Elapsed	-0.023**	-0.003	-0.013	-0.019	-0.010
	(0.010)	(0.016)	(0.023)	(0.017)	(0.022)
Trend	0.009	-0.008	0.001	0.017	0.008
	(0.011)	(0.012)	(0.012)	(0.014)	(0.012)
Observations	1729	1729	1729	1729	1729
R^2	0.932	0.971	0.967	0.971	0.963
Adjusted R^2	0.928	0.969	0.965	0.969	0.961

Note: Robust standard errors clustered at the state level are displayed in parenthesis. Regressions include state and year fixed effects and are weighted by population. Significance levels: * p<0.10; ** p<0.05; *** p<0.01. Years: 1977-2015.

Table 8A: Effect of Decision on Tax Rates (Single Filer, With Children)

	(1) Tax Rate at the 20th Percentile	(2) Tax Rate at the 80th Percentile	(3) Tax Rate at the 95th Percentile	(4) 20th Minus 80th Percentile	(5) 20th Minus 95th Percentile
Post Decision	-0.072	0.147	0.203	-0.219	-0.276
	(0.445)	(0.138)	(0.170)	(0.451)	(0.436)
Observations	1729	1729	1729	1729	1729
R^2	0.698	0.967	0.966	0.900	0.911
Adjusted R^2	0.681	0.965	0.964	0.894	0.907

Note: Robust standard errors clustered at the state level are displayed in parenthesis. Regressions include state and year fixed effects and are weighted by population. Significance levels: * p<0.10; ** p<0.05; *** p<0.01. Years: 1977-2015.

Table 8B: Effect of Decision on Tax Rates (Single Filer, With Children)

	(1) Tax Rate at the 20th Percentile	(2) Tax Rate at the 80th Percentile	(3) Tax Rate at the 95th Percentile	(4) 20th Minus 80th Percentile	(5) 20th Minus 95th Percentile
Post Decision	0.034	0.313**	0.294	-0.280	-0.261
	(0.244)	(0.142)	(0.188)	(0.240)	(0.267)
Post Decision * Yrs. Elapsed	0.012	-0.016	-0.019	0.028	0.032
	(0.066)	(0.019)	(0.025)	(0.057)	(0.060)
Trend	-0.014	-0.005	0.003	-0.009	-0.017
	(0.058)	(0.012)	(0.011)	(0.051)	(0.053)
Observations	1729	1729	1729	1729	1729
R^2	0.698	0.968	0.966	0.900	0.912
Adjusted R^2	0.681	0.966	0.964	0.895	0.907

Note: Robust standard errors clustered at the state level are displayed in parenthesis. Regressions include state and year fixed effects and are weighted by population. Significance levels: * p<0.10; ** p<0.05; *** p<0.01. Years: 1977-2015.

Table 9A: Effect of Decision on Tax Rates (Single Filer)

	(1) 20th Percentile: With Children Minus Without Children	(2) 80th Percentile: With Children Minus Without Children	(3) 95th Percentile: With Children Minus Without Children	(4) Diff. in Diff. 20th Minus 80th	(5) Diff. in Diff. 20th Minus 95th
Post Decision	-0.187	-0.006	-0.000	-0.181	-0.186
	(0.463)	(0.020)	(0.014)	(0.465)	(0.465)
Observations	1729	1729	1729	1729	1729
R^2	0.760	0.775	0.741	0.750	0.754
Adjusted R^2	0.747	0.762	0.727	0.736	0.741

Note: Robust standard errors clustered at the state level are displayed in parenthesis. Regressions include state and year fixed effects and are weighted by population. Significance levels: * p<0.10; ** p<0.05; *** p<0.01. Years: 1977-2015.

Table 9B: Effect of Decision on Tax Rates (Single Filer)

	(1) 20th Percentile: With Children Minus Without Children	(2) 80th Percentile: With Children Minus Without Children	(3) 95th Percentile: With Children Minus Without Children	(4) Diff. in Diff. 20th Minus 80th	(5) Diff. in Diff. 20th Minus 95th
Post Decision	-0.113	0.036	0.018	-0.149	-0.131
	(0.246)	(0.037)	(0.018)	(0.242)	(0.243)
Post Decision * Yrs. Elapsed	0.035	-0.013**	-0.006**	0.048	0.042
	(0.069)	(0.005)	(0.002)	(0.069)	(0.069)
Trend	-0.023	0.003	0.002	-0.026	-0.025
	(0.060)	(0.002)	(0.001)	(0.061)	(0.061)
Observations	1729	1729	1729	1729	1729
R^2	0.761	0.802	0.760	0.752	0.756
Adjusted R^2	0.748	0.791	0.747	0.739	0.743

Note: Robust standard errors clustered at the state level are displayed in parenthesis. Regressions include state and year fixed effects and are weighted by population. Significance levels: * p<0.10; ** p<0.05; *** p<0.01. Years: 1977-2015.

Table 10A: Robustness Checks: Effect on Per-Capita Education Expenditure

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Post Decision	138.61***	138.72**	131.34***	140.71**	140.50**	142.31**	136.65**
	(51.62)	(52.85)	(48.27)	(53.76)	(53.61)	(54.62)	(52.19)
State Presidential Vote	-3.46		-4.24				
	(3.37)		(3.50)				
Composition of State Government		0.61	0.68				
		(0.52)	(0.51)				
Democratic Governor				0.28			0.41
				(0.26)			(0.30)
Democratic Legislature					0.45		0.56
					(0.37)		(0.42)
Entirely Democratic State Government						0.28	-0.30
						(0.33)	(0.35)
Observations	1716	1716	1716	1716	1716	1716	1716
R^2	0.885	0.886	0.887	0.885	0.885	0.885	0.886
Adjusted R^2	0.879	0.879	0.881	0.879	0.879	0.878	0.880

Note: Variables in real 2015 dollars. Robust standard errors clustered at the state level are displayed in parenthesis. Regressions include state and year fixed effects and are weighted by population. Significance levels: * p<0.10; ** p<0.05; *** p<0.01. Years: 1972, 1977-2014.

Table 10B: Robustness Checks: Effect on Per-Capita Education Expenditure

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Post Decision	147.17***	137.95***	130.85***	141.63***	147.49***	146.27***	137.68***
	(36.77)	(40.25)	(38.36)	(37.42)	(41.02)	(38.06)	(41.16)
Post Decision * Yrs. Elapsed	3.56	2.18	2.66	2.24	3.63	3.16	2.49
	(4.36)	(3.99)	(4.18)	(4.10)	(3.73)	(3.90)	(3.94)
Trend	-2.49	-1.05	-1.33	-1.21	-2.40	-1.92	-1.34
	(4.45)	(4.68)	(4.67)	(4.71)	(4.27)	(4.39)	(4.65)
State Presidential Vote	-3.58		-4.32				
	(3.40)		(3.56)				
Composition of State Government		0.60	0.68				
		(0.54)	(0.53)				
Democratic Governor				0.27			0.39
				(0.27)			(0.32)
Democratic Legislature					0.47		0.56
					(0.38)		(0.43)
Entirely Democratic State Government						0.29	-0.28
						(0.33)	(0.36)
Observations	1716	1716	1716	1716	1716	1716	1716
R^2	0.885	0.886	0.887	0.885	0.885	0.885	0.886
Adjusted R^2	0.879	0.879	0.881	0.879	0.879	0.878	0.880

Note: Variables in real 2015 dollars. Robust standard errors clustered at the state level are displayed in parenthesis. Regressions include state and year fixed effects and are weighted by population. Significance levels: * p<0.10; ** p<0.05; *** p<0.01. Years: 1972, 1977-2014.

Table 11A: Robustness Checks: Effect on Per-Capita Education Expenditure

	(1) Unweighted	(2) 20 Pre-Decision and 30 Post-Decision Years	(3) All Years Included in Regression	(4) Log Education Expenditure
Post Decision	180.025**	139.883**	157.931***	0.095***
	(72.151)	(55.524)	(57.310)	(0.029)
Observations	1716	1811	1940	1716
R^2	0.896	0.888	0.888	0.902
Adjusted R^2	0.891	0.883	0.883	0.897

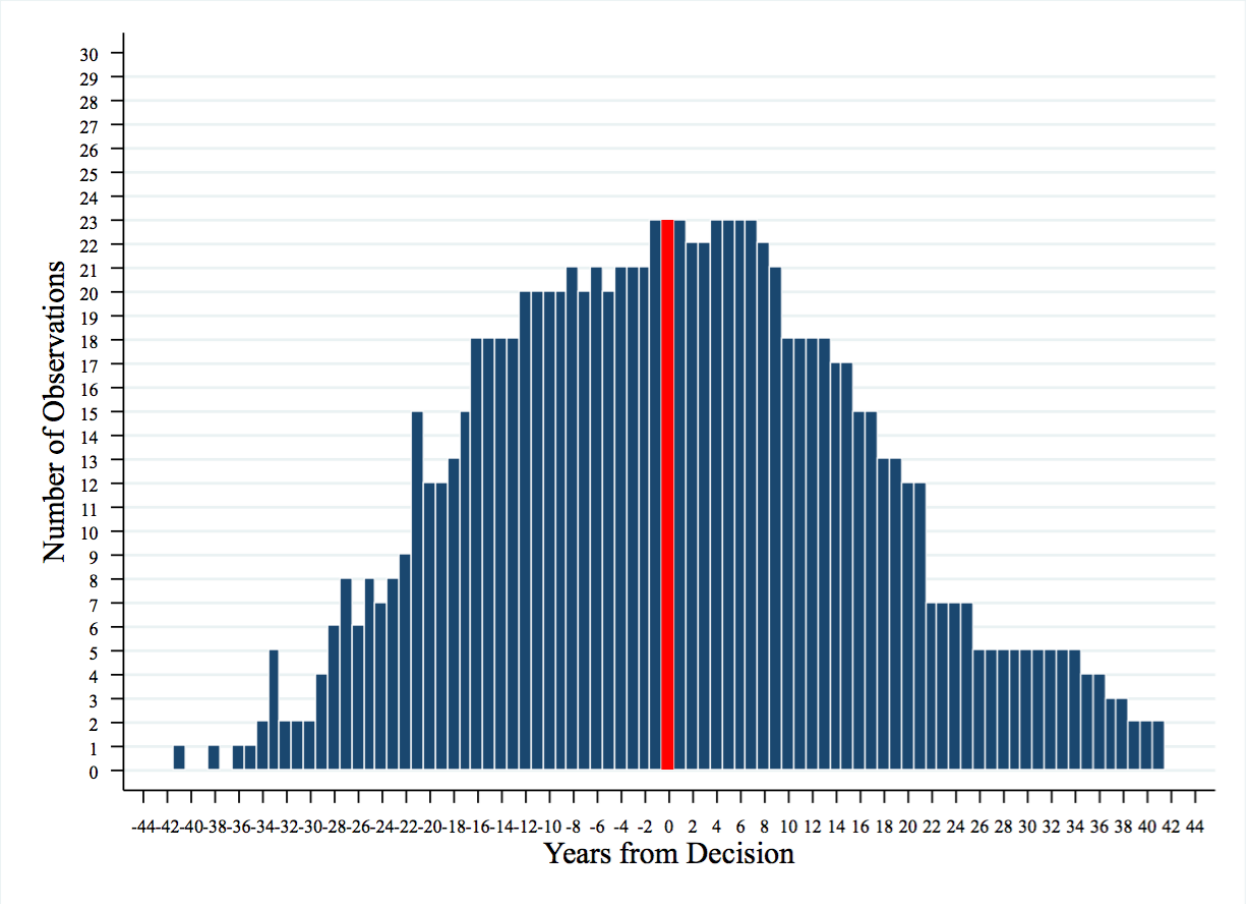
Note: Variables in real 2015 dollars. Robust standard errors clustered at the state level are displayed in parenthesis. Regressions include state and year fixed effects and are weighted by population. Significance levels: * p<0.10; ** p<0.05; *** p<0.01. Years: 1972, 1977-2014.

Table 11B: Robustness Checks: Effect on Per-Capita Education Expenditure

	(1) Unweighted	(2) 20 Pre-Decision and 30 Post-Decision Years	(3) All Years Included in Regression	(4) Log Education Expenditure
Post Decision	190.179***	138.858***	182.696***	0.101***
	(53.482)	(37.279)	(53.786)	(0.028)
Post Decision * Yrs. Elapsed	3.689	3.832	1.864	0.000
	(5.372)	(3.659)	(3.835)	(0.003)
Trend	-2.737	-1.594	-2.650	-0.001
	(4.384)	(4.510)	(4.432)	(0.002)
Observations	1716	1811	1940	1716
R^2	0.896	0.889	0.889	0.902
Adjusted R^2	0.891	0.883	0.884	0.897

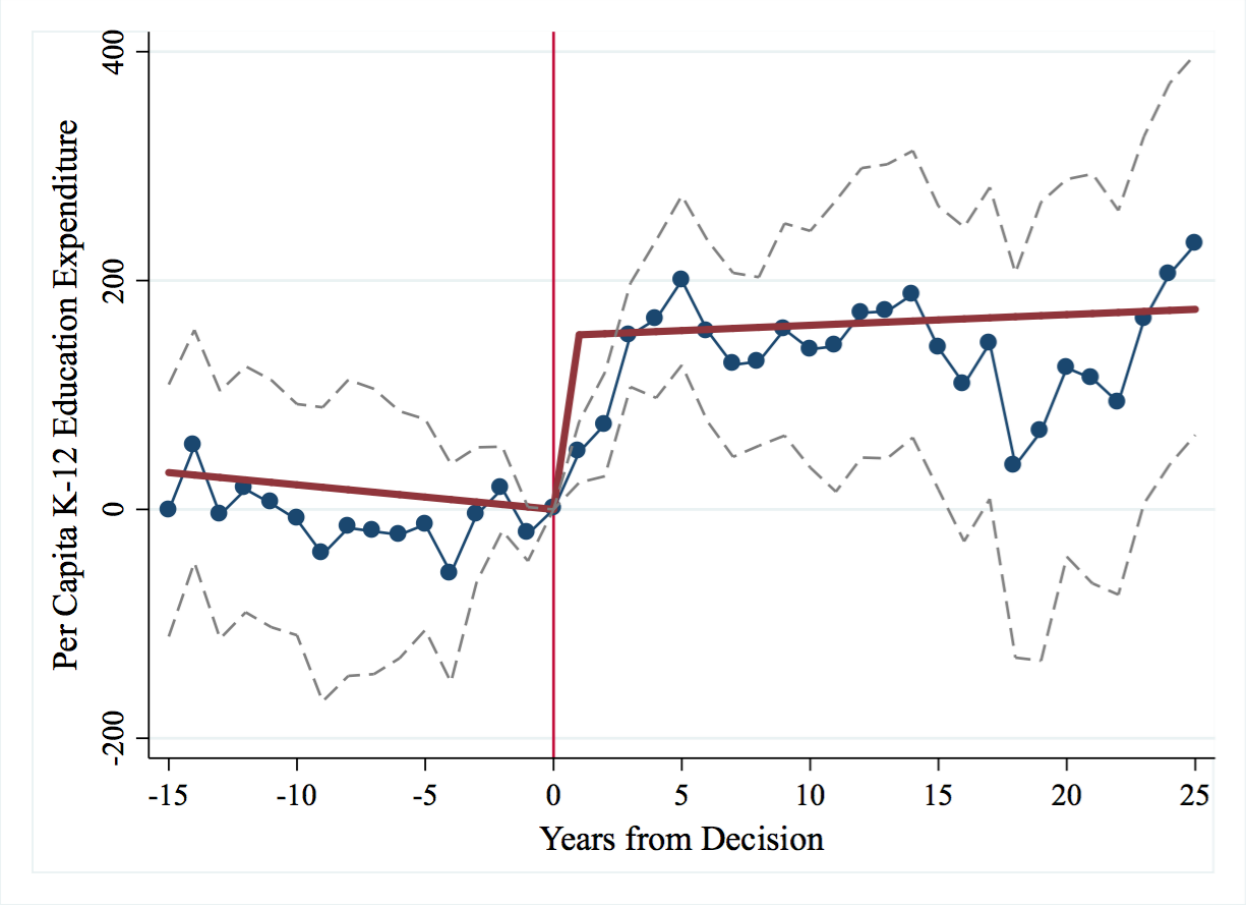
Note: Variables in real 2015 dollars. Robust standard errors clustered at the state level are displayed in parenthesis. Regressions include state and year fixed effects and are weighted by population. Significance levels: * p<0.10; ** p<0.05; *** p<0.01. Years: 1972, 1977-2014.

Figure 1: Distribution of Years in Relation to Decisions



Note: Observations zero years from the decision are shown in red, and total 23 observations. Years: 1972, 1977-2014.

Figure 2: Effect of Decision on Per-Capita K-12 Education Expenditure



Note: The solid red line shows the estimates from the trend specification (equation 2). The blue line with dots shows the nonparametric specification (equation 3). 90% confidence intervals for the nonparametric regression are shown in gray dashes. Regressions include state and year fixed effects and are weighted by population. Per-capita education expenditure is measured in 2015 dollars. Years: 1972, 1977-2014.

Figure 3: Effect of Decision on Average Tax Rates of Single Filers

20th Percentile Income Earners

Figure 3A: Without Children

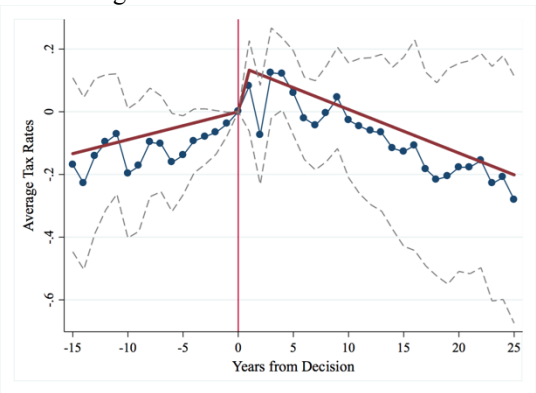
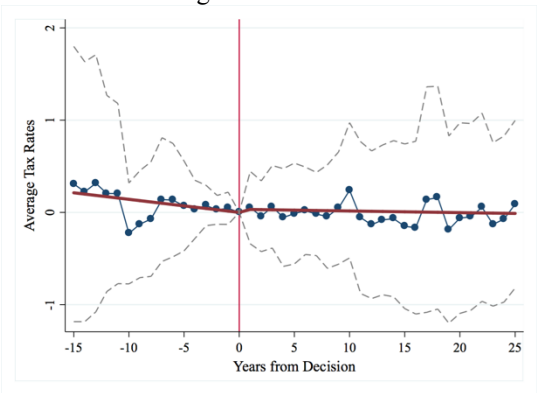


Figure 3B: With Children



80th Percentile Income Earners

Figure 3C: Without Children

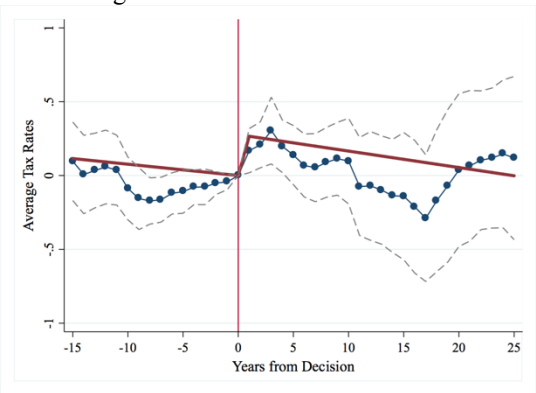
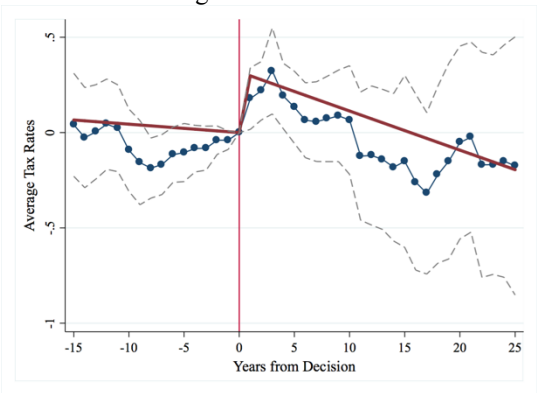


Figure 3D: With Children



95th Percentile Income Earners

Figure 3E: Without Children

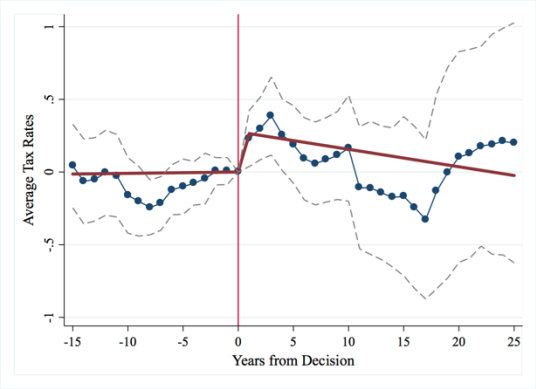
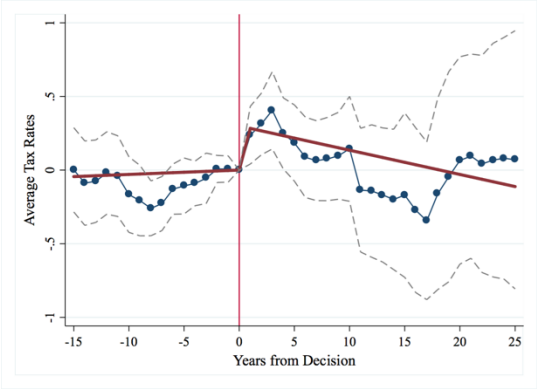


Figure 3F: With Children



Note: The solid red line shows the estimates from the trend specification (equation 2). The blue line with dots shows the nonparametric specification (equation 3). 90% confidence intervals for the nonparametric regression are shown in gray dashes. Regressions include state and year fixed effects and are weighted by population. Years: 1977-2015.

Figure 4: Effect of Decision on Differences in Average Tax Rates of Single Filers

20th Minus 80th Percentile Income Earners

Figure 4A: Without Children

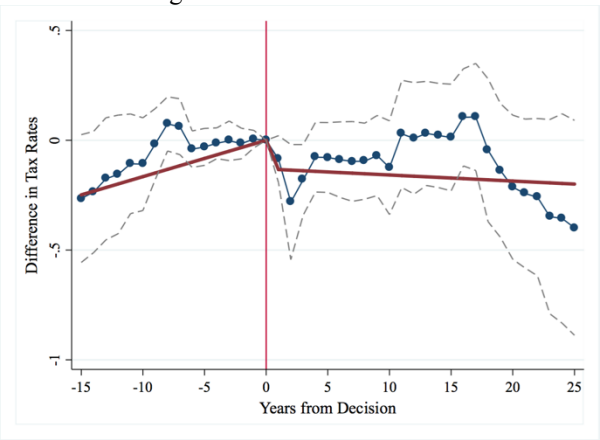
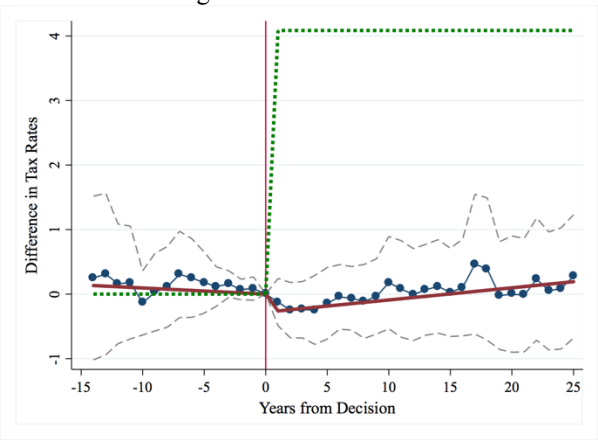


Figure 4B: With Children



20th Minus 95th Percentile Income Earners

Figure 4C: Without Children

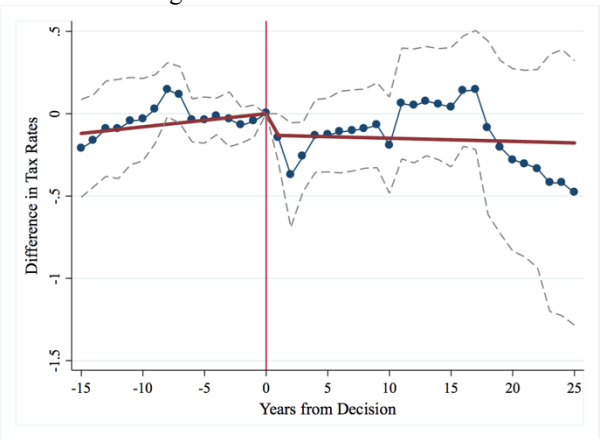
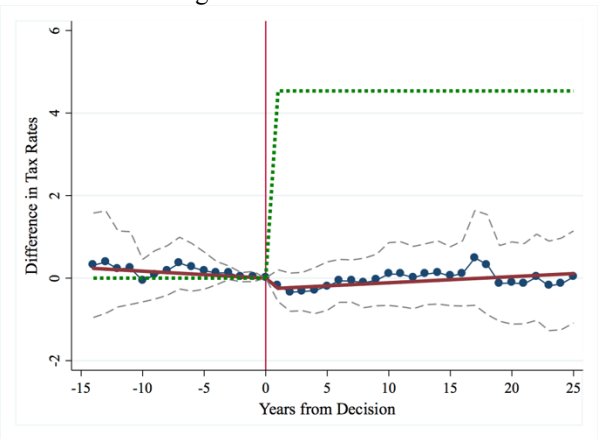


Figure 4D: With Children



Note: The solid red line shows the estimates from the trend specification (equation 2). The blue line with dots shows the nonparametric specification (equation 3). 90% confidence intervals for the nonparametric regression are shown in gray dashes. The dotted green line is an estimate of the amount by which taxes would have to change to offset the distributional impact of the school spending. Regressions include state and year fixed effects and are weighted by population. Years: 1977-2015.

Figure 5: Effect of Decision on Differences in Average Tax Rates Between Single Filers With and Without Children
(With Children Minus Without Children)

Figure 5A: 20th Percentile Income Earners

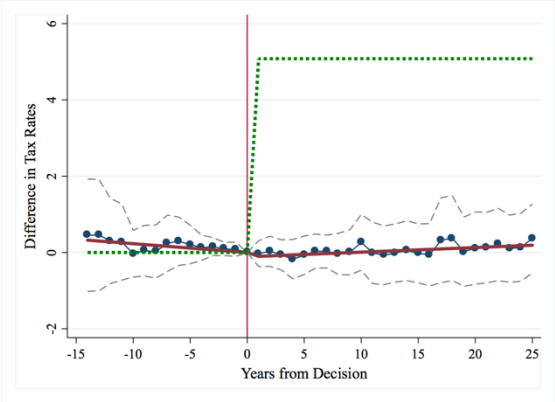


Figure 5B: 80th Percentile Income Earners

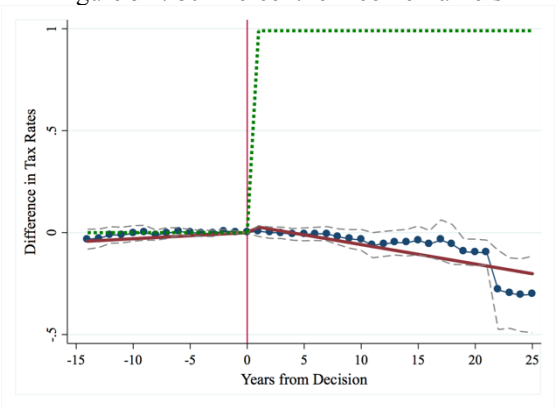
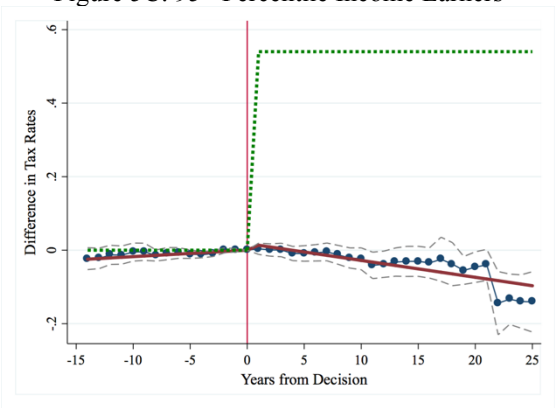


Figure 5C: 95th Percentile Income Earners

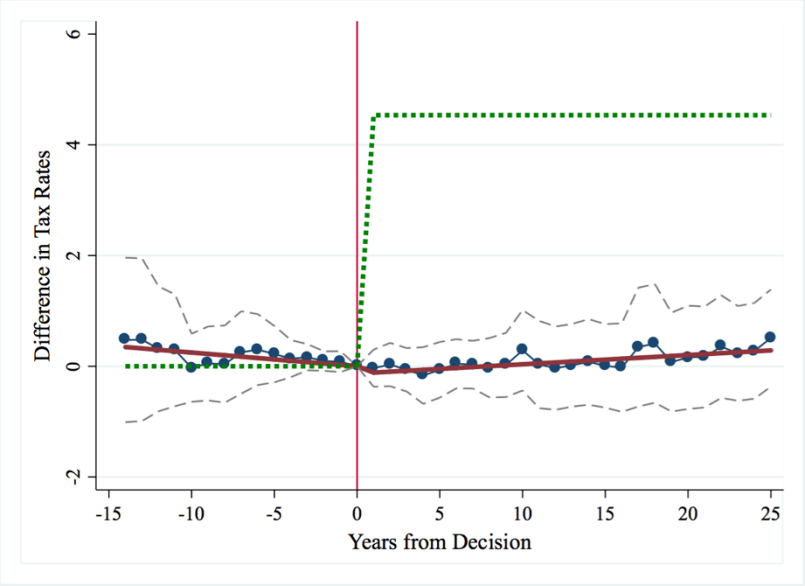


Note: The solid red line shows the estimates from the trend specification (equation 2). The blue line with dots shows the nonparametric specification (equation 3). 90% confidence intervals for the nonparametric regression are shown in gray dashes. The dotted green line is an estimate of the amount by which taxes would have to change to offset the distributional impact of the school spending. Regressions include state and year fixed effects and are weighted by population. Years: 1977-2015.

Figure 6: Effect of Decision on Difference in Difference for Single Filers

Figure 6A: 20th Minus 80th Percentile Income Earners, With Children Minus Without Children

Figure 6B: 20th Minus 95th Percentile Income Earners, With Children Minus Without Children



Note: The solid red line shows the estimates from the trend specification (equation 2). The blue line with dots shows the nonparametric specification (equation 3). 90% confidence intervals for the nonparametric regression are shown in gray dashes. The dotted green line is an estimate of the amount by which taxes would have to change to offset the distributional impact of the school spending. Regressions include state and year fixed effects and are weighted by population. Years: 1977-2015.