# Do Court Mandates Change the Distribution of Taxes and Spending?:

# Evidence from School Finance Litigation

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Abstract: I use an event-study methodology to show how state school financing has responded to court orders to increase funding for schools. I find that states respond by spending $132 per capita more per year on schools in the immediate aftermath. This appears to be financed entirely through increases in taxes. State income tax changes appear to be broad-based across the income distribution and do not appear to target tax filers with children.

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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.[2] But no paper has systematically studied across the country how much state funding for education increased as a result of the court orders or how state legislatures have paid for it.[3] This paper uses an event study methodology to measure how legislatures pay for these mandates, measuring the distributional consequences of the decisions to finance increased expenditures on education.

The answers to these questions matter for at least two reasons. First, how legislatures choose to pay for increased funding on education matters intrinsically. 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 sales taxes, regressive income tax changes, or reductions in other forms of spending that benefit the poor, then the opposite is true.

Second, this paper is the first rigorous test of a key assumption in the economic analysis of law, which tends to assume, implicitly or explicitly, that the distributional consequences of changes in legal rules are offset with changes in taxes and transfers. The reason is simple:

[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] I provide further discussion in a separate paper, *Do Court Orders Matter? The Consequence of School Finance Litigation*, p. 9-13.

[3] Others have studied the impact on local educational expenditures, but not state expenditures, and no one has studied how the state aid was financed. See especially Jackson, Johnson, and Persico (2016).

normative economic analysis of the law usually ignores distributional consequences in making policy recommendations, justified by the argument that taxes and transfers should address distributional concerns.[4] And in recommending laws ignoring distributional concerns, usually based on maximizing efficiency, economic analysis therefore de facto assumes that taxes and transfers *do* address distributional concerns. Otherwise, efficient laws would not in general be optimal. Though whether taxes and transfers do, in fact, respond to offset the distributional effects of changes in legal rules is a key assumption underlying normative analysis in law and economics, surprisingly it is one with little solid empirical work to support it. Here, I provide the first empirical test of what I call *tax-offset assumption*—that the distributional consequences of changes in legal rules are offset through changes in taxes, transfers, or other policies.

Several criteria make for a good test. First, the change in legal rules should be big, so that an offsetting change in taxes should be detectable empirically and so that legislatures would have reason to overcome inertia to enact offsetting taxes. Second, there must be some kind of plausibly exogenous variation in the legal rule across space, time, persons, or otherwise, to exploit econometrically. Third, it should be relatively clear what the economic incidence of the change in legal rule is (i.e., who benefits across the income distribution), so that we have some idea of what the expected change in taxes should be.

School finance redistribution meets the three criteria I laid out. First, the changes are big—very big. A recent analysis of these court orders finds that poor areas received an extra

[4] For commonly used textbooks taking this view, see, for example, RICHARD A. POSNER, ECONOMIC ANALYSIS OF LAW (15-20) (9th ed. 2014) [hereinafter "POSNER, EAL"]; STEVEN SHAVELL, FOUNDATIONS OF ECONOMIC ANALYSIS OF LAW 2-3 (2004) (describing social welfare as the normative basis for analysis in law and economics, but then restricting attention to efficiency by excluding analysis on the distribution of utilities) [hereinafter "SHAVELL, FEAL"]; ROBERT COOTER & THOMAS ULEN, LAW & ECONOMICS 7-8 (6th ed. 2012) (saying that the book "will focus on efficiency rather than distribution" in analyzing the law because of the availability of the tax system for redistribution). Of course, law and economics long precedes the work of Richard Posner. See, for example, the work of Coase in the 1950s and John Commons in the 1920s.

$1,063 per student in spending on education in the aftermath of a school finance decision.[5] Given that the average income of households in those areas is $35,212, and the ratio of households to children is 2.3[6], these changes amounted to an average change in spending of 1.3 percent for households in those areas, a huge increase for one program.

Second, there is plausibly exogenous variation across space and time. 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.

Third, as previously alluded to, there has been work already done measuring the incidence of the school finance decisions—unsurprising, given the hundreds of billions of dollars involved. So, seeing whether the change in taxes matches with the change in spending is relatively easy, at least by the standards of measuring the incidence of changes in legal rules.

Before moving on though I need to address something that puzzles some readers. It may seem strange to study an *increase* in redistribution as a test of the tax offset assumption. However, for the purpose of testing the tax offset assumption, it does not matter which direction the tax offsets are required.[7] If the poor are better-off, then they should be less in need of transfers through the tax system, and taxes should respond. Also, one could view state supreme courts as part of the mechanism for achieving an optimal distribution of taxes and spending, but

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[5] C. Kirabo Jackson, Rucker Johnson & Claudia Persico, *The Effect of School Finance Reforms on the Distribution of Spending, Academic Achievement, and Adult Outcomes*, Q. J. ECON. (forthcoming).

[6] See FEDERAL INTERAGENCY FORUM ON CHILD AND FAMILY STATISTICS, AMERICA'S CHILDREN: KEY NATIONAL INDICATORS OF WELL-BEING (2015) http://www.childstats.gov/americaschildren/tables.asp and STATISTA http://www.statista.com/statistics/183635/number-of-households-in-the-us/.

[7] This is not to say that the results might be different for a different type of legal change. I return to this issue below.

doing so would concede the point that legal rules should take into account distributive effects owing to failures of the legislature to achieve an optimal distribution. That is, it may be reasonable to expect no change in taxes, since legislators may recognize the failure of the tax offset assumption, so when courts accomplish some redistribution on their own, legislators may be happy to let that be rather than enact an offset. But, that very expectation involves a failure of the tax offset assumption.

The results of the paper are as follows. I find that there is no tax offset. To the contrary, I find that school finance reform does indeed lead to increases in school financing, which are financed by tax increases. There is no evidence that the tax increases target the beneficiaries of the school spending and little evidence of other forms of spending declining in response to the decisions.

## I. Background: School Finance Reform in the United States

Schools have long been primarily the responsibility of local governments, though some state and federal involvement is longstanding.[8] For example, the federal Land Ordinance Act of 1795 and the Northwest Ordinances of 1787 set aside funds for school construction.[9] Federal involvement expanded substantially as part of President Lyndon Johnson's "War on Poverty" with the passage in 1965 of the Elementary and Secondary Education Act, which provided funding to schools with disadvantaged students.[10] As a result, the federal share of education spending increased from less than 3 percent in 1958 to about 10 percent in 1968.[11] In most states

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[8] PATRICK J. MCGUINN, NO CHILD LEFT BEHIND AND THE TRANSFORMATION OF FEDERAL EDUCATION POLICY, 1965-2005 25 (2006).
[9] *Id*. at 26.
[10] Pub. L. No. 89-10, 79 Stat. 27 (codified as amended in 20 U.S.C. § 70).
[11] MCGUINN, at 33.

prior to the 1970s, the vast majority of funding for schools came from property taxes raised at the local level.[12]

The dominance of local governments in local school finance began to change in the beginning of the 1970s prompted by litigation in the courts.[13] In the subsequent decades, most state supreme courts began mandating some kind of school finance equalization. Legal scholars have divided this litigation from the 1970s through today into three phases.[14] The first phase included arguments based on the federal Equal Protection Clause. In the path breaking 1971 case *Serrano v. Priest*,[15] the California Supreme Court held that the state's local financing of schools violated the California and U.S. Constitutions' equal protection clauses.[16]

The U.S. Supreme Court could have chosen to follow *Serrano* and require more equalization of funding across school districts as a federal Constitutional matter, but in the 1973 case *San Antonio Independent School District v. Rodriguez* it opted not to do that.[17] Education reformers had hoped that the Court would rule in their favor given the favorable language in

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12 C. Kirabo Jackson, Rucker Johnson & Claudia Persico, *The Effect of School Finance Reforms on the Distribution of Spending, Academic Achievement, and Adult Outcomes* 1 (Nat'l Bureau of Econ. Research, Working Paper No. 20118, 2014).

13 Jackson et al.

14 Heise

15 487 P.2d 1241 (Cal. 1971).

16 After the state legislature acted to equalize education funding, state voters passed Proposition 13, which limited the local property tax to 1% of property value, among other changes including those affecting how property was valued. CAL. CONST art. 13A. Some argue that *Serrano* caused Proposition 13, by reducing the amount that a locality benefited from paying its local taxes. *See* William Fischel, *How Serrano Caused Proposition 13*, 12 J.L. & POL. 607 (1996). *But see* Kirk Stark & Jonathan Zasloff, *Tiebout and Tax Revolts: Did Serrano Really Cause Proposition 13?*, 50 UCLA L. REV. 801 (2003) (challenging the claim of Fischel by offering a different empirical assessment); William Fischel, *Did John Serrano Vote for Proposition 13? A Reply to Stark and Zasloff's "Tiebout and Tax Revolts: Did Serrano Really Cause Proposition 13?"*, 51 UCLA L. REV. 887 (2004) (defending the original proposition that *Serrano* caused Proposition 13). For an excellent overview of Proposition 13, see ARTHUR O'SULLIVAN, TERRI A. SEXTON & STEVEN M. SHEFFRIN, PROPERTY TAXES AND TAX REVOLTS: THE LEGACY OF PROPOSITION 13 (2007). For a doctrinal critique of *Serrano*, see Stephen R. Goldstein, *Interdistrict Inequalities in School Financing: A Critical Analysis of* Serrano v. Priest *and Its Progeny*, 120 PENN. L. REV. 504 (1972). As a result, the endogenous political response to school finance equalization has served largely to limit school funding. I combine school finance reforms across the country in my empirical analysis, both those like California and those unlike it. I also focus on the school finance reform of Connecticut, which does not exhibit the problems that California's has; in particular, if towns in Connecticut choose to spend more on education, they keep the full amount of increase in tax revenue for their own locality.

17 411 U.S. 1 (1973).

cases like *Brown v. Board of Education*, which declared that the "opportunity [of an education] . . . is a right which must be made available to all on equal terms."[18] Over vigorous dissents arguing that the "fundamental rights" to education seemingly guaranteed by previous decisions were not instantiated by the Court, the majority found that the poor do not constitute a suspect class that would trigger the strict scrutiny test under the Equal Protection clause of the U.S. Constitution. Instead, echoing the reasoning of Tiebout, the Court found that local control of schools constitutes a rational reason to maintain local financing, despite the great disparities in taxable property across jurisdictions.

That decision left to the states the issue of school finance equalization, thereby generating most of the variation in funding between states that I exploit in this Article. The cases based only on state constitutional claims constitute the next two waves. In the second wave, "equity" theory cases were argued based on equal protection clauses in state constitutions. These cases tended to focus on spending disparities and input measures like per-pupil spending.[19] The first post-*Rodriguez* state school finance case was *Robinson v. Cahill*[20] in New Jersey. Partly because of doctrinal difficulties in defining what "equal" meant, some scholars argue that these cases have had limited success.[21] Typically, cases required "substantial" equality rather than full equality, perhaps bowing to the reality that equality would be very difficult to achieve.[22]

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[18] 347 U.S. 483, 484 (1954).

[19] Heise, at 1153. After 1983, the cases were aided by the publication *of A Nation at Risk: The Imperative for Educational Reform*, which helped alert Americans to the need for education reform. THE NAT'L COMM'N ON EXCELLENCE IN EDUC., A NATION AT RISK: THE IMPERATIVE FOR EDUCATIONAL REFORM (1983) (criticizing the quality of the American educational system and offering recommendations for improvement).

[20] 303 A.2d 273 (N.J. 1973), *cert. denied*, 414 U.S. 976 (1973).

[21] Heise, at 1162. *See also* JAMES E. RYAN, FIVE MILES AWAY, A WORLD APART 157 (2010) (arguing that school finance litigation has had limited impacts on outcomes overall). *But see* Jackson et al., (showing improvements in educational outcomes and increases in earnings as a result of increases in education spending).

[22] RYAN, at 150.

In the third wave, "adequacy" cases were argued based on the fact that all state constitutions require the state to provide some level of education for children.[23] These decisions challenge not the spending itself, but rather the quality of the education—for not meeting an adequate threshold of quality required by the constitution.[24] For example, in *Rose v. Council for Better Education*,[25] the Kentucky Supreme Court decided what was arguably the first of the adequacy cases,[26] declaring that, on the basis of "adequate national standards," even Kentucky's relatively rich school districts required more funding.[27] This litigation continues to today, with the Washington Supreme Court recently finding the legislature of the state in contempt for not funding schools adequately as required in previous litigation.[28]

As suggested by the Washington state decision, school finance reform has involved a complicated interplay between the courts, seeking to interpret state constitutions, and legislatures, seeking to stave off unfavorable judicial rulings and implement new judicial mandates. For example, by 2009, the New Jersey Court had issued 23 opinions[29] since it first invalidated the state's school finance system in 1973 in *Robinson v. Cahill*.[30] Similarly, Texas's scheme has recently again been declared unconstitutional.[31] As noted by James Ryan, "in no state has one trip to the courthouse been enough to secure long-term relief."[32]

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[23] RICHARD BRIFFAULT & LAURIE REYNOLDS, CASES AND MATERIALS ON STATE AND LOCAL GOVERNMENT LAW 521 (7th ed. 2009). *See also* Peter Enrich, *Leaving Equality Behind: New Directions in School Finance Reform*, 48 VAND. L. REV. 100 (1995) (for an argument in favor of shifting to an adequacy-based approach instead of an equity-based approach, which Enrich argues had proven inadequate).

[24] Some question the strict dichotomy between equity and adequacy cases. *See* RYAN, at 150-51 (arguing that equity cases have adequacy elements and vice versa).

[25] 790 S.W.2d 186 (Ky. 1989).

[26] Heise, at 1163.

[27] 790 S.W.2d at 198.

[28] McCleary v. State, No. 84362-7, at 4-5 (Wash. Sept. 11, 2014) (order holding state legislature in contempt). *See also* Joseph O'Sullivan, *Contempt Ruling Ups Ante in Fight to Fund Public Schools*, SEATTLE TIMES, Sept. 11, 2014, http://seattletimes.com/html/localnews/2024518538_mccclearyorderxml.html.

[29] BRIFFAULT & REYNOLDS, at 515.

[30] 303 A.2d 273 (N.J. 1973).

[31] Texas Taxpayer & Student Fairness Coalition v. Williams, No. D–1–GN–11–003130 (200th Dist. Ct., Travis County, Tex., Aug. 28, 2014). *See also* Terrence Stutz, *Texas' School Finance System Again Overturned in Court*,

These school finance schemes have taken various forms.[33] Although there are a variety of schemes for categorizing the reforms, a recent paper by the economists Kirabo Jackson, Rucker Johnson, and Claudia Persico divide school finance schemes into five non-mutually-exclusive categories.[34] First, foundation plans establish a certain amount of funding, determine how much localities must provide based on local income and wealth, and distribute the difference as state aid. Second, flat grants provide a similar per student grant to all school districts. Third, equalization plans provide more aid to districts with lower incomes (categorical aid) or property values (power equalization plans). Fourth are "reward for effort plans," which provide more aid when districts enact higher tax rates, typically with a greater reward for poorer districts. Finally, some states imposed a spending limit on how much a district could spend, potentially recapturing amounts in excess of the spending limit.[35] A key feature is that the plans have tended to increase state funding of schools.

## II. Data

I use four sources of data. The first is a dataset of years of major state supreme court holdings, which constitute the "event" in the event study; I construct this dataset myself.[36] The

DALLAS MORNING NEWS, Aug. 28, 2014, http://www.dallasnews.com/news/education/headlines/20140828-texas-school-finance-system-again-overturned-in-court.ece.

32 RYAN, at 175.

33 Several potential avenues were not adopted by courts. Despite early expectations that school finance litigation would lead to a prohibition on using the local property tax for funding public schools, no court has required that remedy. *See* Linda Greenhouse, *Enthusiasm Is Waning for Proposals to Reform Property Taxes*, N.Y. TIMES, Dec. 19, 1972, at A1 (describing school finance litigation before the Supreme Court as raising the question whether the property tax is a constitutional source of income for public schools); RYAN, at 174. As well, "no court has required legislatures to . . . change boundary lines so that districts have roughly equal property wealth." *Id*. at 174.

34 *See* Jackson et al.

35 Texas, Kansas, and Vermont use such plans, and they have been very controversial. *See* RYAN, at 154-55.

36 In creating the dataset, I reference Kirabo C. Jackson, Rucker C. Johnson, and Claudia Persico, *The Effects of School Spending on Educational and Economic Outcomes: Evidence from School Finance Reforms*, Working Paper No. 20847. National Bureau of Economic Research (2016) and Julien Lafortune, Jesse Rothstein, and Diane Whitmore Schanzenbach, *School Finance Reform and the Distribution of Student Achievement.* IRLE Working Paper No. 100-16 (2016).

second is U.S. Census data on the income distribution across time.[37] Third, I use the National Bureau of Economic Research's TAXSIM program[38] and the U.S. Census[39] data on income distribution to create a dataset of yearly state income tax rates from 1977 to 2010.[40] With TAXSIM, a program for calculating taxes, I consider a representative unmarried individual without children taking the standard deduction and receiving income only in the form of wages. I create these tax rates for the 20th, 80th, and 95th percentiles in the national income distribution. I produce the average (not the marginal) tax rate, which is the relevant statistic of distributional concerns.[41] Fourth, I use the Annual Survey of Governments from the Census Bureau, which has annual data from 1972[42] and 1977 to 2013 for all state government revenues and expenditures, including breakdowns by type.[43]

Table 1 presents summary statistics of the key variables. Table 1A shows the summary statistics for annual total educational expenditure,[44] divided by state population and inflated to 2015 prices like all other revenue and expenditure numbers using the Bureau of Labor Statistics' CPI-U Index. States spent an average of $1,627 per year per resident on K-12 education in 1972 and 1977 to 2013. (In these summary statistics, as in the regressions, I exclude data on Alaska prior to 1986 due to highly anomalous data.) The Appendix shows some illustrative figures showing per capita educational spending across time, with a line indicating a major state

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[37] Income percentiles come from U.S. CENSUS, HISTORICAL INCOME TABLES: INCOME INEQUALITY, tbl. H-1, https://www.census.gov/hhes/www/income/data/historical/inequality/. Income percentile data come from the Current Population Survey.

[38] NBER, TAXSIM, http://users.nber.org/~taxsim/.

[39] U.S. CENSUS, https://www.census.gov/hhes/www/income/data/historical/household/.

[40] The years 1977 and 2010 are the earliest and latest, respectively, that Stata TAXSIM is available for state income taxes.

[41] What matters for people 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.

[42] For revenue variables, 1972 data are often missing, so I exclude 1972 for those years.

[43] *Annual Survey of Governments*.

[44] 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.

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 $5,617 per person, roughly half of which ($2,379) comes from taxes (and most of the rest of which comes from intergovernmental transfers from the federal government). A little more than one third of state taxes comes from income taxes ($902 per capita), and a little more than one third of state taxes comes from sales taxes ($719 per capita). A small amount ($61 per capita) comes from property taxes.

Table 1C describes the expenditure structure of states, with roughly equivalent revenue and expenditure. States have average expenditure roughly equal to total taxes. Of that expenditure, $3,552 is on items other than K-12 education. States spend $1,000 per capita on "welfare," which means Medicaid, Temporary Assistance for Needy Families and its predecessor, and Supplemental Security Income; of that $365 is the state contribution. States also spend $327 on health care other than Medicaid, including state-run hospitals and community health centers. States also spend $614 per person on higher education, $317 on employee retirement, $169 on unemployment benefits, and $440 on highways. Across all categories of expenditures, states spend an average of $344 per capita on construction. States on average have $3,025 of debt outstanding.

Table 1D – 1F present summary statistics on the structure of state income taxes. Table 1D has the average state income tax rates for single tax filers without children. The average rate is 2.03% at the $20^{th}$ income percentile, 4.11% at the $80^{th}$ percentile, and 4.52% at the $95^{th}$ percentile, leading to average differences between the $20^{th}$ and $80^{th}$ income percentiles of 2.08

percentage points and between the 20th and 95th income percentiles of 2.49 percentage points. Table 1E has analogous rates for those with children, where the structure is substantially more progressive, with an average tax rate of 0.76% at the 20th percentile, 3.89% at the 80th percentile, and 4.39% at 95th percentile, leading to average differences between the 20th and 80th percentiles of 3.14 percentage point and between the 20th and 95th percentiles of 3.64 percentage points.

Table 1F shows the differences at given percentiles between those with and without children: at the 20th, 80th, and 95th percentiles, those with children have a 1.27, 0.22, and 0.12 percentage point lower average income tax rate, respectively.

On average taxes at the 20th percentile of the national income distribution are 2.08 percentage points lower than at the 80th percentile and 2.49 percentage points lower than at the 95th percentile. The progressivity of state income taxes varies widely, from the 20th percentile paying 6.66 percentage points less than the 80th percentile and 8.45 percentage points less than the 95th percentile. These differences reflect states' progressive income tax structure, with an average income tax rate of 2.03% at the 20th percentile, 4.11% at the 80th percentile, and 4.52% at the 95th percentile. I also present 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. The difference in difference is 1.06 percentage points lower at the 80th percentile than the 20th percentile and 1.15 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. In particular, I measure how the difference between state income taxes on the poor and the rich vary by the number of years from a state supreme court opinion, controlling for state and year fixed effects. 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 $\theta_{it}$ is the outcome—typically either per capita spending or revenue or a measure of average tax rates. The main coefficient of interest is $\beta^{jump}$, which measures how much the outcome increases 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 have 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 decsion ($\beta^{phasein}$) when compared to the underlying trend ($\beta^{trend}$). I refer to this specification as specification (2) or the "trend" specification.

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. In my default specification, 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, though I also include more data in robustness checks.) So, for example for $r = k_{min} = -15$, the coefficient $\beta_{-15}$ 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).

I use all data on states between 1977 and 2013. Also, for expenditure data, I include data from 1972; that year is not available for TAXSIM and the revenue data is not consistently available for that year, so I exclude it. If the tax-offset assumption is correct, we should expect to see a positive $\beta^{jump}$, as 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. Similarly, a positive $\beta^{jump}$ for the revenue variables would indicate that the increase in school finance is funded through that form of revenue, and a negative $\beta^{jump}$ for expenditure variables would indicate that a decrease in that form of spending funds the increase in education spending.

Several states have multiple important decisions. For these states, I follow Lafortune, Rothstein, and Schanzenbach (2016)[45] and Card, Mas, and Rothstein (2008)[46] and use the data itself to inform us when the most pivotal event is. In particular, for each state and each decision-year possibility, I run the regression:

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

[45] Julien Lafortune, Jesse Rothstein, and Diane Whitmore Schanzenbach, *School Finance Reform and the Distribution of Student Achievement.* IRLE Working Paper No. 100-16 (2016).

[46] David Card, Alexandre Mas, and Jesse Rothstein, *Tipping and the Dynamics of Segregation.* The Quarterly Journal of Economics 123, no. 1 (2008).

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)[47], and kappa 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 kappa, indicating that it has the largest difference in average spending, following Bai (1997).[48] 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-three states have decisions. Note that the number of states is non-monotonic because I have data from 1972, but not 1973-1976.[49]

Finally, all regressions, unless stated otherwise 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 years do not have less weight than later years.

## 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 $134 per capita in the "jump" specification, which is significant at the 5% level, and $132 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.) There are not statistically significant pre- or post-trends.

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[47] 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.

[48] Jushan Bai, *Estimation of a change point in multiple regression model,* 79(4) Review of Economics and Statistics, 551-563 (1997).

[49] 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.

Robust standard errors are clustered at the state level, as they are for all regressions. Figure 2 shows the nonparametric regression, as well as 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. 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.

Table 3 turns to the financing of this increase in education spending. In the wake of a decision, government revenue increased by $159, which is significant at the 10% level, in the trend regression (and by $253 in the jump regression, which is significant at the 5% level). Of that $159 increase, essentially all is the result of increased taxes—with an increase of $154 in taxes per capita, which is significant at the 5% level. I do not have the power to know with precision the composition of that increase in tax revenue, but the regressions show that $68 come from increases in income tax revenue,[50] $39 from increases in sales tax revenue,[51] and $16 from increases in property tax revenue, though none of those coefficients are statistically significant. The regressions also show a statistically significant increase over time in property tax revenue. The jump regression shows a larger share for income tax, with $119 of the $162 increase in total taxes coming from increases in the income tax, though again that coefficient is not statistically significant.

Tables 4 and 5 turn to how expenditures changed in response to the school finance decisions. In these tables, and those that follow, part A of the table has the jump specification and part B has the trend specification. I will focus on the trend specifications. Column (1) of Table 4B shows that total expenditure increased by $139, roughly the same amount as K-12

[50] Income tax revenue includes not only the personal income tax but also the corporate income tax.
[51] The regressions include general sales tax revenue, but not sales tax revenue from the sales of specific items, like alcohol.

education spending increased ($132 increase in K-12 education spending). I then assess the significant categories of expenditure—construction, healthcare, 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, which is actually positive, and in any case is only significant at the 10% level. 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, primarily the state income tax, Tables 6, 7, and 8 show how the structure of state income taxes change. These tables show the results for single filers; the Appendix has results for married filers, which have similar point estimates, but are often less precisely estimated. I focus on measuring whether those who primarily benefit (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. Tables 6 and 7, columns (1) – (3) describe the changes in tax rates at three points in the income distribution, 20$^{th}$ (column (1)), 80$^{th}$ (column (2)), and 95$^{th}$ (column (3)), for filers without children (Table 6) and filers with children (Table 7). The estimates in Table 6B show statistically significant increases in average tax rates at the 20$^{th}$ percentile (an increase of 0.14 percentage points), but also shows a statistically significant negative post-trend of -0.025. The table also shows a statistically significant increase in tax rates at the 80th percentile (0.27 percentage points). Table 6B shows an even larger increase in taxes at the 95th percentile (0.28 percentage points), but that estimate is not statistically significant. The left-hand-side figures of Figure 3 show graphically how income tax rates changed at these percentiles.

Table 7B 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 figures of Figure 3 show graphically how average income tax rates changed at these percentiles for filers with children.

Columns (4) and (5) of Tables 6 and 7 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 null 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 shows 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 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 0.22 percentage points more for the 20th percentile than the 95th percentile.[52] 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 0.34 percentage points more for 20th percentile earners than 95th percentile earners.[53] In other words, these zeros (and the point estimates in both these cases are almost exactly zero) are precise zeros.

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[52] The point estimate is -0.035 and standard error is 0.153, so -0.035 + 1.645 * 0.153 = 0.22.
[53] The point estimate is -0.033 and standard error is 0.227, so -0. 033 + 1.645 * 0.227 = 0.34.

For filers with children, the point estates are about the same as those without children, but the estimates are noisier.

Table 8, columns (1) – (3), compares the average income tax rates for filers with children to those without children, since the largest beneficiaries of the school finance decisions are those with children. The results in Table 8B show very small, statistically insignificant, jumps—0.02 percentage points at the 20th percentile (though imprecisely estimated), 0.04 at the 80th percentile (precisely estimated with a SE of 0.04), and 0.02 at the 95th percentile (precisely estimated with a SE of 0.02). The results also show high statistically significant, but small in magnitude, downward trends after the decisions for the high income percentiles: -0.014 at the 80th percentile and -0.007 at the 95th percentile, both significant at the 1% level. Figure 5 shows the nonparametric estimates for these same outcome variables. 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.[54]

Table 8, columns (4) – (5), shows the difference in difference estimates: that is, in the case of column (4), the difference between column (1), the difference in tax rates between filers with and without children at the 20th percentile, and column (2), the difference in tax rates between filers with and without children at the 80th percentile. Column (5) repeats the exercise for the difference between column (1) and column (3), for the 95th percentile. The point estimates are extremely small (-0.020 for the 20th versus 80th percentiles and 0.001 for the 20th versus the 95th percentiles), and there are no statistically significant trends. However, the

[54] 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.

standard errors are large, which is unsurprising given the large standard errors in column (1), 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 8. Point estimates are roughly zero, but the confidence intervals are large, and I cannot 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.

## B. Robustness Checks

Tables 9 and 10 include robustness checks. A concern that one might have is that states might be 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. 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 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.[55] Table 9 shows that controlling for each of these variables separately and

[55] 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

together has little effect on the results (although the result is modestly smaller when both controls are included). Interestingly, the coefficient on neither the presidential vote nor the composition of state government is significant, suggesting that—after controlling for state and year fixed effects

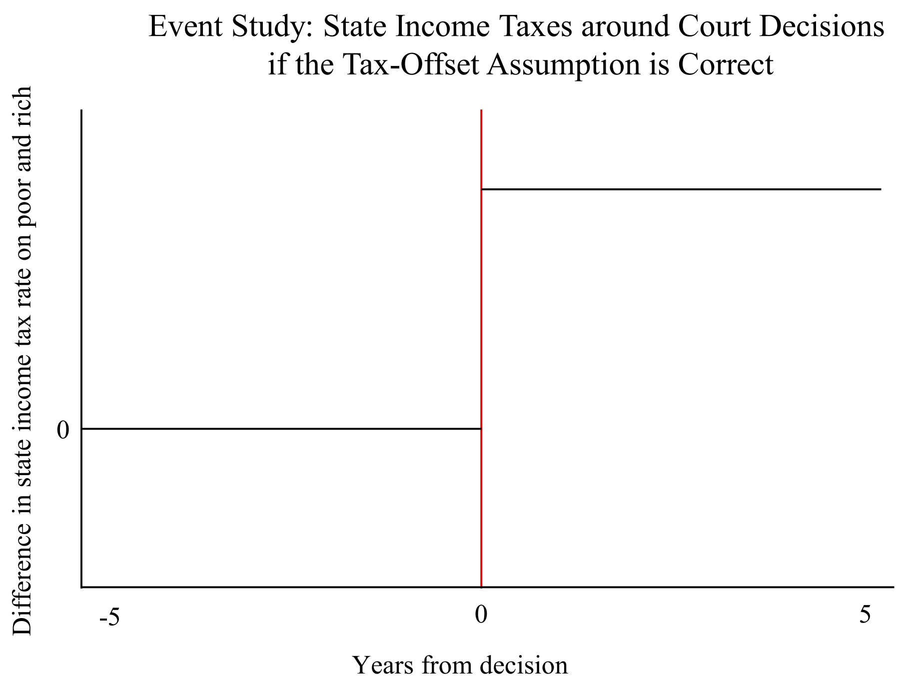
Table 10 includes a variety of other specifications, including not weighting the results, including 20 years before and 30 years after a decision instead of 15 years before and 25 years after, and including all years of data. None of these specifications substantially changes the estimate of the increase in per capita spending. I also include an estimate of the effect on log K-12 education expenditure. 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 9% increase in spending.

## V. Discussion

The main results are straightforward: school finance litigation increases state funding for schools, and there is no evidence that thse increases are offset by regressive changes in spending or taxes. Quite the contrary, there is some evidence that state income taxes become more progressive in response to school finance court decisions.

I now turn to a less straightforward discussion of the meaning of the results for the "tax offset assumption." I begin by discussing the results on the changes in income tax rates, in the context of the tax offset assumption. Importantly, unlike the typical econometric study, my null hypothesis is not 0. Rather, the tax offset assumption makes a stark prediction—that the distributional consequences of the legal rule change will be offset through taxes. Of course, estimating the incidence of the change in taxes is not trivial, but my estimate is that taxes on poor households (i.e., those at the 20th percentile of the income distribution) should increase by 0.74 percentage points more than taxes on rich households (those at the 80th percentile) in the aftermath of a state supreme court opinion requiring school finance equalization, as I will explain shortly. Of course, as I argue elsewhere, full offset is generally *not* a condition of full optimality; the true optimum could be either greater or less than full offset, depending on the distribution of gains and losses and the costs of redistribution.[56] However, it is beyond the scope of the analysis here to determine the optimal offset, so I will assume that the appropriate offset is full, not more or less than full. I demonstrate an idealized image of what the change should look like if the tax-offset null is not rejected in the figure below. Before the state supreme court decision at time 0, there is some trend in the differential tax rate; in this case, I am assuming a flat trend. At time 0, the state supreme court requires more progressive spending. To offset that, the legislature would have to enact regressive tax increases, leading to a jump up in the difference between taxes on the poor and the rich, normalizing the difference at the time of the supreme court decision to zero.

[56] Zachary Liscow, Should Law Be Efficient?, Draft.




For the purpose of calculating the null hypothesis, I begin by assuming that, prior to the supreme court decisions, social welfare was maximized. I do so not because it is necessarily plausible but because to assume otherwise would be to concede the point that redistributing through legal rules can be used to increase social welfare. Then, I assume that households value the increase in spending on their education at the amount spent. They could value it more[57] or less, but I take this to be a reasonable baseline assumption.[58]

The null hypothesis depends upon which regression is under discussion, but is based upon the amount that a given household can be expected to benefit from an increase in education spending. I estimate the amount of spending as follows. First, I use my estimate that per capita education spending jumps up by $132 (in inflation-adjusted 2015 dollars) because of a state

[57] See Stephanie Riegg Cellini, Fernando Ferreira & Jesse Rothstein, *The Value of School Facility Investments: Evidence from a Dynamic Regression Discontinuity Design*, 125 *Q.J. ECON.* 215 (2010) (using evidence from changes in housing prices after close referenda to show that that people value school spending more than dollar-for-dollar).

[58] 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, *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.

supreme court opinion, with little pre-trend before the decision. Not all of this state spending on education actually leads to increased spending on schools, instead being used for other purposes like local tax cuts. I estimate the "flypaper effect" using the canonical paper on the subject, which lists ten studies with an average result of 63.7% of spending being used for the intended purpose.[59]

With those two pieces of data, I calculate the increased education spending as a fraction of income in two ways: 1) differentiating based on the number of children a household has and 2) not differentiating, and instead assuming that policymakers cannot target households with children. Turning to the first method, I make the following calculation. There were 53.89 million children in K-12 school in 2013.[60] With a total us population of 316.5 million, this means that the ratio of total population to school-attending population is 5.87. So, to transform per capita spending to per student spending, I multiply the per capita spending amount by 5.87. Thus, for a filer with two children, this amounts to $1,550 (= $132 * 5.87 * 2). With a 2013 20th percentile income of $20,900, a 80th percentile income of $105,910, and a 95th percentile income of $196,000, and a flypaper effect of 63.7%, this spending amounts to 4.72% of income for 20th percentile filers, 0.93% for 80th percentile filers, and 0.50% for 95th percentile filers.[61] Thus, for 2-child filers, we expect a 3.79 percentage point increase in taxes for the 20th percentile over the 80th percentile, and a 4.32 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

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[59] James Hines & Richard Thaler, *Anomalies: The Flypaper Effect*, 9 J. ECON. PERSP. 217 (1995).
[60] Table 1 at https://www.census.gov/hhes/school/data/cps/2013/tables.html . The calculation assumes that assuming that half of those in preschool or kindergarten were in kindergarten.
[61] See https://www2.census.gov/programs-surveys/demo/tables/p60/249/tablea2.pdf

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" changes of 3.79 and 4.32, respectively. 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 4.72 percentage points at the 20th percentile, 0.93 percentage points for 80th percentile filers, and 0.50 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).

The second way of calculating the expected change in income tax rates assumes that policymakers are unable to target tax changes to those with 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 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, it is telling to do this calculation, in case this constraint is a real one.

So here I find the average number of children per household. With 122.46 million households in 2013,[62] and 53.89 million students, this means that there is an average of 0.44 children per household. So with spending of $774.84 per student and 0.44 students per household, that means that we expect $340.93 in spending per household and, after a flypaper

[62] https://www.statista.com/statistics/183635/number-of-households-in-the-us/

effect of 63.7%, $217.17 per household. This amount is 1.04% of income for the 20th percentile, 0.21% for the 80th percentile, and 0.11% for the 95th percentile. So the expected change in tax rates would be an increase of 0.83 percentage points more for the 20th percentile than the 80th and an increase of 0.93 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.

This back-of-the-envelope calculation is contestable in many ways, though mostly in ways suggesting that my calculated expected tax increase is not too big but rather *too small*, which works against my finding a failure of tax offset. For example, I have estimated a lower flypaper effect in the state of Connecticut in other work.[63] But the biggest set of assumptions concern how state spending is allocated. In particular, I am assuming that poor and rich households receive the same amount of school spending, which is almost certainly 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.[64] 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

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[63] Liscow at 43-48.
[64] Jackson, Johnson & Persico at 5.

spending.[65] Additionally, richer families are far more likely to send their children to private schools and therefore not benefit from the school spending.[66]

But, taking the null hypothesis on its face, the state income tax regressions reject it by a long shot; there is no evidence for tax offset here. Indeed, F-value of the test of the null that the coefficient is 0.74 is 29.56 (p-value < 0.0000). So, depending on how one looks at it, the result either is a precisely-measured zero or a great rejection of the null hypothesis of tax-offset.

Of course, one example of the failure of the tax-offset assumption is not conclusive. What can be said now on this foundational question in law and economics is that there is some significant evidence against the tax-offset assumption. But how strong and significant is this finding? 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 at being 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

[65] U.S. CENSUS BUREAU, HISTORICAL INCOME TABLES: HOUSEHOLDS, tbl. F-9, https://www.census.gov/hhes/www/income/data/historical/families.

[66] U.S. CENSUS BUREAU, CURRENT POPULATION SURVEY: OCTOBER 2012, tbl. 8, http://www.census.gov/hhes/school/data/cps/2012/tables.html.

the event study methodology is the ability to easily see the granular trends before and after a change, and the event study figure evinces no worrying trends.

Second, one might be concerned that types of state taxes other than income taxes did increase. Sales taxes, in particular, form a large percent 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. But as I showed in Table 4 I can reject substantial increases in sales taxes. However, there is evidence that property taxes increase in response to the decisions, and those are likely a regressive tax. There are a couple of reasons that this increase in taxes may not indicate a regressive tax change (though, for these same reasons, it may be less indicative of true increases in school finance). In particular, much of the result appears to be driven by the experience of Vermont, which replaced local property taxes with state property taxes, and most people (outside of wealthy residents of ski towns) experienced a decrease in property taxes as a result, making the tax change progressive.[67]

A third concern is whether there should actually be any increase in taxes at all under the tax-offset assumption, given the uncertain economic incidence of school finance decisions. 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.[68] Indeed, in earlier work, I have produced estimates that a significant

[67] See http://www.schoolfunding.info/states/vt/lit_vt.php3. New Hampshire also implemented a state-wide property tax, and it is less clear if that supplanted a local property tax. See http://www.schoolfunding.info/states/nh/lit_nh.php3.

[68] 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.

share of state spending on schools in Connecticut may have gone to tax reductions.[69] 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[70]) 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.[71] 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 far disproportionately to the poor areas; the disproportionate number of poor people in poor 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 null hypothesis should be even higher—and even further from observed effects—than I estimate.

Of course, other complexities of measuring the incidence of the state aid remain. For example, capitalization into housing prices complicates the analysis. And generally incidence is more complicated in this situation of funds going to governments, rather than individuals, since

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[69] Liscow at 43-48.

[70] 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. *See* Edward Zelinsky, *The Once and Future Property Tax: A Dialogue with My Younger Self*, 23 CARDOZO L. REV. 2199 (2002).

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

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 effects. In any case, this is an example of an unusually large amount of money being transferred to poor areas, with other papers showing the large effects. The fact that the transfers are financial makes them easy to quantify. This relatively easy quantifiability, along with the size, in my view, more than compensate for the other guesswork needed for measuring the incidence of state aid for schools.

A fourth concern is that the state aid for schools is not a purely judicial act, but rather one involving interplay with the legislature. However, this interplay is not a problem for the relevance of the results. Most importantly, the result that all redistribution should take place through taxes and transfers is a general result that does not heed institutional boundaries. That is, the tax-offset assumption is that the legislature will also offset the distributional changes to changes in legal rules enacted by the legislature itself. So the involvement of the legislature is not particularly relevant. The fact that this one requires a quantifiable legislative response in dollars is a feature, not a bug.

A related concern is that the decisions of judges themselves may reflect the redistributionary preferences of the electorate—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.[72] So the electorate's preferences for redistribution could be expressed through the election or retention of judges.

[72] *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.

Elsewhere, the electorate's preferences for redistribution could also enter through the decision to appoint and confirm state supreme court justices.

While electoral influence on judges is fair concern, I have several responses. First, a similar critique could be used against any state supreme court ruling I can think of and certainly against any legislative or executive action. 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.[73] Second, even where there are elections, the intrusion of politics—and therefore the redistributionary preferences of the electorate—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. Most importantly, controlling for changes in the political leanings of the state do not substantially change the results.

Finally, one might be concerned about the external validity of the results. Recall that the tax-offset assumption holds that taxes and transfers respond to offset the distributional effects of changes in legal rules. And that should hold regardless of whether the change in legal rule constitutes a regressive move toward efficiency or not. Nevertheless, one may think that taxes respond differently to progressive than to regressive legal rule changes. Of course, I cannot rule

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

that out, and testing the tax offset assumptions in other circumstances is a useful item for future research.

With this one example of an absence of any tax offset in response to a big change in legal rules, we cannot conclude that taxes never offset the distributional effects of changes in legal rules. The issue is hardly settled. For example, it could even be the case that federal spending or state spending that I do not capture respond in offsetting ways.[74] We need much more evidence on this question. But, I have provided a start to an empirical answer of this question of great importance in a context where finding offset is promising, due to the large size of the legal rule change, the plausibly exogenous variation in changes across states and across time, and the availability of data to measure whether the incidence of the legal rule change is offset.

## VI. Conclusion

This paper has documented for the first time how states respond to school finance orders by increasing their education spending using an event study methodology, and demonstrating the substantial increase in spending that takes place. I also offer evidence for how the court orders are financed: using progressive income tax changes and property tax changes, but not sales tax changes or cuts in spending. It may be the case that school finance court orders have helped drive the increasingly progressive state income taxes across the country and, surprisingly, may have driven a partial return to state property taxes. The results imply that, in this setting, legal changes are effective at changing the distribution of taxes and spending.

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[74] In principle, this concern could be partially addressed by assessing intergovernmental transfers from the federal government, which are in this same dataset, as well as federal spending, which is available in another dataset that tries to capture county-level federal spending.

Interesting future steps may be exploring heterogeneity in response, across time, between Democratic- and Republican-held legislatures, and between equity-based and adequacy-based decisions.

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

| | Mean | S.D. | Min. | Max. |
|---|---|---|---|---|
| Education Expenditure | 1,626.97 | 544.63 | 452.8 | 4,038.48 |
| Post Decision | 0.25 | 0.43 | 0.0 | 1.00 |
| Post Decision * Yrs. Elapsed | 3.32 | 7.46 | 0.0 | 41.00 |
| Observations | 1890 | | | |

Note: Per-Capita K-12 Education Expenditure is shown in 2015 dollars. Years: 1972, 1977-2013

Table 1B: Summary Statistics of Per-Capita Revenue

| | Mean | S.D. | Min. | Max. |
|---|---|---|---|---|
| Total Revenue | 5,617.2 | 2,506.8 | 1,688.8 | 26,511.1 |
| Total Taxes | 2,379.1 | 857.0 | 761.2 | 14,190.1 |
| Income Taxes | 902.4 | 716.8 | 0.0 | 13,384.4 |
| General Sales Taxes | 719.4 | 378.3 | 0.0 | 2,293.2 |
| Property Taxes | 60.6 | 153.3 | 0.0 | 1,649.3 |
| Observations | 1841 | | | |

Note: 2015 Dollars. Years: 1977-2013.

Table 1C: Summary Statistics of Per-Capita Expenditure

| | Mean | S.D. | Min. | Max. |
|---|---|---|---|---|
| Total Expenditure | 5,179.7 | 2,141.7 | 2,171.8 | 19,313.8 |
| Non-Education Total Expenditure | 3,552.7 | 1,707.5 | 1,173.7 | 16,231.8 |
| Construction | 344.2 | 193.5 | 52.9 | 1,717.1 |
| Healthcare | 327.0 | 144.9 | 103.0 | 1,096.8 |
| Total Welfare | 1,000.4 | 525.3 | 158.7 | 3,045.7 |
| State-Level Welfare | 364.7 | 240.1 | -336.1 | 1,341.6 |
| Higher Education | 614.0 | 215.4 | 168.4 | 1,483.8 |
| Employee Retirement | 316.5 | 224.1 | 0.0 | 1,665.5 |
| Unemployment Benefits | 168.6 | 120.1 | 15.0 | 984.3 |
| Highways | 440.2 | 227.7 | 122.4 | 2,309.5 |
| Total Debt Outstanding | 3,025.0 | 2,417.1 | 99.5 | 27,659.8 |
| Observations | 1890 | | | |

Note: 2015 Dollars. Unlike the other categories, “Total Debt Outstanding” is not an expenditure. Years: 1972, 1977-2013.

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

| | Mean | S.D. | Min. | Max. |
|---|---|---|---|---|
| Tax Rate at the 20th Percentile | 2.03 | 1.38 | -0.23 | 6.12 |
| Tax Rate at the 80th Percentile | 4.11 | 2.35 | 0.00 | 8.78 |
| Tax Rate at the 95th Percentile | 4.52 | 2.62 | 0.00 | 10.52 |
| 20th Minus 80th Percentile | -2.08 | 1.58 | -6.66 | 0.07 |
| 20th Minus 95th Percentile | -2.49 | 1.92 | -8.45 | 0.06 |
| Observations | 1841 | | | |

Years: 1977-2013.

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

| | Mean | S.D. | Min. | Max. |
|---|---|---|---|---|
| Tax Rate at the 20th Percentile | 0.76 | 1.62 | -7.14 | 4.68 |
| Tax Rate at the 80th Percentile | 3.89 | 2.23 | 0.00 | 8.47 |
| Tax Rate at the 95th Percentile | 4.39 | 2.55 | 0.00 | 10.16 |
| 20th Minus 80th Percentile | -3.14 | 2.45 | -12.51 | 0.08 |
| 20th Minus 95th Percentile | -3.64 | 2.78 | -13.49 | 0.11 |
| Observations | 1841 | | | |

Years: 1977-2013.

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

| | Mean | S.D. | Min. | Max. |
|---|---|---|---|---|
| 20th Percentile: With Children - Without Children | -1.27 | 1.62 | -9.87 | 0.52 |
| 80th Percentile: With Children - Without Children | -0.22 | 0.22 | -1.15 | 0.18 |
| 95th Percentile: With Children - Without Children | -0.12 | 0.13 | -0.58 | 0.20 |
| Diff. in Diff. 20th - 80$^{th}$ | -1.06 | 1.53 | -9.31 | 0.38 |
| Diff. in Diff. 20th - 95$^{th}$ | -1.15 | 1.57 | -9.59 | 0.43 |
| Observations | 1841 | | | |

Years: 1977-2013.

Table 1G: Summary Statistics of Controls Used in Robustness Checks

| | Mean | S.D. | Min. | Max. |
|---|---|---|---|---|
| State Presidential Vote | 43.92 | 8.82 | 19.6 | 71.80 |
| Composition of State Government | 50.53 | 38.41 | 0.0 | 100.00 |
| Observations | 2411 | | | |

Years: 1972, 1977-2013.

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

| | (1) | (2) |
|---|---|---|
| Post Decision | 133.857** | 132.176*** |
| | (56.579) | (41.257) |
| Post Decision * Yrs. Elapsed | | 4.093 |
| | | (3.850) |
| Trend | | -1.905 |
| | | (4.398) |
| Observations | 1688 | 1688 |
| $R^2$ | 0.881 | 0.882 |
| Adjusted $R^2$ | 0.875 | 0.875 |

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, 1977-2013.

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

| | (1) Total Revenue | (2) Total Taxes | (3) Income Taxes | (4) General Sales Taxes | (5) Property Taxes |
|---|---|---|---|---|---|
| Post Decision | 253.283** | 162.436** | 119.318 | 15.900 | 20.156 |
| | (101.971) | (76.188) | (73.617) | (25.599) | (16.231) |
| Observations | 1658 | 1658 | 1658 | 1658 | 1658 |
| $R^2$ | 0.930 | 0.869 | 0.876 | 0.916 | 0.727 |
| Adjusted $R^2$ | 0.926 | 0.862 | 0.870 | 0.912 | 0.712 |

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: 1977-2013.

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

| | (1) Total Revenue | (2) Total Taxes | (3) Income Taxes | (4) General Sales Taxes | (5) Property Taxes |
|---|---|---|---|---|---|
| Post Decision | 159.067* | 153.631** | 67.667 | 38.089 | 15.724 |
| | (89.405) | (66.347) | (53.199) | (26.532) | (12.206) |
| Post Decision * Yrs. Elapsed | 0.894 | 2.005 | -4.377 | 2.595 | 2.491*** |
| | (12.183) | (6.176) | (7.220) | (3.189) | (0.928) |
| Trend | 7.158 | -0.236 | 6.215 | -3.006 | -0.816 |
| | (10.796) | (4.900) | (5.254) | (2.279) | (0.911) |
| Observations | 1658 | 1658 | 1658 | 1658 | 1658 |
| $R^2$ | 0.930 | 0.869 | 0.877 | 0.917 | 0.731 |
| Adjusted $R^2$ | 0.927 | 0.862 | 0.870 | 0.912 | 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: 1977-2013.

Table 4A: 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 | 257.475** | 123.619 | 11.032 | 36.420 | 78.621** | -13.929 |
| | (119.798) | (100.736) | (22.556) | (25.469) | (39.027) | (26.455) |
| Observations | 1688 | 1688 | 1688 | 1688 | 1688 | 1688 |
| $R^2$ | 0.959 | 0.956 | 0.709 | 0.790 | 0.945 | 0.852 |
| Adjusted $R^2$ | 0.957 | 0.954 | 0.693 | 0.779 | 0.942 | 0.844 |

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, 1977-2013.

Table 4B: 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 | 139.416** | 7.240 | 22.207 | 8.566 | 29.009 | -8.758 |
| | (68.245) | (56.390) | (19.072) | (18.759) | (43.073) | (18.269) |
| Post Decision * Yrs. Elapsed | 3.391 | -0.702 | -0.673 | 0.302 | -3.452 | 3.904 |
| | (13.770) | (12.066) | (2.323) | (2.907) | (5.067) | (2.720) |
| Trend | 7.563 | 9.469 | -0.541 | 2.032 | 5.606 | -2.348 |
| | (12.759) | (11.899) | (1.809) | (1.652) | (5.118) | (3.056) |
| Observations | 1688 | 1688 | 1688 | 1688 | 1688 | 1688 |
| $R^2$ | 0.960 | 0.957 | 0.709 | 0.793 | 0.946 | 0.853 |
| Adjusted $R^2$ | 0.958 | 0.955 | 0.693 | 0.782 | 0.943 | 0.845 |

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, 1977-2013.

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

| | (1) Higher Education | (2) Employee Retirement | (3) Unemployment Benefits | (4) Highways | (5) Total Debt Outstanding |
|---|---|---|---|---|---|
| Post Decision | 34.191 | -6.091 | -4.750 | -2.225 | 216.046 |
| | (21.339) | (27.284) | (11.943) | (22.290) | (362.307) |
| Observations | 1688 | 1688 | 1688 | 1688 | 1688 |
| $R^2$ | 0.904 | 0.926 | 0.830 | 0.766 | 0.877 |
| Adjusted $R^2$ | 0.899 | 0.922 | 0.820 | 0.753 | 0.870 |

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. Unlike the other categories, "Total Debt Outstanding" is not an expenditure. Years: 1972, 1977-2013.

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

| | (1) Higher Education | (2) Employee Retirement | (3) Unemployment Benefits | (4) Highways | (5) Total Debt Outstanding |
|---|---|---|---|---|---|
| Post Decision | 34.718* | -13.540 | -4.825 | 14.075 | 30.285 |
| | (18.369) | (14.661) | (12.985) | (21.763) | (130.602) |
| Post Decision * Yrs. Elapsed | 1.960 | -0.475 | 1.703 | 0.331 | 18.296 |
| | (2.397) | (2.235) | (1.500) | (1.638) | (14.644) |
| Trend | -1.017 | 0.820 | -0.842 | -1.442 | 5.449 |
| | (2.297) | (2.106) | (1.565) | (1.426) | (20.664) |
| Observations | 1688 | 1688 | 1688 | 1688 | 1688 |
| $R^2$ | 0.905 | 0.926 | 0.830 | 0.767 | 0.878 |
| Adjusted $R^2$ | 0.899 | 0.922 | 0.821 | 0.754 | 0.872 |

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. Unlike the other categories, "Total Debt Outstanding" is not an expenditure. Years: 1972, 1977-2013.

Table 6A: 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 | 0.128 | 0.185 | 0.244 | -0.056 | -0.116 |
| | (0.118) | (0.141) | (0.176) | (0.110) | (0.152) |
| Observations | 1658 | 1658 | 1658 | 1658 | 1658 |
| $R^2$ | 0.931 | 0.971 | 0.967 | 0.971 | 0.964 |
| Adjusted $R^2$ | 0.928 | 0.970 | 0.966 | 0.969 | 0.962 |

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: 1977-2013.

Table 6B: 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 | 0.138** | 0.270** | 0.278 | -0.132 | -0.140 |
| | (0.066) | (0.133) | (0.194) | (0.133) | (0.194) |
| Post Decision * Yrs. Elapsed | -0.025** | -0.002 | -0.010 | -0.023 | -0.015 |
| | (0.010) | (0.018) | (0.026) | (0.018) | (0.024) |
| Trend | 0.011 | -0.006 | 0.002 | 0.017 | 0.009 |
| | (0.011) | (0.013) | (0.012) | (0.014) | (0.012) |
| Observations | 1658 | 1658 | 1658 | 1658 | 1658 |
| $R^2$ | 0.933 | 0.971 | 0.967 | 0.971 | 0.964 |
| Adjusted $R^2$ | 0.929 | 0.970 | 0.966 | 0.970 | 0.962 |

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: 1977-2013.

Table 7A: 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 | 0.012 | 0.181 | 0.243 | -0.168 | -0.231 |
| | (0.407) | (0.136) | (0.169) | (0.407) | (0.399) |
| Observations | 1658 | 1658 | 1658 | 1658 | 1658 |
| $R^2$ | 0.693 | 0.967 | 0.966 | 0.905 | 0.916 |
| Adjusted $R^2$ | 0.676 | 0.965 | 0.964 | 0.899 | 0.912 |

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: 1977-2013.

Table 7B: 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 | 0.156 | 0.309** | 0.295 | -0.153 | -0.139 |
| | (0.235) | (0.146) | (0.197) | (0.220) | (0.259) |
| Post Decision * Yrs. Elapsed | 0.008 | -0.016 | -0.017 | 0.024 | 0.025 |
| | (0.066) | (0.021) | (0.027) | (0.057) | (0.060) |
| Trend | -0.015 | -0.003 | 0.004 | -0.012 | -0.019 |
| | (0.059) | (0.012) | (0.012) | (0.052) | (0.054) |
| Observations | 1658 | 1658 | 1658 | 1658 | 1658 |
| $R^2$ | 0.693 | 0.968 | 0.967 | 0.905 | 0.916 |
| Adjusted $R^2$ | 0.676 | 0.966 | 0.965 | 0.900 | 0.912 |

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: 1977-2013.

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

| | (1)<br>20th Percentile: With Children Minus Without Children | (2)<br>80th Percentile: With Children Minus Without Children | (3)<br>95th Percentile: With Children Minus Without Children | (4)<br>Diff. in Diff. 20th Minus 80th | (5)<br>Diff. in Diff. 20th Minus 95th |
|---|---|---|---|---|---|
| Post Decision | -0.116 | -0.004 | -0.001 | -0.112 | -0.115 |
| | (0.419) | (0.021) | (0.013) | (0.419) | (0.420) |
| Observations | 1658 | 1658 | 1658 | 1658 | 1658 |
| $R^2$ | 0.757 | 0.777 | 0.749 | 0.745 | 0.750 |
| Adjusted $R^2$ | 0.744 | 0.764 | 0.735 | 0.731 | 0.736 |

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: 1977-2013.

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

| | (1)<br>20th Percentile: With Children Minus Without Children | (2)<br>80th Percentile: With Children Minus Without Children | (3)<br>95th Percentile: With Children Minus Without Children | (4)<br>Diff. in Diff. 20th Minus 80th | (5)<br>Diff. in Diff. 20th Minus 95th |
|---|---|---|---|---|---|
| Post Decision | 0.018 | 0.039 | 0.018 | -0.020 | 0.001 |
| | (0.233) | (0.035) | (0.018) | (0.229) | (0.231) |
| Post Decision | 0.032 | -0.014*** | -0.007*** | 0.047 | 0.040 |
| * Yrs. Elapsed | (0.069) | (0.005) | (0.002) | (0.069) | (0.069) |
| Trend | -0.026 | 0.003 | 0.002 | -0.029 | -0.028 |
| | (0.061) | (0.003) | (0.002) | (0.062) | (0.062) |
| Observations | 1658 | 1658 | 1658 | 1658 | 1658 |
| $R^2$ | 0.759 | 0.811 | 0.773 | 0.747 | 0.752 |
| Adjusted $R^2$ | 0.745 | 0.800 | 0.760 | 0.733 | 0.738 |

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: 1977-2013.

Table 9A: Robustness Checks: Effect on Per-Capita Education Expenditure

| | (1)<br>Presidential Vote Control | (2)<br>State Government Control | (3)<br>Presidential Vote and State Government Control |
|---|---|---|---|
| Post Decision | 127.389** | 129.585** | 106.193** |
| | (52.862) | (54.480) | (50.502) |
| State Presidential Vote | -4.007 | | -4.328 |
| | (3.381) | | (3.644) |
| Composition of State Government | | 0.574 | 0.609 |
| | | (0.536) | (0.521) |
| Observations | 1688 | 1688 | 1658 |
| $R^2$ | 0.882 | 0.883 | 0.885 |
| Adjusted $R^2$ | 0.876 | 0.876 | 0.879 |

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, 1977-2013.

Table 9B: Robustness Checks: Effect on Per-Capita Education Expenditure

| | (1)<br>Presidential Vote Control | (2)<br>State Government Control | (3)<br>Presidential Vote and State Government Control |
|---|---|---|---|
| Post Decision | 126.269*** | 121.948*** | 101.806*** |
| | (38.627) | (41.493) | (35.316) |
| Post Decision * Yrs. Elapsed | 4.495 | 3.361 | 2.580 |
| | (4.104) | (3.800) | (4.004) |
| Trend | -2.164 | -1.071 | -0.869 |
| | (4.435) | (4.528) | (4.501) |
| State Presidential Vote | -4.120 | | -4.404 |
| | (3.416) | | (3.707) |
| Composition of State Government | | 0.568 | 0.606 |
| | | (0.543) | (0.534) |
| Observations | 1688 | 1688 | 1658 |
| $R^2$ | 0.883 | 0.883 | 0.885 |
| Adjusted $R^2$ | 0.876 | 0.876 | 0.879 |

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, 1977-2013.

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

| | (1)<br>Unweighted | (2)<br>20 Pre-Decision and 30 Post-Decision Years | (3)<br>All Years Included in Regression | (4)<br>Log Education Expenditure |
|---|---|---|---|---|
| Post Decision | 172.374** | 128.562** | 140.014** | 0.090*** |
| | (71.339) | (56.288) | (58.272) | (0.030) |
| Observations | 1688 | 1780 | 1890 | 1688 |
| $R^2$ | 0.896 | 0.886 | 0.887 | 0.900 |
| Adjusted $R^2$ | 0.890 | 0.880 | 0.882 | 0.894 |

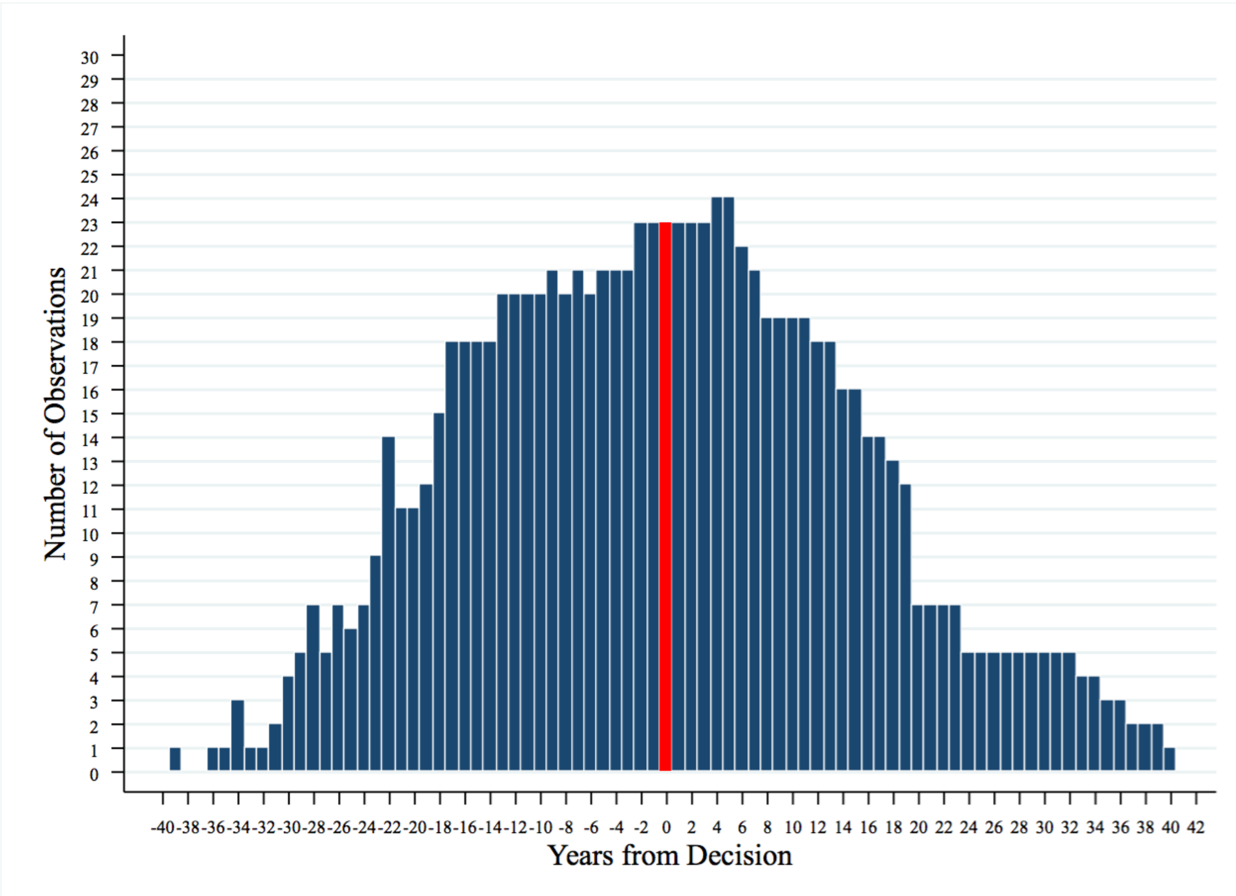
Note: Variables in real 2015 dollars. Robust standard errors clustered at the state level are displayed in parenthesis. Regressions include state and year fixed effects and are weighted by population. Significance levels: * p<0.10; ** p<0.05; *** p<0.01. Years: 1972, 1977-2013.

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

| | (1)<br>Unweighted | (2)<br>20 Pre-Decision and 30 Post-Decision Years | (3)<br>All Years Included in Regression | (4)<br>Log Education Expenditure |
|---|---|---|---|---|
| Post Decision | 165.789*** | 126.489*** | 164.167*** | 0.091*** |
| | (55.353) | (36.151) | (51.548) | (0.030) |
| Post Decision | 6.850 | 5.216 | 2.889 | 0.001 |
| * Yrs. Elapsed | (4.679) | (3.502) | (3.721) | (0.003) |
| Trend | -3.176 | -2.039 | -2.914 | -0.000 |
| | (3.823) | (4.455) | (4.268) | (0.002) |
| Observations | 1688 | 1780 | 1890 | 1688 |
| $R^2$ | 0.896 | 0.887 | 0.888 | 0.900 |
| Adjusted $R^2$ | 0.891 | 0.881 | 0.882 | 0.894 |

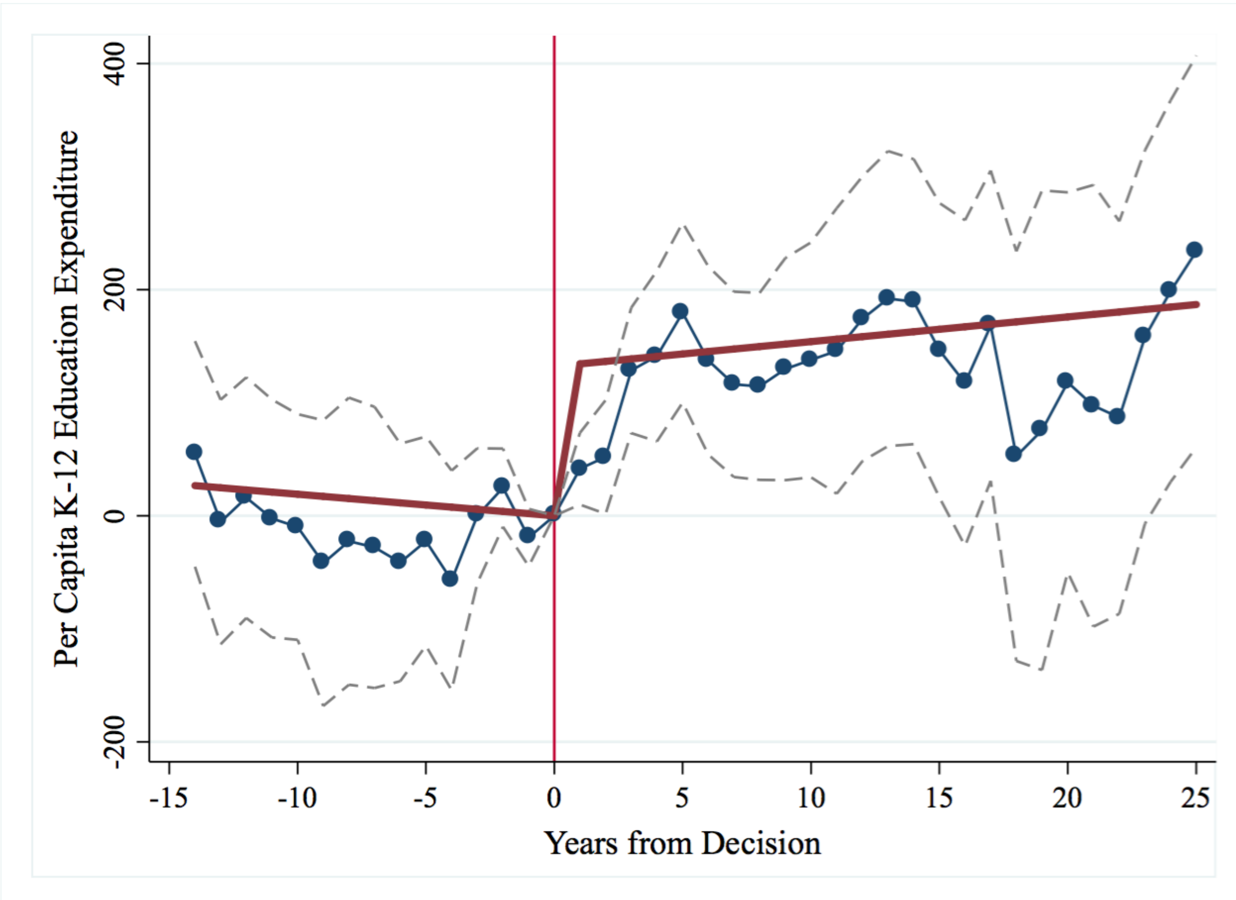
Note: Variables in real 2015 dollars. Robust standard errors clustered at the state level are displayed in parenthesis. Regressions include state and year fixed effects and are weighted by population. Significance levels: * p<0.10; ** p<0.05; *** p<0.01. Years: 1972, 1977-2013.

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, 1977-2013.

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, 1977-2013.

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

20th Percentile Income Earners

Figure 3A: Without Children

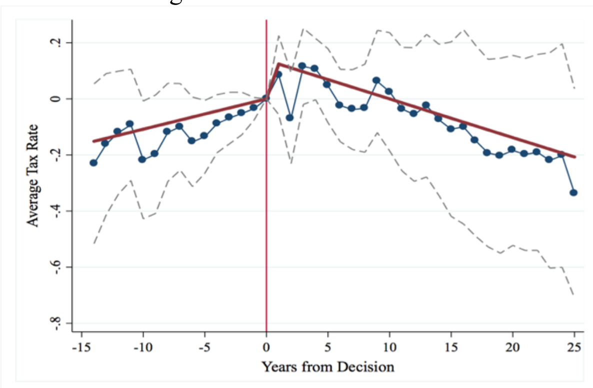
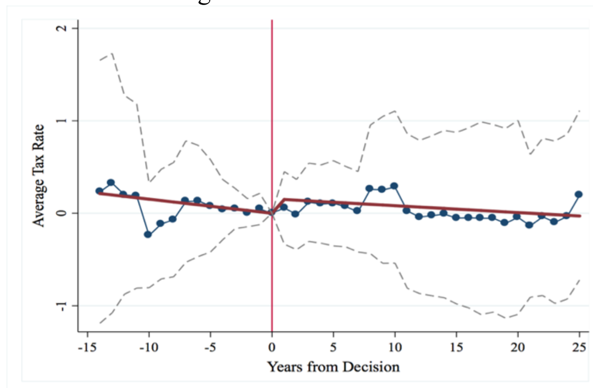



Figure 3B: With Children




80th Percentile Income Earners

Figure 3C: Without Children

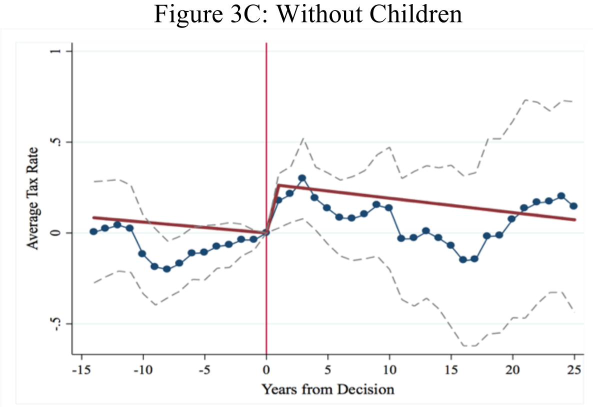
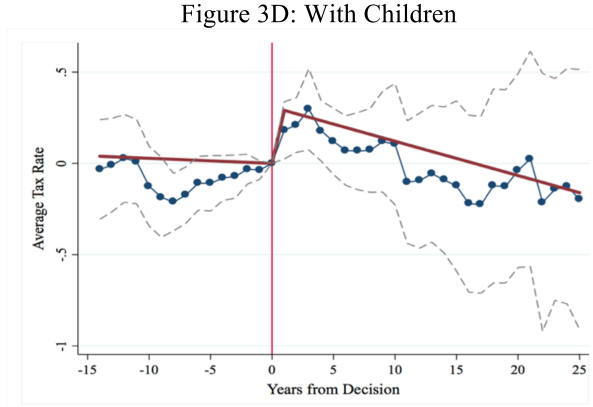



Figure 3D: With Children




95th Percentile Income Earners

Figure 3E: Without Children

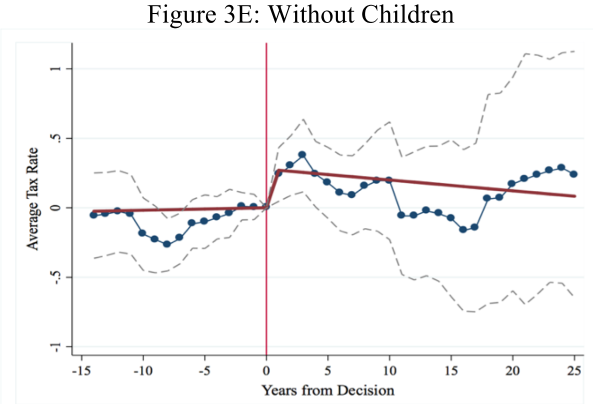
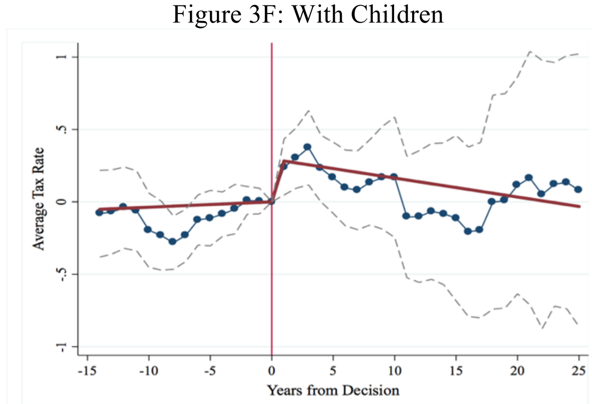



Figure 3F: With Children




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: 1977-2013.

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

20th Minus 80th Percentile Income Earners

Figure 4A: Without Children

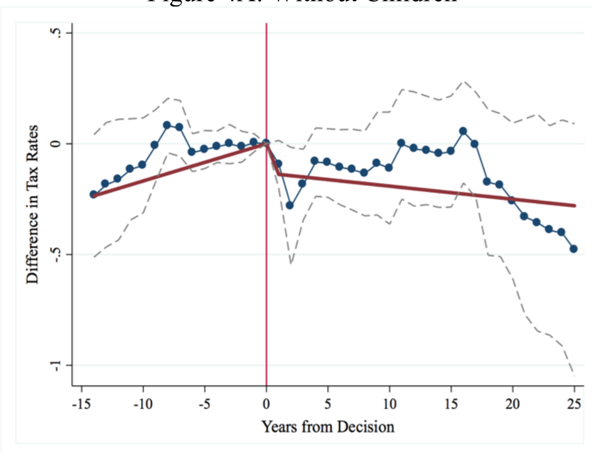
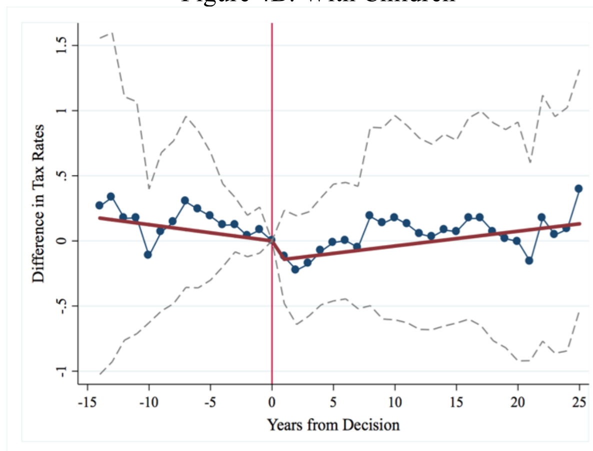



Figure 4B: With Children




20th Minus 95th Percentile Income Earners

Figure 4C: Without Children

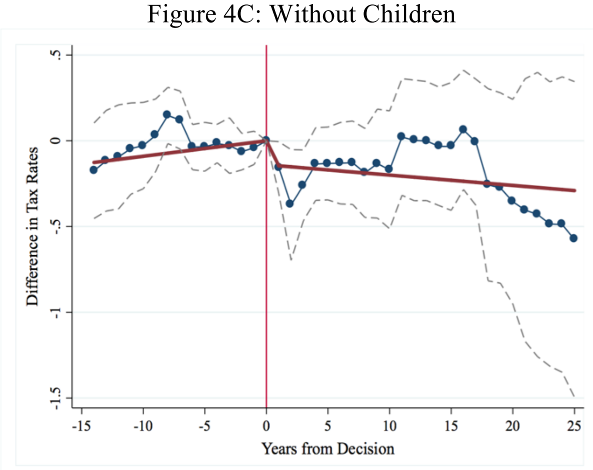
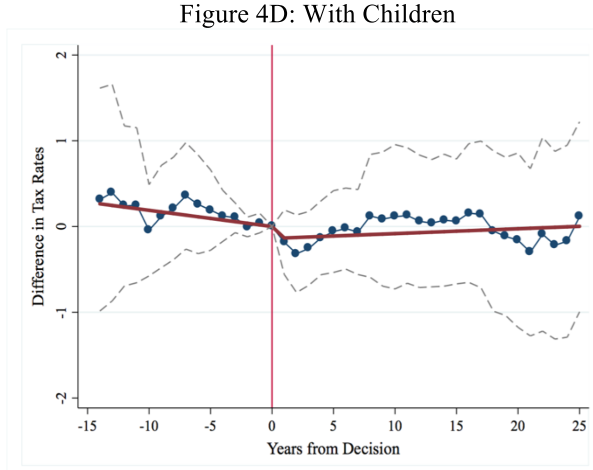



Figure 4D: With Children




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. Regressions include state and year fixed effects and are weighted by population. Years: 1977-2013.

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

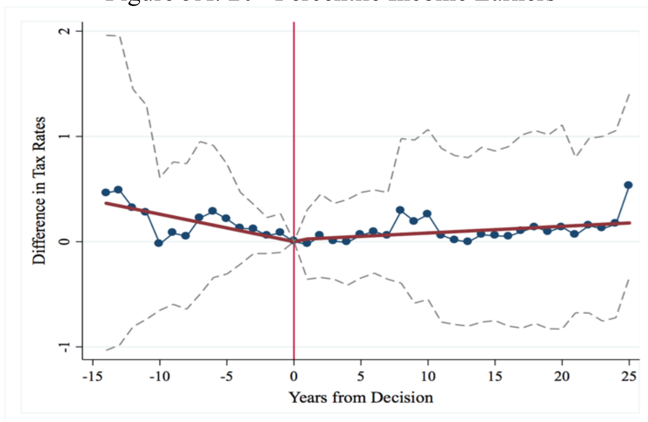



Figure 5B: 80th Percentile Income Earners

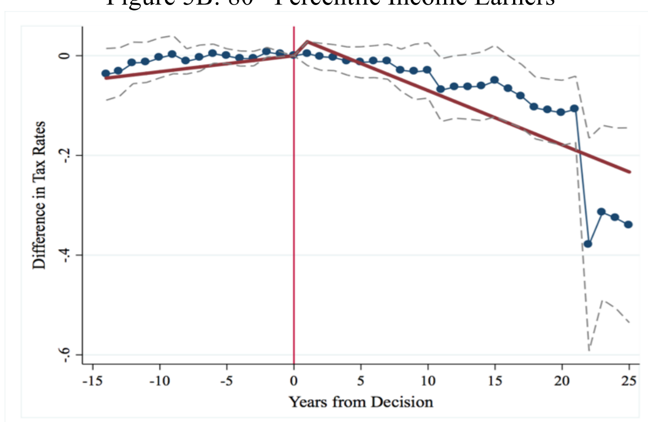
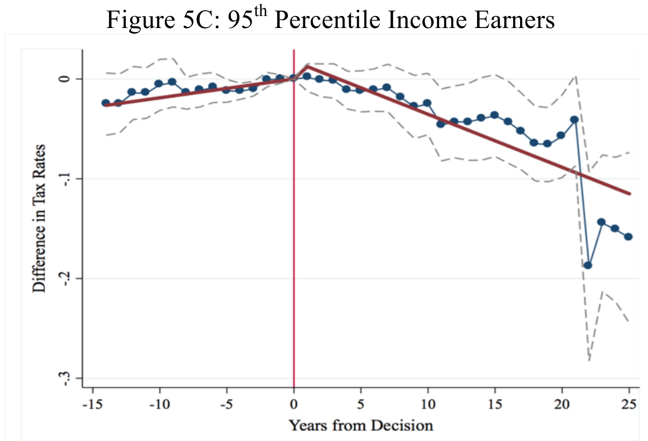



Figure 5C: 95th Percentile Income Earners




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. Regressions include state and year fixed effects and are weighted by population. Years: 1977-2013.

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

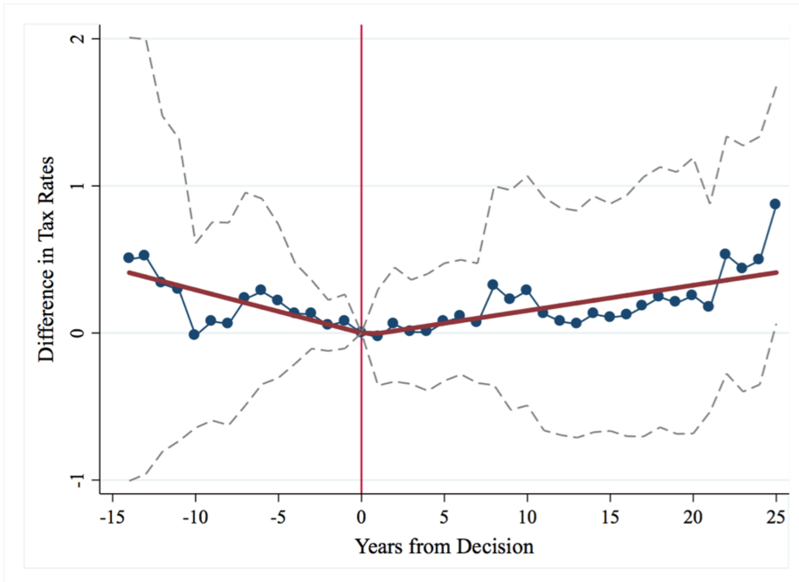
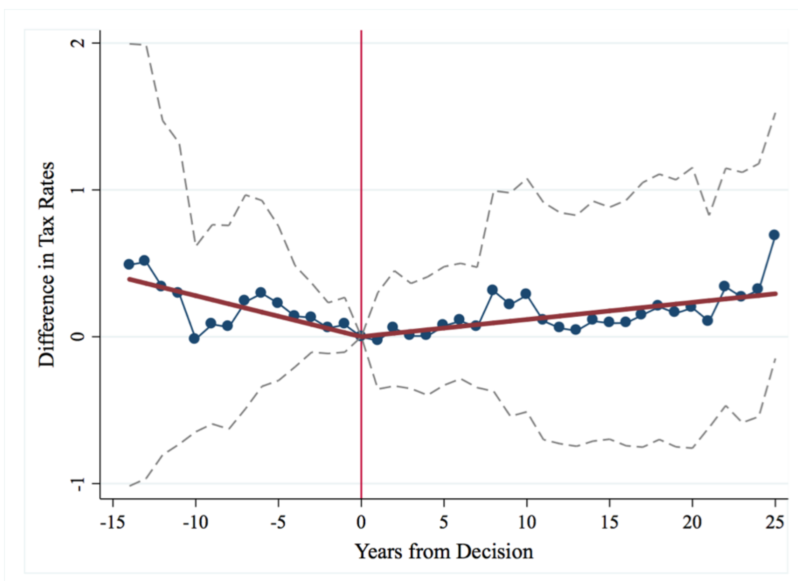



Figure 6B: 20th Minus 95th Percentile Income Earners, With Children Minus Without Children




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. Regressions include state and year fixed effects and are weighted by population. Years: 1977-2013.

Appendix Table 1A: Summary Statistics of Tax Rate Variables (Married, Without Children)

| | Mean | S.D. | Min | Max |
|---|---|---|---|---|
| Tax Rate at the 20th Percentile | 1.12 | 1.10 | -1.12 | 4.13 |
| Tax Rate at the 80th Percentile | 3.63 | 2.11 | 0.00 | 9.11 |
| Tax Rate at the 95th Percentile | 4.20 | 2.41 | 0.00 | 9.99 |
| 20th Minus 80th Percentile | -2.50 | 1.75 | -7.85 | 0.00 |
| 20th Minus 95th Percentile | -3.08 | 2.14 | -8.94 | 0.00 |
| Observations | 1841 | | | |

Years: 1977-2013.

Appendix Table 1B: Summary Statistics of Tax Rate Variables (Married, With Children)

| | Mean | S.D. | Min | Max |
|---|---|---|---|---|
| Tax Rate at the 20th Percentile | 0.76 | 1.62 | -7.14 | 4.68 |
| Tax Rate at the 80th Percentile | 3.89 | 2.23 | 0.00 | 8.47 |
| Tax Rate at the 95th Percentile | 4.39 | 2.55 | 0.00 | 10.16 |
| 20th Minus 80th Percentile | -3.14 | 2.45 | -12.51 | 0.08 |
| 20th Minus 95th Percentile | -3.64 | 2.78 | -13.49 | 0.11 |
| Observations | 1841 | | | |

Years: 1977-2013.

Appendix Table 1C: Summary Statistics of Tax Rate Variables (Married)

| | Mean | S.D. | Min | Max |
|---|---|---|---|---|
| 20th Percentile: With Children - Without Children | -0.37 | 1.42 | -8.03 | 2.92 |
| 80th Percentile: With Children - Without Children | 0.26 | 0.55 | -0.87 | 3.65 |
| 95th Percentile: With Children - Without Children | 0.19 | 0.43 | -0.85 | 2.80 |
| Diff. in Diff. 20th - 80th | 0.63 | 1.52 | -2.96 | 8.30 |
| Diff. in Diff. 20th - 95th | 0.56 | 1.48 | -2.95 | 8.44 |
| Observations | 1841 | | | |

Years: 1977-2013.

Appendix Table 2A: Effect of Decision on Tax Rates (Married, Without Children)

| | (1)<br>Tax Rate at the<br>20th Percentile | (2)<br>Tax Rate at the<br>80th Percentile | (3)<br>Tax Rate at the<br>95th Percentile | (4)<br>20th Minus<br>80th Percentile | (5)<br>20th Minus<br>95th Percentile |
|---|---|---|---|---|---|
| Post Decision | 0.057 | 0.123 | 0.212 | -0.066 | -0.156 |
| | (0.111) | (0.118) | (0.140) | (0.090) | (0.119) |
| Observations | 1658 | 1658 | 1658 | 1658 | 1658 |
| $R^2$ | 0.868 | 0.961 | 0.971 | 0.949 | 0.966 |
| Adjusted $R^2$ | 0.861 | 0.959 | 0.969 | 0.946 | 0.964 |

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: 1977-2013.

Appendix Table 2B: Effect of Decision on Tax Rates (Married, Without Children)

| | (1)<br>Tax Rate at the<br>20th Percentile | (2)<br>Tax Rate at the<br>80th Percentile | (3)<br>Tax Rate at the<br>95th Percentile | (4)<br>20th Minus<br>80th Percentile | (5)<br>20th Minus<br>95th Percentile |
|---|---|---|---|---|---|
| Post Decision | 0.167* | 0.148* | 0.135 | 0.019 | 0.032 |
| | (0.097) | (0.086) | (0.092) | (0.123) | (0.137) |
| Post Decision | 0.012 | 0.007 | 0.002 | 0.005 | 0.010 |
| * Yrs. Elapsed | (0.025) | (0.013) | (0.011) | (0.028) | (0.027) |
| Trend | -0.014 | -0.005 | 0.005 | -0.009 | -0.020 |
| | (0.021) | (0.013) | (0.011) | (0.022) | (0.021) |
| Observations | 1658 | 1658 | 1658 | 1658 | 1658 |
| $R^2$ | 0.868 | 0.961 | 0.971 | 0.949 | 0.966 |
| Adjusted $R^2$ | 0.861 | 0.959 | 0.969 | 0.946 | 0.965 |

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: 1977-2013.

Appendix Table 3A: Effect of Decision on Tax Rates (Married, 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 | 0.012 | 0.181 | 0.243 | -0.168 | -0.231 |
| | (0.407) | (0.136) | (0.169) | (0.407) | (0.399) |
| Observations | 1658 | 1658 | 1658 | 1658 | 1658 |
| $R^2$ | 0.693 | 0.967 | 0.966 | 0.905 | 0.916 |
| Adjusted $R^2$ | 0.676 | 0.965 | 0.964 | 0.899 | 0.912 |

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: 1977-2013.

Appendix Table 3B: Effect of Decision on Tax Rates (Married, 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 | 0.156 | 0.309** | 0.295 | -0.153 | -0.139 |
| | (0.235) | (0.146) | (0.197) | (0.220) | (0.259) |
| Post Decision * Yrs. Elapsed | 0.008 | -0.016 | -0.017 | 0.024 | 0.025 |
| | (0.066) | (0.021) | (0.027) | (0.057) | (0.060) |
| Trend | -0.015 | -0.003 | 0.004 | -0.012 | -0.019 |
| | (0.059) | (0.012) | (0.012) | (0.052) | (0.054) |
| Observations | 1658 | 1658 | 1658 | 1658 | 1658 |
| $R^2$ | 0.693 | 0.968 | 0.967 | 0.905 | 0.916 |
| Adjusted $R^2$ | 0.676 | 0.966 | 0.965 | 0.900 | 0.912 |

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: 1977-2013.

Appendix Table 4A: 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 | -0.044 | 0.058 | 0.031 | 0.102 | 0.075 |
| | (0.403) | (0.079) | (0.063) | (0.408) | (0.401) |
| Observations | 1658 | 1658 | 1658 | 1658 | 1658 |
| $R^2$ | 0.632 | 0.904 | 0.818 | 0.679 | 0.628 |
| Adjusted $R^2$ | 0.612 | 0.899 | 0.808 | 0.662 | 0.608 |

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: 1977-2013.

Appendix Table 4B: Effect of Decision on Tax Rates (Single Filer)

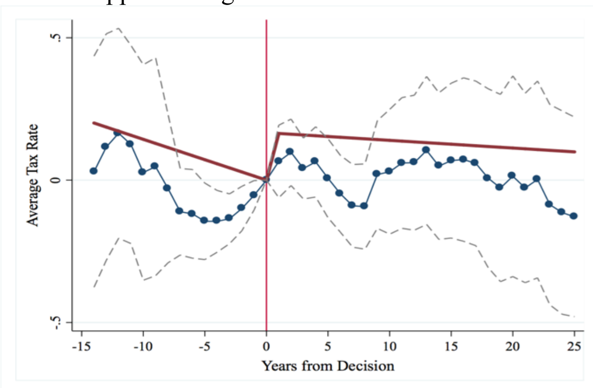
| | (1) 20th Percentile: With Children Minus Without Children | (2) 80th Percentile: With Children Minus Without Children | (3) 95th Percentile: With Children Minus Without Children | (4) Diff. in Diff. 20th Minus 80th | (5) Diff. in Diff. 20th Minus 95th |
|---|---|---|---|---|---|
| Post Decision | -0.010 | 0.161 | 0.160 | 0.171 | 0.171 |
| | (0.247) | (0.148) | (0.143) | (0.264) | (0.257) |
| Post Decision | -0.004 | -0.023 | -0.018 | -0.019 | -0.015 |
| * Yrs. Elapsed | (0.075) | (0.022) | (0.022) | (0.074) | (0.073) |
| Trend | -0.001 | 0.002 | -0.002 | 0.003 | -0.001 |
| | (0.062) | (0.007) | (0.006) | (0.062) | (0.061) |
| Observations | 1658 | 1658 | 1658 | 1658 | 1658 |
| $R^2$ | 0.632 | 0.909 | 0.828 | 0.680 | 0.629 |
| Adjusted $R^2$ | 0.611 | 0.904 | 0.818 | 0.662 | 0.608 |

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: 1977-2013.

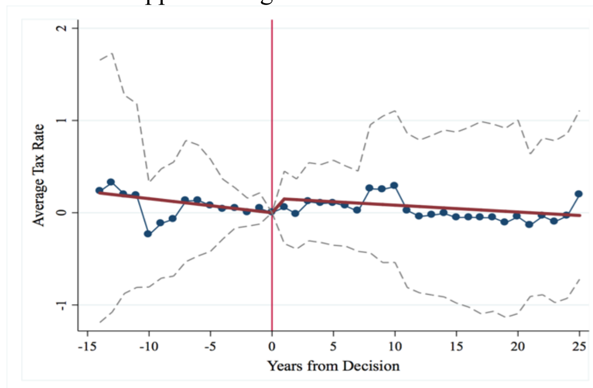
## Appendix Figure 1: Effect of Decision on Average Tax Rates of Married Filers

### 20th Percentile Income Earners

Appendix Figure 1A: Without Children

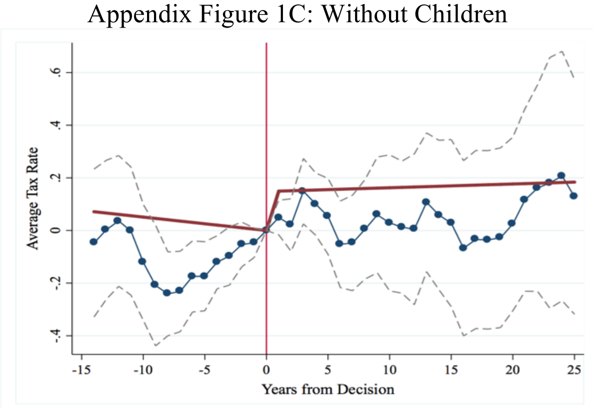



Appendix Figure 1B: With Child

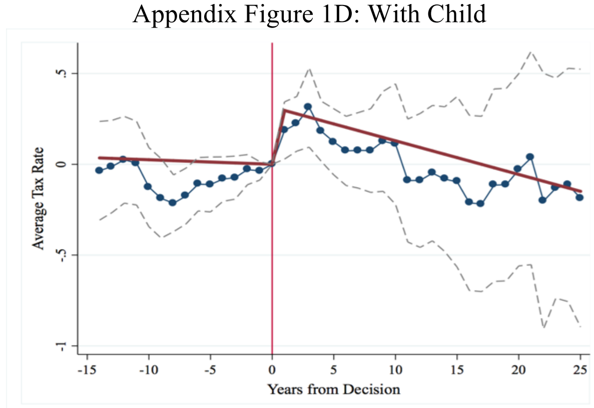



### 80th Percentile Income Earners

Appendix Figure 1C: Without Children

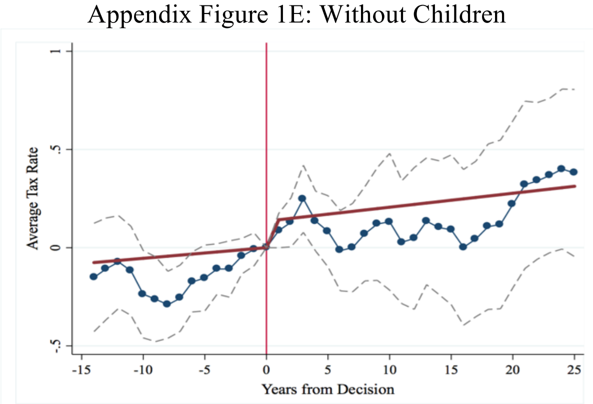



Appendix Figure 1D: With Child

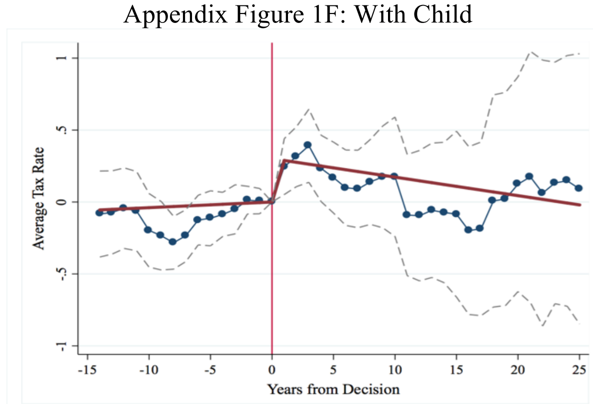



### 95th Percentile Income Earners

Appendix Figure 1E: Without Children




Appendix Figure 1F: With Child

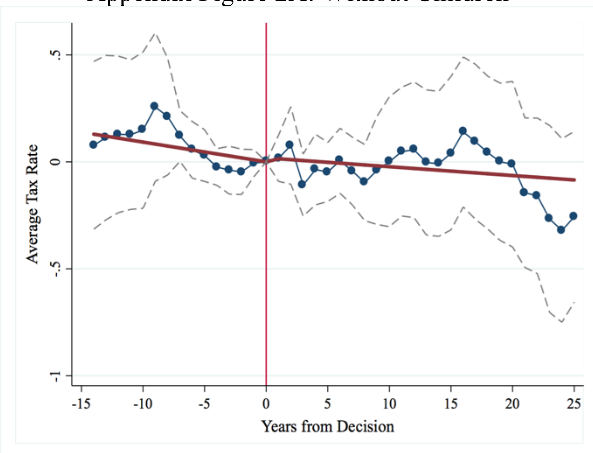



Note: The solid red line shows the estimates from the trend specification (equation 2). The blue line with dots shows the nonparametric specification (equation 3). 90% confidence intervals for the nonparametric regression are shown in gray dashes. Regressions include state and year fixed effects and are weighted by population. Years: 1977-2013.

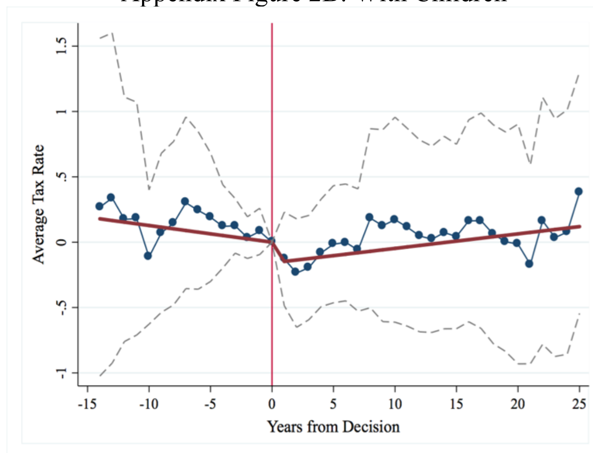
Appendix Figure 2: Effect of Decision on Differences in Average Tax Rates of Married Filers

20th Minus 80th Percentile Income Earners

Appendix Figure 2A: Without Children

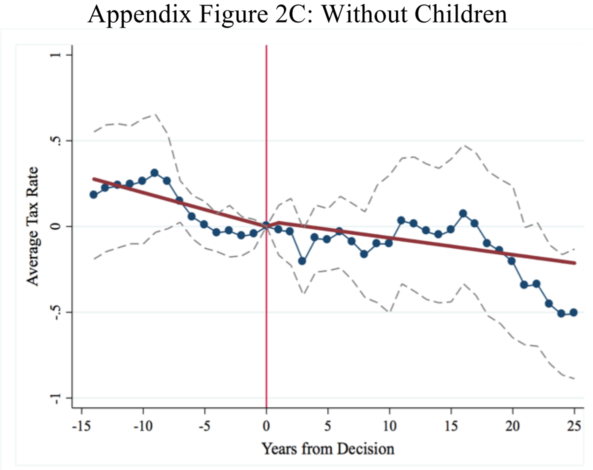



Appendix Figure 2B: With Children

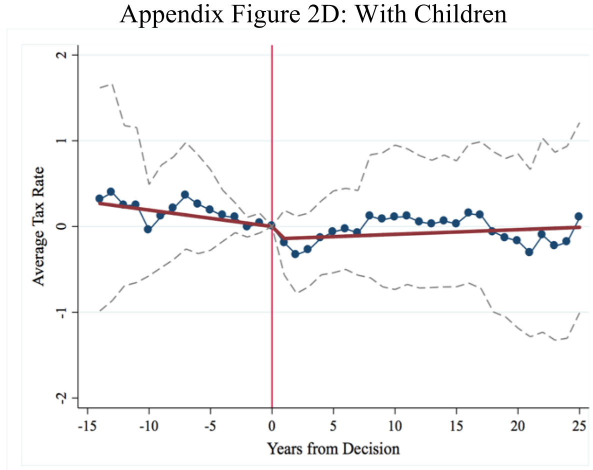



20th Minus 95th Percentile Income Earners

Appendix Figure 2C: Without Children



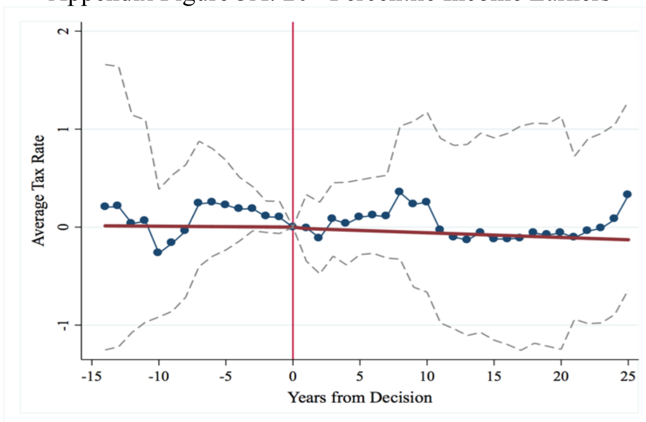

Appendix Figure 2D: With Children



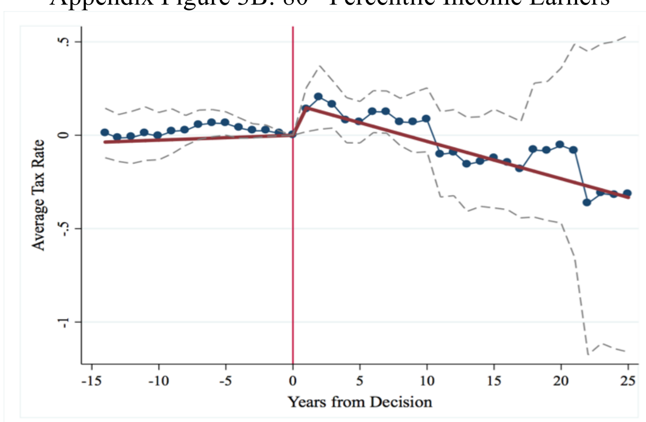

Note: The solid red line shows the estimates from the trend specification (equation 2). The blue line with dots shows the nonparametric specification (equation 3). 90% confidence intervals for the nonparametric regression are shown in gray dashes. Regressions include state and year fixed effects and are weighted by population. Years: 1977-2013.

Appendix Figure 3: Effect of Decision on Differences in Average Tax Rates Between Married Filers With and Without Children
(With Children Minus Without Children)

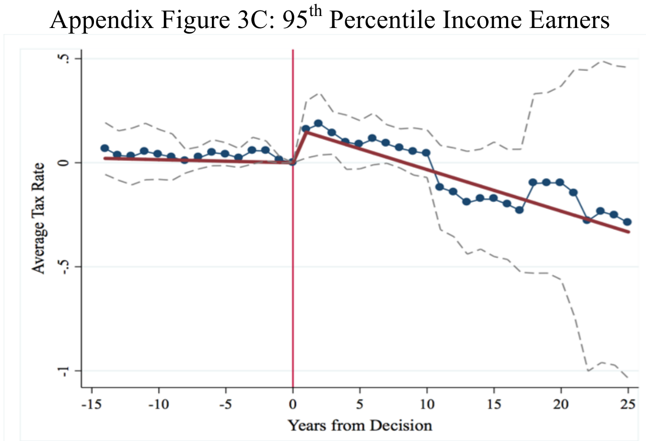
Appendix Figure 3A: 20th Percentile Income Earners




Appendix Figure 3B: 80th Percentile Income Earners



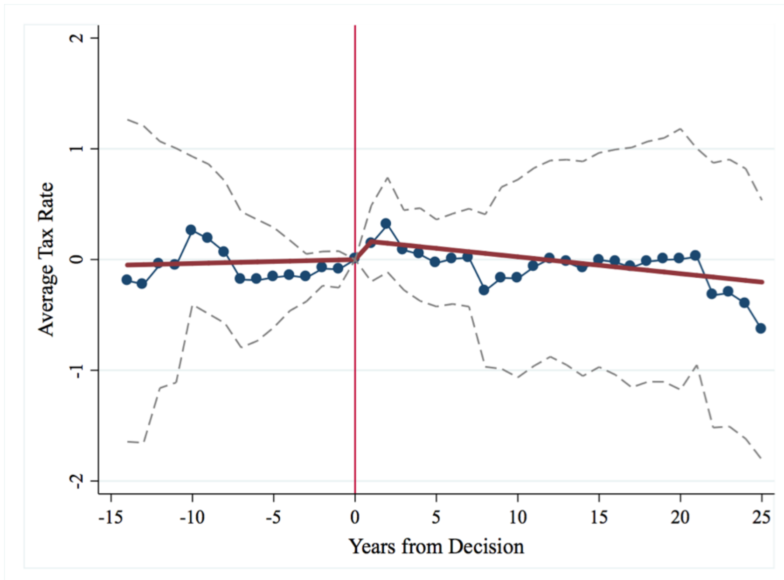

Appendix Figure 3C: 95th Percentile Income Earners



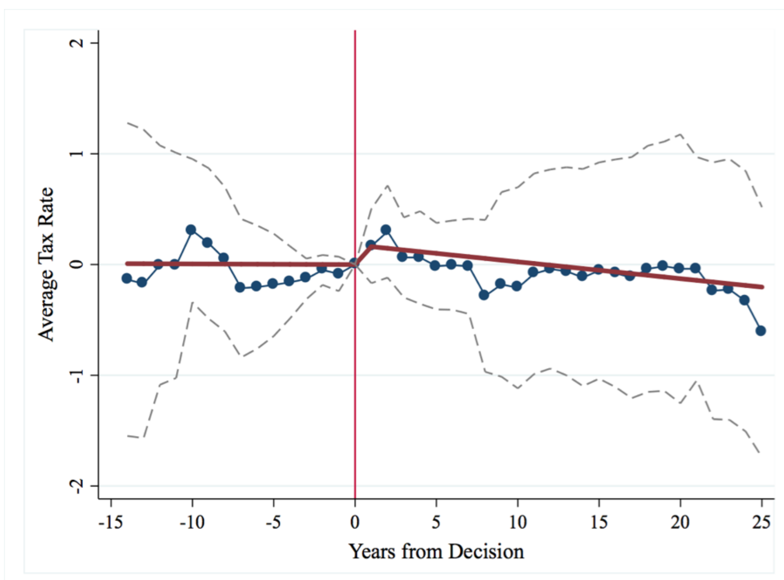

Note: The solid red line shows the estimates from the trend specification (equation 2). The blue line with dots shows the nonparametric specification (equation 3). 90% confidence intervals for the nonparametric regression are shown in gray dashes. Regressions include state and year fixed effects and are weighted by population. Years: 1977-2013.

Appendix Figure 4: Effect of Decision on Difference in Difference for Married Filers

Appendix Figure 4A: 20th Minus 80th Percentile Income Earners, With Children Minus Without Children



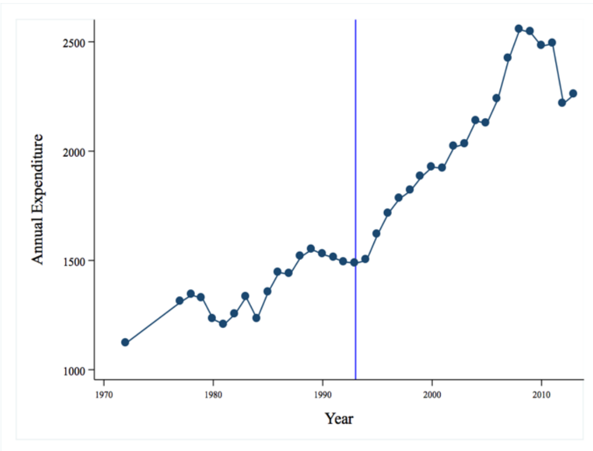

Appendix Figure 4B: 20th Minus 95th Percentile Income Earners, With Children Minus Without Children



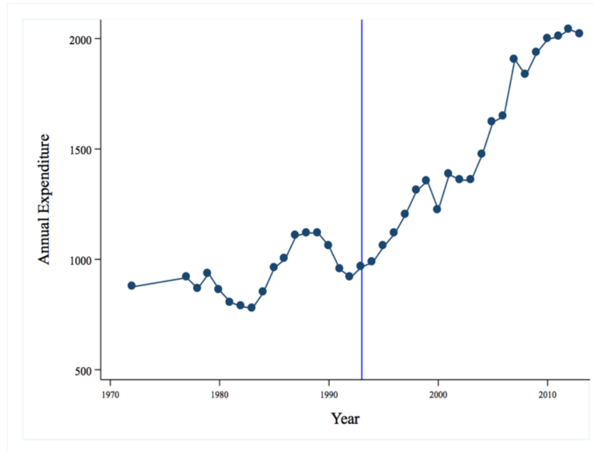

Note: The solid red line shows the estimates from the trend specification (equation 2). The blue line with dots shows the nonparametric specification (equation 3). 90% confidence intervals for the nonparametric regression are shown in gray dashes. Regressions include state and year fixed effects and are weighted by population. Years: 1977-2013.

Appendix Figure 5: Per-Capita K-12 Education Expenditure Over Time

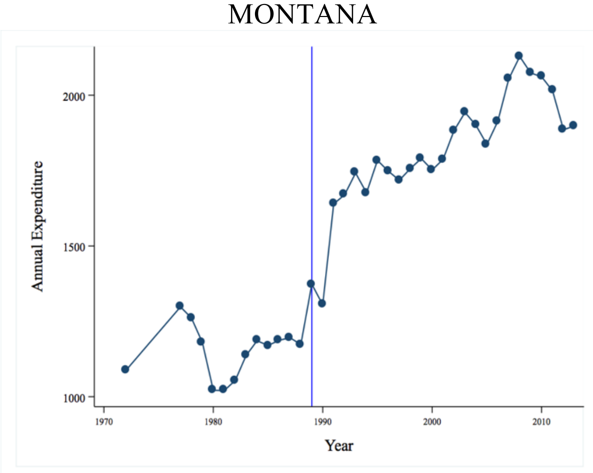
ALABAMA



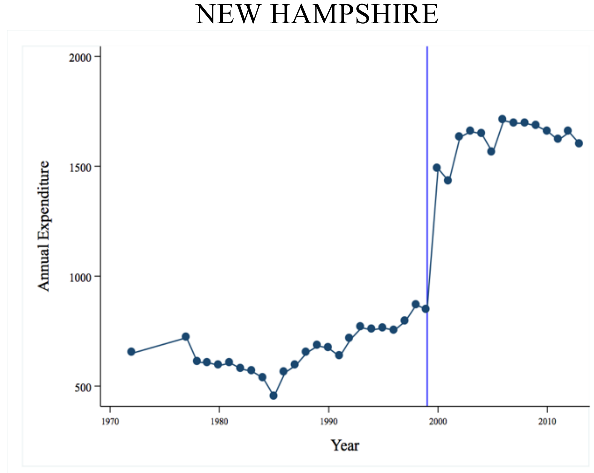

MASSACHUSETTS



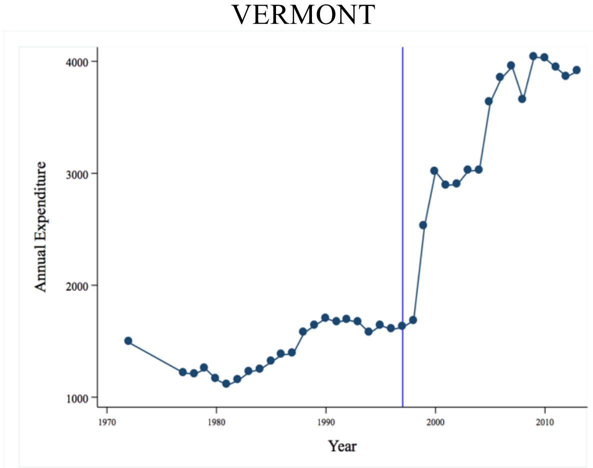

MONTANA




NEW HAMPSHIRE




VERMONT




Note: Per-capita K-12 education expenditure in real 2015 dollars. Decision-year shown using vertical blue line.