School Finance Reform and the Distribution of Student Achievement: Online Appendix

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Julien Lafortune
University of California, Berkeley
julien@econ.berkeley.edu

Jesse Rothstein
University of California, Berkeley
and NBER
rothstein@berkeley.edu

Diane Whitmore Schanzenbach
Northwestern University
and NBER
dws@northwestern.edu

Online Appendix A. Event database

Online Appendix Table A1 lists all of the events in our database, covering the period from 1990 forward. As noted in the text, we include both court orders and major legislative reforms. Columns of the table indicate how our list compares with those of Jackson, Johnson, and Persico (2016) and Corcoran and Evans (2015).

Online Appendix D, below, discusses in depth each case where our list differs from that of Jackson, Johnson, and Persico (2016).

Online Appendix Figure A1 shows which states have and have not had events since 1990.

Online Appendix B. Details of samples and empirical specifications

The primary sources for our outcome measures are the Common Core of Data Local Education Agency (LEA) finance survey (also known as the F-33), for finance outcomes, and the NAEP, for test scores. Each is matched to district mean household income from the 1990 School District Data Book (SDDB), a tabulation of Decennial Census data at the district level. Mean incomes pertain to all households in the district, with and without children and without regard to public school attendance.

Our analysis relies on collapsing the district- and student-level measures to summaries at the state-by-year level. We use three types of summaries: Means for districts in each quintile of family income, the difference in between the first and fifth quintile means, and slopes with respect to log district income. Our methods differ slightly among these; we describe them here.

Sample definitions

All of our samples exclude Hawaii and the District of Columbia, each of which has only one school district.

Our finance analyses exclude district-year observations with enrollment of fewer than 100 students. This removes 8% of district-year observations, with only 0.1% of total enrollment.

We make two additional exclusions aimed at reducing volatility in the per-pupil funding measures. Both total funding and enrollment can vary dramatically from year to year in a district, particularly in small districts, creating enormous swings in per-pupil revenues. We view this variability as likely to reflect measurement error; it is particularly problematic when it derives from large proportional swings in enrollment with more stable funding.

We begin by computing each district's average enrollment over our sample, as well as its average growth rate over our sample period. We exclude from our sample any district-year observation with enrollment (a) more than double the district's average enrollment; (b) more than 15% above or below the prior year or the subsequent year's enrollment; or (c) more than 10% above or below the district's constant-growth-rate trend. In addition, for any district for which more than one-third of annual observations are excluded under these criteria, we exclude all remaining observations as well. Exclusion (a) in particular likely leads us to exclude districts serving newly developed areas, but we are not confident that 1990 incomes are reliable measures of population resources in these districts in any case. Together, these exclusions capture 18% of district-year observations, with 8% of enrollment.

To address volatility in the numerator of our revenue and expenditure measures, we exclude as well district-year observations with per-pupil revenues (respectively, expenditures) greater than 500% or less than 20% of the unweighted state-by-year mean. Only 0.02% of district-year observations are excluded by these rules.

When analyzing mean teacher salaries and pupil-teacher ratios, each of which is somewhat noisily measured, we exclude the top and bottom 2% of districts (unweighted) in each state-year cell.

Finally, our NAEP analyses exclude students in charter schools.

For many purposes, it is useful to have a weight for each district that does not vary over time. We use the geometric mean of the district's enrollment in all available years for this.

Definition of income quintiles

The basis for our income quintile calculations is the finance sample, as defined above. Districts' quintile assignments are treated as fixed over time.

To construct our income quintile cutoffs, we sort districts in a state by their 1990 mean family income and compute the 20^{th}, 40^{th}, 60^{th}, and 80^{th} percentile of the state distribution of 1990 mean family income. These percentiles are based on the districts in our finance sample in 1994 (the first year in which complete CCD data are available), weighted by our stable enrollment count. Districts spanning the quintile cutoffs are assigned with partial weights to each of the relevant quintiles.

Quintile means

Quintile means of our finance measures are computed as weighted averages over the districts in each quintile, weighting each district by its average log enrollment and including only districts that meet the criteria outlined above. The specific districts included can vary slightly over time due to differences in the availability of the dependent variable.

Our NAEP quintile means are similar but weight districts by the sum of the NAEP student weights in the district. Only districts meeting the finance sample restrictions for the relevant year are included.

Income slopes

To construct state-by-year slopes of revenues with respect to district income, we estimate a separate regression for each state and each year. Explanatory variables are the log mean income of the district, based on the 1990 data, the district's log enrollment in that year, and indicators for whether the district is an elementary or a secondary district (unified districts are the excluded category). These regressions are weighted by our stable log enrollment measure and samples are defined as above. The coefficient on the log mean district income is extracted, along with its standard error.

NAEP score-income slopes are computed similarly, using NAEP data aggregated to the district-year-subject-grade level. Separate regressions are estimated for each state-year-subject-grade combination. The district-level regression is weighted by the sum of NAEP student weights in the district, and does not include district type controls. Standard errors account for the NAEP plausible values methodology, as discussed below.

Event study regressions

Once quintile means, between-quintile gaps, and income slopes are constructed at the state-year level, we estimate event-study regressions as described in the main text.

For finance outcomes, where we have a census of school districts, the event study regressions using quintile means as dependent variables are unweighted. For test score outcomes, our quintile mean event study regressions are weighted by the sum of the NAEP student weights within the grade-subject-quintile-year cell. When we examine quintile gaps in scores, we weight by the harmonic mean of the two quintile weights.

All event studies with income slopes as the dependent variable are weighted by the inverse sampling variance of the state-year slope.

NAEP plausible values

NAEP does not report a single test score for each student, but rather reports five plausible values, random draws from the student's posterior distribution. We average these five plausible values before computing quintile means or income slopes. Our estimated standard errors for the income slopes account for the contribution of the sampling from the posterior distribution, following NAEP guidance.

Jacob and Rothstein (2016) point out that the use of NAEP plausible values as dependent variables may create biases, as the measurement error in PVs is not classical. The bias depends on the degree to which the "conditioning variables" in the NAEP model can predict the relevant explanatory variables in the research regression, but likely takes the form of attenuated treatment effects.

Jacob and Rothstein (2016) discuss methods for obtaining unbiased estimates from a single NAEP. We are not aware of methods for avoiding bias in analyses that pool across many NAEP samples. We have verified that cross-sectional regressions of NAEP PVs on measures of district finance, both across and within states, are minimally biased relative to unbiased marginal maximum likelihood estimates based on underlying item responses, and therefore conclude that the use of PVs is unlikely to meaningfully bias our results.

Online Appendix C. Robustness and additional analyses

Online Appendix figures and tables present numerous additional results.

Figure A2 shows the gap in average NAEP scores between the lowest- and highest-income districts, averaged separately across states that did and did not have reform

events since 1990. This is analogous to Figure 2 of the main paper. Reform states had larger test score gaps in 1990, but while the gaps in non-reform states were stable from 1990-2011, the gap shrunk steadily in reform states.

Figure A3 displays coefficients from parametric (equation 2) and non-parametric (equation 3) event study regressions on mean total revenues per pupil (in 2013 dollars), including revenues from state, local, and federal sources. Dependent variables are mean total revenues per pupil (panel A), mean total revenues per pupil in the lowest income quintile of districts (panel B), mean total revenues per pupil in the highest income quintile of districts (panel C), and the difference in mean total revenues per pupil between districts in the bottom and top income quintile in the state (panel D). Parametric estimates correspond to those reported in panel D of Table 3. Mean total revenues per pupil increased significantly following reform events (panel A), with little sign of pre-trends. Revenue increases were concentrated in districts in the lowest income quintiles (panel B), whereas districts in the top income quintiles (panel C) saw only small increases. Thus, the difference in mean total revenues between districts in the bottom and top income quintiles in a state increased significantly following reform events (panel D). All of these effects persisted essentially unchanged for at least 10 years following the events.

Figure A4 plots our event study estimates of impacts on total revenues and mean test scores in each of the five quintiles, along with 95% confidence intervals. Estimates are consistent with effects on test scores that are proportional to effects on revenues across district groups, though the confidence intervals are wide.

Table A2 presents an event study analysis for state-level budgets, on both per-capita and per-pupil bases. Our one-parameter specification (equation 1) is used. Events are associated with sharp increases in both total expenditures and total revenues, though the latter are imprecisely estimated and not significant. There is some indication that non-education expenditures rose following reforms, but estimates are imprecise and this result is not robust. In any event, it does not appear to be concentrated in health and welfare budget items, which seem most likely to confound our estimates. The impacts on total per-pupil education expenditures are larger than in our district-level analyses, though confidence intervals are quite wide. Insofar as this is real effect, it may indicate that some of the new state expenditures involve re-labeling existing funds rather than allocating new funds to education.

Table A3 presents Card and Payne's (2002) analysis of the effect of SFRs on the slope of district revenues with respect to district income. Card and Payne use the income level, in dollars, rather than log income as the explanatory variable in this slope calculation. They compute a single long-difference of this slope for each state, from 1977 to 1992, and regress it on indicators for plaintiff and defense victories in court cases. We construct a similar slope with respect to income levels, and a similar 1990-2012 long difference. Columns 2 and 5 report regressions of this on an indicator for a post-1990 event. Columns 3 and 6 report estimates of our one-parameter event study model. Our estimates of the impacts of court rulings are

directionally similar but somewhat larger than in Card and Payne (2002). We cannot rule out equal effects.

Table A4 reports event study estimates where the dependent variable is the slope of various demographic characteristics of a district's students with respect to district log income (panel A) or the between-quintile gap in mean demographic characteristics (panel B). Log mean income, used in columns 1-2, is measured in 1990, 2000, and 2011. (The slope of log mean income in 1990 with respect to a districts 1990 log mean income is by construction 1 in every state, but this slope can vary in subsequent years.) Minority and free lunch shares, used in columns 3-6, are measured in every year, though free lunch data are missing for some states and years. In five of six cases, we find no indication of an effect of SFR events on the level or trend of demographic composition. The one exception is the low- vs. high-income district gap in student free lunch share, where we find that the relative prevalence of poor students in low-income districts may have declined slightly following SFRs. The estimates are imprecise, however, and the point estimates are quite small: They indicate that the relative free lunch share in low-income districts might have fallen by 3-5 percentage points over the decade following an SFR, far too little to account for the 0.1 standard deviation effect that we find.

Table A5 reports the share of students of various characteristics who are in districts in each quintile. Rows sum to 1. Minority and low-income students are disproportionately represented in bottom-quintile districts, but there are substantial shares in even the highest-income districts.

Table A6 reports event study regressions where the dependent variable is the difference in the revenues of the district attended by the average black (or free lunch, or low scoring) student and the revenues of the district attended by the average white (or non-free-lunch, or high scoring) student. Estimates indicate that the average black student in a state was exposed to less than $200 in additional per pupil revenue relative to the average white student in the same state following an SFR, and that free lunch students got no additional revenues on average.

Table A7 further explores the potential impact of demographic changes on our test score results. We decompose test scores into two components, one capturing demographics and one not, and estimate separate SFR effects on each. Specifically, we estimate an individual-level regression of test scores on student demographic characteristics, pooling NAEP data across years for each grade-subject pair and including year fixed effects. We then construct separate achievement-log district income gradients from the fitted values (excluding the fixed effects) for this regression, representing student characteristics that would be affected by SFRs only through changes in sorting, and from the residuals. Table A7 presents results of our event study analyses of these gradients. We present two decompositions: The first panel uses only race and gender, which are consistently available in each NAEP wave, along with school means of these. The next uses additional covariates, parental education and free lunch status, that are less consistently available,

including indicators for years in which each is unavailable. The first set of variables explains 22% of the variance in student test scores (net of the subject-grade-year effects), while the second set explains 28%.

We find no evidence that reforms affect the demographic component of our test score progressivity measures. Point estimates are less than half the size of our overall test score impacts, and are never significantly different from zero. By contrast, estimated effects on the residual component of test scores are all significant, and about two-thirds the size of the overall impacts. Thus, while we cannot rule out small effects of SFRs on student sorting, the robustness of effects on the residual component supports our interpretation that our results primarily reflect changes in educational production in low-income school districts.

Online Appendix D. Monte Carlo analyses of event studies with multiple events

Our analysis differs from many event study analyses in that states in our sample often experience multiple potential events. Our analytical strategy is predicated on the view that states typically implemented only one actual finance reform during our sample, and that other apparent events represent political and legal maneuvering with little consequence for school spending or student achievement. Accordingly, we develop a procedure for selecting a single potential event in each state and estimate our models as if that were the only event.

In Table 7, however, we present two additional approaches that would be more appropriate if states actually had multiple events. The first uses all identified potential events, creating separate copies of the state's time series for each and analyzing them independently (though clustering standard errors by state), while the second uses only the first court order in a state and ignores any subsequent events (as well as any prior legislative events).

In this Online Appendix, we present Monte Carlo analyses of the performance of these two estimators under two data generating processes (DGPs). In our first DGP, all events have equal effects; in the other DGP, one (randomly chosen) event in each state has an effect, while others do not. Other aspects of the DGP are quite simple: The distribution of potential events is as in the true data, and errors are independent over time within states. (We do not use these Monte Carlos to study our preferred event selection approach, as the performance of that approach is highly dependent on the specific time series properties of the errors.)

Specifically, suppose that state s has J_s potential events that take place in $t = \{t_{s1}, t_{s2}, \dots, t_{sJ_s}\}$. Our first DGP is

$$y_{st} = \frac{1}{J_s}\sum_{j=1}^{J_s} 1\left[t > t_{sj}\right] + \epsilon_{st}.$$

That is, the outcome rises, permanently, by $\frac{1}{J_s}$ following each event. In the second DGP, we choose for each state a random integer j_s^* between 1 and J_s; the DGP is then

$y_{st} = 1\left[t > t_{sj_s^*}\right] + \epsilon_{st}$.

Here, outcomes rise by a full unit following the randomly chosen true event, with no change following other apparent events. Thus, in each DGP the total effect across all events in the state is 1.

We model ϵ_{st} as i.i.d. standard normal random variables. We use the actual sequence of measured events in each state for $\{t_{s1}, t_{s2}, \ldots, t_{sJ_s}\}$. 53% of states have events; among those with at least one, the average state has 2.5 events, and 69% have more than one.

We simulate each DGP 5000 times. We apply our two estimators, one selecting just the first event and one using all events with stacked panels for each, to each simulated sample. In each case, we use the same specifications as are applied to actual data in Table 7, with state(-by-event-copy) and year fixed effects and a single jump following the event, but no state-specific trend terms.

Table A8 shows the results. The two rows correspond to the two DGPs, while the two columns show the two different estimators. The first thing to notice is that the rows are nearly identical: Each estimator yields very similar results whether the DGP includes multiple true events or just one. Second, the first column, in which the estimator is based solely on the first apparent event, yields estimates of the event effect of about 0.79. This is somewhat attenuated from the total event effect, but the bias is not severe. Intuitively, the post event period in this estimator includes some years in which event effects have not yet kicked in (or have only partially kicked in), reducing the contrast with the pre-event period.

In the second column, the estimator that stacks all potential events in a state yields somewhat lower estimates, around 0.58. Here, the intuition is that for any given potential event, some of the pre-event observations reflect the impact of earlier events, and some of the post-event observations do not reflect the impacts of events yet to come. Again, this attenuates the effect.

In general, the pattern of results in Online Appendix Table A8 mirrors that in Table 7. Estimates from the initial event and stacked specifications are both smaller than those from our preferred specification, which focuses on a single event selected as the true one, and (in most results) the attenuation is greater for the stacked specification than for the initial event specification. Overall, we conclude that the three approaches are all likely to work reasonably well in our setting, but that the more "hands off" estimators are likely to understate the true effects of events.

Online Appendix E. Reconciliation of school finance reform tabulations

The literature on school finance reforms has been plagued by a lack of authoritative tabulations of court-ordered reforms, with substantial discrepancies between the tabulations used by different authors. Our tabulation, too, differs from all previous listings.

In an effort to provide clarity to the literature going forward, in this Online Appendix we discuss every case between 1990 and 2011 where our tabulation of court-ordered school finance reforms differs from that of Jackson, Johnson, and Persico (2016; hereafter *JJP*). Many of these discrepancies reflect judgment calls. We have estimated our main results with a number of variants of the event sample, and in general have found little sensitivity of the results; we nevertheless present the basis for our preferred tabulation for completeness.

The states and years for which the two tabulations disagree are:

- Alabama, 1993
- Arizona, 2007
- Connecticut, 1995 & 2010
- Idaho, 1993 & 1998
- Maryland, 1996 & 2005
- Michigan, 1997
- Montana, 1993 & 2008
- New Hampshire, 2006
- New Jersey, 1991, 1998 & 2000
- New Mexico, 1998 & 1999
- Oregon, 2009
- South Carolina, 2005
- Texas, 2004
- Washington, 1991, 2007 & 2010

This includes only cases in scope for both lists but coded differently. This in particular means that we do not discuss our tabulation of legislative school finance reforms, as these are out of JJP's scope. For each state, we discuss only the events where the two tabulations disagree; see Online Appendix Table A1 for a full listing of events in each state.

Alabama[1]

1993: JJP court order; Lafortune-Rothstein-Schanzenbach (LRS) no event

In 1993's *Alabama Coalition for Equity (ACE) v. Hunt*, the public school funding system was found inequitable, on both adequacy and equity grounds, by a lower court, and a remedy order was issued. The remedy negotiated among the parties and ordered by the court, in *ACE v. Folsom*, included equitable and adequate funding,

[1] Case histories from http://schoolfunding.info/2012/01/school-funding-cases-in-alabama/; http://www.encyclopediaofalabama.org/article/h-2045.

and in addition also covered performance-based education, professional development, early childhood programs, and inclusive special education— all to be fully funded within six years. No educational reform package made it through the legislature before the 1994 election season, and education reform became an important issue in the gubernatorial campaign, with incumbent governor Folsom promising reform and compliance with the remedy order and his opponent (and eventual winner) Forrest "Fob" James vowing to fight what he described as a usurpation of executive and legislative powers. Upon appeal, the Alabama Supreme Court decided in 1997 that while schools were inadequately funded, it would decline to issue a remedy order, leaving the funding system unchanged. Because the lower court was overturned and no school finance legislation was passed, we do not code this event as a school finance reform.

Arizona[2]
2007: JJP court order; LRS no event

Flores v. Arizona was first decided in 1992 under Federal law, in the United States District Court for the District of Arizona. The plaintiffs claimed that the state failed to adequately fund programs for English language learners (ELLs). In 2000, the district court found that the state's method and level of funding ELL programs was "arbitrary and capricious" and ordered that the level of state funding for ELL programs bear a rational relationship to the cost of those programs. The parties reached an agreement in 2002, and the court ordered a costing-out study. The state's repeated failure to comply led to a December 2005 order and daily fines that mounted to $21 million before the state enacted additional funding in early March 2006. In August 2006, the Ninth Circuit Court of Appeals, in *Flores v. Rzeslawski*, vacated the 2005 district court judgment and remanded the case so the district court could hold new hearings to determine whether circumstances had changed and required modification of the 2000 court order.

In March 2007 Judge Raner Collins of the U.S. District Court for the District of Arizona ruled that Arizona was in violation of the Equal Educational Opportunities Act (EEOA) by under-funding programs directed towards English learners, invalidating HB 2064, the funding formula passed by the Arizona legislature in response to the court's earlier decision. Judge Collins ordered the state to comply with the order by the end of the 2007 legislative session, but when the legislature failed to do so the judge issued a contempt order. The Ninth Circuit Court of Appeals upheld the ruling in 2008.

In September 2008, the defendants petitioned the U.S. Supreme Court to review the 9th U.S. Circuit Court of Appeals' holding. In 2009's *Horne v. Flores*, 129 S. Ct. 2579 decision, the Supreme Court reversed and directed the District Court to examine

[2] Case history from http://www.clearinghouse.net/detail.php?id=11194; http://www.schoolfunding.info/states/az/lit_az.php3.

several specific factors, including whether non-compliance was statewide.

On March 28, 2013, the plaintiff's statewide claims were dismissed, requiring district-by-district analysis instead. The dismissal was upheld by the Court of Appeals in June 2015.

Because the 2007 ruling was a Federal district court order that the state never complied with and was subsequently overturned by the Supreme Court, we do not code this event as a school finance reform.

Connecticut[3]
1995: JJP court order; LRS no event
2010: JJP court order; LRS no event

In 1996, the State Supreme Court ruled in *Sheff v. O'Neill* (coded by JJP as 1995) that the separation of suburban and Hartford students violated the segregation clause in the Connecticut Constitution, and ordered the State Legislature to take necessary measures to integrate schools and to provide equal educational opportunity to all children. This resulted in a plan by the 1997 State Legislature geared at promoting voluntary school desegregation and magnet schools. Though plaintiffs made adequacy-based arguments, the ruling and subsequent legislation focused on desegregation and not school funding.

In 2010, the Supreme Court ruled in *Coalition for Justice in Education Funding, Inc. v. Rell* that the state's constitution guaranteed all students an adequate education. It did not order changes in school finance, however, but rather sent the case back to a trial court to determine whether the appropriate standard had been met. As of 2013, the case was still pending. We therefore do not code it as a school finance reform order.

Idaho[4]
1993: JJP no event; LRS court order
1998: JJP court order; LRS no event

In the 1993 ruling on *Idaho Schools for Equal Educational Opportunity v. Evans* (ISEEO) (850 P.2d 724), the Idaho Supreme Court found that the state constitution required adequate (but not equitable) school spending. In 1994, the

[3] Case history drawn from https://www.jud.ct.gov/external/news/sheff.htm; http://connecticuthistory.org/sheff-v-oneill-settlements-target-educational-segregation-in-hartford/#sthash.6QnsSrbm.dpuf; https://scholar.google.com/scholar_case?case=10572221569547466633&q=Sheff+v+o+neill+199&hl=en&as_sdt=400006; http://schoolfunding.info/2012/01/school-funding-cases-in-connecticut-2/.
[4] Case history from http://www.educationjustice.org/states/idaho.html; https://nces.ed.gov/edfin/pdf/lawsuits/ISEEO_v_idaho.pdf.

legislature passed Senate Bill 1560 which revised the state funding formula in regard to teacher salaries, allocating more than $90 million to public schools fund this change. We code the 1993 decision as a court-ordered school finance reform.

After the legislative changes, the trial court declared the lawsuit moot, but this decision was overturned by the state Supreme Court, which concluded that whether a "thorough education" was being provided to students was still in question. In 1997, the trial court again dismissed the plaintiffs' claim. The state Supreme Court reversed in part in 1998, in *ISEEO v. State* (976 P.2d 913), remanding the facilities and capital funding portion of the case. The court held that "the Legislature has the duty to provide a means for school districts to fund facilities that offer a safe environment conducive to learning." In 2000 and 2001, the legislature passed minor facilities measures that help property-poor districts, but plaintiffs argued these measures were insufficient. We do not code this as an independent school finance reform, due in part to its limited scope.

Maryland[5]
1996: JJP no event; LRS court order
2005: JJP court order; LRS no event

The ACLU and Baltimore City alleged that Baltimore's students were not receiving an adequate education. In a 1996 summary judgment decision in the consolidated *Bradford v. Maryland State Board of Education* case, the trial court agreed, though the cause of the inadequacies was in dispute. The parties entered into a settlement that provided an increase in state funding for the Baltimore City Public Schools for the next five years. During this period, the "Thornton" Commission on Education Finance, Equity, and Excellence was established to address statewide adequacy in funding. We code the 1996 court order as a school finance reform, in part because Baltimore is such a large district.

In 2004, the Baltimore schools had an accumulated budget deficit of $58 million. In response to a new state law requirement, it cut its budget drastically, and planned a two-year paydown of the deficit. The ACLU returned to court in Bradford, trying to restore funding to Baltimore schools and stop cuts to academic programs impacting students. The Circuit Court ruled that the budget cuts had resulted in reduced educational opportunity to students and that $30 million to $45 million in funds should be restored, preferably with additional revenue from the city and state. The State appealed, arguing that education funding levels are outside court jurisdiction. In 2005, Maryland's highest court ruled against the State's attempt to strike the lower court order, but did not overturn the state law for the deficit paydown. As a result, the additional funding awarded under the Thornton commission would proceed, but since no additional funds were ordered at this time we do not code this as a separate court ordered school finance reform.

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[5] Case history drawn from http://www.schoolfunding.info/states/md/lit_md.php3; http://www.aclu-md.org/uploaded_files/0000/0173/bradford_summary.pdf.

Michigan[6]
1997: JJP court order; LRS no event

Durant v. State of Michigan ("Durant I"), was filed in 1980 and decided in 1997. The major issue was state funding for special education mandates. In its ruling on *Durant I*, the Michigan Supreme Court unanimously held that state government had not properly financed three state-imposed mandates: special education, special education transportation and a school lunch program. The court split awarded monetary damages to local school districts to repay past costs of mandates. Due to the limited nature of the lawsuit, we do not code this as a school finance reform.

Montana[7]
1993: JJP court order; LRS legislative event but no court order
2008: JJP court order; LRS no event

Montana's Supreme Court ruled in *Helena Elementary School District No. 1 v. State* in 1989 that the state's school finance system was unconstitutional. This is outside the scope of our sample. The legislature responded in 1989, then overhauled the formula again in 1993 via House Bill 667. Earlier that year, the *Montana Rural Education Association v. State* case was tried but not decided. Following the legislative action, but still in 1993, the state's First Judicial District Court for Lewis and Clark County ruled that the case was moot due to the new law. It permitted the plaintiffs to argue that the new law remained unconstitutional, but to our knowledge the case ended then. JJP code this as a court order; we code House Bill 667 as a legislative action, but do not code the case as a court order.

In 2005, in *Columbia Falls Elem. Sch. Dist. 6 v. State*, the trial court found that the state was not providing a "quality" education as mandated by the constitution, and in particular it had violated the provision of the state constitution requiring the state to commit to preserve the cultural heritage of American Indians. JJP and LRS each code this as a court-ordered school finance reform. A subsequent 2007 legislative reform (which we code as a legislative event) made substantial changes to the school finance system in light of this ruling.

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[6] Case history from https://www.mackinac.org/8568.
[7] Case history from http://www.mqec.org/school-funding-history/; http://schoolfunding.info/2011/12/school-funding-cases-in-montana/; https://static1.squarespace.com/static/53ab63e1e4b0cb2b67560152/t/55ef5b40e4b064e46223df9f/1441749824419/CF-Decision-II.pdf; http://static1.squarespace.com/static/53ab63e1e4b0cb2b67560152/t/55ef3dcbe4b0adc4e323efbc/1441742283987/Rural_Ed_Assoc-v-State_District_Order_re_Mootness_Issue_1993.pdf; http://leg.mt.gov/content/committees/interim/2005_2006/edu_local_gov/minutes/02242006exhibits/ELG02242006_ex5.pdf.

Suit was filed in 2008 seeking supplemental monetary relief to help districts avoid funding shortfalls in 2009. In December 2008, the district court declined to award any supplemental relief, so we do not code 2008 as a court-ordered school finance reform.

New Hampshire[8]
2006: JJP court order; LRS no event

In September 2006 in *Londonderry School District v. State* the New Hampshire Supreme Court ordered the state to define a "constitutionally adequate education" by June 2007. After recounting the failure to establish this definition in several previous cases (both JJP and LRS code court orders in 1993, 1997, and 1999), the court concluded that it is willing to defer to the legislature one more time, and that "in the absence of action…, a judicial remedy is not only appropriate, but essential" in order to vindicate the constitutional rights of New Hampshire's students.

In the 2006 decision, the Court ordered the State to define a "constitutionally adequate education" by the end of the 2007 legislative session, but deferred to the legislature for appropriate action. We code the 2008 legislative action but not the 2006 court order.

New Jersey[9]
1991: JJP court order; LRS no event
1998: JJP no event; LRS court order
2000: JJP no event; LRS court order

New Jersey's school finance litigation history is extremely complex, with a decades-long exchange between the legislature and the courts. There have been many, many rulings in the *Abbott v. Burke* case in particular. The court ruled in *Abbott II* in 1990 (counted by both JJP and LRS) that state funding statutes failed to ensure adequate funding in the low-wealth "Abbott districts", and noted that students in these districts need programs and services beyond those provided to students in wealthier districts. In response, the legislature passed the Quality of Education Act of 1990 (QEA; LRS code this as a legislative event).

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[8]Case history from https://www.nhbar.org/publications/display-journal-issue.asp?id=365.

[9] Case histories drawn from http://www.schoolfunding.info/resource_center/legal_docs/New%20Jersey/Abbott%20Decisions/Abbott-SupremeCourt_May1997.PDF; http://www.schoolfunding.info/resource_center/legal_docs/New%20Jersey/Abbott%20Decisions/Abbott-SupremeCourt-May-1998.PDF; http://www.schoolfunding.info/resource_center/legal_docs/New%20Jersey/Abbott%20Decisions/Abbott-SupremeCourt-Feb-2002.PDF.

In 1991, the plaintiffs applied to the court to declare the QEA unconstitutional. The court declined to hear the motion at that time. JJP code this as a court order, but we do not. The court did find the QEA unconstitutional in the 1994 *Abbott III* ruling; both JJP and LRS count this event.

In 1998's *Abbott V* ruling, the court required the state to increase funding to ensure parity in per-pupil expenditures between the Abbott districts and the average of the state's 110 successful suburban school districts, and directed the state to conduct a study to determine the needs of Abbott students and the programs necessary to meet those needs. Based on the State's study, the court ordered additional remedial measures for the Abbott children, including preschool for all three- and four-year olds, adequate school facilities, and "supplemental" programs. We code this as a school finance reform, though JJP do not.

After plaintiffs brought another motion alleging the state did not comply with the *Abbott V* ruling, the court provided (in the 2000 *Abbott VI* ruling) more detail on the preschool requirements, including substantive educational standards, certified staff, and a maximum student/teacher ratio of 15:1. We code this as a school finance reform; again, JJP do not.

New Mexico[10]
1998: JJP court order; LRS no event
1999: JJP no event; LRS court order

In 1998, a number of districts brought a capital funding/facilities suit, *Zuni School District v. State,* CV-98-14-II (Dist. Ct., McKinley County Oct. 14, 1999), claiming that the funding system for capital items was unconstitutional. The trial court granted partial summary judgment in favor of plaintiffs and ordered the state to "establish and implement a uniform funding system for capital improvements . . . and for correcting existing past inequities."

The case was filed in 1998 but decided in 1999. JJP code it as a 1998 event, but we code it as a 1999 event based on the decision date.

Oregon[11]
2009: JJP court order; LRS no event

In January 2009, the Oregon Supreme Court found in *Pendleton School District 16R v. State* that the legislature had, in violation of a 2000 constitutional amendment, failed to fund the Oregon public school system at a level sufficient to meet the quality education goals established by law. However, it concluded that the state

[10] Case history from http://www.schoolfunding.info/states/nm/lit_nm.php3; https://nces.ed.gov/edfin/pdf/lawsuits/Zuni_v_%20nm.pdf; http://ielp.rutgers.edu/resources/New_Mexico.
[11] Case history from http://www.educationjustice.org/states/oregon.html.

constitution did not give the court authority to issue an injunction requiring the state to provide sufficient funding to reach those goals. Because the court ruled that the law was not judicially enforceable, and no subsequent legislative actions were taken, we do not code this event as a school finance reform.

South Carolina[12]
2005: JJP court order; LRS no event

In 1999, in *Abbeville County Sch. Dist. v. State*, the South Carolina Supreme Court held that plaintiffs had a valid claim under the state constitution's education clause, interpreted the clause to mean that the legislature must provide children with a "minimally adequate education," and remanded the case for trial. The lower court ruled in 2005 that the state's failed to meet its constitutional requirement by inadequately providing early education programs, but ruled against plaintiff claims requesting relief regarding school buildings and quality teaching. Because the court did not order substantial school finance reform, we do not code a 2005 event.

Both plaintiffs and defendants appealed to the South Carolina Supreme Court, which heard oral argument in 2008 and again in 2012. In 2014, the state supreme court held the state's school funding unconstitutional, declaring that "South Carolina's education funding scheme is a fractured formula denying students ... the constitutionally required opportunity." The court explained that the resources provided failed to produce sufficient educational opportunities. The court explicitly refrained from mandating how the state should remedy the system, but ordered the parties to work together to present a new funding system to the court "within a reasonable time." The 2014 court order meets our definition of a court-ordered school finance reform, but is outside of our sample period so is not included in our tabulation (or in JJP's).

Texas[13]
2004: JJP court order; LRS no event.

A trial court found in *West Orange-Cove Consolidated ISD v. Nelson* (2004) that the Texas school finance system failed to provide "an adequate, suitable and efficient education system" as required by the state constitution, and additionally found the state property tax to be unconstitutional.

In 2005, the state Supreme Court ruled in *Neeley v. West Orange-Cove Indep. Sch. Dist.* that the state property tax was unconstitutional, but held that despite funding inequities the state's education finance system did not violate the constitutional adequacy, efficiency, and suitability requirements. The court wrote that the school finance system displayed deficiencies that could in time render it unconstitutional

[12] Case history from http://www.educationjustice.org/states/southcarolina.html.
[13] Case history from http://caselaw.findlaw.com/tx-supreme-court/1153227.html; http://www.schoolfunding.info/states/tx/McCown.pdf.

under the education article. Because the supreme court did not order reform's, we do not include this case.

Washington[14]
1991: JJP court order; LRS no event
2007: JJP court order; LRS no event
2010: JJP no event; LRS court order

Seattle School District v. State, also known as *Seattle II*, was a 1983 trial court ruling following up on the 1978 *Seattle I* decision that prompted an overhaul of the school finance system and the introduction of the Basic Education Act. *Seattle II* expanded the definition of "basic education" in the state to include special education, and bilingual and remedial programs. The state did not appeal, and the legislature amended the school finance system to include funding for these programs. JJP date this case to 1991. To our knowledge, it occurred in 1983, so does not fall into our sample period.

In *Federal Way Sch. Dist. v. State*, filed in 2006, plaintiffs alleged that the state funding system failed to amply fund education in all school districts and was unconstitutional. In 2007, Judge Michael Heavey held in favor of plaintiffs, finding that the State's method of providing salary funding was unconstitutional. The state Supreme Court, however, issued a narrower ruling in 2009 that a "uniform system" of education governs educational content, teacher certification, instructional hour requirements and the assessment system, but does not require uniform funding of staff salaries. The court did not rule on whether the plaintiffs had "ample" funds under the state constitution. Because the 2009 Supreme Court ruling did not involve finances, we do not code this as an event.

McCleary v. State, filed in 2007, argued that although the state had developed standards for a constitutional "basic education," it was not fully funding that education. In 2010, the Superior Court held that the state funding system was unconstitutional because it neither determined the cost of nor provided the resources needed for a basic education for all children in the state. The court ordered the state to fund a constitutionally adequate education, using stable and dependable state sources. In response, the legislature enacted legislative reforms, and in early 2012 the Washington Supreme Court affirmed the Superior Court ruling. We code the 2010 event, as the legislature acted on it without waiting for it to be upheld by the Supreme Court.

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[14] Case history drawn from
https://www.courts.wa.gov/opinions/pdf/843627.opn.pdf;
http://digitalcommons.law.seattleu.edu/cgi/viewcontent.cgi?article=2290&context=sulr;
https://www.courts.wa.gov/opinions/pdf/843627.opn.pdf;
http://www.schoolfunding.info/states/wa/lit_wa.php3.

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Card, D., & Payne, A. A. (2002). School finance reform, the distribution of school spending, and the distribution of student test scores. *Journal of Public Economics*, *83*(1), 49-82.

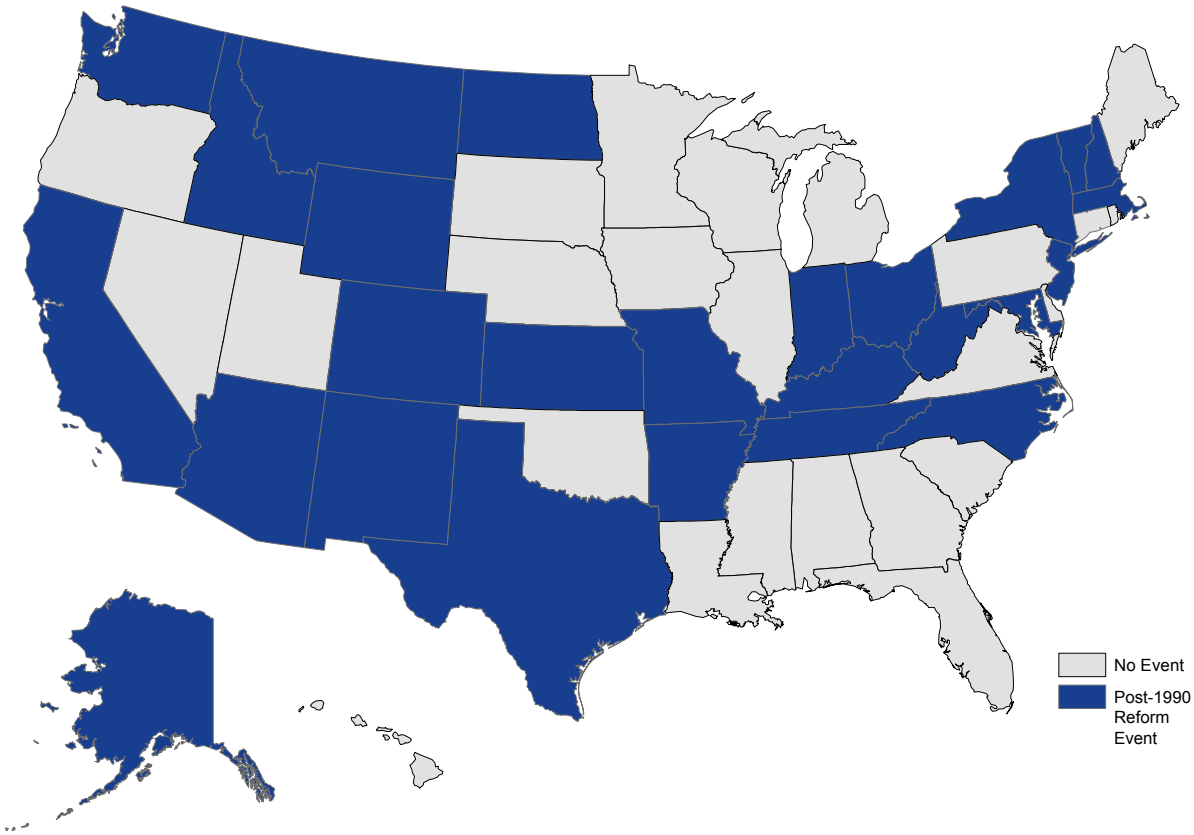
Corcoran, S. P., & Evans, W. N. (2015). Equity, adequacy, and the evolving state role in education finance. In H. F. Ladd and M. E. Goertz, eds., *Handbook of Research in Education Finance and Policy,* 2nd edition. New York: Routledge.

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

Jacob, B. & Rothstein, J. (2016). The measurement of student ability in modern assessment systems. *Journal of Economic Perspectives*, forthcoming.

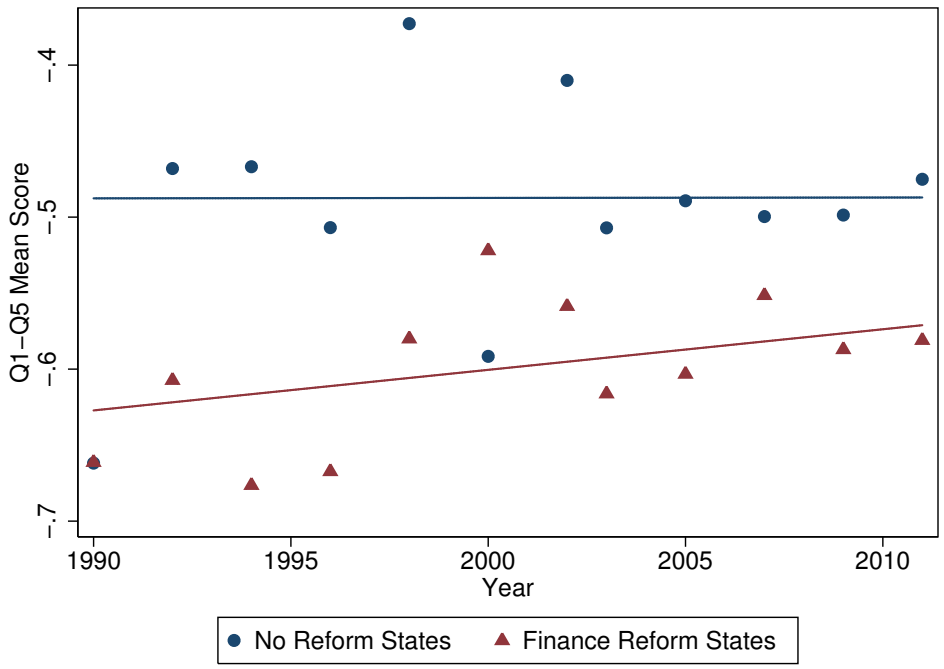
Appendix Figures

Figure A1: Geographic distribution of post-1989 school finance events



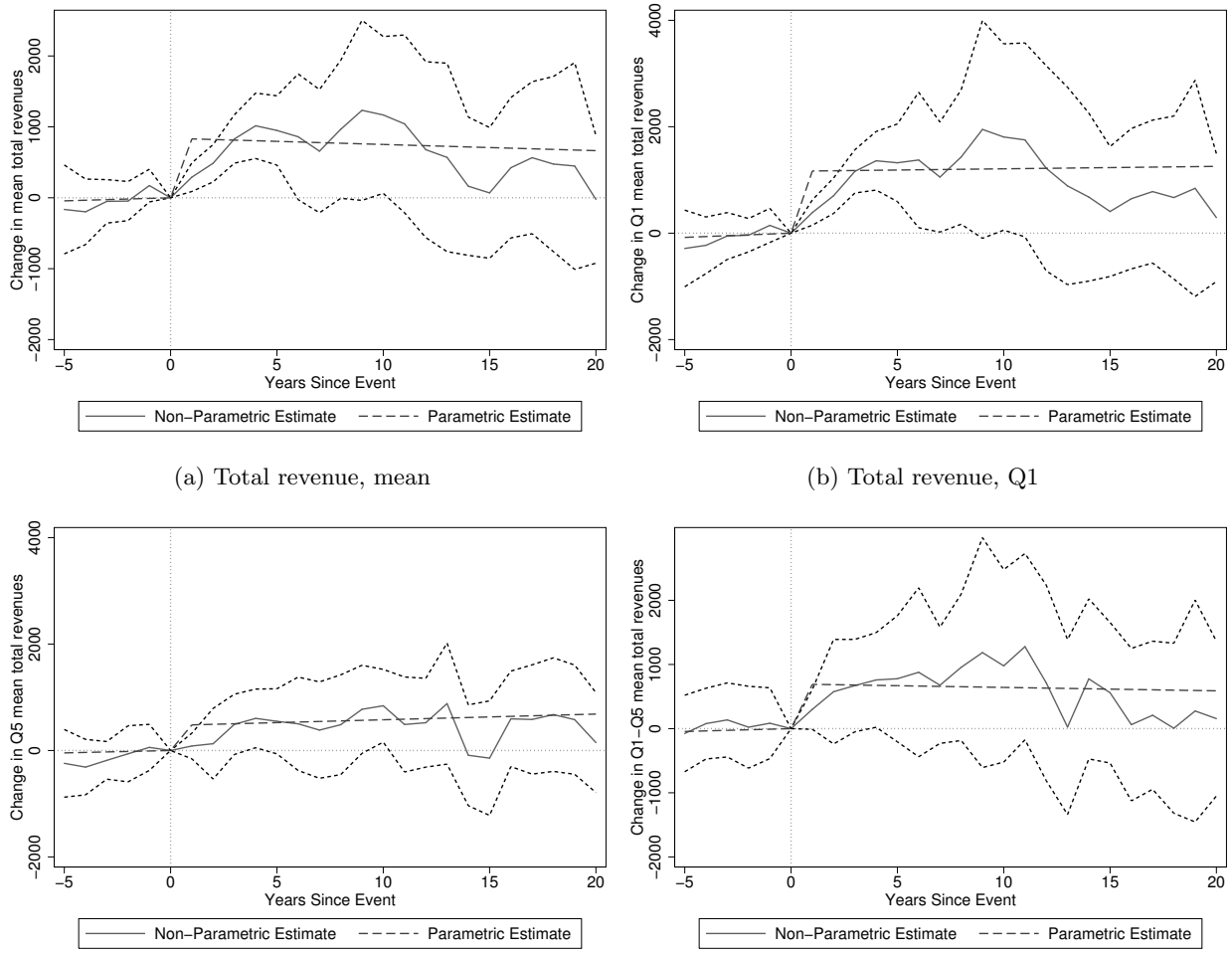
Notes: Map indicates states that had school finance reform events, as listed in Appendix Table A1, between 1990 and 2011.

Figure A2: Gap in average test scores between lowest and highest income districts, by state finance reform status, 1990-2011



Notes: Lowest (Q1) and highest (Q5) income districts are defined as in Figure 1. NAEP observations in districts in each quintile are averaged, using NAEP sampling weights and separately for each grade and subject tested, and the Q1-Q5 difference is computed for each state. State-grade-subject Q1-Q5 differences are averaged separately for each group of states, weighting by the harmonic mean of the sum of the student weights in Q1 and Q5 districts. Lines show best linear fit to the time series.

Figure A3: Event study estimates of effects of school finance reforms on mean total revenues in lowest and highest income districts



(a) Total revenue, mean

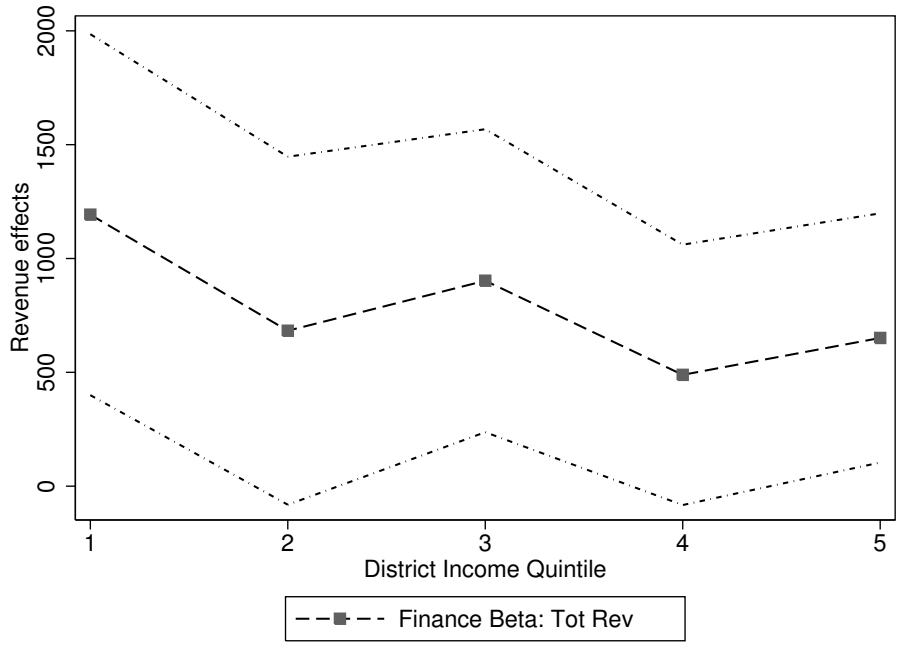
(b) Total revenue, Q1

(c) Total revenue, Q5

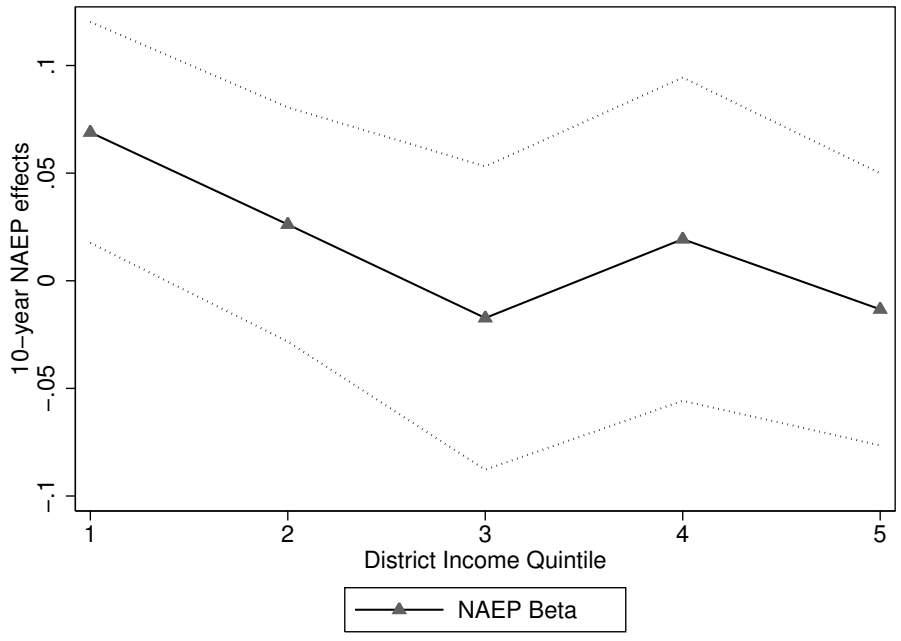
(d) Total revenue, Q1-Q5

Notes: Figure displays coefficients from event study regressions. Dependent variables are mean total revenues per pupil (panel A), mean total revenues per pupil in the lowest income quintile of districts (panel B), mean total revenues per pupil in the highest income quintile of districts (panel C), and the difference in mean total revenues per pupil between districts in the bottom and top income quintile in the state (panel D), all measured in 2013 dollars per pupil. Dashed lines show the three-parameter parametric model (equation 2). Solid lines shows the non-parametric model (equation 3), with the event year (indicated as 0) as the excluded category; dotted lines represent 95% confidence intervals. Estimates for the parametric models are reported in Table 3, panel D, columns 1-4. p values for omnibus hypothesis tests of zero pre-event effects in the non-parametric model in panels A-D are 0.15, 0.40, 0.74, and 0.86, respectively; p-values for zero post-event effects are <0.001 in all panels. In the parametric model, the p-values for the hypothesis that the pre-event trend is zero are 0.79, 0.68, 0.78, and 0.72; for the test that the post-event jump and change in trend is zero they are 0.01, <0.001, 0.22, and 0.01.

Figure A4: Event study estimates for total revenues and test scores by district income group



(a) Total revenue



(b) NAEP

Notes: Figure shows event study estimates from one-parameter parametric models for mean revenues and mean test scores in each quintile. Estimates for quintiles 1 and 5 are shown in Table 3, panel C, columns 1-2, and Table 5, columns 3 and 4. 95% confidence intervals shown by dotted lines. Standard errors are clustered at the state level.

Appendix Tables

Appendix Table A1
Complete Event List, 1990-2011

State	Year	Event	Lafortune, Rothstein & Schanzenbach (2016)	Jackson, Johnson & Persico (2016)	Corcoran & Evans (2015)
Alabama	1993	Alabama Coalition for Equity (ACE) v. Hunt; Harper v. Hunt		X	
Alaska	**1999**	Kasayulie v. State of Alaska	Court	X	
Arizona	1994	Roosevelt v. Bishop	Court	X	
	1997	Hull v. Albrecht	Court	X	
	1998	Hull v. Albrecht	Court	X	
	2007	Flores v. Arizona		X	
Arkansas	1994	Lake View v. Arkansas	Court	X	
	1995	Approved Equitable School Finance Plan (Acts 917, 916, and 1194)	Bill		n/a
	2002	Lake View v. Huckabee	Court	X	X
	2005	Lake View v. Huckabee	Court	X	X
	2007	Various acts resulting from Master's Report findings	Bill		n/a
California	1998	Leroy F. Greene School Facilities Act of 1998	Bill		n/a
	2004	Senate Bill 6, Senate Bill 550, Assembly Bill 1550, Assembly Bill 2727, and Assembly Bill 3001	Bill		n/a
Colorado	**2000**	Bill 181; Various Other Acts	Bill		n/a
Connecticut	1995	Sheff v. O'Neill		X	
	2010	Coalition for justice in Education Funding, Inc. v. Rell		X	n/a
Idaho	**1993**	Idaho Schools for Equal Educational Opportunity v. Evans (ISEEO)	Court		
	1994	Senate Bill 1560	Bill		n/a
	1998	Idaho Schools for Equal Educational Opportunity v. State (ISEEO III)		X	
	2005	Idaho Schools for Equal Educational Opportunity v. Evans (ISEEO V)	Court	X	
Indiana	**2011**	HB 1001 (Pl229)	Bill		n/a

(continued)

Appendix Table A1 (continued)

State	Year	Event	LRS (2016)	JJP (2016)	CE (2015)
Kansas	1992	The School District Finance and Quality Performance Act	Bill		n/a
	2005	Montoy v. State; Montoy v. State funding increases	Both	X	X
Kentucky	(1989)	Rose v. Council for Better Education, Inc.	Court	X	X
	1990	Kentucky Education Reform Act (HB 940)	Bill		n/a
Maryland	1996	Bradford v. Maryland State Board of Education	Court		
	2002	Bridge to Excellence in Public Schools Act (BTE) (Senate Bill 856)	Bill		n/a
	2005	Bradford v. Maryland State Board of Education		X	(upheld)
Massachusets	**1993**	McDuffy v. Secretary of the Executive Office of Education; Massachusetts Education Reform Act	Both	X	X
Michigan	1997	Durant v. State of Michigan		X	
Missouri	**1993**	Committee for Educational Equality v. State of Missouri; Outstanding Schools Act (S.B. 380)	Both	X	
	2005	Senate Bill 287	Bill		n/a
Montana	1993	House Bill 667	Bill	X	
	2005	Columbia Falls Elementary School v. State	Court	X	X
	2007	M.C.A. § 20-9-309	Bill		n/a
	2008	Montana Quality Education Coalition v. Montana		X	n/a
New Hampshire	1993	Claremont New Hampshire v. Gregg	Court	X	
	1997	Claremont School District v. Governor	Court	X	X
	1998	Opinion of the Justices--School Financing (Claremont III)			X
	1999	Claremont v. Governor (Claremont III); RSA chapter 193-E	Both	X	X
	2000	Opinion of the Justices--School Financing (Claremont VI)			X
	2002	Claremont School District v. Governor	Court	X	X
	2006	Londonderry School District v. New Hampshire		X	
	2008	SB 539	Bill		n/a

(continued)

Appendix Table A1 (continued)

State	Year	Event	LRS (2016)	JJP (2016)	CE (2015)
New Jersey	1990	The Quality Education Act; Abbot v. Burke	Both	X	X
	1991	Abbott v. Burke		X	
	1994	Abbott v. Burke	Court	X	X
	1996	Comprehensive Educational Improvement and Financing Act of 1996	Bill		n/a
	1997	Special Master's Report; Abbott v. Burke	Bill		X
	1998	Abbott v. Burke	Court		X
	2000	Abbott v. Burke	Court		
	2008	The School Funding Reform Act of 2008	Bill		n/a
New Mexico	1998	Zuni School District v. State		X	
	1999	Zuni School District v. State	Court		
	2001	Deficiencies Corrections Program; Public School Capital Outlay Act	Bill		n/a
New York	2003	Campaign for Fiscal Equity, Inc. v. State	Court	X	X
	2006	Campaign for Fiscal Equity, Inc. v. State	Court	X	
	2007	Education Budget and Reform Act	Bill		n/a
North Carolina	**1997**	Leandro v. State	Court	X	
	2004	Hoke County Board of Education v. State	Court	X	X
North Dakota	**2007**	SB 2200	Bill		n/a
Ohio	**1997**	DeRolph v. Ohio	Court	X	X
	2000	DeRolph v. Ohio; Increased school funding (see 93 Ohio St.3d 309)	Both	X	X
	2001	DeRolph v. Ohio			X
	2002	DeRolph v. Ohio	Court	X	X
Oregon	2009	Pendleton School District 16R v. State		X	n/a
South Carolina	2005	Abbeville County School District v. State		X	

(continued)

Appendix Table A1 (continued)

State	Year	Event	LRS (2016)	JJP (2016)	CE (2015)
Tennessee	1992	The Education Improvement Act	Bill		n/a
	1993	Tennessee Small School Systems v. McWherter	Court	X	X
	1995	Tennessee Small School Systems v. McWherter	Court	X	X
	2002	Tennessee Small School Systems v. McWherter	Court	X	X
Texas	1991	Edgewood Independent School District v. Kirby	Court	X	X
	1992	Carrolton-Farmers Branch ISD v. Edgewood Independent School District	Court	X	X
	1993	Senate Bill 7	Bill		n/a
	2004	West Orange-Cove ISD v. Nelson		X	
	2005	West Orange-Cove Consolidated ISD v. Neeley			X
Vermont	1997	Brigham v. State	Court	X	X
	2003	Revisions to Act 68; H.480	Bill		n/a
Washington	1991	Seattle II		X	
	2007	Federal Way School District v. State		X	
	2010	McCleary v. State	Court	n/a	n/a
West Virginia	**1995**	Tomblin v. Gainer	Court	X	
Wyoming	1995	Campbell County School District v. State	Court	X	X
	1997	The Wyoming Comprehensive Assessment System; The Education Resource Block Grant Model	Bill		n/a
	2001	Campbell II; Recalibration of the MAP model	Bill	X	n/a

Notes: Table lists all events included in any of the Lafortune-Rothstein-Schanzenbach (2016); Jackson-Johnson-Persico (2016); or Corcoran-Evans (2015) event lists, from 1990 onward. Xs indicate events that appear in the relevant event list; n/a indicates events that were out of scope for the relevant list, either because they were too recent or because it included only court cases and not legislative events. In Lafortune et al. column, events are classified as "court," "bill," or "both"; rows without an entry are not included in our event database but are included in one of the comparison samples. Bold years indicate the single event per state selected by our algorithm (see text). Appendix D discusses discrepancies between Lafortune et al. and Jackson et al. lists.

Table A2: Event studies for state budgets

	Per capita	Per pupil
Tax revenues:		
Total revenues	235	2,736
	(258)	(2,044)
Expenditures:		
General expenditures	290**	2,536*
	(138)	(1,505)
Education expenditures	114	1,029
	(70)	(643)
General expenditures (less education)	176**	1,508
	(90)	(977)
Health + welfare expenditures	73	514
	(49)	(457)
General expenditures (less education, health, welfare)	103	993
	(77)	(700)

Notes: Table shows estimates from the one-parameter event study specification (equation (1)) for state budgetary aggregates. State and year fixed effects are included. Standard errors are clustered at the state level.

Table A3: Comparison to Card-Payne

	State revenues (per capita)			Total revenues (per capita)		
	1997-1992 (CP)	1990-2012 (LRS)		1997-1992 (CP)	1990-2012 (LRS)	
	Long diff	Long diff	Event study	Long diff	Long diff	Event study
Court Ruling:						
Upheld	-0.81			0.20		
	(0.67)			(0.52)		
Unconstitutional	-1.89***			-1.10**		
	(0.62)			(0.48)		
Selected Events:						
Post Event		-2.06	-2.25**		-2.44	-1.61
		(2.24)	(0.89)		(4.73)	(2.38)

Notes: This table shows results using slopes from a regression of per capita state or total funding on district mean household income (note: district mean income here is in *levels*, not logs). Columns 1 and 4 are from table 4 of Card and Payne (2002) and show the long difference from 1977-1992 in the level-level slope coefficient. In columns 2 and 5, we replicate the Card and Payne specification using data from 1990 and 2012. Columns 3 and 6 show estimated effects from the one parameter event study specification (equation (1)) where level-level per capita slope coefficients are the dependent variables. Standard errors are clustered at the state level.

Table A4: Event study for log income, race, free lunch

(a) Income gradients

	Log mean income		Minority share		Free lunch share	
	(1)	(2)	(3)	(4)	(5)	(6)
Post Event * Yrs Elapsed	-0.0010	0.0008	0.0021	0.0017	0.0058	0.0089
	(0.0029)	(0.0040)	(0.0013)	(0.0015)	(0.0064)	(0.0071)
Trend		-0.0026		0.0008		-0.0023
		(0.0042)		(0.0008)		(0.0031)
Post Event		0.0193		-0.0042		-0.0247
		(0.0368)		(0.0051)		(0.0293)
Observations	147	147	1046	1046	958	958
p(post-event=post-event*trend=0)	0.72	0.87	0.10	0.51	0.37	0.42
State FEs	X	X	X	X	X	X
Yr FEs	X	X	X	X	X	X

(b) Q1-Q5 difference

	Log mean income		Minority share		Free lunch share	
	(1)	(2)	(3)	(4)	(5)	(6)
Post Event * Yrs Elapsed	-0.0017	-0.0008	-0.0012	-0.0016	-0.0035*	-0.0051**
	(0.0029)	(0.0035)	(0.0018)	(0.0020)	(0.0019)	(0.0024)
Trend		-0.0004		0.0003		0.0018
		(0.0035)		(0.0016)		(0.0021)
Post Event		-0.0073		0.0034		-0.0050
		(0.0290)		(0.0085)		(0.0154)
Observations	145	145	1045	1045	962	962
p(post-event=post-event*trend=0)	0.55	0.95	0.49	0.72	0.08	0.07
State FEs	X	X	X	X	X	X
Yr FEs	X	X	X	X	X	X

Notes: Table presents event study specifications where the dependent variable is the slope of the indicated demographic characteristic with respect to the district's 1990 log mean household income (panel A) or the gap between the average for districts in the bottom and top quintiles of the 1990 income distribution (panel B). Minority share and free lunch share are available annually from the Common Core of Data (though missing in some states and some years); log mean income is available from the Census in 1990 and 2000 and from the American Community Survey in 2007-11 (coded as 2011). Standard errors are clustered at the state level.

Table A5: Stratification of race, FRL, & achievement, by quintile

	Q1	Q2	Q3	Q4	Q5
Black	0.24	0.24	0.24	0.17	0.11
Black/Hispanic	0.24	0.23	0.24	0.17	0.11
White	0.20	0.20	0.18	0.20	0.22
Free/reduced-price lunch	0.32	0.23	0.20	0.15	0.09
25th pctl or below (NAEP)	0.27	0.21	0.22	0.17	0.13
75th pctl or above (NAEP)	0.14	0.15	0.17	0.22	0.32

Note: Table shows fraction of students of various groups in districts in various quintiles of the state's district income distribution. Each row sums to 1. Racial and free lunch shares are computed using CCD district-level data for the year 1994. The distribution of high- and low-achieving students is based on the 2003 NAEP data, which is the first year of comprehensive data for all grades and subjects.

Table A6: Event studies for district-mean resource gaps by race, FRL, & achievement

	Black/White		Free Lunch		25th/75th Pctl (NAEP)	
	St. Rev	Tot. Rev	St. Rev	Tot. Rev	St. Rev	Tot. Rev
Post Event	196	195	-32	-33	143	193*
	(160)	(164)	(193)	(219)	(141)	(101)
Observations	1047	1047	938	938	1509	1509
State FEs	X	X	X	X	X	X
Yr FEs	X	X	X	X		
Sub-gr-yr FEs					X	X

Note: In columns 1 and 2, the dependent variable in event study specifications is the average per-pupil revenue in the district attended by the average black student, less that in the district attended by the average white student in the same state. In columns 3 and 4, analogous revenue gaps are constructed for free/reduced-price lunch and non-free/reduced-price lunch students. In columns 5 and 6, analogous revenue gaps are constructed for students scoring at or below the 25th percentile in the NAEP, and students scoring at or above the 75th percentile in the NAEP. The *Post Event* coefficient shows the estimated event effect from parametric event study model without controlling for prior trends. State and year fixed effects are included in columns 1-4. State and grade-subject-year fixed effects are included in columns 5 and 6. Standard errors are clustered at the state level.

Table A7: Impacts of student sorting on student achievement results

	Q1-Q5 difference	Slope
Baseline Estimates	0.008**	-0.010***
	(0.004)	(0.003)
Decomposition 1: Common covariates		
Predicted score	0.003	-0.003
	(0.004)	(0.004)
Residual score	0.005**	-0.007**
	(0.002)	(0.003)
Decomposition 2: Richer covariates		
Predicted score	0.004	-0.004
	(0.004)	(0.003)
Residual score	0.004*	-0.006***
	(0.002)	(0.002)

Notes: First row repeats estimates from Table 6, columns 2 and 5. In subsequent rows, dependent variables are modified. We estimate student-level regressions of NAEP scores on student demographic characteristics, with year fixed effects, then compute predicted and residual test scores. We compute separate slopes with respect to district income and quintile gaps for the predicted and residual test scores, and estimate separate event study regressions for each. In decomposition 1, student demographic characteristics are race/ethnicity and gender, along with school means (in the NAEP sample) of each. Decomposition 2 adds indicators for students whose parent is a college graduate and for free or reduced-price lunch receipt, along with indicators for NAEP samples where these variables are unavailable and school means of each. Standard errors are clustered at the state level.

Table A8: Multiple events robustness: Monte Carlo simulations

	First event	All events (stacked)
DGP 1: Constant event effect		
Post coefficient	0.789	0.577
DGP 2: Only one event		
Post coefficient	0.788	0.577

Notes: Table reports estimates of average post-event "jump" coefficient from Monte Carlo simulations using the empirical distribution of event dates, in which some states had multiple school finance reform events. Column 1 shows estimates from event study models estimated using only the first event in a state. Column 2 shows estimates using all events in a state, stacking panels and adding a joint state-panel copy fixed effect (see table 7 column 2). In both columns, estimates are from parametric event study models with a single coefficient (equation 1). Row 1 shows estimates from a simulated DGP where every event in a state has a constant effect. Row 2 shows estimates from a DGP where only one event (randomly chosen within state) has an effect. In both DGPs the total event effect over all events within a state is equal to 1. All DGPs include i.i.d. error terms and are simulated 5000 times.