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School Finance Reforms, Teachers' Unions, and the Allocation of School Resources

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Abstract

School finance reforms caused some of the most dramatic increases in intergovernmental aid from states to local governments in U.S. history. We examine whether teachers' unions affected the fraction of reform-induced state aid that passed through to local spending and the allocation of these funds. Districts with strong teachers' unions increased spending nearly dollar-for-dollar with state aid, and spent the funds primarily on teacher compensation. Districts with weak unions used aid primarily for property tax relief, and spent remaining funds on hiring new teachers. The greater expenditure increases in strong union districts led to larger increases in student achievement.

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I. Introduction

The school finance reforms that occurred across the U.S. beginning in the early 1970's caused some of the largest transfers from states to local governments in U.S history. Recent work has linked these reforms to sustained improvements in student achievement, and long-run increases in educational attainment, earnings, and intergenerational mobility (Jackson, Johnson, & Persico, 2016; Hyman, 2017; Lafortune, Rothstein, & Schanzenbach, 2018; Candelaria & Shores, 2018; Biasi, 2017). However, some of the earliest and most fundamental questions regarding school finance reforms were not about their effects on student outcomes. Rather, early studies focused on the effect of school finance reforms on the distribution of school spending across districts and whether local school districts responded to increases in state aid by reducing local taxing effort (Murray, Evans, & Schwab, 1998; Hoxby, 2001; Card & Payne, 2002). These studies found a substantial incidence of "flypaper," with most of the increases in state aid translating into increased education spending.

The finding that state aid from school finance reforms tended to "stick where it hit" contributes to a larger literature on the flypaper effect, in which some studies find very little or no evidence of local effort crowd-out of intergovernmental aid (Dahlberg et al., 2008; Feiveson, 2015), while others find substantial or near total crowd-out (Knight, 2002; Gordon, 2004; Lutz, 2010; Cascio, Gordon, & Reber, 2013). One leading explanation for the flypaper effect is about local politics, and specifically, that special interest groups influence the allocation of resources by lobbying for intergovernmental grants to be spent on the preferred good (Inman, 2008; Singhal, 2008). In education, teachers' unions are the most prominent special interest group, and an extensive literature examines their impact on the size of school district budgets, district resource allocations, and student outcomes (Hoxby, 1996; Lovenheim, 2009; Frandsen, 2016; Lovenheim & Willen, 2018). However, despite the long-standing interest in how teachers' unions and school finance reforms have affected school spending and student achievement, the question of whether and how teachers' unions influenced local responses to school finance reforms remains unexplored.

In this paper, we provide the first evidence on whether the strength of local teachers' unions influenced: 1) the extent to which school finance reform-induced increases in state aid translated into increased education spending by local districts, 2) the allocation of these expenditures across different inputs to education production, and 3) the effect of reform-induced increases in state aid on student achievement. We combine National Center for Education Statistics (NCES) and Schools and Staffing Survey (SASS) school district data from 1986 through 2012 on revenue, expenditures, staffing, and teacher salaries with data on the timing of statewide school finance reforms and information on state teachers' union power. Our primary measure of teachers' union power is based on an index that incorporates administrative and survey data across several areas related to teachers' union strength.[1]

We use the plausibly-exogenous timing of statewide school finance reforms as an instrument for state aid and examine whether the effects of reform-induced increases in state aid on total and local revenue, expenditures, and the allocation of resources differ by state teachers' union power. Finally, we assemble microdata from the National Assessment of Educational Progress (NAEP) to examine whether any differential effects of the reforms on education spending by teachers' union power also translate into differential effects on student achievement.

[1] The index was created by researchers at the Fordham Institute. We also use more traditional measures of state teachers' union power that rely solely on state public sector collective bargaining laws and right-to-work status.

We find that unions played a critical role in determining both the amount of state aid that translated into education expenditures and the allocation of these funds. Consistent with a basic model of teachers' union preferences, school districts in states with the strongest teachers' unions increased education expenditures nearly one-for-one with increases in state aid in response to school finance reforms, whereas states with the weakest unions reduced local tax effort by approximately 80 cents on the dollar. Districts in strong teachers' union states allocated more of the additional spending toward increasing teacher salaries, while districts in weak union states spent the money primarily on teacher hiring. Spending in non-instructional areas such as capital outlays, administration, and classroom support also increased more in strong teachers' union states than in states with weak teachers' unions. Finally, we find that the larger expenditure increases in strong teachers' union states translated into larger impacts on student achievement: ten years after a reform, students in low-income districts in weak teachers' union states scored 0.08 standard deviations (SDs) higher, but scored 0.16 SDs higher in strong teachers' union states.

While our methodology is similar to recent papers exploiting the plausibly exogenous timing of school finance reforms across states (e.g., Jackson et al., 2016; Lafortune et al., 2018), an additional threat to the validity of our analysis is the potential endogeneity of state teachers' union power. We show that our results are robust to two alternative identification strategies that address this potential threat: 1) a border discontinuity analysis where we restrict our sample to districts along state borders where there are differences in teachers' union power but not in observed population characteristics; and 2) directly controlling for heterogeneity in the effects of school finance reforms by key state-level predictors of union power, such as share voting for the Democratic presidential candidate, and median household income. The robustness of our results to these alternative strategies suggests that we are identifying the effects of teachers' unions, and not unobserved differences across states with strong versus weak teachers' unions. We also show that our results are robust to alternative ways of categorizing school finance reforms, including using a stacked difference-in-differences estimation strategy that includes all reforms for states that experienced multiple reforms.

Our results provide important insights to the school finance reform literature. Early studies found that a dollar of state aid increased district education spending by 50-65 cents (e.g., Card & Payne, 2002), while more recent work shows achievement gains for low-income districts on the order of 0.1 SDs 10 years post-reform (Lafortune et al., 2018). We find similar mean flypaper effects and achievement gains, but show that these mask dramatic heterogeneity driven by the strength of local teachers' unions. This heterogeneity is so stark that it is consequential for assessing the success of the school finance reform movement, suggesting that in the absence of teachers' unions, the reforms would have had little impact on school resources or student achievement, leading instead to large increases in property tax relief. These findings are consistent with Inman's (2008) argument that local politics is the primary explanation for the flypaper phenomenon, and specifically, that local unions or other special interest groups ensure intergovernmental grants "stick where they hit."

It is also possible that strong teachers' unions used their power to influence the design of school finance reforms in a way that would limit the degree of local crowd-out. As Hoxby (2001) notes, school finance reforms are quite heterogeneous in their design, with some states implementing reforms that level-up spending and others implementing reforms that level-down spending. While it is possible that our results are driven in part by the influence of teachers' unions on the specific design elements of reforms, we find little evidence that the type of reform implemented by states is correlated with state teachers' union power.[2]

Finally, our results build on the labor economics literature examining the effects of teachers' unions (Hoxby, 1996; Lovenheim, 2009; Frandsen, 2016; Lovenheim & Willen, 2018). We find large and important impacts of unions on the size and allocation of school district budgets and on student outcomes. Perhaps most interestingly, we demonstrate that in the context of this historically important school finance reform movement, teachers' unions acted in a manner consistent with special interests, namely maximizing the welfare of their members. Yet, the outcome of this rent-seeking behavior aligned with the objectives of the school finance reform movement, ensuring that the reforms were effective in reducing inequality across school districts in education resources and student achievement.

II. Teachers' Unions, School Spending, and the Allocation of Resources

The neoclassical view of intergovernmental grants suggests that when communities receive a lump sum grant from a higher-level government, they treat that grant the same as an equivalent increase in private income. Thus, intergovernmental grants should increase spending by the same amount as an equivalent increase in income. A large literature, however, has found that intergovernmental grants tend to increase government spending by much more than an equivalent increase in income, a finding commonly referred to as the flypaper effect.

[2] Specifically, we used the coding of reforms provided by Jackson et al. (2014) to classify all the school finance reforms in our sample into six groups: 1) flat grants (FG), 2) minimum foundation plans (MFP), 3) equalizations plans (EP), 4) local effort equalizations (LE), 5) spending limits (SL), and 6) full state funding (FS). We then examined the correlation between the type of reform implemented and our primary measure of teachers' union power. As detailed in the online appendix, these correlations tended to be quite low ranging from -0.16 to 0.068, and whether they are positive or negative does not consistently support the hypothesis that unions would favor reform types that discourage local crowd out.

Scholars have provided several explanations for the flypaper effect, including: matching grants being misclassified as exogenous lump-sum aid, endogeneity and omitted variable bias in econometric specifications, voter ignorance about intergovernmental grants, and finally, local politics (Hines & Thaler, 1995; Inman, 2008). Among these alternative explanations, Inman (2008) suggests that the most likely explanation for the flypaper effect is politics. Specifically, several studies have developed models that focus on the role of special interest groups, such as unions, as an explanation for the flypaper effect (Dougan & Kenyon, 1988; Singhal, 2008; Seig & Wang 2013). In these models, interest group lobbying leads to an allocation of resources that favors spending on the good preferred by the interest group.

In education, teachers' unions are the most prominent special interest group. Thus, the theoretical models discussed above predict that after an increase in intergovernmental aid brought about by a school finance reform (SFR) teachers' unions will lobby to direct intergovernmental aid toward school spending and away from property tax relief, leading to the classic flypaper effect (see Appendix Figure 1a).[3] Furthermore, note that regardless of whether teachers' unions are primarily rent-seeking or simply interested in maximizing school quality, they will use their political power to advocate for higher school spending. If teachers' unions are primarily rent-seeking, then increasing the size of the budget allows them to bargain for higher teacher salaries or other items that disproportionally benefit teachers.[4] Similarly, if unions are

[3] See the online appendix for more discussion about why this would occur. Appendix figure Ia illustrates the choice problem of a district facing an increase in intergovernmental grant aid and shows that districts under union influence would increase spending by more than their marginal propensity to spend out of income.

[4] Specifically, as shown in Appendix Figure Ib, if teachers' unions are primarily rent-seeking they may bargain for a larger share of any budget increase to be allocated towards inputs that primarily benefit teachers, such as teacher salaries, as opposed to other inputs that may be more efficient in raising student achievement, such as class size reductions.

primarily interested in maximizing school quality, and additional resources lead to higher student achievement, unions will again advocate for higher school spending.

II. Data

Our primary data source is the Local Education Agency (i.e., School District) Finance Survey (F-33) maintained by the National Center for Education Statistics (NCES). The F-33 surveys contain detailed annual revenue and expenditure data for all school districts in the United States for the period 1990-91 to 2011-12. We augment this data with earlier versions of the F-33 survey provided by the U.S. Census for the years 1986-87 to 1989-90. For this period, 1986 – 2011[5], we also utilize the annual NCES Common Core of Data (CCD) school district universe surveys that provide student enrollments and staff counts for every school district.

We restrict our sample in several ways. First, note that we aim to examine whether teachers' unions affect the degree to which inter-governmental aid "sticks where it hits," i.e., the flypaper effect. As discussed in Inman (2008), one of the explanations for why prior studies have found strong evidence of a flypaper effect is that researchers may have misclassified matching grants as lump sum grants. Furthermore, we acknowledge that SFRs vary in their design and intended impacts (Hoxby, 2001). Thus, to avoid misclassifying matching grants as lump sum aid, and to focus as much as possible on similarly designed SFRs, we omit Kansas, Kentucky, Missouri, and Texas since these states implemented "reward for local effort" (matching grant) formulas as part of their SFRs. We also omit Michigan and Wyoming because these states adopted SFRs that eliminated local discretion over funding. Second, because the NCES F-33 financial data tends to be noisy, particularly for small districts, we follow Gordon (2004) and Lafortune et al. (2018) and exclude small districts (with enrollment below 250

[5] Here and subsequently, we refer to a school year by its fall year, i.e., 2011 refers to the 2011-12 school year.

students) from the analysis.[6] Finally, in our preferred specifications, we omit the final three years (2009-2011) of our sample due to the severe and potentially confounding influence of the Great Recession on school finances during that time (Evans, Schwab, & Wagner, 2017). We show in Appendix Table 7 that our results are robust to this sample restriction.

We combine the school district financial data with data on median household income, fraction black, fraction urban, and fraction of adults 25 and older with a Bachelor's degree from the Special School District Tabulations of the 1980 Census.[7] We obtained a comprehensive list of SFRs from Jackson et al. (2016) and Lafortune et al. (2018). Our primary coding of these SFRs is based on the coding structure developed by Lafortune et al. (2018), though we differ from their coding in a few cases. We show in Appendix Table 7 that our results are robust to using a stacked difference-in-differences strategy that uses all SFRs for states with multiple reforms (including the reforms where we differ from Lafortune et al. (2018)), and to using only court-ordered reforms, as in Jackson et al. (2016).[8]

Finally, our primary teachers' union power measure is based on an index created by researchers at the Fordham Institute (Winkler, Scull, & Zeehandelaar, 2012). The index combines administrative and original survey data across five areas related to teachers' union power: 1) resources and membership; 2) involvement in politics; 3) scope of bargaining; 4) state policies; and 5) perceived influence. Many of the index components are measured as of 2012, after the SFRs in our sample, raising concerns that some components may be endogenous to the reforms. After carefully reviewing all of the index components, the only ones we believe

[6] See the online appendix for a more detailed discussion of our data and sample restrictions.
[7] These data are missing for approximately 3.5% of the districts in our sample. Rather than excluding these districts, we matched school districts to counties and then replaced the missing district-level values of each variable with their county-level equivalent.
[8] See Appendix Table 1 for a listing of the school finance reforms used in our main analysis.

would have been directly influenced by SFRs are the measures related to school spending included in the "resources and membership" category. We therefore drop these variables from the index and recalculate it without them.[9]

Figure Ia shows a state map of the U.S. by this continuous measure of state teachers' union power, with states ranging from weakest teachers' union power (white) to strongest teachers' union power (dark grey). The strongest teachers' union states tend to be in the Northeast, Great Lakes area of the Midwest, and the Pacific census division, while the weakest teachers' union states tend to be in the South. As such, these types of states look quite different from one another. Table 1 shows the sample means of the variables we use in our analysis for all of the states in our sample and by high (above median) versus low (below median) state teachers' union power. Stronger teachers' union states have higher per-pupil revenues and expenditures, are more heavily urban, and have higher teacher salaries and household income.

To address possible concerns about endogeneity or subjectivity of the continuous teachers' union power measure, we supplement our analysis with measures of state teachers' union power that utilize state laws implemented prior to our sample period. Specifically, our first alternative measure is an indicator for whether a state mandates collective bargaining (CB), as defined in the NBER Public Sector CB Law Data Set, developed by Valletta and Freeman (1988) and updated by Kim Rueben. As our second alternative measure, we augment the information on state CB laws with information on state right-to-work (RTW) status, obtained from the National Conference of State Legislatures.[10] In this more flexible alternative

[9] In practice, this makes very little difference as these spending measures compose only 6.7% of the weight of the index. See Appendix Figure II, taken from Winkler, Scull, and Zeehandelaar (2012), for a concise overview of the index components and their relative weightings.

[10] Right-to-Work laws are in place in twenty-eight states and prohibit employees in unionized workplaces from being required to join a union or to pay union agency fees, thus potentially reducing the power of unions by reducing

union power index, states first receive a value of zero if CB is prohibited, a value of one if CB is allowed but not mandatory, and a value of two if CB is mandatory. Then, a state's value on the index is increased by one if they are not RTW. This index thus has four values. The weakest union power states are CB prohibited and RTW, and have a value of zero (=0+0). The strongest union power states are CB mandatory and not RTW, and have a value of three (=2+1).[11]

Figure Ib shows a state map of the U.S. by our first alternative teachers' union power measure of whether or not a state mandates collective bargaining, with CB mandatory states shaded dark grey and CB non-mandatory states (where CB is either prohibited or allowed, but not mandatory) shaded white. Figure Ic shades states from white to dark grey for the weakest to strongest union states according to our second alternative measure. While there are some exceptions, the geographic patterns of state union power using these alternative measures are similar to the pattern for the continuous measure shown in Figure Ia.[12] We prefer the continuous index over the alternative measures, because it provides a much finer measure of teachers' union power with a unique value for each state, and thus more variation across states to exploit. However, we show that the pattern of results that we find is similar regardless of which teachers' union power measure we employ.

their membership and resources. The recent U.S. Supreme Court decision in *Janus vs. AFSCME* effectively made the remaining 22 states Right-to-Work, but this change occurred after our sample period.

[11] To avoid endogenous changes in union power, both of our alternative union power measures are based on the CB and RTW laws that were in place in 1987, the first year of our sample time frame. We note, however, that for our main analytic sample that spans the years 1987-2008, only one state adopted a Right-to-Work law (Oklahoma) and two states changed their collective bargaining laws (Alabama and New Mexico).

[12] Appendix Table 2 provides values by state for all three teachers' union power measures. The three measures are strongly positively correlated with a correlation of 0.69 for the continuous and dichotomous measure, 0.75 for the continuous and four-value measure, and 0.89 for the dichotomous and four-value measure.

IV. Empirical Framework

To examine the effect of SFR-induced intergovernmental grants on school district expenditures and resource allocations, and whether state teachers' union power led to heterogeneity, we estimate models of the following form:

$$y_{ist} = \beta_0 + \beta_1 Rev_{ist} + \beta_2 (Rev_{ist} * Union_s) + X_{is}\theta_t\kappa_1 + X_{is}\theta_t Union_s \kappa_2 + \delta_i + \lambda_{rt} + Q_{is}\theta_t + \mu_{ist}, \quad (1)$$

where y_{ist} denotes an outcome of interest for district *i* in state *s* in year *t*; Rev_{ist} denotes state aid per-pupil; $Union_s$ is a measure of the teachers' union power in state *s*; X_{is} is a vector of school district characteristics at baseline interacted with a linear time trend, θ_t; δ_i is a vector of school district fixed effects; λ_{rt} is a vector of census region-by-year fixed effects; Q_{is} is a set of indicators for whether a district was in the 1st, 2nd, or 3rd tercile of the within-state distribution of school district median household income in 1980 (we discuss these indicators in more detail below); and μ_{ist} is a random disturbance term. In all specifications, we cluster the standard errors at both the school district and state-year level.[13]

In our most parsimonious specification, X_{is} includes 1986 district enrollment and 1980 district median income. We then add 1980 district fraction black, fraction urban, and fraction of adults 25 and older who have a Bachelor's degree. We exclude time-varying characteristics because they could be affected by the SFRs (i.e., endogenous controls). Therefore, we include each characteristic interacted with a linear time trend to allow for differential trending by districts with different baseline values of these characteristics. We additionally include $X_{is}\theta_t Union_s$, to allow these trends to differ by state union power. Finally, in all specifications

[13] Following Bertrand, Duflo, and Mullainathan (2004), we cluster the standard errors at the district level to account for serially correlated error terms, but we also cluster at the state-year level to account for spatial correlation. We consider this to be a conservative approach, but additionally report the results clustering at the state level in Panel C of Appendix Table 4.

we include an indicator for whether the district is subject to a binding tax or expenditure limit, given that such limits have been shown to affect local government fiscal behavior (see Dye & McGuire, 1997).

As noted by Jackson et al. (2016) and Lafortune et al. (2018) among others, the amount of intergovernmental state aid allocated to districts is likely endogenous. To isolate potentially exogenous variation in state aid, we use the timing of adoption of SFRs as instrumental variables and estimate two first-stage models, where the first model is:

$$\begin{aligned} Rev_{ist} = {} & \alpha_0 + \alpha_1(Q1_{is} * SFR_{st}) + \alpha_2(Q2_{is} * SFR_{st}) + \alpha_3(Q3_{is} * SFR_{st}) + \\ & \alpha_4(Q1_{is} * SFR_{st} * Union_s) + \alpha_5(Q2_{is} * SFR_{st} * Union_s) + \alpha_6(Q3_{is} * SFR_{st} * Union_s) + \\ & X_{is}\theta_t\pi_1 + X_{is}\theta_t Union_s\pi_2 + \delta_i + \lambda_{rt} + Q_{is}\theta_t + \varepsilon_{ist}, \end{aligned} \quad (2)$$

and the second model is identical to equation (2), but where the dependent variable is $Rev_{ist} * Union_s$. In equation (2), SFR_{st} is an indicator for whether state s implemented a SFR in year t and all subsequent years, and $Q1_{is}$, $Q2_{is}$ and $Q3_{is}$ denote indicators for whether a district was in the 1st, 2nd, or 3rd tercile of the within-state distribution of school district median household income in 1980. We separate the effects of SFRs by within-state 1980 income terciles because reforms were designed to differentially impact state aid for low- and high-income districts, with the goal of equalizing school funding.[14] Given that other factors could be changing over time across these district terciles, we include $Q_{is}\theta_t$, the tercile dummies interacted with a linear time trend in equations (1)-(2), to allow for differential trending across these terciles.

[14] We show in Panel B of Appendix Table 4 that our results are robust to using a just-identified model that includes only the bottom tercile SFR effect and its interaction with union power as instruments.

A. *Dynamic Event Study Specifications*

To provide evidence that SFRs induce exogenous variation in state aid to school districts, we also estimate an event study model of the following form:

$$y_{ist} = \sum_{j=-6}^{10} \gamma_j T_{j,st} + \delta_i + \lambda_t + \eta_{ist}, \quad (3)$$

where, $T_{j,st}$ represents a series of lead and lag indicator variables for when state s implemented a SFR, η_{ist} is a random disturbance term and all other terms are as defined as above. We re-center the year of adoption so that $T_{0,st}$ always equals one in the year in which state s implemented a SFR. We include indicator variables for 2 to 6 or more years prior to implementation of a SFR ($T_{-6,st}, T_{-5,st}, T_{-4,st}, T_{-3,st}, T_{-2,st}$), the year of implementation, $T_{0,st}$, and 1 to 10 or more years after implementation ($T_{1,st} - T_{10,st}$). Note that T_{-6st} equals one in all years that are 6 or more years prior to the implementation of a SFR, and $T_{10,st}$ equals one in all years that are 10 or more years after the implementation of a SFR. The omitted category is the year just prior to a state implementing a SFR, $T_{-1,st}$.

The coefficients of primary interest in equation (3) are the $\gamma_j's$, which represent the difference-in-differences estimates of the impact of SFRs on state aid in each year from t_{-6} to t_{+10}. The estimated coefficients on the lead treatment indicators ($\gamma_{-6}, \ldots, \gamma_{-2}$) provide evidence on whether state aid was trending pre-reform. If reforms induce exogenous variation in state aid, these lead treatment indicators should generally be small in magnitude and statistically insignificant. The lagged treatment indicators ($\gamma_{+1}, \ldots, \gamma_{+10}$) allow the effect of SFRs on state aid to evolve slowly over time.

V. Results

We begin our analysis by showing that SFRs led to exogenous increases in state aid. Specifically, we estimate the event study model from equation (3) for the full sample of school districts and also separately for school districts in each within-state median income tercile. We then plot the estimated $\gamma_j's$ and associated 95% confidence intervals. Figure IIa (all districts) shows that after a SFR, state aid increases to between \$500 and \$1,000 per-pupil above the pre-reform level, and remains at this level through at least 10 years after the reform. Importantly, there is no evidence of trending state aid prior to SFRs. Figure IIb shows more dramatic effects for districts in the bottom income tercile, where state aid increases by between \$1,000 and \$1,500 per-pupil. Figures IIc and IId show the effects for the middle and top income tercile districts, where both groups experience increases of between \$500 and \$850 per-pupil, though the effects are not statistically different from zero for the top-tercile districts. Importantly, there is no evidence of trending state aid prior to the reforms in any of the figures.[15] Having established that the timing of SFRs appears to have been exogenous, we move to our two-stage-least-squares (2SLS) framework to estimate the effects of SFR-induced increases in state aid.

A. Effects of State Aid on Revenues and Expenditures

We present estimates from the second stage of our Instrumental Variables (IV) analysis in Table 2.[16] Columns 1 and 2 in Panel A show the effects of a SFR-induced one dollar increase

[15] Appendix Figures III and IV present similar event study pictures, plotting state aid, total revenue, local revenue, and current expenditures at the 25th and 75th percentile of state union power. In no case do we find evidence of differential pre-trends by union power.

[16] The first-stage results are presented in Appendix Table 3. The pattern of results matches closely with those seen in Figure II, with increases of \$1,089, \$592, and \$578 per-pupil for districts at the bottom, middle, and top income tercile, respectively. There are statistically significant differences in the effects of SFRs on state aid by union power, with stronger union states seeing larger increases in aid to poorer districts and smaller increases to richer districts. This suggests that unions may influence reform design at the state level, at least as far as the level and distribution of resulting state aid to districts, to be more progressive. The first-stage F-statistic for the regression of state aid on the instruments is 23, and for the regression of state aid interacted with union power on the instruments is 36.

in state aid on school district total revenue. Before adding the expanded controls, the results reported in column 1 reveal that for a state with the mean value of union power (index=0), total revenue increases by 64 cents with every dollar increase in state aid, while a one SD increase in teachers' union power leads to a 32 cent larger increase in total revenue. This pattern of results is similar after adding the expanded controls – a 68 cent increase at the mean level of union power, and a 30 cent larger increase given a one SD increase in union power (column 2). These results demonstrate that while total revenue goes up by two thirds of a dollar for every dollar increase in state aid at the mean level of union power, there is substantial heterogeneity in the degree of crowd-out depending on the strength of a state's teachers' union.

As property tax relief is the likely source of crowd-out, we next examine the effects of increased state aid on local revenue (Table 2, columns 3 and 4). Using our preferred specification with the additional controls, districts in a state with mean teachers' union power reduce local revenue by 29 cents for each additional dollar of state aid, with a 27 cent smaller reduction (i.e., only a two cent reduction) in states with teachers' union power one standard deviation higher and a 0.56 cent reduction (i.e., 29 + 27 cents) in local revenue among states with teachers' union power one standard deviation lower. These results explain most of the heterogeneity in total revenue increases by union power – districts in weak teachers' union states substantially reduce their local tax effort in response to the windfall of state aid, whereas districts in states with stronger teachers' unions do so to a far lesser degree.

Finally, we examine the extent to which these revenue effects translate into effects on education expenditures. Using our preferred specification (Table 2, column 6), we find that a SFR-induced dollar increase in state aid translates into a 50 cent increase in current education expenditures at the mean level of state teachers' union power. This is similar to the mean flypaper effect estimated in the earlier SFR literature (e.g., Card & Payne 2002). However, we find that the increase is 19 cents larger (or smaller) given a one SD higher (or lower) level of teachers' union power, suggesting substantial heterogeneity in the flypaper effect by the strength of a state's teachers' unions.

In Figures IIIa-IIIc we plot the estimated coefficients reported in Table 2 at each vigintile (i.e., 20 percentiles) of the union power index.[17] Figure IIIa presents the results from this exercise where total revenue is the outcome. For states with very low teachers' union power (near the 10th percentile), total revenue increases by only 10 cents for every dollar of SFR-induced state aid. In contrast, for states with very high union power (90th percentile), total revenue increases nearly dollar-for-dollar with increases in state aid. The heterogeneity in total revenue across union power percentiles is explained by heterogeneity in local revenue: in states near the 10th percentile of union power, school districts reduced local tax effort by about 80 cents for every dollar of SFR-induced state aid, while in states near the 90th percentile of union power, there is very little change in local taxing effort due to SFR-induced increases in state aid (Figure IIIb). Finally, the heterogeneity in total revenue across the union power distribution also translated into similar heterogeneity in educational expenditures (Figure IIIc). Taken together, the results reported in Table 2 and Figures IIIa-IIIc reveal that differences in state teachers' union power were highly influential in shaping the extent to which the state aid increases from SFRs translated into changes in total revenues and expenditures for education.[18]

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[17] The teachers' union power distribution is skewed such that the top of the distribution is one standard deviation above the mean and the bottom of the distribution is two standard deviations below the mean. We report in the bottom two rows of Table 2 the coefficients and standard errors at the 25th and 75th percentile of union power.

[18] Appendix Table 4 presents OLS effects of state aid. Similar to Jackson et al. (2016), we find that the OLS results are strikingly different than the instrumental variable estimates. This finding highlights the importance of identifying exogenous changes in state aid to identify the effects of state aid on resource allocations.

B. Boosting Teacher Compensation or Shrinking Class Size

The aforementioned results suggest that teachers' unions played a powerful role in determining the pass-through rate of SFR-induced state aid increases to education expenditures. However, unions may also shape the allocation of resources to different inputs. For example, unions may prefer to spend a larger share of any increase in state aid on teacher compensation than on teacher employment (see Appendix Figure 1b). We next examine the effect of SFR-induced increases in state aid on class size and teacher salaries, and whether these effects differ by the power of a state's teachers' unions.

First, we examine effects on the pupil teacher ratio (PTR), which is our measure of class size. A one thousand dollar increase in state aid reduces the PTR by 0.84 pupils among districts in a state with the mean value of union power (Table 2, column 8, Panel A). This represents a 5.2% decrease in class size, relative to the sample mean of 16.3 students.

Recall that our results imply that SFR-induced increases in state aid led to substantially larger increases in per-pupil expenditures in stronger union states. As a result, if money is not being spent differently, then some share of these increases should be spent on teacher hiring. We should therefore expect to find greater class size reductions in states with stronger teachers' unions if unions do *not* alter the allocation of school resources between teacher hiring and raising teacher salaries. On the contrary, we find no statistically significant difference in the effect on class size by teachers' union power. If anything, there is suggestive evidence that there was *less* of a class size reduction in the stronger union states by 0.144 pupils (standard error of 0.118), suggesting that unions alter the allocation of resources away from teacher hiring.

We next examine the effects of SFR-induced state aid increases on teacher compensation. Teacher salaries are typically a lock-step schedule based on level of experience and education. While district average teacher salaries are provided in the CCD, these conflate changes to the teacher salary schedule with changes in hiring of new teachers that are usually paid less than the average teacher in the district. Information on teacher salary schedules are not available in our primary CCD data so we use salary schedule information from the Schools and Staffing Survey (SASS), which surveys a random cross-section of school districts every few years about staffing, salaries, and other school, district, teacher, and administrator information. We focus on base teacher salary, which is available in every wave and is particularly informative about average teacher salaries given the high rate of teacher attrition and relatively large degree of compression in teacher wages. Unfortunately, given the limited number of years and overlap of districts across waves, we lose about 91 percent of our sample size.[19] Consequently, we exclude the controls interacted with the linear time trend, given the limited number of years in the sample with which to estimate the trend.

We find that a one dollar increase in state aid leads to a statistically insignificant 32 cent increase in teacher salaries for districts in a state with mean teachers' union power, and a statistically significant 51 cent larger increase for districts in states with one SD higher teachers' union power. Consistent with our basic conceptual framework, stronger teachers' unions appear to focus the increases in education expenditures more on increasing teacher salaries than on hiring new teachers.[20] Taken together, these findings suggest that teachers'

[19] Appendix Table 5 shows the number of district observations by state and year used in this analysis. The mean, median, 25th, and 75th percentile of district observations by state and year is 66, 64, 45, and 82 districts, respectively. In Panel D of Appendix Table 4, we show that the results for revenues, expenditures, and class size are robust to restricting to the SASS sample of district-years.

[20] Figures IIId and IIIe plot the estimated impacts on class size and teacher salaries, respectively, at each vigintile of the union power index.

unions affect not only the fraction of SFR-induced increases in state aid that pass through to spending, but also the allocation of the spending increases across inputs.

C. Alternative Measures of State Teachers' Union Power

While we prefer the continuous measure of state teachers' union power, we examine whether the results are robust to using our alternative measures of state teachers' union power that avoid any possible concerns about endogeneity or subjectivity of the continuous measure. Our first measure is simply an indicator variable for whether a state mandates collective bargaining (CB). Thus, in Panel B of Table 2, the main state aid term reflects the effect of a dollar increase in state aid for CB non-mandatory states. For CB mandatory states, the effect is calculated by adding the coefficients on the main and interaction terms. Our second alternative measure incorporates CB and RTW status, taking on four values from zero (weakest union) to three (strongest). Thus, in panel C, the main state aid term reflects the effect of a dollar increase in state aid for the weakest union power states with a value of zero on this index. For states with a value of 1 for the measure, the result is calculated by adding the coefficients on the main and interaction terms. The effect for the strongest states are calculated by adding the main coefficient to three times the coefficient on the interaction term.

The pattern of results based on these two alternative measures of union power is broadly similar to those with the continuous measure. For example, in Panel B, districts in CB non-mandatory states experience a statistically insignificant 9 cent increase in total revenue, while the increase in CB mandatory states is 75 (=9+66) cents. Similarly, in Panel C, total revenue increases by 18 cents (insignificant) in states with the weakest unions, and by 75 cents (= 0.179 + [3 x 0.191]), in states with the strongest unions. In columns 5 and 6 we find small and statistically insignificant changes in current expenditures in CB non-mandatory states (panel B) or the weakest union states (panel C) but statistically significant increases of approximately 56 cents in CB mandatory states or the strongest union states. While the results for base salary are statistically imprecise in both panels B and C, the overall pattern of results using these alternative measures is similar to that found when using the continuous index, thus reducing potential concerns about the subjectivity or endogeneity of that index.

D. Possible Teachers' Union Endogeneity

One concern with the results presented thus far is that our measures of teachers' union power may be correlated with state-specific unobservables that also influence education spending and the allocation of education resources. For example, state teachers' union power may be correlated with unobserved state population characteristics, such as voter sentiment about the appropriate level and allocation of K-12 education spending. As a result, voters in states with strong teachers' unions might choose to spend more on education and allocate educational resources differently than states without strong teachers' unions regardless of the teachers' unions themselves. This concern is partially allayed by the inclusion of district fixed effects, which control for any unobserved district- or state-level factors to the extent that they are time invariant. However, there may be unobserved time varying differences causing the heterogeneity we detect. We now present results from two strategies that attempt to address this potential endogeneity of state teachers' union power. We move forward using the continuous union power index and our preferred specification that includes the expanded set of controls.

Our first strategy is a border discontinuity design that focuses on districts in counties along state borders. The assumption (which we support empirically) is that while school districts along these borders differ in terms of their states' teachers' union power, they are otherwise similar along both observable and unobservable dimensions due to their geographic proximity. If our results are robust to this sample change, this would provide confidence that any differences in the effects of state aid in these two types of districts is driven by differences in union power and not unobserved factors.

We use two different state border samples. First, we restrict to counties where the county centroid is less than 50 miles from the nearest state border. This strategy includes some counties not adjacent to a state border in geographically small states, and excludes some counties adjacent to a border in large states with geographically large counties. We alternatively restrict to only counties adjacent to state borders.[21]

To implement the border discontinuity analysis, we restrict the sample to school districts in the counties close to state borders and then re-estimate equations (1)-(2) replacing the region-by-year fixed effects with border-by-year fixed effects, where a border spans two states and includes counties on both sides of the border. The inclusion of the border-by-year fixed effect ensures that we are making comparisons across states within a given border.

To provide evidence that the border discontinuity sample provides a sample of districts that are similar according to their observed characteristics, we conduct a series of balancing tests by estimating cross-sectional models of the form:

$$C_{is,1990} = \rho_0 + \rho_1 Union_s + \gamma_b + v_{is}, \tag{4}$$

where $C_{is,1990}$ denotes a 1990 characteristic of school district i in state s, and γ_b, is a border fixed effect. Since we analyze SFRs that occurred during the 1990's we base our balancing test on pre-determined district characteristics as of 1990. The coefficient of primary interest in equation (4) is ρ_1, which represents the average difference in $C_{is,1990}$ by state union power

[21] See Appendix Figure V for a county map of the U.S. with the border samples shaded grey.

among districts located close to the border. If focusing on the border discontinuity sample leads to a more homogenous set of districts, then ρ_1 should be statistically insignificant or at least substantially smaller in magnitude when compared to estimates obtained from equation (4) that are based on the main sample of school districts and exclude the border fixed effects.

We first present the results from estimating equation (4) on the main sample (Table 3, columns 1 and 2). We find that districts in states with stronger teachers' unions are more likely to vote democratic in presidential elections, be more densely populated, and have higher median household income, lower fraction below poverty, and higher educational attainment.

We now restrict our sample to districts in counties whose centroid is within 50 miles of a state border and re-estimate equation (4), including border fixed effects and thus comparing districts along the same state border (Table 3, columns 3 and 4). The sample appears much better balanced: most of the point estimates shrink dramatically. In fact, the only coefficients that remain marginally statistically significant are the coefficient on population density, which shrinks to approximately half of its previous magnitude, and the coefficient on fraction non-white, which shrinks to approximately one third of its previous magnitude. The pattern is similar when we instead restrict the sample to districts in counties that are adjacent to a state border (columns 5 and 6). These balancing tests provide encouraging evidence that our border sub-samples and specifications significantly reduce observed and therefore, hopefully, unobserved differences across districts by state teachers' union power.

We present results from the border analysis in Table 4. Panel A restricts to counties within 50 miles of a state border, while Panel B restricts to border counties. The pattern of results is nearly identical to that in our main analysis: districts in states with stronger teachers' unions reduce their local tax effort to a smaller extent than states with weak unions, translating into more of the state aid going toward education expenditures. Districts in states with stronger teachers' unions also spend less on reducing class size and more on increasing teacher salaries. While the magnitude of the point estimates varies to some extent, and we again lose statistical precision for the salary results, the pattern is generally robust across both border samples.

One concern with the border analysis is that there are both state-level and district-level sources of union endogeneity, and the border analysis only addresses confounders at the district-level. This concern motivates our second strategy, which involves controlling directly for heterogeneity of the effects of state aid by observable state characteristics that are highly correlated with state teachers' union power and may also influence how districts choose to allocate reform-induced increases in state aid. Specifically, we augment equation (1), the second stage of our two-stage-least-squares estimation strategy, by adding terms $Rev_{ist} * Char_s$ and estimating specifications of the form:

$$y_{ist} = \beta_0 + \beta_1 Rev_{ist} + \beta_2(Rev_{ist} * Union_s) + \beta_3(Rev_{ist} * Char_s) + X_{is}\theta_t\kappa_1 + X_{is}\theta_t Union_s\kappa_2 + \delta_i + \lambda_{st} + Q_{is}\theta_t + \mu_{ist}, \quad (5)$$

where $Char_s$ includes one of three baseline state characteristics that are shown in columns 1 and 2 of Table 3 to be highly correlated with state teachers' union power: 1988 presidential democratic vote share, 1990 median income, and 1990 fraction of adults 25 years of age and older with a Bachelor's degree or higher. Note that because $Char_s$ is interacted with state aid, we instrument for the interaction term $Rev_{ist} * Char_s$ using a first stage specification that is identical to equation (2) except the dependent variable is now the $Rev_{ist} * Char_s$ interaction term.[22] If β_2 withstands the addition of these union power correlates interacted with state aid,

[22] Further, there are three additional instruments, namely, $Q1_{is} * SFR_{st} * Char_s$, $Q2_{is} * SFR_{st} * Char_s$, and $Q3_{is} * SFR_{st} * Char_s$.

this provides reassurance that β_2 identifies the effects of union power and not unobserved characteristics associated with union power.

Panel A of Table 5 presents results based on specifications where we interact state aid with the state share voting democratic in the 1988 presidential election. While the point estimates change somewhat in magnitude, controlling for heterogeneity by democratic vote share does not change the pattern of results. In panel B we interact state aid with state 1990 median income, and in panel C we interact state aid with 1990 fraction BA or higher. Again the results are largely robust to both of these additions.[23]

Finally, as shown in columns 1 and 2 of Table 3, there are other characteristics that are strongly correlated with union power. To account for those characteristics, we regressed our union power index on all of these state-level characteristics.[24] We then predict union power and re-estimate equation (5) using that predicted union power index for $Char_s$. We once again find that our results are largely robust to the inclusion of this additional interaction term, the main exceptions being that we lose statistical significance for the interaction of state aid and union power for expenditures and base teacher salary. Some loss of precision is not surprising given the strong correlations between this group of covariates and union power. However, the fact that our results are largely robust to the inclusion of state aid interacted with this index that captures all of the observed covariates highly correlated with union power is reassuring.

[23] Appendix Table 6 shows that the results are robust to simultaneously including two of these characteristics at a time, instrumenting for each separately.

[24] We include in this regression the following seven characteristics: 1988 Democratic vote share, and 1990 median household income, population density, poverty, fraction non-white, fraction B.A. or higher, and fraction less than high school.

E. School Finance Reform Coding and Sample Restriction Robustness

In this section we explore the robustness of our results to decisions about the way we code SFRs and restrict the sample (results shown in Appendix Table 7). First, we implement a stacked difference-in-differences design where instead of choosing one reform from each state that experienced a reform, we include all identified reforms, creating separate panels for each. This check implicitly tests robustness to the few differences between our coding of SFRs and those of Lafortune et al. (2018), given that these differences reflect choices over which reform is the "primary" reform in states that experience multiple reforms. Second, we exclude the handful of reforms that are not court-ordered. Third, we include the years spanning the Great Recession (2009-2011). Fourth, we include states that adopted matching aid formulas. Fifth, we drop all states that did not experience a SFR during our sample period.

Finally, recall that we drop Michigan and Wyoming because they adopted reforms which effectively eliminated local discretion over funding. However, a number of states also adopted reforms that imposed a limit on how much a district may spend on education. As noted by Jackson et al. (2014), such reforms are likely to reduce spending per-pupil, particularly for the highest income districts in a state for whom spending limits are most likely to be binding. To examine that possibility, we drop districts in the top tercile of 1980 household income from our sample. Our results are generally robust to all of these checks.[25]

[25] While the pattern is similar when we add in the states that adopted matching aid formulas, given that three of the four states that implemented matching aid formulas tend to be weak union states, we now find somewhat less crowd-out than before among states with weaker unions, which is expected given that the introduction of matching aid would at least partially offset any crowd-out effect.

F. Effects by Expenditure Type

In this section we estimate effects separately by expenditure sub-categories. This accomplishes two goals. First, it provides us with an alternative approach to examining whether teachers' unions favor spending state aid increases on class size reductions (i.e., teacher hiring) or on increasing teacher compensation. Specifically, note that instructional expenditures are primarily composed of expenditures on teacher compensation. Furthermore, recall that in Table 2, we find that reform-induced increases in state aid have similar effects on class size in both strong and weak union states. Thus, if we find that reform-induced increases in state aid have a larger effect on instructional expenditures in strong union states than weak union states, this would suggest that the strong union states must be spending more of the marginal dollar of increased instructional spending on raising teacher compensation.

The second reason we explore effects by expenditure subcategories is that while we focus our examination of the allocation of resources on teacher salary increases and class size reductions, other inputs to education production can be important as well. Thus, we examine how much of each dollar of SFR-induced state aid passes through to various subsets of expenditures, for example, current expenditures versus capital outlay, and among current expenditures, instructional versus non-instructional spending.

In Table 6, we find a similar pattern of results for instructional expenditures as we did for current expenditures, with a 32 cent increase in weak teachers' union states (25^{th} percentile) and 44 cent increase in strong union states (75^{th} percentile). The similarly sized or marginally smaller class size reduction in the strong teachers' union states, along with this larger increase in instructional expenditures, suggests that districts in strong union states focused more on increasing teacher compensation than did districts in weak union states.

We also find heterogeneity by teachers' union strength in the effects of SFR-induced increases in state aid on non-instructional expenditures (column 4) and on capital outlays (column 5), though the interaction of state aid and union power is statistically insignificant for the latter. Districts in strong union states see a 34 cent increase in non-instructional spending and 19 cent increase in capital outlays for every dollar increase in state aid compared to only a 23 cent and 14 cent increase, respectively, in weak union states. Thus, while there are important differences in how teachers' union power affects instructional spending, there are also important differences across these other spending categories. This suggests teachers' unions prefer not only higher teacher salaries, but also increases in non-instructional items that may improve working conditions, such as classroom, curricular, and administrative support, as well as school infrastructure improvements.

G. Effects on Student Achievement

To examine whether the differences in spending by teachers' union power translated into differences in student performance, we use restricted-access microdata from the National Assessment of Educational Progress (NAEP). The NAEP provides representative samples of math and reading test scores in grades four and eight from over 100,000 students nationwide every other year since 1990. Following Lafortune, Rothstein, and Schanzenbach (LRS, 2018), we standardize the individual scores by subject and grade to the distribution in the first tested year, and then aggregate the microdata to the district-subject-grade-year level, weighting the individual scores by the individual NAEP weight.[26] Unlike effects on expenditures, effects of

[26] For more details about the NAEP microdata, please see the online appendix, as well as LRS (2018) and Jacob and Rothstein (2016). Note that LRS (2018) further aggregate their data to the state-by-district income quintile-by-subject-by-grade-by-year level. We leave the data at the district-subject-grade-year level to be consistent with our prior analyses, which are all at the district level, but show in Appendix Table 8 that the results are robust to aggregating the data to the state-by-district income quintile-by-subject-by-grade-by-year level as in LRS (2018).

the reforms on achievement are not expected to appear immediately. Consequently, we modify our main specification in two ways. First, we focus on the reduced form impact of the reforms instead of instrumenting for spending. Second, we allow the impact to evolve linearly during the post-reform period instead of including a single post indicator as we do in our first stage analyses. Specifically, we estimate the following specification:

$$NAEP_{ijgst} = \phi_0 + \phi_1 YearsPost_{st} + \phi_2 YearsPost_{st} * Union_s$$
$$+ X_{is}\theta_t\kappa_1 + X_{is}\theta_t Union_s\kappa_2 + \pi_{jg} + \delta_i + \lambda_{rt} + Q_{is}\theta_t + \zeta_{ijgst}, \qquad (6)$$

where $NAEP_{ijgst}$ is the average score in district i, in tested subject j and grade g, in state s, and year t; $YearsPost_{st}$ equals zero for non-reform states and for reform states prior to the reform, and equals the number of years since the reform in reform states; π_{jg} is a vector of subject-by-grade fixed effects; ζ_{ijgst} is a random disturbance term; and all other terms are as defined in equation (1). As before, we cluster the standard errors at both the district and state-year level.

Table 7 presents the reduced form effects of SFRs on achievement. Without including the union interaction, we find an overall impact of SFRs of 0.007 standard deviations (SDs) per year, or 0.07 SDs ten years after a reform. This impact is driven by increases of 0.009 SDs per year in districts in the bottom tercile of within state median income.[27] These effects, however, mask important heterogeneity. When we include the union interaction for all districts, there is a 0.009 SD per year impact at the mean level of state teachers' union power, and a statistically significantly larger 0.004 effect for one standard deviation higher union power. For low-income districts, the effect is 0.011 SDs per year at the mean union power level, and 0.006 SDs greater for a 1 SD higher level of union power. This translates to an effect of 0.008 SDs per year, or

[27] LRS (2018) do not report the effect for all districts, but in column 3 of their Table 5, they find an increase of 0.007 SDs per year in their bottom quintile districts – comparable to the effect we find of 0.009.

0.08 SDs ten years post-reform, for weak teachers' union states (25th percentile). For strong union states (75th percentile), the effect is twice as large, or 0.016 SDs per year (0.16 SDs higher ten years post-reform). The effect among the top income tercile districts is smaller and not significantly different by teachers' union power.[28]

In Appendix Figure VI, we show event-study pictures that, as in LRS (2018), show no pre-trend in achievement followed by a steady post-reform increase in test scores driven by the lowest income districts. As in Table 7, a gap in test scores between weak and strong union states emerges after the SFRs, with the effects concentrated among the lowest income districts. These findings suggest that the larger expenditure increases in strong teachers' union states in response to SFRs translated into larger student achievement gains.

While a thorough exploration of the mechanisms behind these achievement impacts is difficult in this context, we conduct back-of-the-envelope calculations to understand the extent to which effects are due to changes in class size versus spending on other inputs. We use the results from the Tennessee STAR class size experiment, which reduced class sizes by 33% and increased achievement by 0.22 SDs, as a benchmark to estimate the fraction of our achievement results that are due to class size reductions (Krueger, 1999). We find that almost half of the achievement increase among weak union states (25th percentile) is due to class size reduction, while only a quarter of the increase is due to class size in strong union states (75th percentile).[29]

[28] Appendix Table 9 shows that the results are essentially identical excluding the baseline controls (as in LRS (2018)) or including only the basic, not expanded, set of controls.

[29] In weak-union states, state aid increased by about $1000 post-reform in low-income districts (see Appendix Figure IIIc), class sizes shrunk by 5.7% per $1000 of state aid, and achievement in low-income districts increased by 0.08 SDs. So, this 17.3% (=5.7/33) of the STAR class size reduction would improve achievement by 0.038 SDs (=0.17*0.22), or 47.5% (=0.038/0.08) of the achievement gains from SFRs. In strong-union states, state aid increased by about $1500 post-reform in low-income districts, class sizes shrunk by 4.4% per $1000 of state aid, so 6.6% post-reform (=4.4*[1500/1000]), and achievement in low-income districts increased by 0.16 SDs. So, this 20% (=6.6/33) of the STAR class size reduction would improve achievement by 0.044 SDs (=0.20*0.22), or 27.5% (=0.04.4/0.16) of the achievement gains from SFRs.

These informal calculations suggest that the additional spending on inputs to education production other than class size reduction in strong-union states was an important mechanism behind the larger achievement gains. However, evidence from the literature on the impacts of inputs such as teacher salaries, capital spending, and current non-instructional spending is too mixed and inconclusive for us to confidently disentangle the relative importance of each. While we cannot completely identify all the mechanisms, the magnitudes of our estimates are consistent with the recent literature finding that money matters in education production.[30]

VI. Conclusion

School finance reforms led to some of the largest intergovernmental transfers from states to local school districts in U.S. history. In spite of the importance of understanding how school finance reforms affected local spending decisions, and the strong theoretical connection between teachers' unions and resource allocation, the question of whether and how teachers' unions influenced local governments' allocation of additional state aid remains unexplored by previous work. In this paper, we examine the role of teachers' unions in determining the extent to which school finance reform-induced increases in state aid translated into increased education spending by local districts and the allocation of these expenditures.

Our results suggest unions played a critical role in determining both the amount of state aid that translated into education expenditures, as well as the allocation of these funds. School districts in states with the strongest teachers' unions increased education expenditures nearly one-for-one with increases in state aid in response to school finance reforms, whereas states

[30] LRS (2018) finds that $1000 increased spending from SFRs improves achievement by between 0.12 and 0.24 SDs for low-income districts 10 years after a reform. We estimate a 0.16 SD effect from an approximately $1000 increase in current expenditure increases 10 years after a reform in strong union states (see Appendix Figure IVd), and a 0.08 SD effect from the approximately $500 expenditure increase in weak union states. These informal calculations suggest that the achievement impacts in both types of states fall within the expected range from LRS (2018).

with the weakest teachers' unions substantially reduced local tax effort, with education expenditures increasing less than 25 cents on the dollar. Furthermore, the school spending in strong teachers' union states was allocated more toward increasing teacher salaries, while districts in weaker teachers' union states spent the money primarily on hiring new teachers. We find that achievement gains due to the reforms were significantly larger in strong teachers' union states than they were in weak teachers' union states. Our results are robust to strategies that address the potential endogeneity of teachers' union strength, suggesting that we are identifying the effects of the teachers' unions, and not unobserved cross-state differences correlated with union power.

Our results have several implications. First, our results support local politics as an important explanation for the flypaper effect, and specifically, the strength of local unions in ensuring that grants stick where they hit. Second, our finding that reform-induced increases in state aid led to significantly larger increases in educational expenditures in states with strong teachers' unions provides an important new perspective on the effectiveness of the SFR movement that began in the 1970's: the recent studies documenting the success of these reforms mask the critical insight that in the absence of teachers' unions, the reforms would have led to large increases in property tax relief with little change for schools or students.

That said, our results are subject to several caveats. First, as noted previously, it is possible that our results are driven in part by the influence of strong teachers' unions on the specific design elements of reforms. Strong teachers' unions may have used their influence to advocate for specific structures in the school finance reforms that would discourage local crowd-out or level-up school spending. While we find little evidence that that the type of reform implemented by states is correlated with state teachers' union power, it is nevertheless still possible that strong teachers' unions influenced the design features of reforms in a way that affected both the amount of state aid that passed through to local expenditures and the allocation of those expenditures. Second, some states bundled other policy changes into their school finance reform efforts.[31] The differential achievement effects by union power that we identify may have been partly driven by unions advocating for (or against) other reforms such as school accountability and school choice policies that states implemented in conjunction with their school finance reforms.[32]

Finally, our results provide an important perspective on the impacts of teachers' unions. In response to the large increases in state aid induced by SFRs, teachers' unions appear to have acted primarily in a manner consistent with the objective of maximizing the welfare of their members, namely by increasing the size of school district budgets and channeling increases in state aid toward teacher compensation. However, the outcome of this rent-seeking behavior aligned with the objectives of the SFR movement, ensuring that the reforms were effective in reducing inequality across school districts in education resources and student achievement.

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[31] For example, in response to the 1993 court ruling in McDuffy v. Secretary of the Executive Office of Education, the Massachusetts state legislature bundled school finance reform with several other reforms including, state oversight of school performance, examination-based assessments and data collection, and the authorization of charter schools.

[32] To examine this concern, we collected the year that every state authorized charter schools or inter-district choice (if ever). We then include indicators for these reforms along with their interactions with union power in our achievement specifications. The results are essentially identical to those reported in Table 7 (see Appendix Table 10).

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Table 1: Summary Statistics

	Full Sample		Strong Union States		Weak Union States	
	Mean	St. Dev.	Mean	St. Dev.	Mean	St. Dev.
	(1)	(2)	(3)	(4)	(5)	(6)
Per-Pupil Outcomes						
Total Revenue	10,890	3,814	11,704	4,083	9,200	2,431
Local Revenue	5,217	3,760	5,919	4,108	3,762	2,305
Current Expenditures	9,347	3,091	10,051	3,323	7,887	1,817
Other Outcomes						
Pupil-Teacher Ratio	16.3	3.1	16.6	3.4	15.9	2.6
Base Instructional Salary	37,305	5,329	39,289	5,809	35,038	3,555
Control Variables						
Baseline Enrollment	3,751	15,112	3,393	16,795	4,495	10,781
Median Income in 1980	17,204	5,327	18,495	5,506	14,527	3,708
Fraction Urban in 1980	0.550	0.299	0.608	0.289	0.430	0.282
Fraction Black in 1980	0.066	0.110	0.048	0.074	0.102	0.154
Fraction BA or Higher in 1980	0.137	0.090	0.149	0.097	0.113	0.064
Number of States	42		21		21	
Number of Districts	9,177		6,111		3,066	
Number of Observations	181,756		122,635		59,121	

Notes: The sample is all school districts in the continental U.S., excluding Kansas, Kentucky, Michigan, Missouri, Texas, and Wyoming, from 1986 through 2008. All dollar amounts are in 2015 dollars. Strong (weak) union states are those above (less than or equal to) the median value of the state union power measure described in the text.

Table 2: Effects of State Aid by Teacher Union Power

	Total Revenue		Local Revenue		Current Expenditures		Pupil-Teacher Ratio		Base Salary
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A. Union Power Index (Continuous)									
State Aid	0.644***	0.675***	-0.325***	-0.291***	0.484***	0.498***	-0.832***	-0.838***	0.322
	(0.077)	(0.078)	(0.074)	(0.076)	(0.075)	(0.078)	(0.141)	(0.144)	(0.324)
State Aid * Union	0.324***	0.302***	0.277***	0.270***	0.211***	0.193***	0.172	0.144	0.505**
	(0.068)	(0.067)	(0.063)	(0.064)	(0.063)	(0.066)	(0.113)	(0.118)	(0.248)
Estimated Effect at:									
25th Pctle. of Union Index	0.476***	0.518***	-0.468***	-0.431***	0.375***	0.398***	-0.921***	-0.912***	0.060
	(0.095)	(0.089)	(0.089)	(0.086)	(0.077)	(0.076)	(0.164)	(0.160)	(0.428)
75th Pctle. of Union Index	0.884***	0.899***	-0.119	-0.091	0.640***	0.641***	-0.705***	-0.731***	0.696***
	(0.076)	(0.086)	(0.075)	(0.086)	(0.093)	(0.104)	(0.147)	(0.163)	(0.225)
Panel B. Mandatory CB Status (0, 1)									
State Aid	0.195	0.091	-0.677***	-0.746***	0.062	-0.026	-1.074***	-1.046***	-0.351
	(0.175)	(0.200)	(0.154)	(0.173)	(0.142)	(0.164)	(0.356)	(0.393)	(0.732)
State Aid * Union	0.552***	0.655***	0.436***	0.508***	0.494***	0.586***	0.260	0.200	0.893
	(0.158)	(0.191)	(0.137)	(0.165)	(0.130)	(0.159)	(0.326)	(0.377)	(0.586)
Panel C. Alternative Union Power Index (0, 1, 2, 3)									
State Aid	0.175	0.179	-0.671***	-0.650***	0.084	0.089	-1.455***	-1.482***	-0.343
	(0.180)	(0.189)	(0.157)	(0.165)	(0.140)	(0.149)	(0.413)	(0.466)	(0.781)
State Aid * Union	0.195***	0.191***	0.147***	0.139**	0.159***	0.157***	0.235*	0.241	0.318
	(0.059)	(0.067)	(0.052)	(0.059)	(0.049)	(0.056)	(0.134)	(0.158)	(0.239)
Observations	181,756		181,756		181,756		179,862		16,598
Expanded Controls	No	Yes	No	Yes	No	Yes	No	Yes	No

Notes: The sample is as in Table 1. All results are from 2SLS/IV models where the endogenous variables of interest are state aid and its interaction with state teacher union power ("Union"). The instruments are an indicator for school finance reform adoption interacted with 1980 district median income terciles and those variables further interacted with "Union." Each column and panel presents results from a separate regression where the dependent variable is listed in the top row. All specifications include: 1) controls for baseline district enrollment and 1980 district median income interacted with a linear time trend as well as those two variables interacted with both a linear time trend and the union power measure, 2) an indicator for whether the state-year is subject to a binding tax or expenditure limit, 3) district fixed effects, 4) census region-by-year fixed effects, and 5) 1980 district median income tercile dummies interacted with a linear time trend. Columns 2, 4, 6 and 8 add additional controls for 1980 district fraction of the population black, fraction urban, and fraction with a BA or higher, each interacted with a linear time trend, as well as those same variables interacted with both a linear time trend and the union power measure. Robust standard errors, clustered at both the district and state-year level, in parentheses.
* significant at 10%, ** significant at 5%, *** significant at 1%.

Table 3: State Border Sample Balancing Tests

	Full Sample		Counties Less Than 50 Miles from State Border		Counties Adjacent to State Border	
	Union Coef.	P-Value	Union Coef.	P-Value	Union Coef.	P-Value
	(1)	(2)	(3)	(4)	(5)	(6)
County-Level Democratic Vote Shares						
Dem Vote Share 1984	3.254**	0.011	-1.11	0.733	1.31	0.621
Dem Vote Share 1988	3.340***	0.001	-0.59	0.869	2.64	0.348
Dem Vote Share 1992	4.036***	0.000	0.73	0.752	1.98	0.299
1990 District-Level Characteristics						
Total Population	-2,017	0.612	-1,578	0.757	1,327	0.786
Population Density	92.16**	0.024	46.37*	0.063	60.56*	0.053
Number of Households	-874	0.556	-658	0.735	443	0.811
Median HH Income	4602***	0.000	1207	0.388	815	0.475
Fraction Non-White	-0.039*	0.076	0.015*	0.086	0.004	0.679
Fraction Below Poverty	-0.029***	0.007	0.001	0.836	0.002	0.595
Fraction Unemployed	0.009	0.508	0.006	0.248	0.007	0.234
Fraction Population 65 Plus	-0.002	0.658	0.002	0.467	0.007**	0.049
Fraction Less Than HS	-0.041***	0.000	-0.005	0.508	-0.007	0.362
Fraction HS Degree	0.004	0.615	-0.011	0.357	-0.005	0.608
Fraction Some College	0.013*	0.057	0.005	0.557	0.002	0.691
Fraction BA or Higher	0.024***	0.000	0.011	0.300	0.010	0.309
Fraction Homeowner	-0.004	0.625	-0.002	0.779	-0.003	0.640
Number of Districts	9,177		5,148		3,154	

Notes: Each point estimate is from a separate district-level (cross-sectional) regression of the listed county or district characteristic on our continuous state teacher union power measure. Columns 1 and 2 include the full sample of districts used in Tables 1-2. Columns 3 and 4 restrict to districts in counties whose centroid is less than 50 miles from a state border. Columns 5 and 6 restrict to counties adjacent to a state border. Columns 3-6 include state border fixed effects. Robust standard errors are clustered by state in columns 1-2, and by state-by-border in columns 3-6.
* significant at 10%, ** significant at 5%, *** significant at 1%.

Table 4: State Border Sample Analysis

	Total Revenue	Local Revenue	Current Expenditures	Pupil-Teacher Ratio	Base Salary
	(1)	(2)	(3)	(4)	(5)
Panel A. Counties 50 Miles From State Border					
State Aid	0.730***	-0.256***	0.570***	-0.871***	0.433**
	(0.088)	(0.085)	(0.089)	(0.134)	(0.217)
State Aid * Union	0.229***	0.237***	0.215***	0.187**	0.198
	(0.061)	(0.061)	(0.055)	(0.093)	(0.189)
Panel B. Counties Adjacent to State Border					
State Aid	0.657***	-0.342***	0.505***	-0.806***	0.345
	(0.094)	(0.091)	(0.095)	(0.120)	(0.228)
State Aid * Union	0.238***	0.243***	0.238***	0.123	0.109
	(0.070)	(0.068)	(0.067)	(0.101)	(0.205)
Observations - Panel A	102,589	102,589	102,589	101,143	9,677
Observations - Panel B	62,213	62,213	62,213	61,458	5,991
Border-by-Year FEs	Yes	Yes	Yes	Yes	Yes
Expanded Controls	Yes	Yes	Yes	Yes	No

Notes: Each column and panel presents results from a separate 2SLS/IV regression where the dependent variable is listed in the top row and the specification matches Panel A from Table 2. The sample in Panel A includes only counties whose centroid is within 50 miles from the state border. The sample in Panel B includes only counties that are adjacent to a state border. All specifications include the controls and fixed effects (FEs) listed in the Table 2 notes, except that the region-by-year FEs are replaced with border-by-year FEs, where a border includes counties on both sides of a state border. Robust standard errors, clustered at both the district and state-year level, in parentheses.
* significant at 10%, ** significant at 5%, *** significant at 1%.

Table 5: Effects Controlling for Heterogeneity by State-Level Union Power Correlates

	Total Revenue	Local Revenue	Current Expenditures	Pupil-Teacher Ratio	Base Salary
	(1)	(2)	(3)	(4)	(5)
Panel A. 1988 Democrat Vote Share					
State Aid	0.636***	-0.332***	0.507***	-0.928***	0.394
	(0.088)	(0.085)	(0.079)	(0.162)	(0.350)
State Aid * Union	0.258***	0.227***	0.162**	0.135	0.473**
	(0.064)	(0.062)	(0.064)	(0.117)	(0.239)
Panel B. 1990 Median Income					
State Aid	0.700***	-0.270***	0.466***	-0.916***	0.143
	(0.082)	(0.079)	(0.075)	(0.153)	(0.407)
State Aid * Union	0.353***	0.317***	0.165***	0.036	0.388*
	(0.070)	(0.068)	(0.063)	(0.117)	(0.224)
Panel C. 1990 Fraction BA or Higher					
State Aid	0.671***	-0.300***	0.410***	-0.888***	0.224
	(0.073)	(0.070)	(0.077)	(0.148)	(0.418)
State Aid * Union	0.350***	0.313***	0.156**	0.066	0.472**
	(0.060)	(0.058)	(0.065)	(0.121)	(0.215)
Panel D. Predicted Union Index					
State Aid	0.751***	-0.212***	0.618***	-0.825***	0.290
	(0.078)	(0.078)	(0.073)	(0.140)	(0.300)
State Aid * Union	0.274***	0.244***	0.106	0.167	0.309
	(0.079)	(0.076)	(0.070)	(0.135)	(0.237)
Observations	181,756	181,756	181,756	179,862	16,598
Expanded Controls	Yes	Yes	Yes	Yes	No

Notes: The sample is as in Table 1. Each column and panel presents results from a separate 2SLS/IV regression where the dependent variable is listed in the top row and the specification matches Panel A from Table 2. All specifications include the controls and fixed effects listed in the Table 2 notes. Panel A further controls for state aid interacted with the 1988 state share voting for the Democratic presidential candidate, instrumented for by the school finance reform and income tercile dummies interacted with the vote share. Panel B replaces the 1988 vote share with 1990 state median income, Panel C replaces it with 1990 fraction of adults 25 years of age and older with a Bachelors degree or higher, and Panel D replaces it with the linear prediction of union power fitted from a regression of union power on the seven state-level covariates in Table 3 that are correlated with union status. Robust standard errors, clustered at both the district and state-year level, in parentheses.
* significant at 10%, ** significant at 5%, *** significant at 1%.

Table 6: Effects by Expenditure Type

	Total Expenditures	Current Expenditures			Capital Outlays
		All Current	Instruction	Non-Instruction	
	(1)	(2)	(3)	(4)	(5)
State Aid	0.656***	0.498***	0.371***	0.272***	0.162***
	(0.088)	(0.078)	(0.056)	(0.062)	(0.060)
State Aid * Union	0.200**	0.193***	0.098**	0.091**	0.043
	(0.078)	(0.066)	(0.043)	(0.044)	(0.045)
Estimated Effect at:					
25th Pctle. of Union Index	0.552***	0.398***	0.321***	0.225***	0.140***
	(0.092)	(0.076)	(0.055)	(0.062)	(0.053)
75th Pctle. of Union Index	0.804***	0.641***	0.444***	0.339***	0.194**
	(0.110)	(0.104)	(0.071)	(0.076)	(0.081)
Sample Mean	10,987	9,347	5,749	3,463	1,019
Observations	181,756	181,756	181,756	181,636	180,822
Expanded Controls	Yes	Yes	Yes	Yes	Yes

Notes: The sample is as in Table 1. Each column presents results from a separate 2SLS/IV regression where the dependent variable is listed in the top rows and the specification matches Panel A from Table 2. All specifications include the controls and fixed effects listed in the Table 2 notes. Robust standard errors, clustered at both the district and state-year level, in parentheses.
* significant at 10%, ** significant at 5%, *** significant at 1%.

Table 7: Reduced Form Effects of School Finance Reforms on Student Achievement

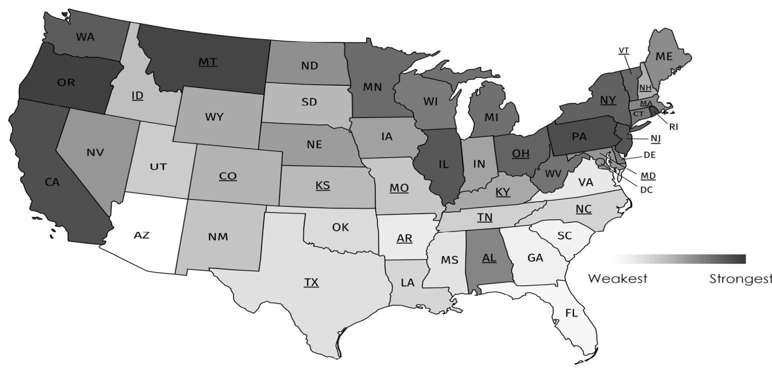
	All Districts		Bottom Tercile		Top Tercile	
	(1)	(2)	(3)	(4)	(5)	(6)
Years Post-Reform	0.007***	0.009***	0.009***	0.011***	0.004*	0.006***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Years Post-Reform * Union		0.004*		0.006**		0.002
		(0.002)		(0.003)		(0.002)
Estimated Effect at:						
25th Pctle. of Union Index		0.007***		0.008***		0.005**
		(0.002)		(0.002)		(0.002)
75th Pctle. of Union Index		0.012***		0.016***		0.007***
		(0.003)		(0.004)		(0.003)
Observations	64,901		17,159		27,328	
Expanded Controls	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The sample is at the district-subject-grade-year level. Each column presents results from a separate regression of weighted mean NAEP scores on a linear post-reform trend (columns 1, 3, and 5), and the post-reform trend interacted with our measure of union power (columns 2, 4, and 6). All specifications include the controls and fixed effects listed in the Table 2 notes. Robust standard errors, clustered at both the district and state-year level, in parentheses.

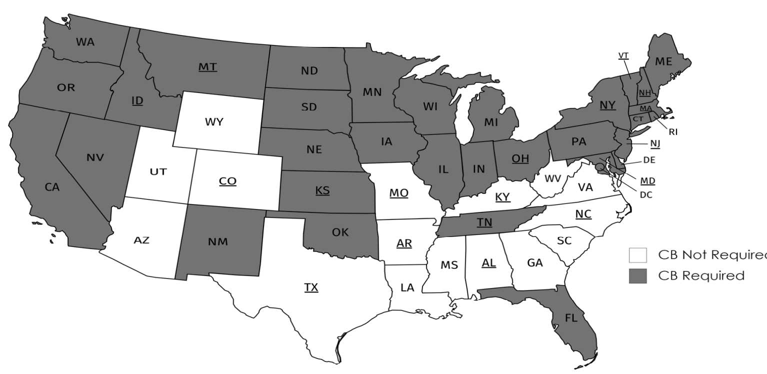
* significant at 10%, ** significant at 5%, *** significant at 1%.

Figure I: United States Map, by State Teacher Union Power

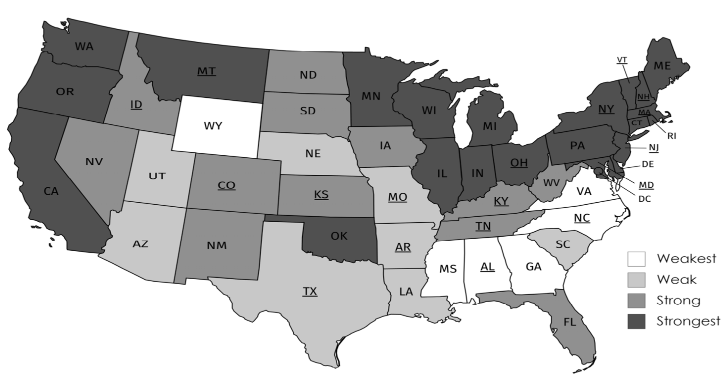
(a) Continuous Teacher Union Power Index



(b) Mandatory Collective Bargaining (CB) Status



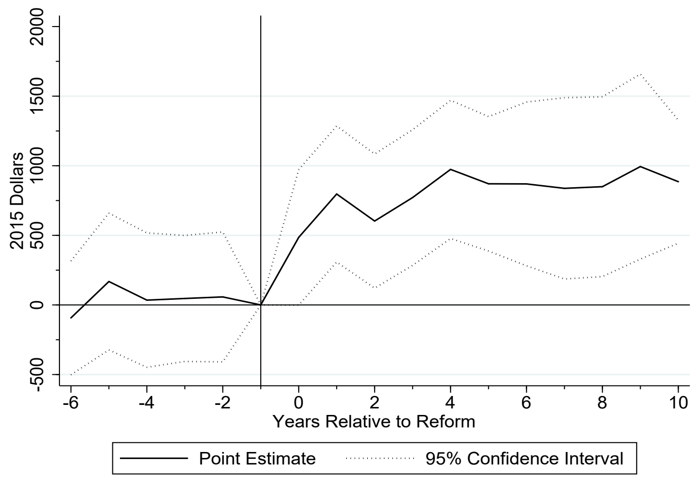
(c) CB and Right-to-Work Index



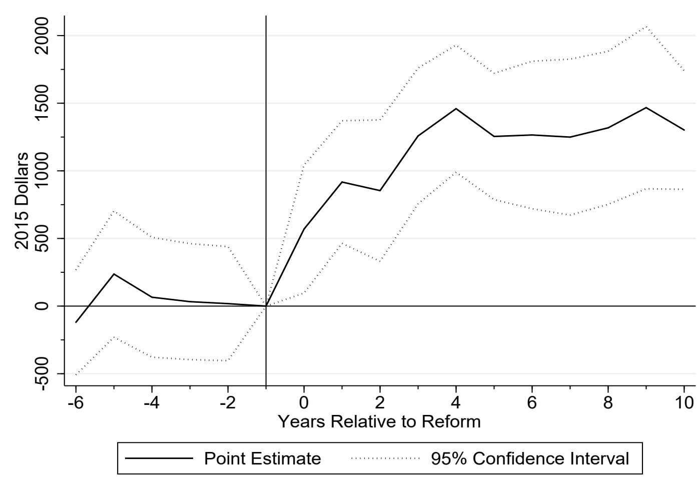
Notes: Map shows states by their values for the three teacher union power measures used in this paper. Figure (a) shows states by the continuous teacher union power index provided by the Fordham Institute (2012); figure (b) by their public sector collective bargaining (CB) law status; and (c) by the four-value index incorporating CB law and right-to-work status. States that experienced a school finance reform have an underlined state abbreviation.

Figure II: Effects of School Finance Reforms on State Aid, by District Income Tercile

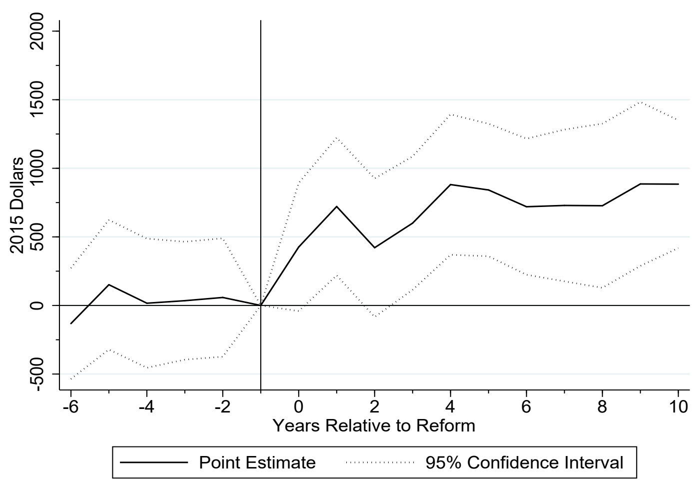
(a) All Districts



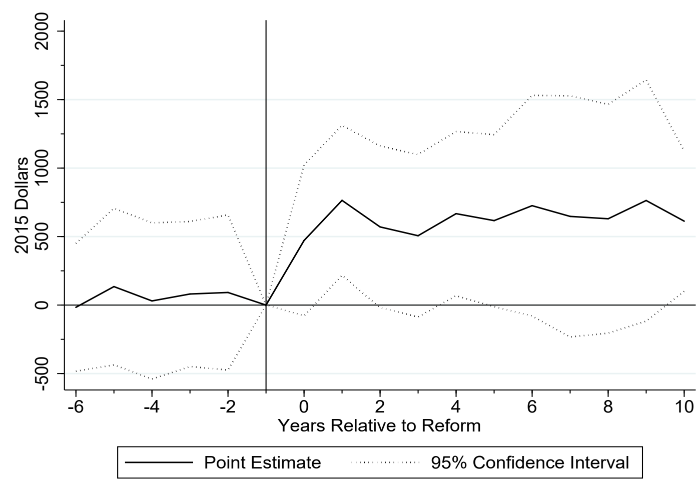
(b) Bottom Income Tercile



(c) Middle Income Tercile

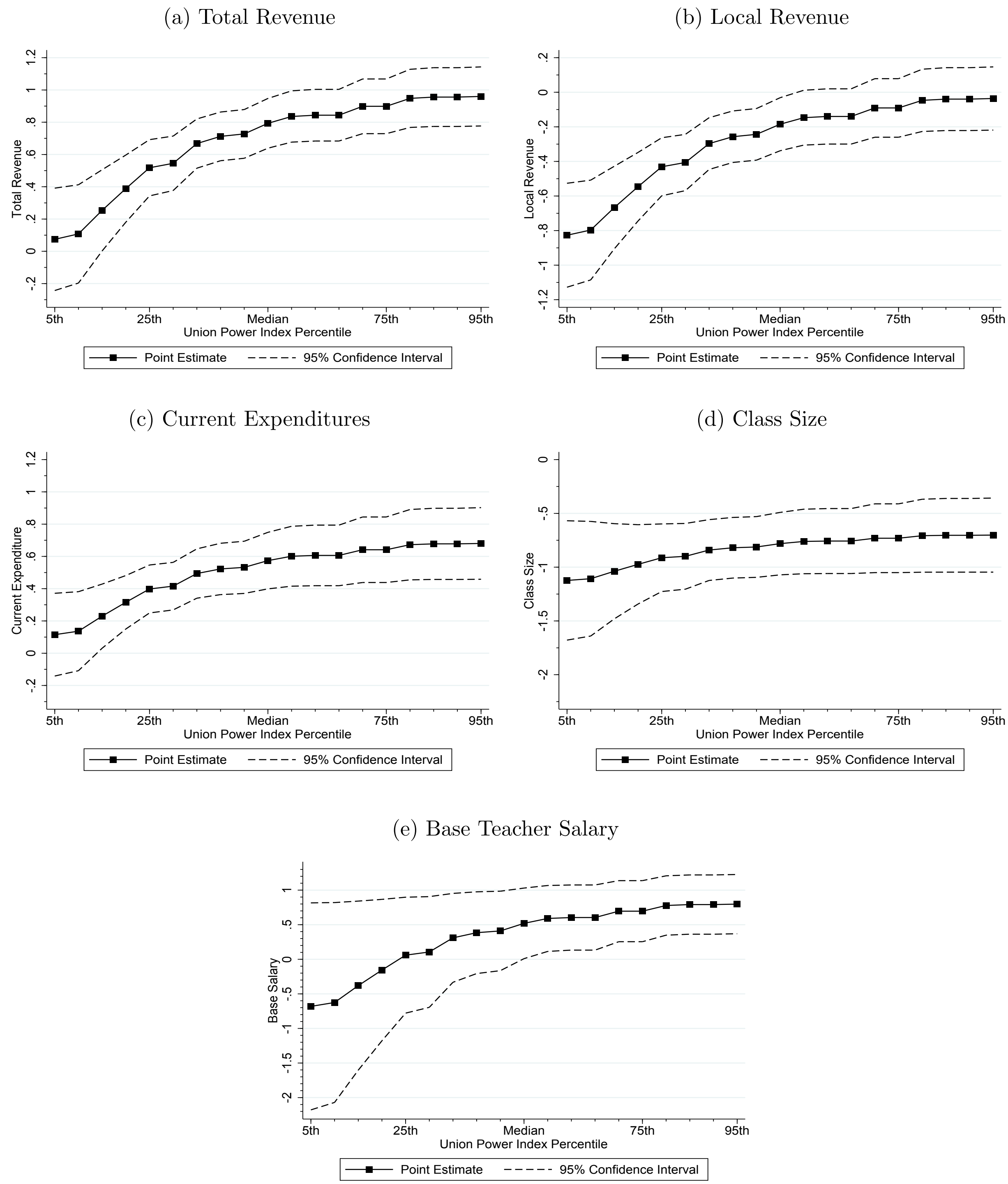


(d) Top Income Tercile



Notes: Figures show event study estimates of the effects of school finance reforms on per-pupil state aid to school districts, by 1980 district income tercile. Solid lines are point estimates, and dashed lines are 95% confidence intervals.

Figure III: Effects of School Finance Reforms by State Teacher Union Power Percentile



Notes: Each figure shows point estimates (solid line) and 95% confidence intervals (dashed lines) from 2SLS regressions of the dependent variable on state aid per-pupil and aid interacted with our continuous state teacher union power index. The figures show the calculated point estimate at percentiles of the union power measure. For example, Figure (a) shows that for every dollar increase in state aid due to school finance reforms in states with the weakest teacher unions, total revenue increases by about 10 cents. For states with the strongest teacher unions, it increased nearly 1-for-1.

School Finance Reforms, Teachers' Unions, and the Allocation of School Resources: Online Technical Appendix

School District Financial Data

Our primary data source is the Local Education Agency Finance Survey (F-33) maintained by the National Center for Education Statistics (NCES) for the period 1990-91 through 2011-12. We augment this data with earlier versions of the F-33 survey provided by the U.S. census for the years 1986-87 through 1989-90. We limit the sample to traditional school districts, namely elementary, secondary and unified school systems, and thus drop charter schools, college-grade systems, vocational or special education systems, non-operating school systems and educational service agencies. We also drop a small number of observations associated with the following types of educational agencies: 1) Regional education services agencies, or county superintendents serving the same purpose; 2) State-operated institutions charged, at least in part, with providing elementary and/or secondary instruction or services to a special-needs population; 3) Federally operated institutions charged, at least in part, with providing elementary and/or secondary instruction or services to a special-needs population; and 4) other education agencies that are not a local school district. We also drop Hawaii and the District of Columbia from the sample, both of which are comprised of a single school district.

As noted by Gordon (2004) and Lafortune et al. (2018) among others, the F-33 finance data tends to be noisy and thus we impose several additional restrictions to reduce noise in the finance data. First, we restrict the sample to school districts with enrollment of 250 students or more in every year of our sample. This removes 20% of district-year observations but only 1.2% of total enrollment. Second, following Lafortune et al. (2018) we exclude any district-year observation with enrollment more than double the district's average enrollment over the entire sample period, as well as district-year observations with enrollment that is more than 15% above or below the prior year or the subsequent year's enrollment. Combined these additional restrictions remove only 1.2% of district-year observations.

We also impose several restrictions that are based on the values of the finance variables. First, we drop district-year observations if the reported values of our finance outcome measures (e.g. total revenue, total expenditures, state aid) are less than zero. Second, following Lafortune et al. (2018) we drop district-year observations for the per-pupil revenue or expenditures variables that are at least five times greater or five times smaller than the state-by-year mean of the variable. These restrictions remove less than 1.1% of district-year observations.

Finally, we used the consumer price index to deflate all of the per-pupil revenue and expenditure variables we utilize into constant 2015 dollars.

Non-Financial Data

We merge the F-33 finance data with several other data sources. First, we merge the finance data with data from the annual common core of data (CCD) school district universe surveys that provide staff counts for every school district. We then construct district-level estimates of the pupil-teacher ratio by dividing total full time equivalent teachers (FTE) by total district enrollment.[1] Second, we merge the finance data with the Census of Population and Housing, 1980: Summary Tape File 3F for School Districts, to obtain data on district-level 1980 median household income, fraction black, fraction urban, and fraction of adults 25 and older with a Bachelor's degree. Third, we merge our data with the 1980 Census of Population and Housing county estimates. We then use 1980 county-level estimates on median household income, fraction black, fraction urban, and fraction of adults 25 and older with a Bachelor's degree to replace the approximately 3.5% of district-level observations that are missing for each of these variables with their county-level equivalent. Fourth we merge our data with information on whether and when a state enacted a binding tax and expenditure limitation on local school districts. Following Jackson et al. (2016), information on the timing of enactment of tax and expenditure limits is from Downes and Figlio (1998). We supplement and cross-checked this measure with information on more recent limitations from Winters (2008) and from the Advisory Commission on Intergovernmental Relations (1995). Finally, we merge our data with indicators for the four census regions in the United States, namely the Northeast, South, Midwest and West.

[1] In our main analysis we utilize the full sample of districts with valid pupil teacher ratios. However, because staff counts tend to be noisy, we also followed Lafortune et al. (2018) and set values of the pupil teacher ratio that were in the top or bottom 2% of the within state-year distribution to missing. Imposing this restriction led to coefficient estimates that were qualitatively and quantitatively similar to those reported in the text.

NAEP Data

We use restricted-access microdata from the National Assessment of Educational Progress (NAEP) to examine student achievement. The NAEP, commonly referred to as the "the Nation's report card," has been implemented every other year since 1990 by the U.S. Department of Education. In each wave, representative samples of school districts from across the U.S. are required to have their students take the NAEP math and reading test in grades four and eight.[2] We restrict the data to the NAEP reporting sample and to public schools. Rather than providing a single score for each student, NAEP provides random draws from each student's estimated posterior ability distribution based on their test performance and background characteristics. We use the mean of these five draws for each student, essentially creating an Empirical Bayes "shrunken" estimate of the student's latent ability. We then standardize the mean score by subject and grade to the first year each subject and grade was tested. We then aggregate these individual-level scores to the district-subject-grade-year level, weighting the individual scores by the individual NAEP weight. Finally, we merge the data to our primary dataset using the National Center for Education Statistics (NCES) unique district ID that is available in the Common Core of Data (CCD) and in the NAEP data from 2000 onward. Prior to 2000, the NAEP data did not include this unique district ID. NCES provided us with a crosswalk that they developed in collaboration with Westat to link the NAEP district ID and the NCES district ID for those earlier years.[3]

Simple Model of School District Response to State Aid

Appendix Figure Ia illustrates the potential effect teachers' unions may have on the size of school district budgets by focusing on the choice problem facing a school district before and after an increase in intergovernmental aid brought about by a school finance reform.[4] The innermost budget constraint illustrates the case where the school district receives no intergovernmental aid and allocates total district income, *M*, freely between private consumption,

[2] The NAEP also tests other subjects such as writing, science, and economics, but we focus on math and reading because they were tested most consistently across years.
[3] Thank you to Daniel McGrath at the U.S. Department of Education Institute of Education Sciences (IES) for his assistance locating and working with this crosswalk file.
[4] Cascio et al. (2013) provide a graphical illustration similar to Appendix Figure Ia to illustrate the effect on an increase in federal Title I spending.

X, and spending on schools, S.[5] Given resident preferences, the district maximizes utility at point A, which leads to school spending of S_1 per-pupil. The introduction of intergovernmental aid in the amount of G per-pupil causes a parallel shift in the budget constraint to M+G. If teachers' unions have no effect on local fiscal policies, the school district then chooses to move to point B, associated with indifference curve U_i, which leads to school spending of S_2 per-pupil. Note that in this case school spending increases by the marginal propensity to spend out of income, which leads to a relatively small increase in S and a larger increase in X.

Now consider a teachers' union whose members have preferences like those depicted by indifference curve U_j. As noted by Rose and Sonstelie (2010), the primary way that teachers' unions impose their preferences onto districts is by using their political and financial resources to help ensure that school boards are comprised of individuals sympathetic to their preferences, thus gaining control over both the size and allocation of the district budget.[6] In this case, the union would direct intergovernmental aid in favor of its preferences, and the district will choose to move to point C, which leads to school spending of S_3 per-pupil. School spending rises by much more than the marginal propensity to spend out of income, leading to the classic flypaper effect: intergovernmental grant revenue is diverted away from property tax relief and towards increased school spending.

Finally, note that if teachers' unions are primarily rent-seeking, then increasing the size of the budget allows them to bargain for higher teacher salaries or other items that disproportionally benefit teachers. Specifically, as shown in Appendix Figure Ib, if teachers' unions are primarily rent-seeking they may bargain for a larger share of any budget increase to be allocated towards inputs that primarily benefit teachers, such as teacher salaries, as opposed to other inputs that may be more efficient in raising student achievement, such as class size reductions. Of course, even if unions are benevolent actors primarily interested in promoting student interests and school quality, they may still bargain for higher teacher salaries if higher salaries increase school productivity. Ultimately, arguments can be made that increased spending

5 For simplicity we normalize the prices of both X and S to one.

6 In the typical U.S. school district, the school board votes on the property tax rate and district budget size. Board members are elected town citizens that must weigh additional school resources against other town needs and increased property tax burden. See Moe (2006) for evidence that teachers' unions are successful at getting the candidates that they back elected to school boards. Specifically, he finds that the effect of union endorsement on the probability of getting elected is roughly equivalent to the effect of being an incumbent.

in any expenditure subcategories, such as current instructional, current non-instructional, and capital, could benefit teachers and improve student achievement.

School Finance Reform Design and Union Power

To investigate whether teachers' unions influence school finance reform design at the state level, we used the coding of reforms developed by Jackson, Johnson & Persico (2014) (henceforth JJP). Appendix Table D.1 of JJP provides information on the funding formula used before a SFR and the funding formula used after a SFR. We used that information to classify all the school finance reforms in our sample into the six groups categorized by JJP: 1) flat grants (FG), 2) minimum foundation plans (MFP), 3) equalizations plans (EP), 4) local effort equalizations (LE), 5) spending limits (SL), and 6) full state funding (FS). We did this for all the school finance reforms that occurred during our sample timeframe. Thus, if a state had more than one reform, we coded the type of reform they implemented after each SFR. This yields 72 observation for a "stacked" sample of states where states can have more than one SFR, which is the same procedure we used for our stacked difference-in-differences robustness check. Table 1 summarizes the types of reforms implemented using the abbreviations listed above.

Table 1: Types of Reforms

Type of SRF	Mean
MFP	0.534
EP	0.411
LE	0.123
FG	0.137
SL	0.219
FS	0.027

We then calculated the correlation between our union power measure and the type of reform implemented to examine if there was any systematic pattern to the type of reforms implemented in weaker versus stronger union states. Table 2 shows the correlation between the state union power index (higher number implies stronger union) and types of SFRs.

Table 2: Correlations Between Union Power Index and Types of SFRs

Type of SRF	Correlation
MFP	-0.035
EP	0.068
LE	-0.161
FG	-0.103
SL	-0.068
FS	0.045

As the table illustrates, there is little evidence of a systematic relationship between the type of reform implemented and state teachers' union power: the correlations between state union power and the type of reform implemented are all relatively low. Furthermore, we find inconsistent evidence as to whether stronger unions advocate for reforms that would discourage local crowd-out. Flat grant (FG) reforms would be *more* susceptible to local crowd-out and we do find evidence that strong union states are less likely to implement such reforms. On the other hand, local effort (LE) or matching grant reforms would discourage local crowd-out but strong union states are less likely to implement these reforms.

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Appendix Table 1: Complete School Finance Reform Event List

State	Year	Type	Event
(1)	(2)	(3)	(4)
Alabama	**1993**	**Court**	**Alabama Coalition for Equity (ACE) v. Hunt; Harper v. Hunt**
Arkansas	**1994**	**Court**	**Lake View v. Arkansas**
Arkansas	2002	Court	Lake View v. Huckabee
Arkansas	2005	Court	Lake View v. Huckabee
Colorado	**1994**	**Legislative**	**Public School Finance Act of 1994**
Colorado	2000	Legislative	Bill 181; Various Other Acts
Idaho	**1993**	**Court**	**Idaho Schools for Equal Educational Opportunity v. Evans (ISEEO)**
Idaho	1998	Court	Idaho Schools for Equal Educational Opportunity v. State (ISEEO III)
Idaho	2005	Court	Idaho Schools for Equal Educational Opportunity v. Evans (ISEEO V)
Kansas	**2005**	**Court**	**Montoy v. State; Montoy v. State funding increases**
Kentucky	**1989**	**Court**	**Rose v. Council for Better Education, Inc.**
Maryland	1996	Court	Bradford v. Maryland State Board of Education
Maryland	**2002**	**Legislative**	**Bridge to Excellence in Public Schools Act (BTE) (Senate Bill 856)**
Massachusetts	**1993**	**Court**	**McDuffy v. Secretary of the Executive Office of Education; Massachusetts Education Reform Act**
Missouri	**1993**	**Court**	**Committee for Educational Equality v. State of Missouri; Outstanding Schools Act (S.B. 380)**
Montana	1993	Bill	House Bill 667
Montana	**2005**	**Court**	**Columbia Falls Elementary School v. State**
New Hampshire	1993	Court	Claremont New Hampshire v. Gregg
New Hampshire	1997	Court	Claremont School District v. Governor
New Hampshire	**1999**	**Court**	**Claremont v. Governor (Claremont III); RSA chapter 193-E**
New Hampshire	2002	Court	Claremont School District v. Governor
New Jersey	1990	Court	The Quality Education Act; Abbot v. Burke
New Jersey	1996	Legislative	Comprehensive Educational Improvement and Financing Act of 1996
New Jersey	**1998**	**Court**	**Abbott v. Burke**
New York	2003	Court	Campaign for Fiscal Equity, Inc. v. State
New York	**2006**	**Court**	**Campaign for Fiscal Equity, Inc. v. State**
North Carolina	**1997**	**Court**	**Leandro v. State**
North Carolina	2004	Court	Hoke County Board of Education v. State
Ohio	**1997**	**Court**	**DeRolph v. Ohio**
Ohio	2000	Court	DeRolph v. Ohio; Increased school funding (see 93 Ohio St.3d 309)
Ohio	2002	Court	DeRolph v. Ohio
Tennessee	1992	Legislative	The Education Improvement Act
Tennessee	**1995**	**Court**	**Tennessee Small School Systems v. McWherter**
Tennessee	2002	Court	Tennessee Small School Systems v. McWherter
Texas	**1989**	**Court**	**Edgewood Independent School District v. Kirby**
Vermont	**1997**	**Court**	**Brigham v. State**
Vermont	2003	Legislative	Revisions to Act 68; H.480

Notes: List includes all school finance reform events that we include in the stacked difference-in-difference model presented in Appendix Table 7. Bolded reforms are those used in our main analyses.

Appendix Table 2: State Teacher Union Power, by State and Union Power Measure

State	Collective Bargaining	Right-to-Work	CB and RTW Index	Fordham Index	
				Index	Rank
(1)	(2)	(3)	(4)	(5)	(6)
Alabama	Prohibited	Yes	0	2.25	18
Arizona	Allowed	Yes	1	0.72	48
Arkansas	Allowed	Yes	1	1.02	44
California	Mandatory	No	3	2.84	5
Colorado	Allowed	No	2	1.78	30
Connecticut	Mandatory	No	3	2.37	15
Delaware	Mandatory	No	3	2.30	17
Florida	Mandatory	Yes	2	0.99	47
Georgia	Prohibited	Yes	0	1.01	45
Idaho	Mandatory	Yes	2	1.66	33
Illinois	Mandatory	No	3	2.72	7
Indiana	Mandatory	No	3	1.93	26
Iowa	Mandatory	Yes	2	1.99	25
Kansas	Mandatory	Yes	2	1.69	32
Kentucky	Allowed	No	2	1.91	27
Louisiana	Allowed	Yes	1	1.29	39
Maine	Mandatory	No	3	2.20	20
Maryland	Mandatory	No	3	2.13	22
Massachusetts	Mandatory	No	3	2.24	19
Michigan	Mandatory	No	3	2.45	13
Minnesota	Mandatory	No	3	2.50	12
Mississippi	Prohibited	Yes	0	1.08	42
Missouri	Prohibited	No	1	1.52	35
Montana	Mandatory	No	3	3.06	2
Nebraska	Mandatory	Yes	2	2.01	24
Nevada	Mandatory	Yes	2	2.05	23
New Hampshire	Mandatory	No	3	1.86	29
New Jersey	Mandatory	No	3	2.82	6
New Mexico	Allowed	No	2	1.54	34
New York	Mandatory	No	3	2.61	9
North Carolina	Prohibited	Yes	0	1.38	38
North Dakota	Mandatory	Yes	2	2.17	21
Ohio	Mandatory	No	3	2.59	10
Oklahoma	Mandatory	No	3	1.26	40
Oregon	Mandatory	No	3	3.18	1
Pennsylvania	Mandatory	No	3	2.85	4
Rhode Island	Mandatory	No	3	2.86	3
South Carolina	Allowed	Yes	1	1.00	46
South Dakota	Mandatory	Yes	2	1.75	31
Tennessee	Mandatory	Yes	2	1.44	37
Texas	Prohibited	Yes	0	1.11	41
Utah	Allowed	Yes	1	1.48	36
Vermont	Mandatory	No	3	2.55	11
Virginia	Prohibited	Yes	0	1.06	43
Washington	Mandatory	No	3	2.72	8
West Virginia	Allowed	No	2	2.44	14
Wisconsin	Mandatory	No	3	2.33	16
Wyoming	Prohibited	Yes	0	1.91	28

Notes: This table lists values by state for each of the teacher union power measures used in the paper. The list includes all states in the continental U.S., excluding D.C. The teacher union power index in columns 5 and 6 is a slightly modified version of the index from Fordam Foundation's publication "How Strong Are U.S. Teacher Unions? A State-by-State Comparison" (2012) by Winkler, Scull, and Zeehandelaar, and ranges from 0 to 3.

Appendix Table 3: First-Stage Estimates by Union Power Measure

	Union Power Index (Continuous)		Mandatory CB Status (0/1)		Alt. Union Power Index (0, 1, 2, 3)	
	State Aid	Aid *Union	State Aid	Aid *Union	State Aid	Aid *Union
	(1)	(2)	(3)	(4)	(5)	(6)
SFR * Q1	1089***	67	504***	-243***	382***	-1154***
	(109)	(79)	(95)	(48)	(119)	(220)
SFR * Q2	592***	-117	325***	-62	430***	-162
	(103)	(75)	(84)	(43)	(106)	(187)
SFR * Q3	578***	-126	291***	153***	416***	340
	(118)	(85)	(100)	(58)	(124)	(251)
SFR * Union * Q1	164*	1321***	759***	1547***	305***	1716***
	(96)	(93)	(156)	(133)	(69)	(183)
SFR * Union * Q2	-166*	593***	337**	731***	74	713***
	(93)	(85)	(150)	(132)	(66)	(178)
SFR * Union * Q3	-179*	319***	351*	441***	70	477**
	(97)	(101)	(183)	(163)	(80)	(220)
F-Statistic	23	36	19	28	22	21
Observations	181,756	181,756	181,756	181,756	181,756	181,756
Expanded Controls	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The sample is as in Table 1. Each column presents results from a separate regression where the dependent variable is state aid per-pupil in columns 1, 3 and 5, and state aid per-pupil interacted with the union power measure listed in the column headers in columns 2, 4, and 6. All specifications include the complete set of controls and fixed effects listed in the Table 2 notes. Robust standard errors, clustered at both the district and state-year level, in parentheses.
* significant at 10%, ** significant at 5%, *** significant at 1%.

Appendix Table 4: OLS, Just-Identified IV, and State-Level Clustering

	Total Revenue	Local Revenue	Currrent Expenditures	Pupil-Teacher Ratio	Base Salary
	(1)	(2)	(3)	(4)	(5)
Panel A. OLS Instead of IV					
State Aid	0.750***	-0.267***	0.236***	-0.150***	0.148***
	(0.027)	(0.027)	(0.015)	(0.014)	(0.049)
State Aid * Union	-0.000	0.007	-0.041***	0.044**	0.082
	(0.019)	(0.019)	(0.014)	(0.018)	(0.055)
Panel B. Just-Identified IV					
State Aid	0.752***	-0.202***	0.513***	-0.757***	0.519**
	(0.071)	(0.070)	(0.077)	(0.125)	(0.261)
State Aid * Union	0.255***	0.217***	0.178***	0.143	0.302*
	(0.054)	(0.051)	(0.054)	(0.100)	(0.181)
Panel C. State-Level Clustering					
State Aid	0.675***	-0.291	0.498***	-0.838***	0.322
	(0.180)	(0.181)	(0.176)	(0.292)	(0.521)
State Aid * Union	0.302**	0.270*	0.193	0.144	0.505
	(0.136)	(0.136)	(0.150)	(0.217)	(0.393)
Panel D. SASS Sample Only					
State Aid	0.629***	-0.323***	0.575***	-1.094***	0.322
	(0.118)	(0.115)	(0.107)	(0.252)	(0.324)
State Aid * Union	0.294***	0.232**	0.162**	0.482**	0.505**
	(0.101)	(0.092)	(0.081)	(0.229)	(0.248)
Panel E. Drop Tercile 3 Districts					
State Aid	0.708***	-0.268***	0.484***	-0.841***	0.448
	(0.073)	(0.070)	(0.074)	(0.137)	(0.291)
State Aid * Union	0.269***	0.213***	0.175***	0.229*	0.436*
	(0.071)	(0.067)	(0.068)	(0.130)	(0.208)
Observations (Panel A, B, C)	181,756	181,756	181,756	179,862	16,598
Observations (Panel D)	16,598	16,598	16,598	16,498	16,598
Observations (Panel E)	118,711	118,711	118,711	117,474	10,120
Expanded Controls	Yes	Yes	Yes	Yes	No

Notes: The sample is as in Table 1. Each column and panel presents results from a separate regression where the dependent variable is listed in the top row. Panel A estimates ordinary least squares (OLS) models where we do not instrument for state aid and its interaction with state teacher union power ("Union"). Panel B estimates IV models where instead of six instruments there are only two, the interaction of SFR with the tercile 1 dummy and their interaction with Union. Panel C estimates the main model clustering the standard errors at the state level. Panel D drops all districts not in the SASS sample, and Panel E drops all districts in the third tercile of within-state median income. All specifications include the controls and fixed effects listed in the Table 2 notes. Robust standard errors, clustered at both the district and state-year level in Panels A, B, D, and E, and at the state level in Panel C, in parentheses.
* significant at 10%, ** significant at 5%, *** significant at 1%.

Appendix Table 5: SASS Estimation Sample Cell Sizes

State	1987	1990	1993	1999	2003	2007	All Years
AL	81	93	94	83	86	82	519
AZ	0	73	76	71	64	59	343
AR	70	86	54	77	76	82	445
CA	181	144	160	176	158	159	978
CO	44	52	30	55	44	50	275
CT	44	81	66	60	60	73	384
DE	14	16	14	13	13	14	84
FL	49	51	53	47	58	66	324
GA	71	77	44	77	71	66	406
ID	51	58	61	59	61	58	348
IL	128	100	103	82	64	71	548
IN	109	107	105	99	87	98	605
IA	81	106	86	83	83	89	528
LA	53	54	54	56	55	48	320
ME	41	56	79	70	64	71	381
MD	17	20	19	17	19	22	114
MA	98	100	116	71	77	70	532
MN	69	91	73	91	90	101	515
MS	81	107	60	93	98	93	532
MT	34	47	48	39	56	52	276
NE	40	51	49	57	47	53	297
NV	14	15	15	13	10	14	81
NH	43	49	47	47	47	35	268
NJ	78	83	39	82	81	70	433
NM	32	45	34	40	52	48	251
NY	129	101	115	96	71	66	578
NC	77	82	71	70	59	69	428
ND	27	39	37	36	34	36	209
OH	152	109	112	90	85	74	622
OK	79	120	33	137	135	145	649
OR	61	72	71	64	55	69	392
PA	132	136	108	103	100	86	665
RI	27	31	30	25	21	25	159
SC	52	61	60	54	57	63	347
SD	47	55	42	53	47	44	288
TN	70	90	79	72	59	55	425
UT	28	32	29	32	28	28	177
VT	16	24	20	25	4	13	102
VA	70	74	74	68	66	70	422
WA	68	86	87	85	67	81	474
WV	39	54	53	47	42	52	287
WI	94	103	92	104	94	100	587
All States	2,691	3,031	2,692	2,819	2,645	2,720	16,598

Notes: Table provides the number of districts by state and year in the main SASS analysis. The overall mean, median, 25th, and 75th percentile for the count of distrcts by state and year in the sample are 66, 64, 45, and 83, respectively.

Appendix Table 6: Effects Controlling for Heterogeneity by Union Power Correlates (Two at a Time)

	Total Revenue	Local Revenue	Current Expenditures	Pupil-Teacher Ratio	Base Salary
	(1)	(2)	(3)	(4)	(5)
Panel A. Vote Share and Median Income					
State Aid	0.658***	-0.313***	0.489***	-0.977***	0.209
	(0.089)	(0.085)	(0.077)	(0.167)	(0.428)
State Aid * Union	0.310***	0.276***	0.130**	0.043	0.363*
	(0.068)	(0.066)	(0.064)	(0.121)	(0.217)
Panel B. Vote Share and BA or Higher					
State Aid	0.664***	-0.314***	0.415***	-1.046***	0.265
	(0.090)	(0.086)	(0.087)	(0.181)	(0.526)
State Aid * Union	0.344***	0.308***	0.179***	0.007	0.474**
	(0.059)	(0.057)	(0.063)	(0.116)	(0.215)
Panel C. Median Income and BA or Higher					
State Aid	0.644***	-0.322***	0.394***	-0.860***	0.036
	(0.062)	(0.059)	(0.065)	(0.136)	(0.409)
State Aid * Union	0.276***	0.243***	0.096*	0.099	0.389*
	(0.054)	(0.053)	(0.055)	(0.107)	(0.232)
Observations	181,756	181,756	181,756	179,862	16,598
Expanded Controls	Yes	Yes	Yes	Yes	No

Notes: The sample is as in Table 1. Each column and panel presents results from a separate 2SLS/IV regression where the dependent variable is listed in the top row and the specification matches Panel A from Table 2. All specifications include the controls and fixed effects listed in the Table 2 notes. Panel A controls simultaneously for state aid interacted with both the 1988 state share voting for the Democratic presidential candidate and 1990 median income, separately instrumented for by the school finance reform (SFR) and income tercile dummies interacted with each. Panel B replaces 1990 median income with 1990 fraction BA or higher. Panel C controls for 1990 median income and 1990 fraction BA or higher. Robust standard errors, clustered at both the district and state-year level, in parentheses.
* significant at 10%, ** significant at 5%, *** significant at 1%.

Appendix Table 7: School Finance Reform Coding and Sample Robustness

	Total Revenue	Local Revenue	Current Expenditures	Pupil-Teacher Ratio	Base Salary
	(1)	(2)	(3)	(4)	(5)
Panel A. Stacked Diff-in-Diff Design					
State Aid	0.693***	-0.284***	0.537***	-0.661***	0.124
	(0.074)	(0.073)	(0.073)	(0.114)	(0.267)
State Aid * Union	0.328***	0.300***	0.258***	0.020	0.333*
	(0.049)	(0.049)	(0.048)	(0.079)	(0.174)
Panel B. Court-Ordered Reforms Only					
State Aid	0.703***	-0.275***	0.521***	-0.886***	0.393
	(0.078)	(0.077)	(0.079)	(0.147)	(0.326)
State Aid * Union	0.284***	0.262***	0.176***	0.184	0.467*
	(0.065)	(0.064)	(0.065)	(0.120)	(0.249)
Panel C. Include Great Recession					
State Aid	0.801***	-0.172**	0.632***	-0.788***	0.437
	(0.068)	(0.067)	(0.068)	(0.117)	(0.281)
State Aid * Union	0.204***	0.211***	0.161***	0.271**	0.413*
	(0.060)	(0.054)	(0.057)	(0.107)	(0.225)
Panel D. Include KS, KY, MO, TX					
State Aid	0.786***	-0.191**	0.553***	-0.896***	0.637**
	(0.076)	(0.074)	(0.076)	(0.150)	(0.298)
State Aid * Union	0.120**	0.108**	0.095*	0.124	0.137
	(0.053)	(0.051)	(0.053)	(0.103)	(0.221)
Panel E. Drop Untreated States					
State Aid	0.574***	-0.373***	0.347***	-0.625***	0.261
	(0.115)	(0.119)	(0.107)	(0.169)	(0.313)
State Aid * Union	0.195***	0.193***	0.120**	0.044	0.369**
	(0.058)	(0.057)	(0.054)	(0.112)	(0.160)
Panel F. Drop Top Income Districts					
State Aid	0.708***	-0.268***	0.484***	-0.841***	0.448
	(0.073)	(0.070)	(0.074)	(0.137)	(0.291)
State Aid * Union	0.269***	0.213***	0.175***	0.229*	0.436**
	(0.071)	(0.067)	(0.068)	(0.130)	(0.208)
Observations - Panel A	279,938	279,938	279,938	276,328	23,575
Observations - Panel B	181,756	181,756	181,756	179,862	16,598
Observations - Panel C	214,974	214,974	214,974	213,000	19,739
Observations - Panel D	214,958	214,958	214,958	213,058	19,069
Observations - Panel E	71,022	71,022	71,022	69,831	6,048
Observations - Panel F	118,711	118,711	118,711	117,474	10,120
Expanded Controls	Yes	Yes	Yes	Yes	No

Notes: Each column and panel presents results from a separate 2SLS/IV regression where the dependent variable is listed in the top row. All specifications include the controls and fixed effects listed in the Table 2 notes. Panel A uses a stacked difference-in-differences specification, which uses all SFRs instead of choosing one from each state (see text for details). Panel B only includes court-ordered school finance reforms. Panel C changes the sample to include the years 2009-2011. Panel D changes the sample to include Kansas, Kentucky, Missouri, and Texas. Panel E drops states that never had a SFR. Panel F drops districts in the top tercile of within-state 1980 median income. Robust standard errors, clustered at both the district and state-year level, in parentheses.
* significant at 10%, ** significant at 5%, *** significant at 1%.

Appendix Table 8: Effects on Achievement, Aggregating NAEP to State-Year-Grade-Subject Level

	All Districts		Bottom Tercile		Top Tercile	
	(1)	(2)	(3)	(4)	(5)	(6)
Years Post-Reform	0.008***	0.009***	0.006**	0.012***	0.003	0.004*
	(0.002)	(0.002)	(0.003)	(0.003)	(0.002)	(0.002)
Years Post-Reform * Union		0.003		0.008**		0.001
		(0.003)		(0.004)		(0.002)
Estimated Effect at:						
25th Pctle. of Union Index		0.008***		0.008***		0.003
		(0.002)		(0.003)		(0.002)
75th Pctle. of Union Index		0.012***		0.018***		0.005
		(0.004)		(0.005)		(0.003)
Observations	1,126		1,126		1,126	
Expanded Controls	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The sample is at the state-subject-grade-year level. Each column presents results from a separate regression of weighted mean NAEP scores on a linear post-reform trend (columns 1, 3, and 5), and the post-reform trend interacted with our measure of union power (columns 2, 4, and 6). All specifications include the controls and fixed effects listed in the Table 2 notes. Robust standard errors, clustered at the state-year level, in parentheses.

* significant at 10%, ** significant at 5%, *** significant at 1%.

Appendix Table 9: Reduced Form Effects of School Finance Reforms on Student Achievement Using Alternative Control Sets

	All Districts		Bottom Tercile		Top Tercile		All Districts		Bottom Tercile		Top Tercile	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Years Post-Reform	0.007***	0.008***	0.010***	0.012***	0.003*	0.004*	0.007***	0.008***	0.010***	0.013***	0.004*	0.004*
	(0.002)	(0.002)	(0.002)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Years Post-Reform * Union		0.005**		0.008***		0.002		0.005**		0.009***		0.001
		(0.002)		(0.003)		(0.002)		(0.002)		(0.003)		(0.002)
Estimated Effect at:												
25th Pctle. of Union Index		0.006***		0.008***		0.003		0.006***		0.008***		0.004
		(0.002)		(0.002)		(0.002)		(0.002)		(0.002)		(0.002)
75th Pctle. of Union Index		0.012***		0.018***		0.006		0.012***		0.019***		0.005
		(0.003)		(0.004)		(0.004)		(0.003)		(0.004)		(0.004)
Observations	64,901		17,159		27,328		64,901		17,159		27,328	
Basic Controls	No	No	No	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The sample is at the district-subject-grade-year level. Each column presents results from a separate regression of weighted mean NAEP scores on a linear post-reform trend (odd columns), and the post-reform trend interacted with our measure of union power (even columns). Columns 1-6 include no controls. Columns 7-12 include our basic set of controls. Robust standard errors, clustered at both the district and state-year level, in parentheses.

* significant at 10%, ** significant at 5%, *** significant at 1%.

Appendix Table 10: Effects of SFRs on Student Achievement Controlling for School Choice Policies

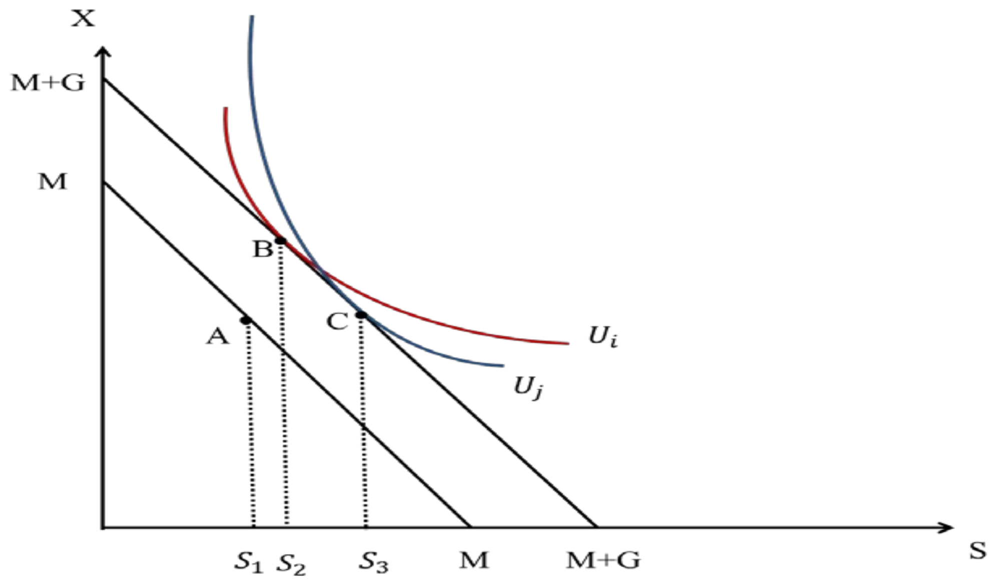
	All Districts		Bottom Tercile		Top Tercile	
	(1)	(2)	(3)	(4)	(5)	(6)
Years Post-Reform	0.007***	0.009***	0.009***	0.012***	0.004**	0.007***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Years Post-Reform * Union		0.004**		0.007***		0.002
		(0.002)		(0.003)		(0.002)
Estimated Effect at:						
25th Pctle. of Union Index		0.007***		0.008***		0.006**
		(0.002)		(0.002)		(0.002)
75th Pctle. of Union Index		0.012***		0.017***		0.008***
		(0.003)		(0.004)		(0.003)
Observations	64,901		17,159		27,328	
Expanded Controls	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The sample is at the district-subject-grade-year level. Each column presents results from a separate regression of weighted mean NAEP scores on a linear post-reform trend (columns 1, 3, and 5), and the post-reform trend interacted with our measure of union power (columns 2, 4, and 6). All specifications include the controls and fixed effects listed in the Table 2 notes, plus indicators for statewide charter school and inter-district choice policies and their interactions with union power. Robust standard errors, clustered at both the district and state-year level, in parentheses.

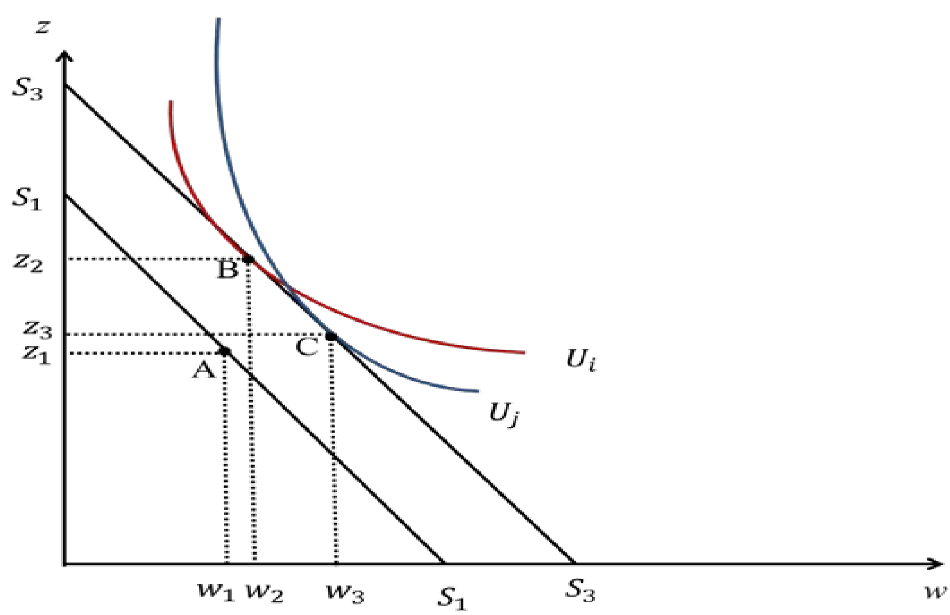
* significant at 10%, ** significant at 5%, *** significant at 1%.

Appendix Figure I: School District Responses to Intergovernmental Aid

(a) School Spending vs. Private Consumption



(b) Teacher Salaries vs. Other Inputs



Notes: Figure (a) shows the choice problem facing a school district before and after an increase in intergovernmental grant aid. Spending on schools is S and private consumption is X, where the price of both is normalized to one. The district has M income, and G is the amount of aid. Figure (b) shows the resource allocation choice problem facing a school district before and after an expansion of their budget from S_1 to S_3. The district chooses between teacher salaries, w, and a composite input, z, where the price of both is normalized to one. The teachers union's preferences are U_j.

Appendix Figure II: Union Power Index Components and Weightings

Area	Major Indicator and % of Total Score		Sub-Indicator and % of Total Score	
AREA 1: RESOURCES & MEMBERSHIP **20%**	1.1: Membership	6.7%	1.1.1: What percentage of public school teachers in the state are union members?	6.7%
	1.2: Revenue	6.7%	1.2.1: What is the total yearly revenue (per teacher in the state) of the state-level NEA and/or AFT affiliate(s)?	6.7%
	1.3: Spending on education	6.7%	1.3.1: What percentage of state expenditures (of state general funds, state restricted funds, state bonds, and federal "pass-through" funds) is directed to K-12 education?	2.2%
			1.3.2: What is the total annual per-pupil expenditure (of funds from federal, state, and local sources) in the state?	2.2%
			1.3.3: What percentage of total annual per-pupil expenditures is directed to teacher salaries and benefits?	2.2%
AREA 2: INVOLVEMENT IN POLITICS **20%**	2.1: Direct contributions to candidates and political parties	6.7%	2.1.1: What percentage of the total contributions to state candidates was donated by teacher unions?	3.3%
			2.1.2: What percentage of the total contributions to state-level political parties was donated by teacher unions?	3.3%
	2.2: Industry influence	6.7%	2.2.1: What percentage of the contributions to state candidates from the ten highest-giving sectors was donated by teacher unions?	6.7%
	2.3: Status of delegates	6.7%	2.3.1: What percentage of the state's delegates to the Democratic and Republican conventions were members of teacher unions?	6.7%
AREA 3: SCOPE OF BARGAINING **20%**	3.1: Legal scope of bargaining	6.7%	3.1.1: What is the legal status of collective bargaining?	3.3%
			3.1.2: How broad is the scope of collective bargaining?	3.3%
	3.2: Automatic revenue streams	6.7%	3.2.1: What is the unions' legal right to automatically collect agency fees from non-members and/or collect member dues via automatic payroll deductions?	6.7%
	3.3: Right to strike	6.7%	3.3.1: What is the legal status of teacher strikes?	6.7%
AREA 4: STATE POLICIES **20%**	4.1: Performance pay	2.9%	4.1.1: Does the state support performance pay for teachers?	2.9%
	4.2: Retirement	2.9%	4.2.1: What is the employer versus employee contribution rate to the teacher pension system?	2.9%
	4.3: Evaluations	2.9%	4.3.1: What is the maximum potential consequence for veteran teachers who receive unsatisfactory evaluation(s)?	1.4%
			4.3.2: Is classroom effectiveness included in teacher evaluations? If so, how is it weighted?	1.4%
	4.4: Terms of employment	2.9%	4.4.1: How long before a teacher earns tenure? Is student/teacher performance considered in tenure decisions?	1.0%
			4.4.2: How are seniority and teacher performance considered in teacher layoff decisions?	1.0%
			4.4.3: What percentage of the teaching workforce was dismissed due to poor performance?	1.0%
	4.5: Class size	2.9%	4.5.1: Is class size restricted for grades 1-3? If so, is the restriction larger than the national average (20)?	2.9%
	4.6: Charter school structural limitations	2.9%	4.6.1: Is there a cap (limit) placed on the number of charter schools that can operate in the state (or other jurisdiction) and/or on the number of students who can attend charter schools?	1.0%
			4.6.2: Does the state allow a variety of charter schools: start-ups, conversions, and virtual schools?	1.0%
			4.6.3: How many charter authorizing options exist? How active are those authorizers?	1.0%
	4.7: Charter school exemptions	2.9%	4.7.1: Are charter schools automatically exempt from state laws, regulations, and teacher certification requirements (except those that safeguard students and fiscal accountability)?	1.4%
			4.7.2: Are charter schools automatically exempt from collective bargaining agreements (CBAs)?	1.4%

Notes: Figure continued on next page...

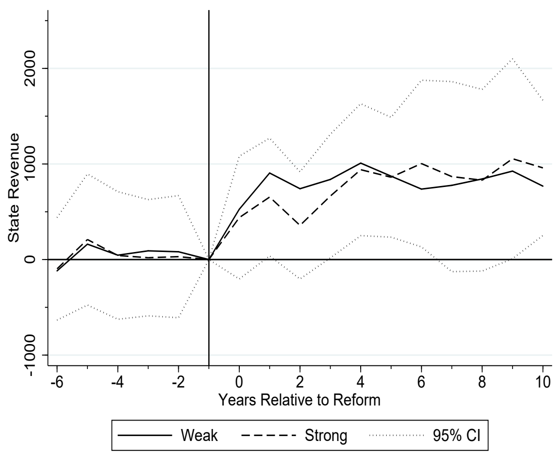
Appendix Figure II: Union Power Index Components and Weightings (...continued)

Area	*Major Indicator and % of Total Score*		*Sub-Indicator and % of Total Score*	
AREA 5: PERCEIVED INFLUENCE 20%	5.1: Relative influence of teacher unions	4.0%	5.1.1: How do you rank the influence of teacher unions on education policy compared with other influential entities?	4.0%
	5.2: Influence over campaigns	4.0%	5.2.1: How often do Democrat candidates need teacher union support to get elected?	2.0%
			5.2.2: How often do Republican candidates need teacher union support to get elected?	2.0%
	5.3: Influence over spending	4.0%	5.3.1: To what extent do you agree that, even in times of cutbacks, teacher unions are effective in protecting dollars for education?	2.0%
			5.3.2: Would you say that teacher unions generally make concessions to prevent reductions in pay and benefits, or fight hard to prevent those reductions?	2.0%
	5.4: Influence over policy	4.0%	5.4.1: To what extent do you agree that teacher unions ward off proposals in your state with which they disagree?	1.0%
			5.4.2: How often do existing state education policies reflect teacher union priorities?	1.0%
			5.4.3: To what extent were state education policies *proposed* by the governor during your state's latest legislative session in line with teacher union priorities?	1.0%
			5.4.4: To what extent were legislative *outcomes* of your state's latest legislative session in line with teacher union priorities?	1.0%
	5.5: Influence over key stakeholders	4.0%	5.5.1: How often have the priorities of state education leaders aligned with teacher union positions in the past three years?	2.0%
			5.5.2: Would you say that teacher unions typically compromise with policymakers to ensure that their preferred policies are enacted, or typically need not make concessions?	2.0%

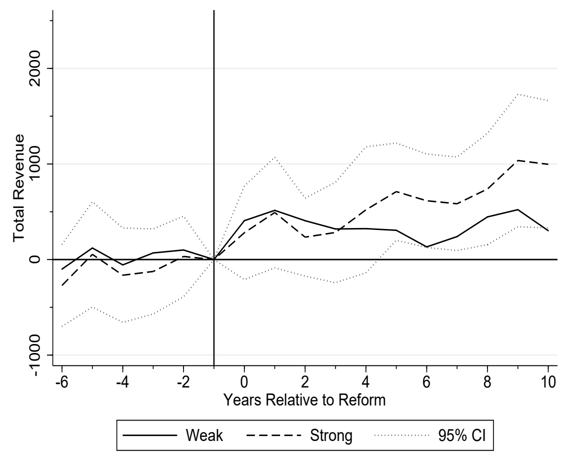
Notes: This figure is taken from Winkler, Scull, and Zeehandelaar (2012). It shows the components that comprise the primary teacher union power measure used in this paper and the relative weighting that each component receives. Our measure excludes components 1.3.1, 1.3.2, and 1.3.3, because they are likely influenced by school finance reforms, and thus endogenous. We instead increase the weight received by components 1.1.1 and 1.2.1 to 10 percent each, leaving the total weight for area 1 unchanged at 20 percent.

Appendix Figure III: Effects of Reforms on State Aid and Total Revenue, by Union Power

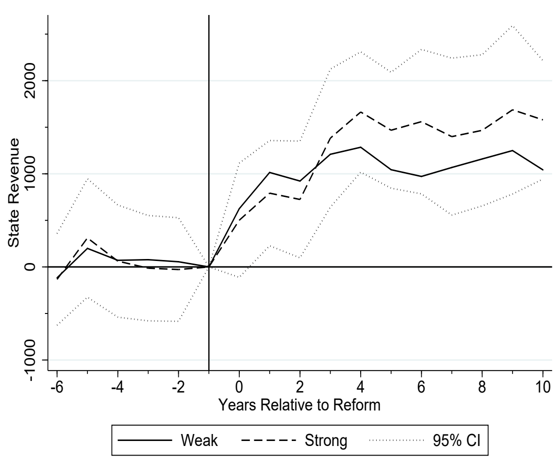
(a) State Aid - All Districts



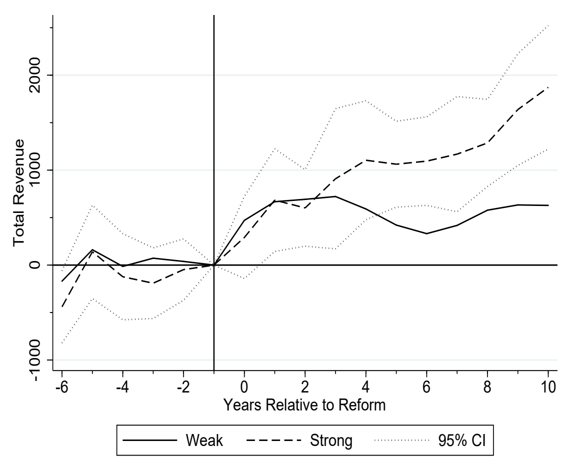
(b) Total Revenue - All Districts



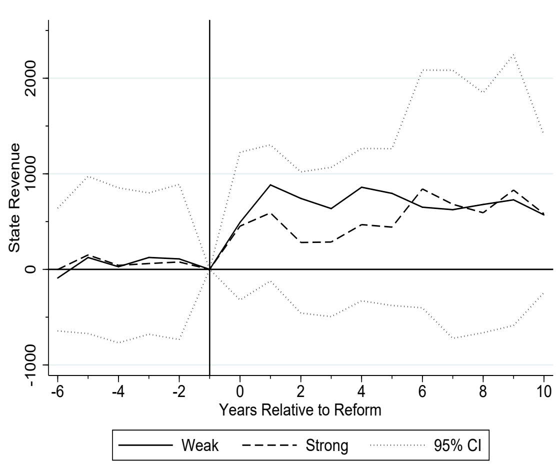
(c) State Aid - Bottom Income Tercile



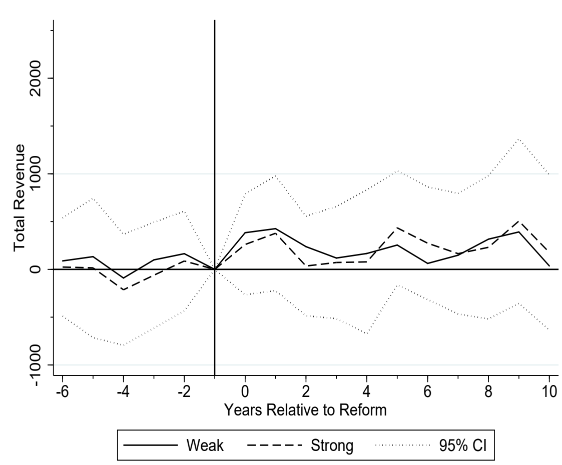
(d) Total Revenue - Bottom Income Tercile



(e) State Aid - Top Income Tercile



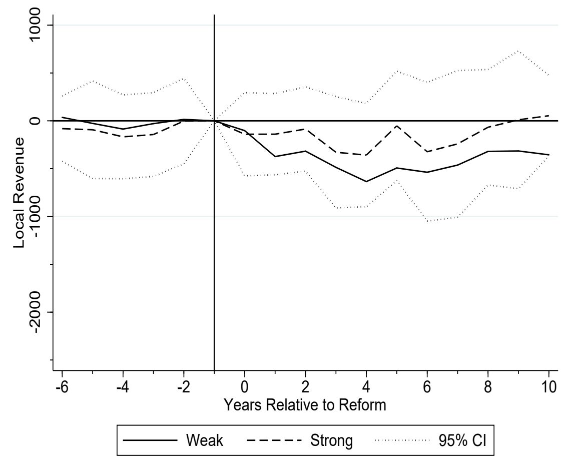
(f) Total Revenue - Top Income Tercile



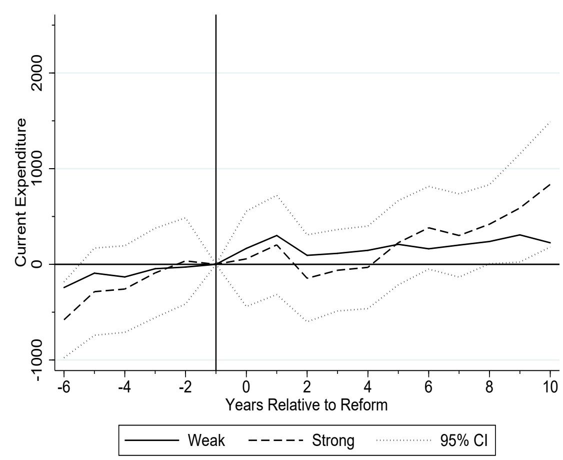
Notes: Figures (a), (c), and (e) show reduced form effects of school finance reforms on state aid to districts in states at the 25th and 75th percentiles of union power, denoted weak and strong, respectively. 95% confidence intervals are shown for the strong union point estimates. Figures (b), (d), and (f) show effects on district total revenue.

Appendix Figure IV: Effects of Reforms on Local Revenue and Current Expenditures, by Union Power

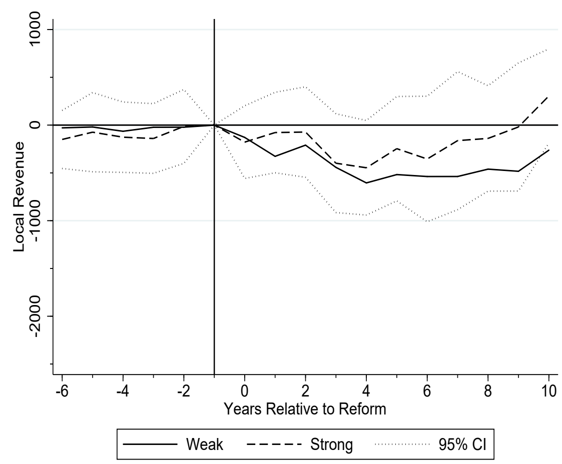
(a) Local Revenue - All Districts



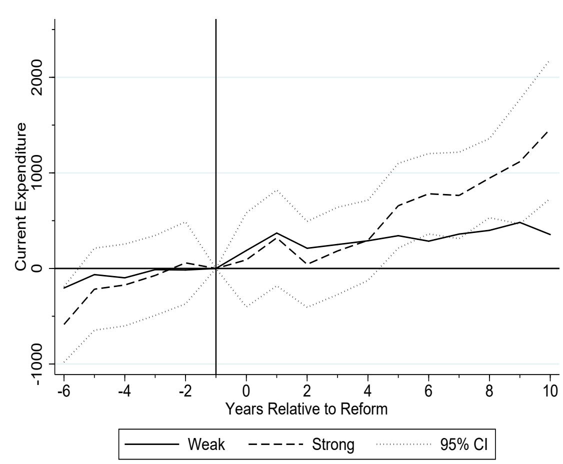
(b) Current Expenditure - All Districts



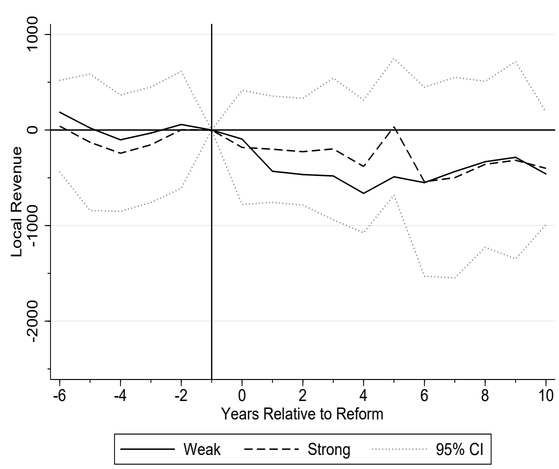
(c) Local Revenue - Bottom Income Tercile



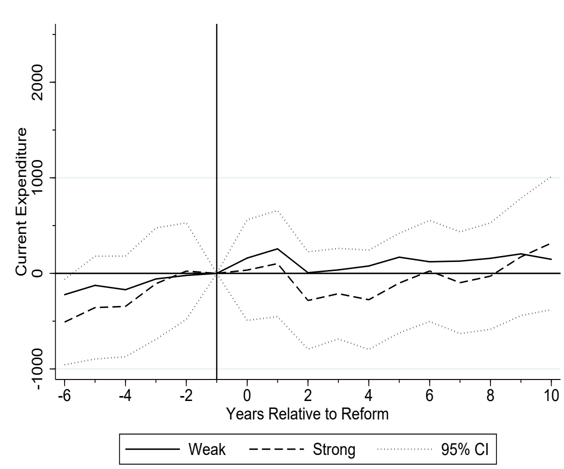
(d) Current Expenditure - Bottom Income Tercile



(e) Local Revenue - Top Income Tercile



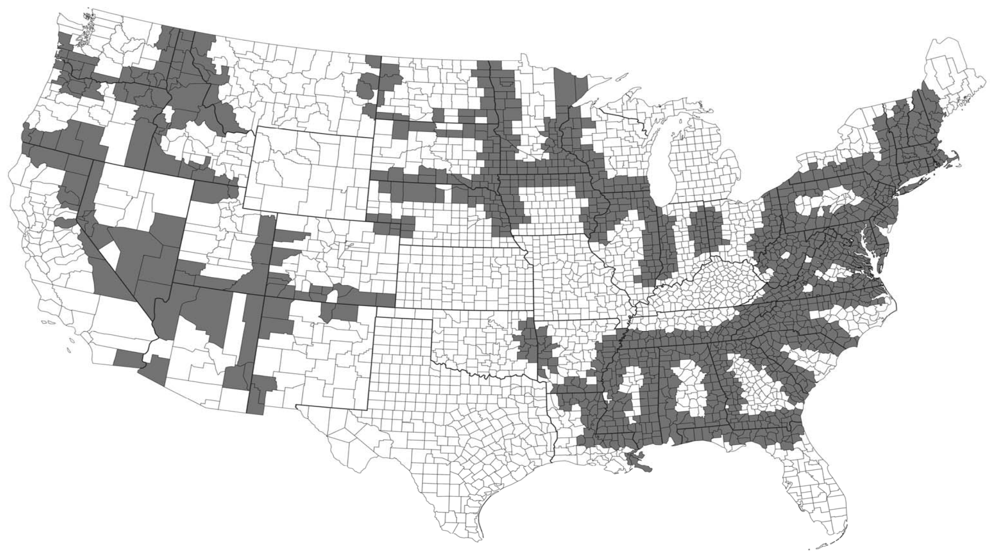
(f) Current Expenditure - Top Income Tercile



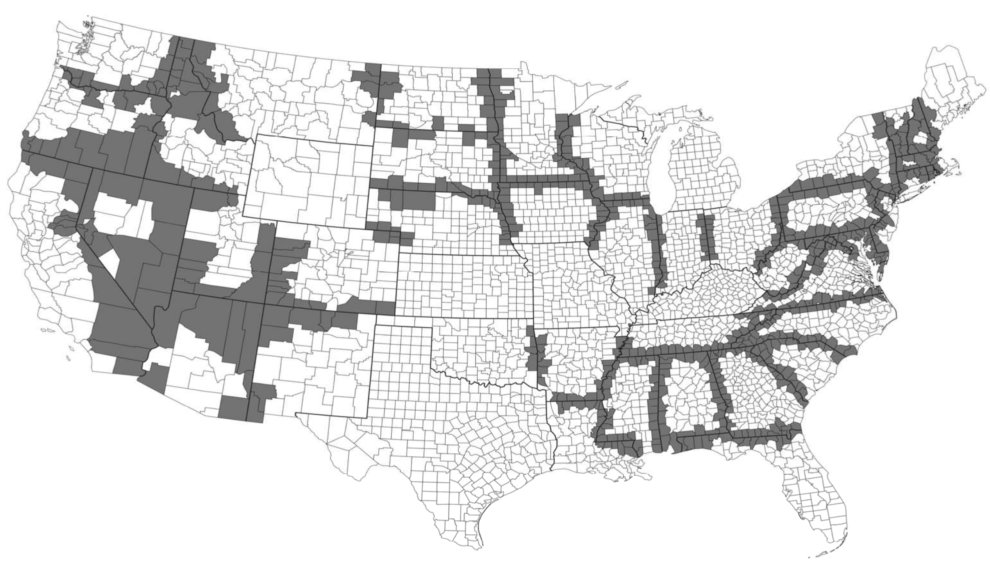
Notes: Figures (a), (c), and (e) show reduced form effects of school finance reforms on district local revenue in states at the 25th and 75th percentiles of union power, denoted weak and strong, respectively. 95% confidence intervals are shown for the strong union point estimates. Figures (b), (d), and (f) show effects on district current expenditures.

Appendix Figure V: United States County Map with Highlighted State Border Samples

(a) Counties <50 Miles from Border



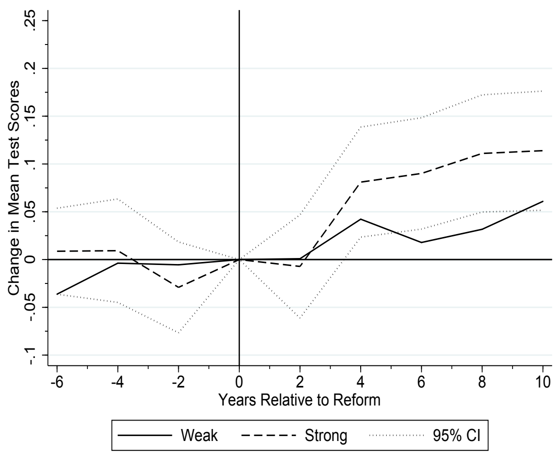
(b) Counties Adjacent to Border



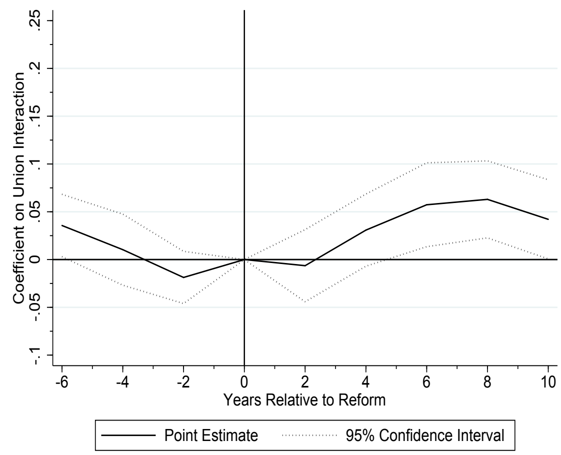
Notes: Map shows counties in our analysis sample whose centroid is within 50 miles of a state border (a), or that is adjacent to a state border (b). Note that our analysis sample excludes Kansas, Kentucky, Michigan, Missouri, Texas, and Wyoming.

Appendix Figure VI: Effects of School Finance Reforms on Achievement, by Union Power

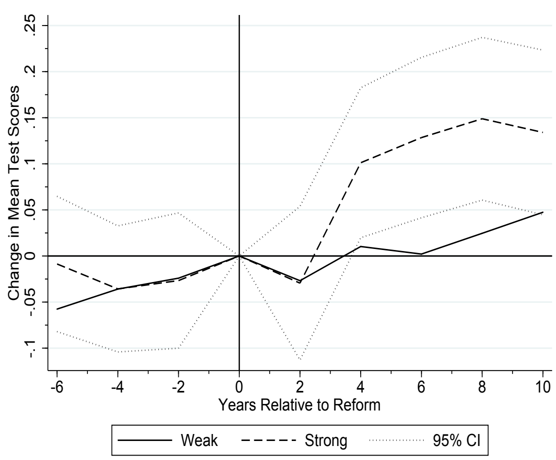
(a) Weak vs Strong Union States - All Districts



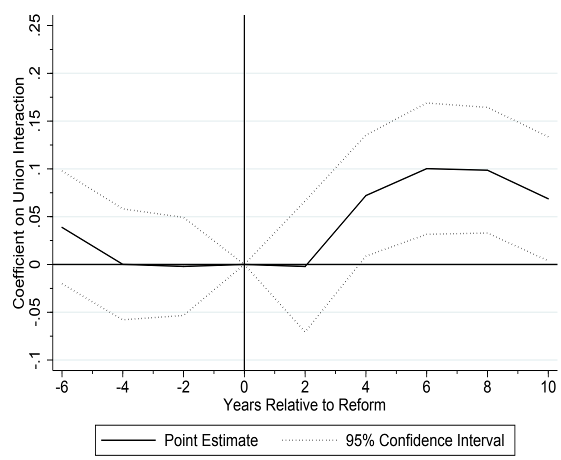
(b) Union Interaction Coefficient - All Districts



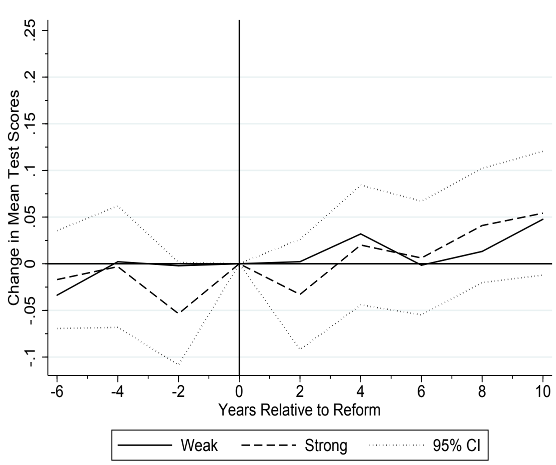
(c) Weak vs Strong Union - Bottom Income Tercile



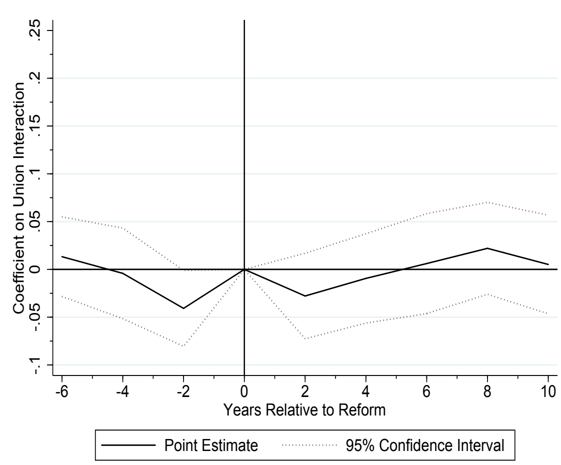
(d) Interaction Coeff. - Bottom Income Tercile



(e) Weak vs Strong Union - Top Income Tercile



(f) Interaction Coeff. - Top Income Tercile



Notes: Figures (a), (c), and (e) show reduced form effects of school finance reforms on district achievement in states at the 25th and 75th percentiles of union power, denoted weak and strong, respectively. 95% confidence intervals are shown for the strong union point estimates. Figures (b), (d), and (f) plot the coefficient and 95% confidence interval on the union power interaction from the reduced form regression.