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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 teacher unions affected the fraction of reform-induced state aid that passed through to local spending and the allocation of these funds. Districts with strong teacher 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, 2001; 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, 2017). 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 teacher union power. 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 teacher union power. Our primary measure of teacher union power is based on an index created by researchers at the Fordham Institute that incorporates administrative and survey data across several areas related to teacher union strength. We also use more traditional measures of state teacher union power that rely solely on state public sector collective bargaining laws and right-to-work status. 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 teacher union power also translate into differential effects on student achievement.

We find that previous estimates in the school finance reform literature mask important heterogeneity. Regardless of the teacher union power measure that we use, 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 teacher 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 teachers' unions reduced local tax effort by approximately 80 cents on the dollar. Districts in strong teacher union states allocated more of the additional spending toward increasing teacher salaries, while districts in weak teacher union states spent the money primarily on teacher hiring. Spending in non-instructional areas such as classroom support and school and district administration also increased more in strong teacher union states than in states with weak teachers' unions. Finally, we find that the larger expenditure increases in strong teacher union states translated into larger impacts on student achievement: ten years after a reform, students in low-income districts in weak teacher union states scored 0.08 standard deviations higher, but scored 0.16 standard deviations higher in strong teacher 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 teacher union power. We show that our results are robust to two alternative identification strategies that address this potential threat: 1) 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; and 2) a border discontinuity analysis where we restrict our sample to districts along state borders where there are differences in teacher union power but not in observed population characteristics. 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 new and important insights to the literature examining the effects of school finance reforms.[1] 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 standard deviations 10 years after a reform (Lafortune et al., 2018). We find a similar mean flypaper effect and achievement gains, but show that these mask dramatic heterogeneity driven by the strength of local teachers' unions. This finding supports local politics as an explanation for the flypaper phenomenon, and specifically, that local unions or other special interest groups ensure intergovernmental grants "stick where they hit."

Finally, our results build on the labor economics literature examining the effects of teachers' unions (Hoxby, 1996; Lovenheim, 2009; Frandsen, 2016; Lovenheim & Willen, 2017). Consistent with Hoxby (1996) and Lovenheim and Willen (2017), 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 and the Flypaper Effect

The neoclassical view of intergovernmental grants suggests that when communities receive a lump sum grant from a higher-level government, they would treat that grant the same as an equivalent increase in private income. As a result, intergovernmental grants should increase government spending by the same amount as an equivalent increase in private income.[2] A large and influential literature, however,

[1] In addition to the national studies on school finance reform discussed previously, a number of authors have examined the effects on school finance reforms in individual states. Examples include Downes (1992) and Sonstelie, Brunner, and Ardon (2000) in the case of California, Clark (2003) in Kentucky, Guryan (2001) in Massachusetts, and Papke (2005), Epple and Ferreyra (2008), Chaudhary (2009), Roy (2011), Chakrabarti and Roy (2015), and Hyman (2017) in the case of Michigan.

[2] According to Hines and Thaler (1995), most estimates of the marginal propensity of local governments to spend out of income are between \$0.05 to \$0.10.

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.[3]

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 presented 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).[4] In these models, interest group lobbying leads to an allocation of resources that favors spending on the good preferred by the interest group. For example, Seig and Wang (2013), develop a model in which public sector unions endorse candidates that are sympathetic to the objectives of the union. Their model predicts that if unions can mobilize enough resources to ensure that the candidate they endorse gets elected, then local public spending will increase. Consistent with their model, Seig and Wang (2013) find that challengers of incumbents in municipal elections strongly benefit from union endorsements and union-endorsed challengers that win elections tend to increase spending and adopt more pro-union policies.

Figure I 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.[5] 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, *X*, and spending on schools, *S*.[6] 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*.

[3] See Hines and Thaler (1995) and Inman (2008) for a review of this literature.
[4] A related set of studies focus on asymmetric information between voters and elected officials (Filimon, Romer, & Rosenthal, 1982; Strumpf, 1998). These models assume local bureaucrats have better information about the size of intergovernmental grants and windfall revenues than voters. Rent-seeking bureaucrats then exploit this asymmetric information to expand the size of the budget beyond the level desired by the decisive voter.
[5] Cascio et al. (2013) provide a graphical illustration similar to Figure I to illustrate the effect on an increase in federal Title I spending.
[6] For simplicity we normalize the prices of both *X* and *S* to one.

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.[7] 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 whether teachers' unions are primarily rent-seeking or are benevolent actors wishing to maximize school quality, they should still attempt to redirect intergovernmental grant revenue away from property tax relief and towards school spending. For example, if teachers' unions are primarily interested in maximizing school quality, and additional resources lead to higher student achievement, unions will use their political power to advocate for higher school spending. Similarly, 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.[8] Thus, regardless of teachers' unions' objective function, it is in their best interest to ensure that state aid "sticks where it hits."

III. 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 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. For this same period, 1986 – 2011[9], 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.

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[7] 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.
[8] Specifically, as shown in Appendix Figure I, 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.
[9] Here and subsequently, we refer to a school year by its fall year, i.e., 2011 refers to the 2011-12 school year.

We restrict our sample in several ways. First, note that one of our primary objectives is 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 school finance reforms 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 school finance reforms, we omit Kansas, Kentucky, Missouri and Texas since these states implemented "reward for local effort" (matching grant) formulas as part of their school finance reforms. We also omit Michigan and Wyoming because these states adopted school finance reforms that effectively 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 students) from the analysis.[10] 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 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.[11] We obtained a comprehensive list of school finance reforms 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 Table 7 that our results are robust to using a stacked difference-in-differences strategy that uses all school finance reforms 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).[12]

Finally, our primary teacher union power measure is based on an index created as part of a study by researchers at the Fordham Institute (Winkler, Scull, & Zeehandelaar, 2012). The index combines administrative and original survey data across five areas related to teacher union power: 1) resources and membership; 2) involvement in politics; 3) scope of bargaining; 4) state policies; and 5)

[10] See the Online Technical Appendix for a more detailed discussion of our data and sample restrictions.
[11] 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.
[12] See Appendix Table 1 for a listing of the school finance reforms used in our main analysis.

perceived influence. Many of the index components are measured as of 2012, which is after the school finance reforms 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 would have been directly influenced by school finance reforms 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.[13]

Figure IIa shows a state map of the U.S. by this continuous measure of state teacher union power, with states ranging from weakest teacher union power (white) to strongest teacher union power (dark grey). The strongest teacher union states tend to be in the Northeast, Great Lakes area of the Midwest, and the Pacific census division, while the weakest teacher 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 teacher union power. Stronger teacher 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 teacher union power measure, we supplement our analysis with more basic measures of state teacher union power that utilize state laws implemented prior to our sample period. Specifically, our first alternative measure is an indicator variable for whether or not a state mandates collective bargaining (CB), as defined in the NBER Public Sector Collective Bargaining Law Data Set, developed by 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.[14] In this more flexible alternative teacher 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

[13] In practice, this makes very little difference as these spending measures compose only 6.7% of the weight of the index. We thank Amber Northern (formerly Winkler), Janie Skull, and Dara Shaw (formerly Zeehandelaar) for generously sharing the index and all of its underlying components. See Appendix Figure II, taken from Winkler, Scull, and Zeehandelaar (2012), for a concise overview of the index components and their relative weightings.
[14] 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 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.

(=0+0). The strongest union power states are CB mandatory and not RTW, and have a value of three (=2+1).

Figure IIb shows a state map of the U.S. by our first alternative teacher 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 IIc 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 IIa.[15] We prefer the continuous index over the more basic measures, because it provides a much finer measure of teacher union power with a unique value for each state, and thus more variation across states that we can exploit. However, we show that the pattern of results that we find is similar regardless of which teacher union power measure we employ.

IV. Empirical Framework

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

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

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 teacher 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.[16] In all specifications, we cluster the standard errors at both the school district and state-year level.[17]

[15] Appendix Table 2 provides values by state for all three teacher union power measures. The three measures are strongly positively correlated with a correlation index 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.

[16] Note that because we include district fixed effects in all specifications, the level effect of $Union_s$ is omitted from equation (1) since it would be perfectly correlated with the district fixed effects.

[17] 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

In our most parsimonious specification, X_{is}, the vector of school district characteristics, 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 Bachelors degree. We do not include time-varying versions of these characteristics because they could be affected by the school finance reforms and would thus be endogenous controls. Therefore, we include each characteristic interacted with a linear time trend to allow for differential trending of the outcome variable 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 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).[18]

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 therefore use the timing of adoption of school finance reforms as instrumental variables and estimate first-stage models of the following form:

$$\begin{aligned} Rev_{ist} = {} & \alpha_0^1 + \alpha_1^1(Q1_{is} * SFR_{st}) + \alpha_2^1(Q2_{is} * SFR_{st}) + \alpha_3^1(Q3_{is} * SFR_{st}) + \\ & \alpha_4^1(Q1_{is} * SFR_{st} * Union_s) + \alpha_5^1(Q2_{is} * SFR_{st} * Union_s) + \alpha_6^1(Q3_{is} * SFR_{st} * Union_s) + \\ & X_{is}\theta_t\pi_1^1 + X_{is}\theta_t Union_s\pi_2^1 + \delta_i + \lambda_{rt} + Q_{is}\theta_t + \varepsilon_{ist}^1 \end{aligned} \tag{2}$$

$$\begin{aligned} (Rev_{ist} * Union_{st}) = {} & \alpha_0^2 + \alpha_1^2(Q1_{is} * SFR_{st}) + \alpha_2^2(Q2_{is} * SFR_{st}) + \alpha_3^2(Q3_{is} * SFR_{st}) + \\ & \alpha_4^2(Q1_{is} * SFR_{st} * Union_s) + \alpha_5^2(Q2_{is} * SFR_{st} * Union_s) + \alpha_6^2(Q3_{is} * SFR_{st} * Union_s) + \\ & X_{is}\theta_t\pi_1^2 + X_{is}\theta_t Union_s\pi_2^2 + \delta_i + \lambda_{rt} + Q_{is}\theta_t + \varepsilon_{ist}^2 \end{aligned} \tag{3}$$

where SFR_{st} is an indicator for whether state *s* implemented a school finance reform 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 out the effects of SFRs by within-state 1980 income terciles because reforms were typically designed to (and did) have very different impacts on state aid for low- and high-income districts, with

consider this to be a conservative approach, but additionally report the results clustering at the state level in Panel C of Appendix Table 4.

[18] 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).

the goal of equalizing school funding between districts.[19] Given that other factors could be changing over time across these district terciles aside from the effects of the school finance reforms, we include $Q_{is}\theta_t$, the tercile dummies interacted with a linear time trend in equations 1-3, to allow for differential trending of the outcome variable across these terciles.

A. Dynamic Event Study Specifications

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

$$y_{ist} = \sum_{j=-6}^{10} \gamma_j T_{j,st} + \delta_i + \lambda_t + \eta_{ist}, \qquad (4)$$

where, $T_{j,st}$ represents a series of lead and lag indicator variables for when state *s* implemented a school finance reform, 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 school finance reform. We include indicator variables for 2 through 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 through 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 similarly, $T_{10,st}$ 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 (4) are the $\gamma_j{}'s$, which represent the difference-in-differences estimates of the impact of school finance reforms 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 prior to the time a state adopted a school finance reform. If school finance 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 school finance reforms on state aid to evolve slowly over time.

[19] 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, rather than our over-identified model that includes six instruments and two endogenous variables.

V. Results

We begin our analysis by showing that school finance reforms (SFRs) led to exogenous increases in state aid. Specifically, we estimate the event study model from equation (4) 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 from these regressions. Figure IIIa (all districts) shows that after a school finance reform, state aid increases quickly 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 school finance reforms. Figure IIIb 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 IIIc and IIId show the effects for the middle and top income tercile districts, where both sets of districts 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.

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. We present the first-stage results in Table 2, using our continuous measure of state teacher union power, which for ease of interpretation, we standardize to be mean zero and standard deviation one.[20] Columns 1 and 2 of Table 2 present results based on the estimation of equation (2) where the dependent variable is state aid per-pupil. The results reported in column 1 include as controls baseline enrollment and 1980 district median income both interacted with the linear time trend, each of those interactions further interacted with the union power index, and finally, an indicator for binding tax or expenditure limits. In column 2 we add in our expanded controls, which include 1980 district fraction black, fraction urban, and fraction BA or higher interacted with the linear time trend and each of these interactions further interacted with the union power index. Columns 3 and 4 present results based on specifications identical to those reported in columns 1 and 2 except the dependent variable is now state aid per-pupil interacted with the union power measure.

As shown in column 1, districts that were in the lowest tercile of 1980 median income and in a state with the mean level of teacher union power (where the standardized index equals zero) experienced an increase of \$1,131 per-pupil; the increase was \$602 for the middle tercile and \$590 for the top tercile. The increase for districts with a one standard deviation higher level of state teacher union power was \$205 larger for the bottom income tercile, \$148 smaller for the middle tercile, and \$170

[20] First stage results for our two alternative measures of teacher union power, presented in Appendix Table 3, show a similar pattern and yield similar *F*-statistics.

smaller for the top tercile. In column 2, we show that the pattern of results is similar when we add the expanded controls. The first-stage *F*-Statistics for these two specifications are 24 and 23, respectively.[21] In sum, school finance reforms provide plausibly exogenous, large, and statistically significant increases in state aid.

A. Effects of State Aid on Revenues and Expenditures

We present estimates from the second stage of our IV analysis in Table 3. Columns 1 and 2 in Panel A show the effects of a SFR-induced one dollar increase 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 teacher 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. In the bottom two rows of Panel A, we present the estimated effects at the 25th and 75th percentiles of the distribution of teacher union power.[22] Weak teacher union states (near the 25th percentile of union power) increase total revenue by 52 cents for a one dollar increase in state aid, whereas strong teacher union states (near the 75th percentile) increase revenue by 90 cents on the dollar.

As property tax relief is the likely source of crowd-out, we next examine the effects of increased state aid on local revenue (Table 3, columns 3 and 4). Using our preferred specification, which includes the additional controls, districts in a state with mean teacher 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 teacher union power one standard deviation higher and a 0.56 cent reduction (i.e., 29 + 27 cents) in local revenue among states with teacher union power one standard deviation lower. States at the 25th percentile of teacher union power reduce local revenue by 43 cents for each dollar of state aid and states at the 75th percentile reduce local revenue by only 9 cents on the dollar. These results explain most of the heterogeneity in total revenue increases by union power – districts in

[21] There is relatively little effect of SFRs on the interaction of state aid and union power at the mean level of union power, but large effects, with the same pattern by tercile, for a one standard deviation increase in union power (column 4, *F*-statistic of 36).

[22] The teacher 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.

weak teacher union states substantially reduce their local tax effort in response to the windfall of state aid, whereas districts in states with stronger teacher unions do so to a far lesser degree.

Finally, we examine the extent to which these revenue effects translate into effects on education expenditures (Table 3, columns 5 and 6). Using our preferred specification (column 6), we find that a SFR-induced dollar increase in state aid translates into a 66 cent increase in total education expenditures at the mean level of state teacher union power. This is similar to the mean flypaper effect for SFR-induced increases in state aid for education expenditures estimated in the earlier school finance reform literature (e.g., Card & Payne 2002). However, we find that the increase is 20 cents larger (or smaller) given a one SD higher (or lower) level of teacher union power, suggesting substantial heterogeneity in the flypaper effect by the strength of a state's teachers' unions. Weak teacher union states (at the 25th percentile) increase total expenditures by 55 cents, while strong union states (75th percentile) increase expenditures by 80 cents.

In Figures IVa-IVc we plot the estimated coefficients reported in Table 3 at each vigintile (i.e., 20 percentiles) of the union power index. Figure IVa presents the results from this exercise where total revenue is the outcome. For states with very low teacher 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 teacher union power (near the 90th percentile), total revenue increases nearly dollar-for-dollar with increases in state aid. As shown in Figure IVb, 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. Finally, as shown in Figure IVc, the heterogeneity in total revenue across the union power distribution also translated into similar heterogeneity in educational expenditures. Taken together, the results reported in Table 3 and Figures IVa-IVc reveal that differences in state teacher 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.[23]

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. This result is

[23] 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 points to the importance of identifying exogenous changes in state aid to identify the effects of state aid on resource allocations.

consistent with our basic conceptual framework showing that teachers' unions could cause school boards to spend more of a windfall from intergovernmental grants on education. However, our conceptual framework also suggests that unions may 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 1). 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. As shown in column 8 of Panel A, 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.[24] This represents a 5.2% decrease in class size, relative to the sample mean of 16.3 students. If there was no impact of unions on the allocation of resources across inputs to education production, then we would expect there to be greater class size reductions in states with stronger teachers' unions, given the larger increases in expenditures in those states. We do not find this to be the case. Specifically, we find no statistically significant difference in the effect on class size by teacher union power, and if anything, there is slight evidence of the opposite, with *less* of a class size reduction in the stronger union states by 0.144 pupils (standard error of 0.118).[25] For states at the 25th percentile of teacher union power, there is a 0.91 pupil decrease, and for states at the 75th percentile, the decrease is 0.73 pupils. Taken together, these findings suggests that teachers' 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, with stronger unions causing less of the marginal dollar to be allocated toward 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 years of experience and whether or not a teacher has a Master's degree. 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. Because information on district teacher salary

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[24] To aid in interpretation given that the effects of a $1 increase in state aid on PTR are negligible, we multiply PTR by 1,000 so that we can interpret these results as the effects of a $1,000 increase in state aid.

[25] We can strongly reject (p-value <0.000) that the 0.144 coefficient on the union interaction equals -0.255, which is what we would expect it to equal if there was the identical pattern of heterogeneity for impacts on class size as we saw for impacts on expenditures. The derivation of the -0.255 estimate is as follows: The coefficient on the state aid*union interaction for expenditures was 0.200, or 30% as large as the 0.656 coefficient on the main state aid term. Multiplying 30% by the -0.838 coefficient on the main state aid term in the class size regression yields -0.255. We can alternatively use the lower 95% confidence interval for the 0.200 coefficient on the state aid*union interaction for expenditures, which equals 0.047. This is 7.2% (instead of 30%) as large as the 0.656 coefficient. Multiplying 0.838 by 7.2% yields -0.060 (instead of -0.255), and we can still marginally reject (p-value 0.084) that the 0.144 class size interaction coefficient equals -0.060.

schedules are not available in our primary CCD data, we use salary schedule information from the U.S. Department of Education 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.[26] We focus on district 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. 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 teacher union power, and a statistically significant 51 cent larger increase for districts in states with one SD higher teacher union power. For states at the 25th percentile of teacher union power, there is a statistically insignificant and near-zero (6 cent) increase in salaries. There is a statistically significant 70 cent increase for states at the 75th percentile. Consistent with our basic conceptual framework, stronger teacher unions appear to focus the increases in education expenditures more on increasing teacher salaries than on hiring new teachers.

C. Alternative Measures of State Teacher Union Power

While we prefer the continuous measure of state teacher union power, we examine whether the pattern of results is similar using our more basic measures of state teacher union power that avoid any possible concerns about endogeneity or subjectivity of the continuous measure. As noted previously, our first alternative measure is simply an indicator variable for whether or not the state mandates collective bargaining (CB). Thus, in Panel B of Table 3, the main state aid term reflects the effect of a dollar increase in state aid for states that are CB non-mandatory. For states that are CB mandatory, the effect is calculated by adding the coefficients on the main term and the interaction term.

The pattern of results is broadly similar to those with the continuous measure. 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 (Table 3, panel B, column 2). Local revenue decreases by 75 cents in CB non-mandatory states, and by 24 cents (= -75+51) in CB mandatory states. Total expenditures increase by 17 (statistically insignificant) and 70 cents (= 17+53), respectively. Class sizes shrink by 1.0 pupils in CB non-mandatory states, and by 0.8 (= -1.0+0.2) pupils in CB mandatory

[26] The SASS was conducted during 1987, 1990, 1993, 1999, 2003, 2007, and 2011, which aligns nicely with our sample period.

states. We find a small, statistically insignificant decrease in teacher base salaries in CB non-mandatory states, but a larger (though still statistically insignificant) coefficient on the interaction term of 0.9, suggesting a 54 cent increase in teacher salaries in CB mandatory states.

Our second alternative measure incorporates CB status and RTW status and takes on four values from zero (weakest union states) to three (strongest union states). 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 term and interaction term. The effect for the strongest union power states are calculated by adding the main coefficient to three times the coefficient on the interaction term.

We find the same pattern of results using this more comprehensive alternative measure. For every dollar increase in state aid due to school finance reforms, 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 union power (Table 3, Panel C, column 2). The weakest union states experience a reduction in local revenue of 65 cents, and (insignificant) increase in total expenditures of 33 cents. In the strongest union states, local revenue decreases by 23 cents (= -0.650 + [3 x 0.139]), and total expenditures increase by 70 cents (= 0.329 + [3 x 0.124]). Class sizes decrease by 1.5 pupils and 0.8 pupils in the weakest and strongest union states, respectively. Finally, a dollar of SFR-induced state aid has a small, negative, and statistically insignificant impact on base teacher salaries among districts in states with the weakest unions, but an increase of 61 cents (= -0.343 + [3 x 0.318]) among districts located in states with the strongest unions. While the results for base salary are statistically imprecise, the overall pattern of results using these two alternative teacher union power 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 Teacher Union Endogeneity

One concern with the results presented thus far is that our measures of teacher union power may be correlated with state-specific unobservables that also influence education spending and the allocation of education resources. For example, state teacher 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 teacher unions might chose to spend more on education and allocate educational resources differently than states without strong teacher unions regardless of the teacher unions themselves. This concern is at least 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, even after for controlling for the fixed effects and observed characteristics, there may be unobserved differences that could be causing the heterogeneity we detect. In this section we present results from two strategies that attempt to address this potential endogeneity of state teacher union power. We move forward using the continuous teacher union power index and our preferred specification that includes the expanded set of controls.

Our first strategy designed to address the potential endogeneity of teacher union power involves controlling directly for heterogeneity of the effects of state aid by observable state characteristics that are highly correlated with state teacher union power and may also influence how districts choose to allocate reform-induced increases in state aid. Specifically, we add terms $Rev_{ist} * Char_s$ to our estimating equations. For example, the second stage of our two-stage-least-squares estimation strategy changes from equation (1) to:

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

where $Char_s$ includes one of three baseline state characteristics that are highly correlated with state teacher 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 (3) except the dependent variable is now the $Rev_{ist} * Char_s$ interaction term.[27] If β_2 withstands the addition of these state teacher union power correlates interacted with state aid, this would provide reassurance that β_2 is identifying the effects of teacher union power and not unobserved state characteristics associated with union power.[28]

Panel A of Table 4 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 for total or local revenue, expenditures, class size, or base salary. 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 either of these additions. Finally, in Panel D we

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

[28] Our use of additional interaction terms to control for other factors that may be correlated with the interaction term of primary interest (in our case $Rev_{ist} * Union_s$) is similar in spirit to the methodology used by Cutler and Glaeser (1997) in the context of the effects of racial segregation on the schooling and employment outcomes of blacks and Brueckner and Neumark (2014) in the context of the effect of amenities on public sector worker rent extraction.

interact state aid separately with all three of these state-level characteristics, and we once again find that our results are largely robust to the inclusion of these additional interaction terms, the only exception being that we lose statistical significance for the interaction of state aid and union power for base teacher salary. This loss of precision is not surprising given the strong correlations between these three covariates and union power. Nevertheless, the fact that the pattern of results is robust to the inclusion of state aid interacted with these additional covariates that are highly correlated with state teacher union power is reassuring.

Our second strategy to address the potential endogeneity of teacher union power is a border discontinuity design that focuses on school districts in counties along state borders. The assumption (which we will support empirically) is that while school districts along these borders differ in terms of their states' teacher union power, they are otherwise quite similar along both observable and unobservable dimensions due to their geographic proximity. If our results are robust to this change in sample, the similarity of these adjoining school districts that differ in their state teacher union power would provide confidence that any differences in the effects of state aid in these two types of districts is driven by the difference in union power and not other 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. Figure Va shows a map of U.S. counties shading these counties grey. 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 (see Figure Vb).

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 1-3 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 characteristis, we conduct a series of balancing tests by estimating cross-sectional models of the form:

$$C_{is,1990} = \rho_0 + \rho_1 Union_s + \gamma_b + \upsilon_{is}, \tag{6}$$

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 school finance reforms that occurred during the 1990's we base our balancing test on pre-determined characteristics of districts as of 1990. The coefficient of primary interest in equation (6) is ρ_1 which represents the average difference in $C_{is,1990}$ by state teacher union power 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 (6) that are based on the main sample of school districts (and estimated without the border fixed effect).

We begin by presenting the results from estimating equation (6) on the main sample of districts (Table 5, columns 1 and 2). We find that districts in states with stronger teacher 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.[29] Clearly there are important differences between districts in states with strong teacher unions and districts in states with weak ones.

We now restrict our sample to districts in counties whose centroid is within 50 miles of a state border and re-estimate equation (6), including border fixed effects and thus comparing districts along the same state border (Table 4, 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 7 and 8). 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 teacher union power.

We present results from the border analysis in Table 6. Panel A restricts the sample to counties within 50 miles of a state border, while Panel B restricts the sample to border counties. The pattern of results are nearly identical to those found in our main analysis: districts in states with stronger teacher unions reduce their local tax effort to a smaller extent than states with weak teacher unions, and this translates into more of the state aid going toward education expenditures. Districts in states with stronger teacher 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.

[29] These point estimates are all statistically significant, most of which at the 1% level.

The robustness of the results to these two conceptually different strategies to address possible union power endogeneity is reassuring that the heterogeneity that we document is due to the effects of teachers' unions and not due to other unobserved characteristics across states.

E. School Finance Reform Coding and Sample Restriction Robustness

In this section, we present four checks to examine the robustness of our results to decisions about the way we code school finance reforms and restrict the sample. In Panel A, we present results from 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 LRS (2018), given that these differences reflect choices over which reform is the "primary" reform in states that experience multiple reforms. In Panel B, we test whether our results are sensitive to excluding the handful of reforms that are not court-ordered, as Jackson et al. (2016) argue that court-ordered reforms are more likely to be exogenous. Once again, the results presented in panels A and B are robust: while the magnitude of the estimates vary somewhat across the different specifications, the same patterns emerge and all of the previously statistically significant coefficients remain significant.

In panel C, we test whether our results are sensitive to excluding the Great Recession (2009-2011). The results are robust to including these years in our sample, with the only exception being that we lose statistical precision for total expenditures. Finally, our main estimation sample omits Kansas, Kentucky, Missouri and Texas because those states adopted matching aid formulas as part of their school finance reforms. To examine how our results change when we include states that adopted matching aid formulas, in panel D we include these states in the estimation sample. Not surprisingly, while the main pattern of results is the same, including states that adopted matching aid formulas changes the magnitudes of the estimated coefficients in the revenue and expenditures specifications. 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 separate out the previously estimated effects on total expenditure into 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 3, 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 besides teachers can be important to education production. Thus, we examine how much of each dollar of school finance reform-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.

We find a similar, though slightly less dramatic pattern of results for instructional expenditures as we did for total expenditures, with a 32 cent increase in weak teacher union states (25th percentile) and 44 cent increase in strong union states (75th percentile). Note that the similarly sized or marginally smaller class size reduction in the strong teacher union states, along with this larger increase in instructional expenditures, suggests that districts in strong union states focused more on increasing teacher compensation than districts in weak union states.

We find dramatic heterogeneity by teacher union strength in the effects of school finance reform-induced increases in state aid on non-instructional expenditures (column 4). There is a similar pattern for capital outlays (column 5), though the interaction of state aid and union power is statistically insignificant. Mean per-pupil spending in these two categories ($3,463 and $1,019, respectively) is lower than spending in instructional expenditures ($5,749) and current expenditures ($9,347). Yet, 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 teacher 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

In order to examine whether the differences in education spending by teacher union power presented thus far translate 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.[30] Unlike effects on expenditures, and as shown in LRS (2018), effects of 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:

$$\begin{aligned} NAEP_{ijgst} &= \phi_0 + \phi_1 YearsPost_{st} + \phi_2 YearsPost_{st} * Union_s \\ &\quad + X_{is}\theta_t\kappa_1 + X_{is}\theta_t Union_s\kappa_2 + \pi_{jg} + \delta_i + \lambda_{rt} + Q_{is}\theta_t + \zeta_{ijgst}, \end{aligned} \tag{7}$$

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 both 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 9 presents the reduced form effects of school finance reforms on achievement. Without including the union interaction, we find an overall impact of school finance reforms 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.[31] 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 teacher union power, and a statistically

[30] For more details about the NAEP microdata, please see the Online Technical 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.

[31] 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.

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 0.08 SDs ten years post-reform, for weak teacher 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 teacher union power.[32]

In Appendix Figure III, we show event-study pictures grouping years into pairs to increase statistical precision. As in LRS (2018), the figures 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 9, the figures show a gap in test scores between weak and strong union states that emerges after the school finance reforms, with the effects concentrated among the lowest income districts. These findings suggest that the larger expenditure increases in strong teacher union states in response to school finance reforms translated into larger student achievement gains.

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.

We find that the previous estimates in the school finance reform literature mask important heterogeneity. Specifically, 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 teacher unions increased education expenditures nearly one-for-one with increases in state aid in response to school finance reforms, whereas states with the weakest teacher unions substantially reduced local tax effort, with education expenditures increasing less than 25 cents on the dollar. Furthermore, the school spending in strong teacher union states was

[32] Appendix Table 5 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.

allocated more toward increasing teacher salaries, while districts in weaker teacher union states spent the money primarily on hiring new teachers. We find that achievement gains due to the reforms were significantly larger in strong teacher union states than they were in weak teacher union states. Our results are robust to strategies that address the potential endogeneity of teacher union strength, suggesting that we are identifying the effects of the teachers' unions, and not unobserved cross-state differences correlated with state teacher union power.

Our results have several implications. First, our results support the hypothesis that an important explanation for the flypaper effect is local politics, and specifically, the strength of local unions or other special interest groups 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 school finance reform movement that began in the 1970's.

Finally, our results provide an important perspective on the impacts of teachers' unions. In response to the large increases in state aid induced by school finance reforms, 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 school finance reform movement, ensuring that the reforms were effective in reducing inequality across school districts in education resources and student achievement.

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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
Total Expenditures	10,987	4,116	11,836	4,422	9,226	2,632
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: First-Stage Estimates

	Dependent Variable			
	State Aid		State Aid * Union	
Instruments:	(1)	(2)	(3)	(4)
SFR * Q1	1131***	1089***	183**	67
	(108)	(109)	(85)	(79)
SFR * Q2	602***	592***	-48	-117
	(103)	(103)	(76)	(75)
SFR * Q3	590***	578***	-77	-126
	(121)	(118)	(90)	(85)
SFR * Union * Q1	205**	164*	1376***	1321***
	(99)	(96)	(102)	(93)
SFR * Union * Q2	-148	-166*	582***	593***
	(93)	(93)	(89)	(85)
SFR * Union * Q3	-170*	-179*	212*	319***
	(99)	(97)	(110)	(101)
F-Statistic	24	23	34	36
Observations	181,756	181,756	181,756	181,756
Expanded Controls	No	Yes	No	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 and 2 and state aid per-pupil interacted with state union power in columns 3 and 4. 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 and 4 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: Effects of State Aid by Teacher Union Power

	Total Revenue		Local Revenue		Total 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***	0.640***	0.656***	-0.832***	-0.838***	0.322
	(0.077)	(0.078)	(0.074)	(0.076)	(0.086)	(0.088)	(0.141)	(0.144)	(0.324)
State Aid * Union	0.324***	0.302***	0.277***	0.270***	0.230***	0.200**	0.172	0.144	0.505**
	(0.068)	(0.067)	(0.063)	(0.064)	(0.077)	(0.078)	(0.113)	(0.118)	(0.248)
Estimated Effect at:									
25th Pctle. of Union Index	0.476***	0.518***	-0.468***	-0.431***	0.520***	0.552***	-0.921***	-0.912***	0.060
	(0.095)	(0.089)	(0.089)	(0.086)	(0.097)	(0.092)	(0.164)	(0.160)	(0.428)
75th Pctle. of Union Index	0.884***	0.899***	-0.119	-0.091	0.811***	0.804***	-0.705***	-0.731***	0.696***
	(0.076)	(0.086)	(0.075)	(0.086)	(0.100)	(0.110)	(0.147)	(0.163)	(0.225)
Panel B. Mandatory CB Status (0, 1)									
State Aid	0.195	0.091	-0.677***	-0.746***	0.230	0.166	-1.074***	-1.046***	-0.351
	(0.175)	(0.200)	(0.154)	(0.173)	(0.182)	(0.207)	(0.356)	(0.393)	(0.732)
State Aid * Union	0.552***	0.655***	0.436***	0.508***	0.481***	0.531***	0.260	0.200	0.893
	(0.158)	(0.191)	(0.137)	(0.165)	(0.167)	(0.201)	(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***	0.288	0.329	-1.455***	-1.482***	-0.343
	(0.180)	(0.189)	(0.157)	(0.165)	(0.188)	(0.202)	(0.413)	(0.466)	(0.781)
State Aid * Union	0.195***	0.191***	0.147***	0.139**	0.144**	0.124*	0.235*	0.241	0.318
	(0.059)	(0.067)	(0.052)	(0.059)	(0.064)	(0.072)	(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 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 4: Effects Controlling for Heterogeneity by State-Level Union Power Correlates

	Total Revenue	Local Revenue	Total Expenditures	Pupil-Teacher Ratio	Base Salary
	(1)	(2)	(3)	(4)	(5)
Panel A. 1988 Democrat Vote Share					
State Aid	0.636***	-0.332***	0.627***	-0.928***	0.394
	(0.088)	(0.085)	(0.085)	(0.162)	(0.350)
State Aid * Union	0.258***	0.227***	0.145*	0.135	0.473**
	(0.064)	(0.062)	(0.075)	(0.117)	(0.239)
Panel B. 1990 Median Income					
State Aid	0.700***	-0.270***	0.651***	-0.916***	0.143
	(0.082)	(0.079)	(0.087)	(0.153)	(0.407)
State Aid * Union	0.353***	0.317***	0.207***	0.036	0.388*
	(0.070)	(0.068)	(0.075)	(0.117)	(0.224)
Panel C. 1990 Fraction BA or Higher					
State Aid	0.671***	-0.300***	0.623***	-0.888***	0.224
	(0.073)	(0.070)	(0.089)	(0.148)	(0.418)
State Aid * Union	0.350***	0.313***	0.210***	0.066	0.472**
	(0.060)	(0.058)	(0.078)	(0.121)	(0.215)
Panel D. Include All Three					
State Aid	0.714***	-0.257***	0.663***	-0.998***	0.039
	(0.078)	(0.073)	(0.086)	(0.171)	(0.508)
State Aid * Union	0.254***	0.223***	0.111*	0.111	0.406
	(0.056)	(0.054)	(0.067)	(0.122)	(0.258)
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 3. 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, and Panel C replaces it with 1990 fraction of adults 25 years of age and older with a Bachelors degree or higher. Panel D includes all three variables separately interacted with state aid, and corresponding instruments for each. Robust standard errors, clustered at both the district and state-year level, in
* significant at 10%, ** significant at 5%, *** significant at 1%.

Table 5: 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 1-4. 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 6: State Border Sample Analysis

	Total Revenue	Local Revenue	Total Expenditure	Pupil-Teacher Ratio	Base Salary
	(1)	(2)	(3)	(4)	(5)
Panel A. Counties 50 Miles From State Border					
State Aid	0.730***	-0.256***	0.688***	-0.871***	0.433**
	(0.088)	(0.085)	(0.093)	(0.134)	(0.217)
State Aid * Union	0.229***	0.237***	0.210***	0.187**	0.198
	(0.061)	(0.061)	(0.069)	(0.093)	(0.189)
Panel B. Counties Adjacent to State Border					
State Aid	0.657***	-0.342***	0.585***	-0.806***	0.345
	(0.094)	(0.091)	(0.093)	(0.120)	(0.228)
State Aid * Union	0.238***	0.243***	0.201***	0.123	0.109
	(0.070)	(0.068)	(0.074)	(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 3. 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 7: School Finance Reform Coding and Sample Robustness

	Total Revenue	Local Revenue	Total Expenditures	Pupil-Teacher Ratio	Base Salary
	(1)	(2)	(3)	(4)	(5)
Panel A. Stacked Diff-in-Diff Design					
State Aid	0.693***	-0.284***	0.642***	-0.661***	0.124
	(0.074)	(0.073)	(0.086)	(0.114)	(0.267)
State Aid * Union	0.328***	0.300***	0.302***	0.020	0.333*
	(0.049)	(0.049)	(0.062)	(0.079)	(0.174)
Panel B. Court-Ordered Reforms Only					
State Aid	0.703***	-0.275***	0.656***	-0.886***	0.393
	(0.078)	(0.077)	(0.088)	(0.147)	(0.326)
State Aid * Union	0.284***	0.262***	0.200**	0.184	0.467*
	(0.065)	(0.064)	(0.079)	(0.120)	(0.249)
Panel C. Include Great Recession					
State Aid	0.801***	-0.172**	0.889***	-0.788***	0.437
	(0.068)	(0.067)	(0.086)	(0.117)	(0.281)
State Aid * Union	0.204***	0.211***	0.127	0.271**	0.413*
	(0.060)	(0.054)	(0.085)	(0.107)	(0.225)
Panel D. Include KS, KY, MO, TX					
State Aid	0.786***	-0.191**	0.735***	-0.896***	0.637**
	(0.076)	(0.074)	(0.097)	(0.150)	(0.298)
State Aid * Union	0.120**	0.108**	0.132**	0.124	0.137
	(0.053)	(0.051)	(0.066)	(0.103)	(0.221)
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
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. Robust standard errors, clustered at both the district and state-year level, in parentheses.
* significant at 10%, ** significant at 5%, *** significant at 1%.

Table 8: 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 3. 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%.

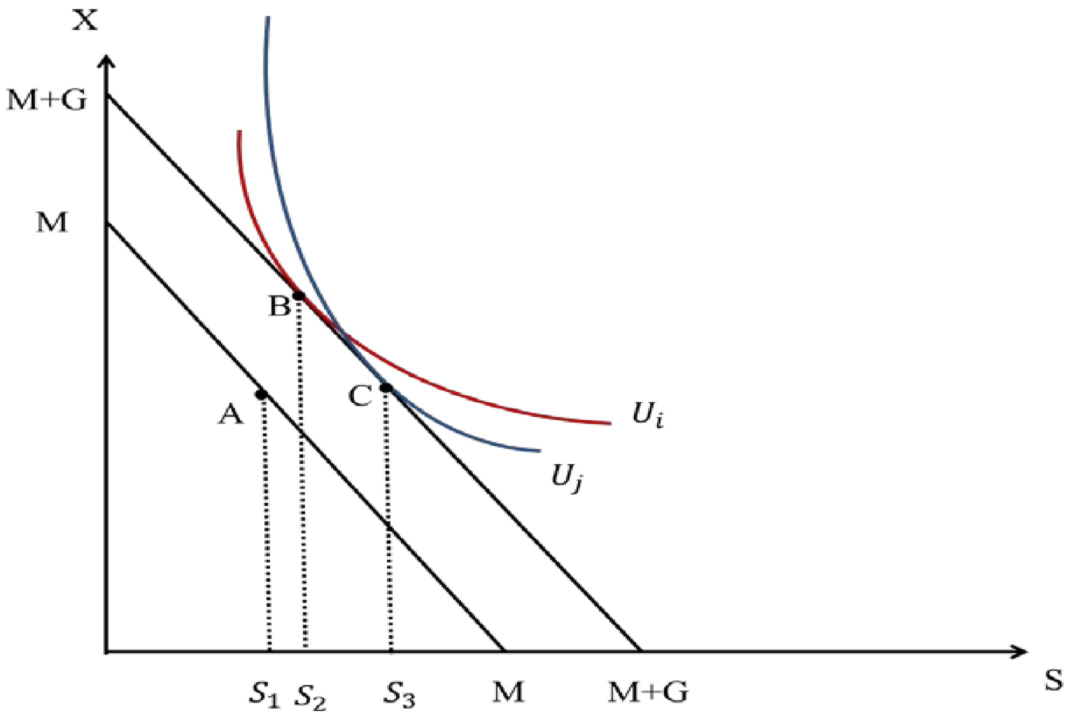
Table 9: Reduced Form Effects of School Finance Reforms on Student Achievement

	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%.

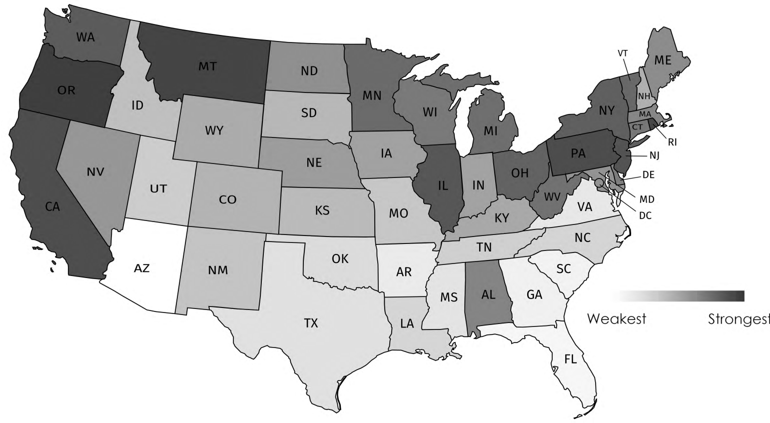
Figure I: School District Responses to Intergovernmental Aid



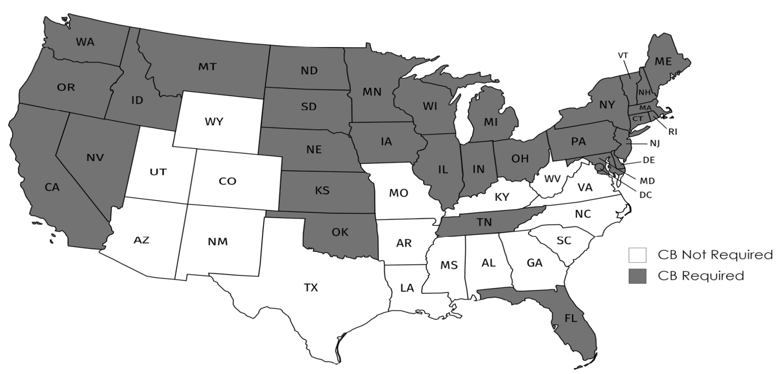
Notes: Figure shows the choice problem facing a school district before and after an increase in intergovernmental grant aid. Spending on schools is S and private consumption is X, where the price of both is normalized to one. The district has M income, and G is the amount of aid. Without any influence from the teachers' union, the district would move from point A to point B, but under the influence of the union, which has preferences U_j, the district would move to point C, leading to the classic flypaper effect where more of the aid is spent on school spending.

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

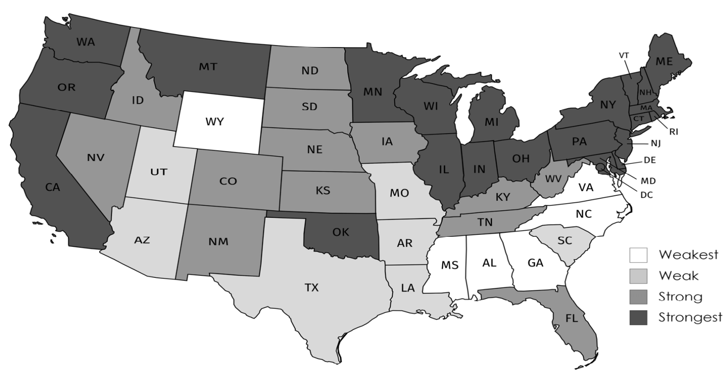
(a) Continuous Teacher Union Power Index



(b) Mandatory Collective Bargaining (CB) Status

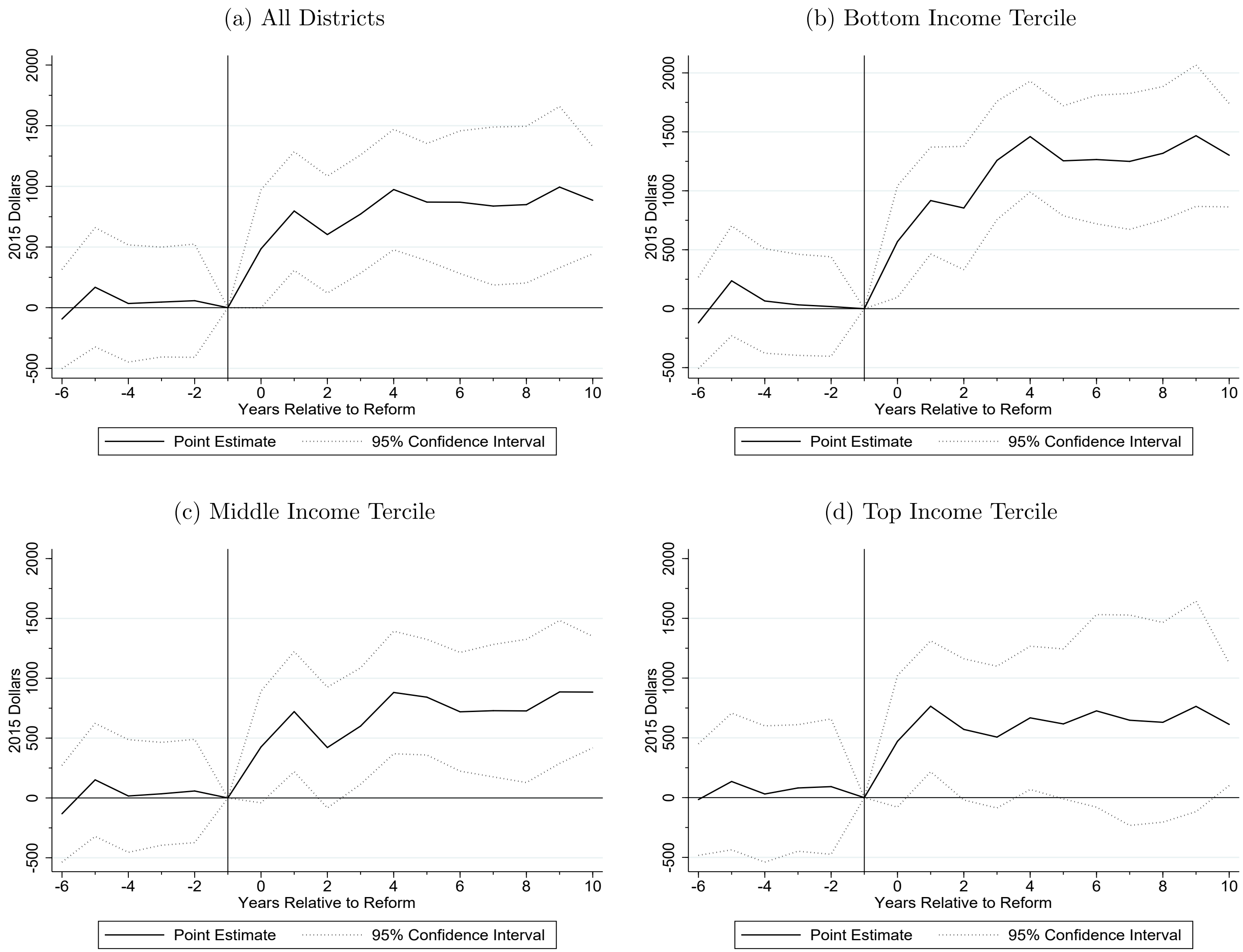


(c) CB and Right-to-Work Index



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.

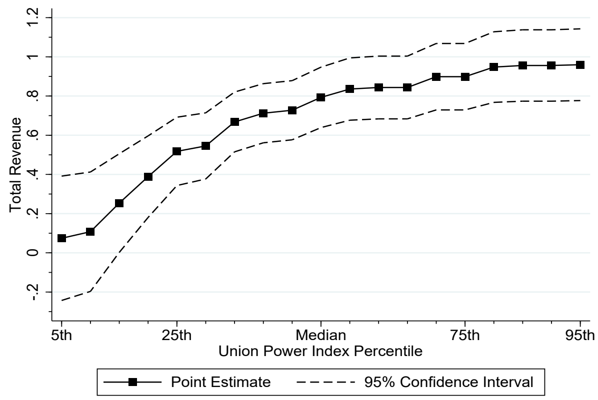
Figure III: Effects of School Finance Reforms on State Aid, by District Income Tercile



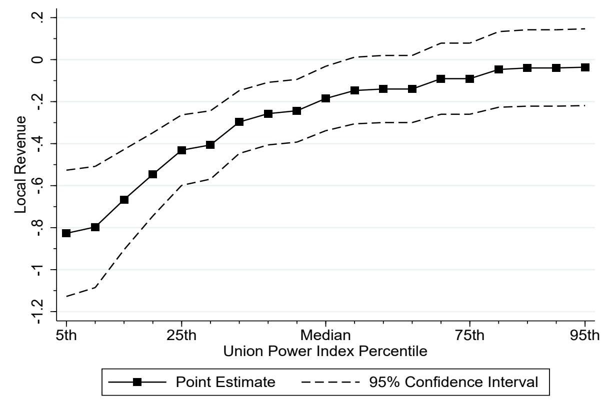
Notes: Figures show event study estimates of the effects of school finance reforms on per-pupil state aid to school districts, by 1980 district income tercile. Solid lines are point estimates, and dashed lines are 95% confidence intervals.

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

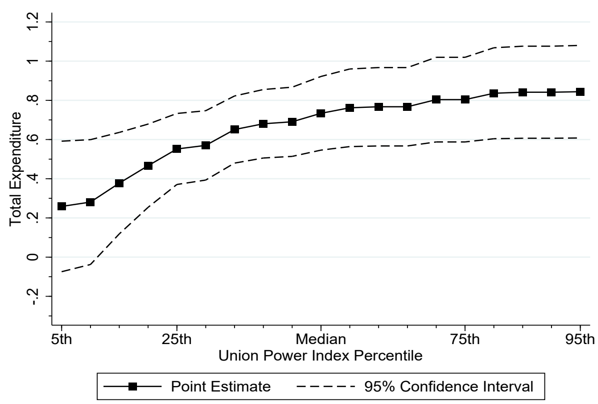
(a) Total Revenue



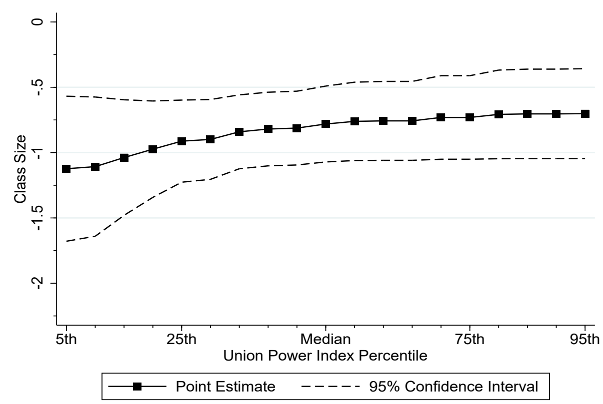
(b) Local Revenue



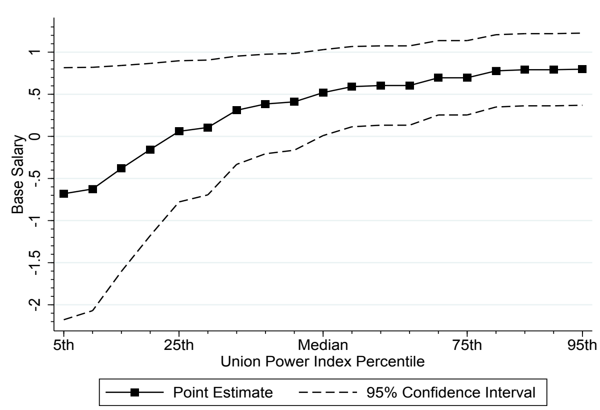
(c) Total Expenditures



(d) Class Size



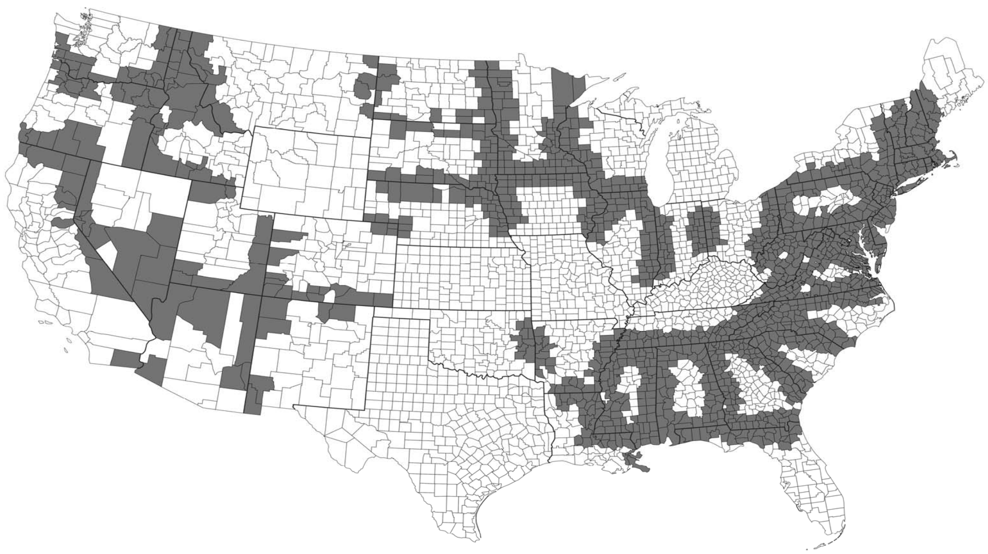
(e) Base Teacher Salary



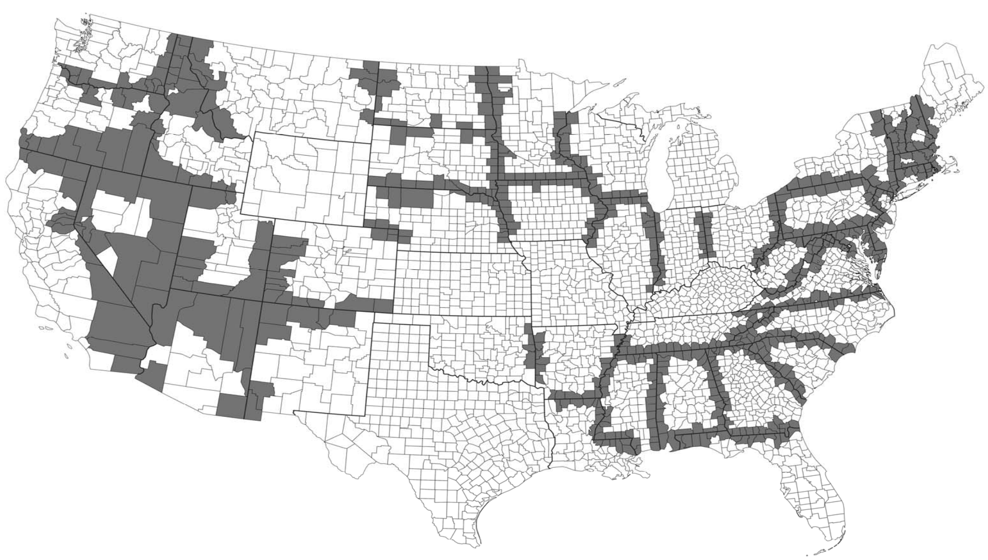
Notes: Each figure shows point estimates (solid line) and 95% confidence intervals (dashed lines) from 2SLS regressions of the dependent variable on state aid per-pupil and aid interacted with our continuous state teacher union power index. The figures show the calculated point estimate at percentiles 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.

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.

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 exclusion 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 and 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.[33] 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.

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 scores in grades four and eight.[34] 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 students' 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 students' latent ability. We then

[33] 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.

[34] 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.

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.[35]

[35] 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.

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 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	20
Arizona	Allowed	Yes	1	0.72	51
Arkansas	Allowed	Yes	1	1.02	47
California	Mandatory	No	3	2.84	6
Colorado	Allowed	No	2	1.78	33
Connecticut	Mandatory	No	3	2.37	17
Delaware	Mandatory	No	3	2.30	19
Florida	Mandatory	Yes	2	0.99	50
Georgia	Prohibited	Yes	0	1.01	48
Idaho	Mandatory	Yes	2	1.66	36
Illinois	Mandatory	No	3	2.72	8
Indiana	Mandatory	No	3	1.93	29
Iowa	Mandatory	Yes	2	1.99	28
Kansas	Mandatory	Yes	2	1.69	35
Kentucky	Allowed	No	2	1.91	30
Louisiana	Allowed	Yes	1	1.29	42
Maine	Mandatory	No	3	2.20	22
Maryland	Mandatory	No	3	2.13	24
Massachusetts	Mandatory	No	3	2.24	21
Michigan	Mandatory	No	3	2.45	15
Minnesota	Mandatory	No	3	2.50	13
Mississippi	Prohibited	Yes	0	1.08	45
Missouri	Prohibited	No	1	1.52	38
Montana	Mandatory	No	3	3.06	3
Nebraska	Mandatory	Yes	2	2.01	27
Nevada	Mandatory	Yes	2	2.05	26
New Hampshire	Mandatory	No	3	1.86	32
New Jersey	Mandatory	No	3	2.82	7
New Mexico	Allowed	No	2	1.54	37
New York	Mandatory	No	3	2.61	10
North Carolina	Prohibited	Yes	0	1.38	41
North Dakota	Mandatory	Yes	2	2.17	23
Ohio	Mandatory	No	3	2.59	11
Oklahoma	Mandatory	No	3	1.26	43
Oregon	Mandatory	No	3	3.18	2
Pennsylvania	Mandatory	No	3	2.85	5
Rhode Island	Mandatory	No	3	2.86	4
South Carolina	Allowed	Yes	1	1.00	49
South Dakota	Mandatory	Yes	2	1.75	34
Tennessee	Mandatory	Yes	2	1.44	40
Texas	Allowed	Yes	1	1.11	44
Utah	Allowed	Yes	1	1.48	39
Vermont	Mandatory	No	3	2.55	12
Virginia	Prohibited	Yes	0	1.06	46
Washington	Mandatory	No	3	2.72	9
West Virginia	Allowed	No	2	2.44	16
Wisconsin	Mandatory	No	3	2.33	18
Wyoming	Prohibited	Yes	0	1.91	31

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 2. 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	Total Expenditures	Pupil-Teacher Ratio	Base Salary
	(1)	(2)	(3)	(4)	(5)
Panel A. OLS Instead of IV					
State Aid	0.750***	-0.267***	0.655***	-0.150***	0.148***
	(0.027)	(0.027)	(0.025)	(0.014)	(0.049)
State Aid * Union	-0.000	0.007	-0.007	0.044**	0.082
	(0.019)	(0.019)	(0.022)	(0.018)	(0.055)
Panel B. Just-Identified IV					
State Aid	0.752***	-0.202***	0.708***	-0.757***	0.519**
	(0.071)	(0.070)	(0.085)	(0.125)	(0.261)
State Aid * Union	0.255***	0.217***	0.180***	0.143	0.302*
	(0.054)	(0.051)	(0.066)	(0.100)	(0.181)
Panel C. State-Level Clustering					
State Aid	0.675***	-0.291	0.656***	-0.838***	0.322
	(0.180)	(0.181)	(0.161)	(0.292)	(0.521)
State Aid * Union	0.302**	0.270*	0.200	0.144	0.505
	(0.136)	(0.136)	(0.150)	(0.217)	(0.393)
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 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. 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 and B, and at the state level in Panel C, in parentheses.
* significant at 10%, ** significant at 5%, *** significant at 1%.

Appendix Table 5: Effects Controlling for Heterogeneity by Union Power Correlates (Two at a Time)

	Total Revenue	Local Revenue	Total Expenditures	Pupil-Teacher Ratio	Base Salary
	(1)	(2)	(3)	(4)	(5)
Panel A. Vote Share and Median Income					
State Aid	0.658***	-0.313***	0.629***	-0.977***	0.209
	(0.089)	(0.085)	(0.086)	(0.167)	(0.428)
State Aid * Union	0.310***	0.276***	0.154**	0.043	0.363*
	(0.068)	(0.066)	(0.075)	(0.121)	(0.217)
Panel B. Vote Share and BA or Higher					
State Aid	0.664***	-0.314***	0.603***	-1.046***	0.265
	(0.090)	(0.086)	(0.095)	(0.181)	(0.526)
State Aid * Union	0.344***	0.308***	0.188**	0.007	0.474**
	(0.059)	(0.057)	(0.073)	(0.116)	(0.215)
Panel C. Median Income and BA or Higher					
State Aid	0.644***	-0.322***	0.598***	-0.860***	0.036
	(0.062)	(0.059)	(0.074)	(0.136)	(0.409)
State Aid * Union	0.276***	0.243***	0.140**	0.099	0.389*
	(0.054)	(0.053)	(0.064)	(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 3. All specifications include the controls and fixed effects listed in the Table 2 notes. Panel A controls simaltaneously 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%.

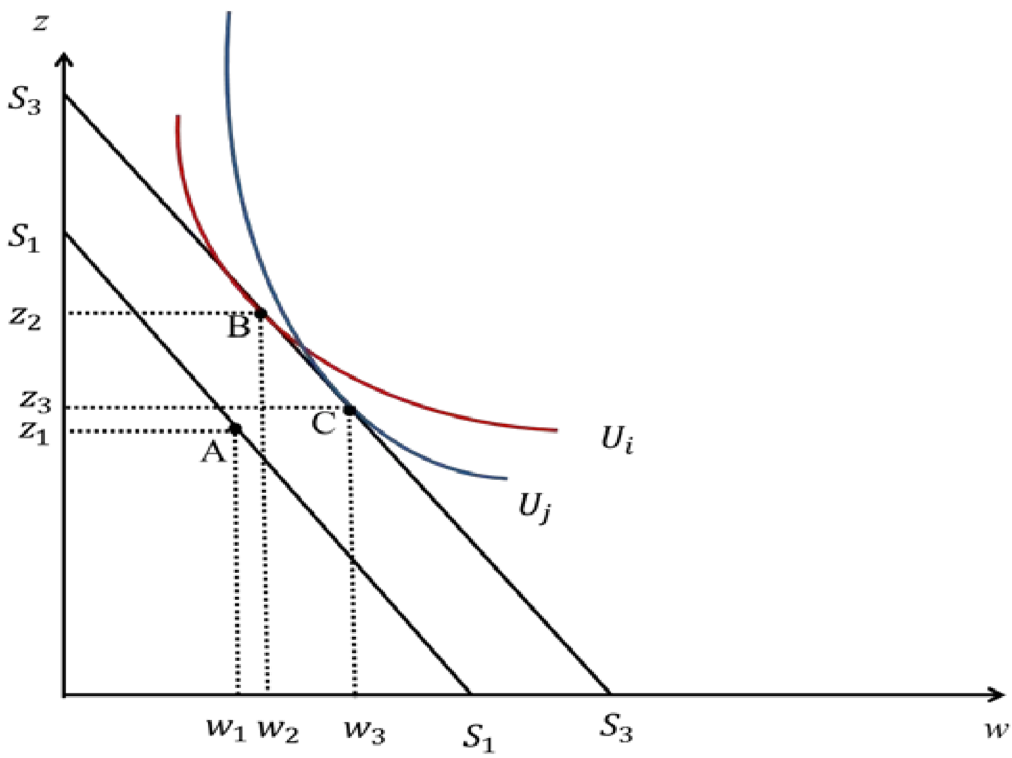
Appendix Table 6: Reduced Form Effects of School Finance Reforms on Student Achievement Using Alternative Control Sets

	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 Figure I: School District Allocation Decisions



Notes: Figure 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. Assuming no effect of teachers' unions, the district would move from point A to point B. Under the influence of the union, which has preferences U_j, the district would move to point C, leading to a larger share of the budget being spent on w.

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...

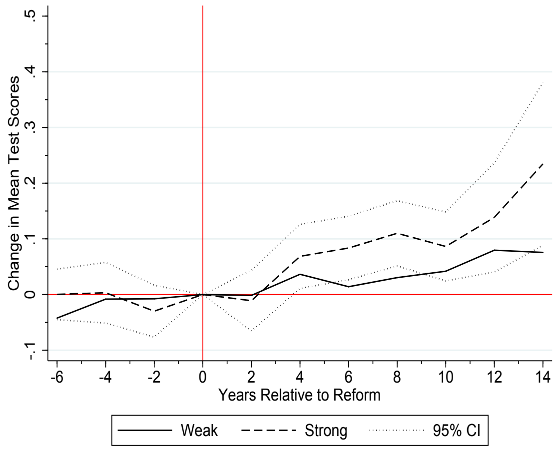
Appendix Figure II: Union Power Index Components and Weightings (...continued)

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%

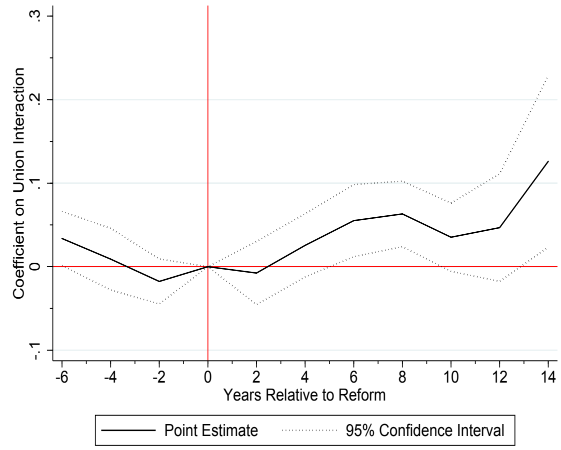
Notes: This figure is taken from Winkler, Scull, and Zeehandelaar (2012). It shows the components that comprise the primary teacher union power measure used in this paper and the relative weighting that each component receives. Our measure excludes components 1.3.1, 1.3.2, and 1.3.3, because they are 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 School Finance Reforms on Achievement, by Union Power

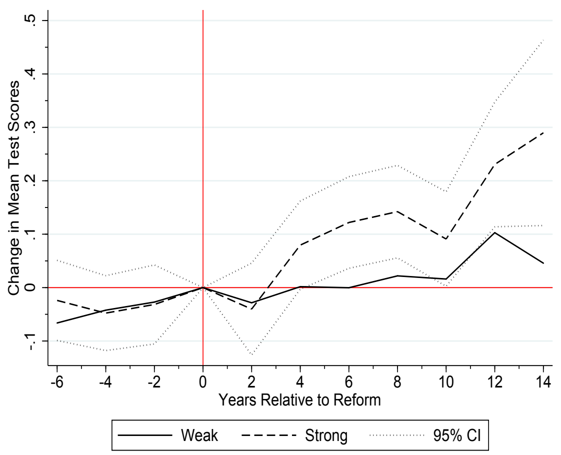
(a) Weak vs Strong Union States - All Districts



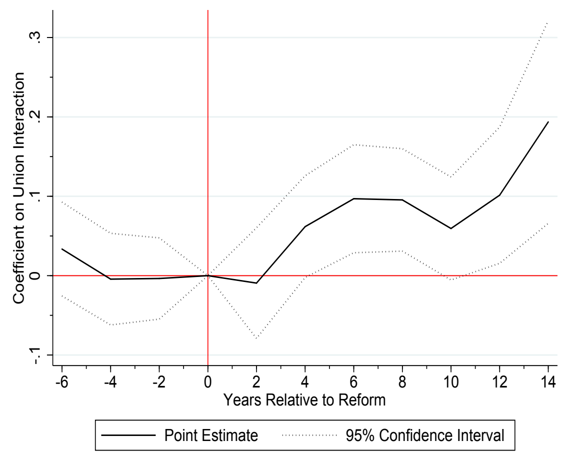
(b) Union Interaction Coefficient - All Districts



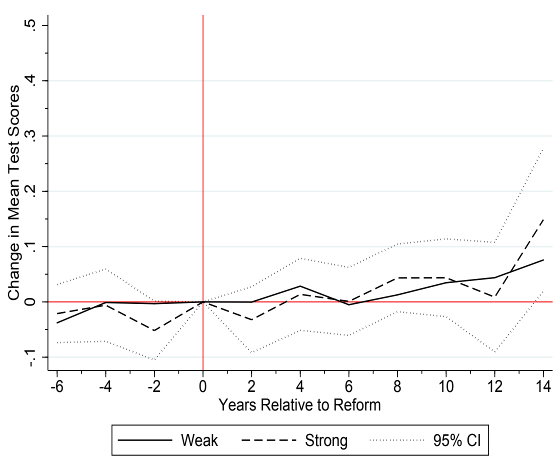
(c) Weak vs Strong Union - Bottom Income Tercile



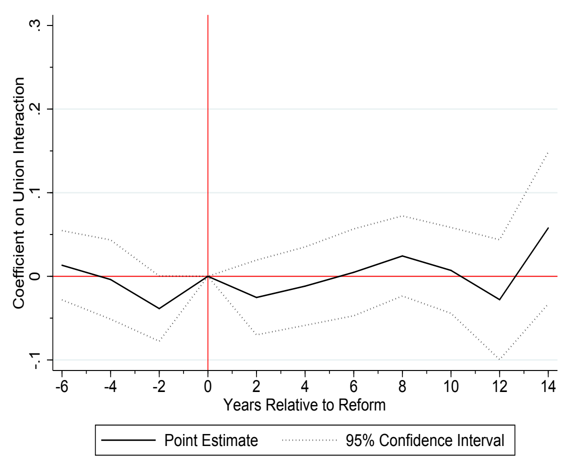
(d) Interaction Coeff. - Bottom Income Tercile



(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.