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

Eric Brunner, Joshua Hyman, and Andrew Ju*

November 30, 2017

Abstract

School finance reforms led to some of the largest intergovernmental transfers from states to local school districts in U.S. history. This paper shows that the strength of local teacher unions had a dramatic impact on both the fraction of these transfers that passed through to education funding and on the allocation of these funds. Our identification strategy exploits plausibly exogenous timing of reforms across states, and compares how reform effects differ across various measures of state teacher union power. In states with the strongest teacher unions, school districts increased education expenditures nearly one-for-one with increases in state aid, and spent the funds primarily on teacher compensation. In states with the weakest teacher unions, districts reduced local taxing effort by about 75 cents for every dollar increase in state aid, and spent the remaining funds primarily on hiring new teachers.

---

* Eric J. Brunner, Department of Public Policy, University of Connecticut, 10 Prospect Street, 4th Floor, Hartford, CT 06103, eric.brunner@uconn.edu; Joshua Hyman, Department of Public Policy, University of Connecticut, 10 Prospect Street, 4th Floor, Hartford, CT 06103, joshua.hyman@uconn.edu; Andrew Ju, Department of Economics, University of Connecticut, 341 Mansfield Road, Unit 1063, Storrs, CT 06269-1063, andrew.ju@uconn.edu.

Acknowledgements: We are grateful to Randal Reback and Steve Ross for helpful conversations and discussion. We thank seminar participants at Columbia University and audience members at the 2017 Association for Education Finance and Policy (AEFP) conference and the Association for Public Policy Analysis and Management (APPAM) conference for helpful comments.

I. Introduction

The school finance reforms that occurred across the U.S. beginning in the early 1970's caused some of the most important and far-reaching changes to school finance in American history. A recent wave of important studies has renewed research interest in these reforms, with these studies finding persistent increases in student achievement (Lafortune, Rothstein, & Schanzenbach, 2016) and long-run impacts on educational attainment (Hyman, 2017) and earnings (Jackson, Johnson, & Persico, 2016). One of the earliest and most fundamental questions regarding school finance reforms, however, was not about the effects on student outcomes. Rather, given that school finance reforms represent one of the largest transfers from states to local governments in U.S history, earlier studies focused on examining the effect of school finance reforms on the distribution of education spending and the fraction of state aid from these reforms that passes through to spending (Murray, Evans, and Schwab, 1998; Hoxby, 2001; Card and Payne, 2002). These studies generally find a high incidence of a "flypaper effect," with most of the increases in state aid translating into increased education spending.

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. Hoxby (1996) develops a simple model illustrating the potential effects of teachers' unions on school district budgets and resource allocations, and Inman (2008) argues that special interest groups, such as unions, are a leading explanation for the flypaper phenomenon. In spite of this theoretical connection between unions and resource allocation, and the prominent role of teachers' unions in the U.S. education landscape, the question of whether and how teachers' unions influenced local governments' allocation of additional state aid due to school finance reforms remains unexplored.

We combine National Center for Education Statistics (NCES) school district data from 1986 through 2012 on revenue, expenditures, staffing, and enrollment, with data on the timing of statewide school finance reforms and information on state teacher union power. We use the timing of plausibly-exogenous 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 various factors across several domains related to teacher union strength. We also use a more traditional measure of state teacher union power that relies solely on state mandatory public sector collective bargaining laws and right-to-work status.

We find that the previous estimates in the school finance reform literature mask important heterogeneity. Regardless of the teacher union power measure 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 about 75 cents on the dollar. The additional school spending in strong teacher union states was allocated primarily toward teacher salaries, while districts in weak teacher union states spent the money primarily on teacher hiring. Finally, spending in non-instructional areas such as classroom support, school administration, and capital outlays, also increased more in strong teacher union states than in states with weak teachers' unions.

While our methodology is similar to recent papers exploiting the plausibly exogenous timing of school finance reforms across states (Jackson et al., 2016; Lafortune et al., 2016), 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 endogeneity: 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 the teachers' unions, and not unobserved differences across districts in states with strong versus weak teachers' unions. We also show that our results are robust across different 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 contribute to three distinct strands of literature. First, we provide important new insights to the economics of education literature examining school finance reforms.[1] Early studies examining school finance reforms, such as Card and Payne (2002), found that a dollar of state aid increased district education spending by 50-65 cents. We find a similar mean flypaper effect, but show that this mean effect masks considerable heterogeneity driven by state teacher union power. Second, our

[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 (200) in the case of California and Papke (2005), Chaudhary (2009), Epple and Ferreyra (2008), Roy (2011), and Chakrabarti and Roy (2015) in the case of Michigan.

results contribute to the public finance literature on the effect of federal and state intergovernmental grants on local spending decisions. Several recent papers find evidence of significant crowding-out of state and federal grants on local spending (Lutz, 2010; Cascio et al., 2013; Gordon, 2004; Knight, 2001). In contrast, Feiveson (2015) and Dahlberg et al. (2008) find little evidence that federal intergovernmental grants crowd-out local spending. We show that, consistent with Inman (2008), one source of this heterogeneity in crowd-out is local politics, and specifically, the strength of local unions or other special interest groups in ensuring the grants "stick where they hit."

Finally, our results build on the labor economics literature examining the effects of teachers' unions. Lovenheim (2009) and Frandsen (2016) find little effect of unions on teacher compensation, while Hoxby (1996) finds that unions increase teacher wages and expenditures per student. We build on these studies by examining how unions affect the allocation of an exogenous funding increase, as opposed to routine changes in year-to-year budgets. We find larger effects than Hoxby (1996), suggesting that teachers' unions most influence spending amounts and allocations when there is a windfall of new funds to distribute, rather than incremental year-to-year budget changes.

In the remainder of this paper, we first turn to some background literature and develop a simple conceptual framework in section II. We describe the data in Section III, and the empirical framework in section IV. In Section V, we provide our results, and we conclude in Section VI.

II. Conceptual Framework

How do teachers' unions affect school district budgets and the allocation of school resources? The answer to that question depends critically on the objective function of teachers' unions. As noted by Lovenheim (2009) a central objective of any labor union is to maximize the welfare of its members. Consequently, teachers' unions may bargain for higher teacher salaries, better working conditions and other items that disproportionately benefit teachers. Under this rent-seeking view, teachers' unions may advocate for an inefficient allocation of resources that favors increases in educational inputs that benefit primarily teachers over increases in inputs that raise school productivity. Alternatively, teachers may be primarily interested in promoting student interests and school quality. Under this view, teachers' unions would maximize the welfare of its members by bargaining for an allocation of school resources that maximizes school productivity. Of course, these two competing views are not mutually exclusive. Even if teachers' unions are primarily interested in maximizing school quality, they may still bargain for higher teacher salaries, smaller class sizes, etc., if changes in these inputs increase school productivity.

Furthermore, whether teachers' unions are purely rent seeking or benevolent actors wishing to maximize school quality, unions may still attempt to use their political power to increase the size of the

budget and thus school spending. For example, if teachers' unions are primarily rent-seeking, increasing the size of the budget allows the union to bargain for even higher teacher salaries and better working conditions. Similarly, if teachers' unions are primarily interested in maximizing school quality, and additional resources lead to higher student achievement, unions will again use their political power to advocate for higher school spending.

While the objective function of teachers' unions is critical to understanding how unions are likely to affect school budgets and the allocation of school resources, a second critical factor is the bargaining power of unions. As noted by Moe (2006), Rose and Sonstelie (2010) and Strunk (2010), the primary source of union bargaining power comes from a union's ability to influence local school board elections and hence the composition of the school board with whom it must bargain. If teachers' unions can use their political power, financial resources, and the effort exerted by union members to help ensure that school boards are comprised of individuals sympathetic to their preferences, they gain control over both the size of the budget and how that budget is allocated across various school inputs.

A number of recent papers provide evidence consistent with the view that teachers' unions influence the outcomes of school board elections and thus affect the size of school budgets and the allocation of resources. Seig and Wang (2013), develop a theoretical model in which public sector unions can 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, local public spending will increase. Consistent with that hypothesis, Seig and Wang (2013) find that challengers of incumbents in municipal elections strongly benefit from union endorsements and union endorsed challengers that win election tend to increase spending and adopt more pro-union policies. Similarly, Moe (2006) provides 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.[2]

Indeed, the political power of special interest groups such as teachers' unions has been hypothesized to be an explanation of the flypaper effect; the commonly observed phenomena that "money tends to stick where it hits." The neoclassical view of intergovernmental grants suggests that when communities receive a lump sum grant from a higher 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. A large and

[2] There is also evidence that teachers are disproportionately more likely to vote in local school referenda and school board elections. See for example, Moe (2006) and Anzia (2011). Given that voter turnout during such local elections tends to be quite low, teachers can have a significant impact on the outcomes of such elections.

influential literature, however, has found that intergovernmental grants tend to increase government spending by much more than an equivalent increase in income.

Scholars have provided a number of explanations for the flypaper effect including: matching grants being misclassified as exogenous aid, endogeneity and omitted variable bias in econometric specifications, voter ignorance about intergovernmental grants and finally, politics (Inman, 2008; Hines and Thaler, 1995). Among these alternative explanations, Inman (2008) suggests that the most likely explanation for the flypaper effect is politics, and notes that "the flypaper effect is a consequence of an inability of citizens to write complete "political contracts" with their elected officials."

The theoretical literature on the political explanation for the flypaper effect has several strands. One stand focuses on asymmetric information between voters and elected officials (Filimon, Romer, and Rosenthal, 1982; Strumpf, 1998). In these models it is assumed that 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. Another strand focuses on the role of special interest groups (Dougan and Kenyon, 1988; Singhal, 2008; Seig and Wang 2013). In these models, interest group lobbying leads to an allocation of resources that favors spending on the good preferred by the interest group. Regardless of which explanation more accurately accounts for the observed flypaper effect, the literature suggests that teachers' unions, through their ability to influence the composition of local school boards and local elections, may be able to influence the size of the school budget and redirect intergovernmental grant revenue away from property tax relief and towards increased school spending.

Figures 1a and 1b illustrate the potential effect teachers' unions can have on school budgets and the allocation of school resources.[3] Figure 1a illustrates the choice problem facing a school district before and after an increase in intergovernmental aid brought about by a school finance reform. 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*.[4] 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

[3] Cascio et al (2013) provide a graphical illustration similar to Figure 1a to illustrate the effect on an increase in federal Title 1 spending while Hoxby (1994) and Feiveson (2015) present graphical illustrations similar to Figure 1b to illustrate the effect of teachers' unions on the allocation of school resources.

[4] For simplicity we normalize the prices of both *X* and *S* to one.

school spending of S_2 per-pupil. Note that in this case the school district increases spending on schools by its marginal propensity to consume out of income, which leads to a relatively small increase in *S* and a larger increase in *X*.[5] Now consider a teachers' union whose members have preferences like those depicted by indifference curve U_j. If the union can use its political power, lobbying effort, and influence over local school board elections to push the allocation of resources in favor of its preferences, the district will choose to move to point *C*, which leads to school spending of S_3 per-pupil. In that case, school spending rises by much more than the marginal propensity to consume out of income, leading to the classic flypaper effect.

Figure 1b illustrates the resource allocation choice problem facing a district when their budget is originally set a S_1 per-pupil (no intergovernmental aid) and then expands to S_3 per-pupil following the introduction of an intergovernmental grant as in Figure 1a. Here we consider two inputs to the educational production function, teacher salaries, *w*, and a composite input, *z*, where the price of both inputs is normalized to one. Prior to the introduction of intergovernmental aid the district choses the allocation of resources depicted by point A, and thus allocates w_1 to teacher salaries. Assuming teachers' unions have no effect on the allocation of resources, after the introduction of inter-governmental aid, the school district chooses the bundle of inputs it believes will maximize school quality, which is associated with point *B* on indifference curve U_i. This leads to a small increase in teacher salaries and a larger increase in spending on the composite input. However, if teachers' unions are able to use their bargaining power to push the allocation of resources in favor of their preferences, the district will choose to move to point C, associated with indifference curve U_j. In this case, a larger share of the budget is allocated towards teacher salaries and a smaller share towards the composite input.

III. Data

Our primary data source is the Local Education Agency (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 year 1986-87 – 1989-90. For this same period, 1986 – 2011[6], we also utilize the annual

[5] According to Hines and Thaler (1995), most estimates of the marginal propensity to consumer out of income for governments are between $0.05 to $0.10.

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

common core of data (CCD) school district universe surveys that provide student enrollments and staff counts for every school district.

We restrict our sample in several ways. First, one of our primary objectives is to examine whether strong teachers' unions affect the degree to which inter-governmental aid "sticks where it hits" (i.e. the flypaper effect). As noted by Inman (2008), one of the explanations for why prior studies have tended to find strong evidence of a flypaper effect is that researchers may have misclassified matching grants as lump sum. To avoid this issue, 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. Second, we omit Michigan and Wyoming since these states adopted school finance reforms that effectively eliminated local discretion over funding as part of their school finance reforms. Third, because the NCES F33 financial data tends to be noisy, particularly for small districts, we follow Gordon (2004) and Lafortune et al. (2017) and exclude small districts (with enrollment below 250 students) from the analysis.[7] Finally, in our preferred specifications, we omit the final three years (2009-2011) of our sample due the severe and potentially confounding influence of the Great Recession on school finances during that time (Evans, Schwab, and Wagner, 2017). We show 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 the population at or below the poverty level from the Special School District Tabulations of the 1980 Census.[8] We obtained a comprehensive list of school finance reforms from Jackson et al. (2016) and Lafortune et al. (2016). Our primary coding of these SFRs is based on the coding structure developed by Lafortune et al. (2016), though we differ from their coding in a few cases. We show that our results are robust to using the Lafortune et al. (2016) coding structure as well as to using a stacked difference-in-differences strategy that uses all school finance reforms. See Appendix Table 1 for a listing of the school finance reforms used in our main analysis.

Finally, we collect data on state teacher union power. Our primary measure is based on an index created as part of a study by researchers at the Fordham Institute, a Washington D.C.-based education policy think-tank (Winkler, Scull, and 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) perceived influence. Many of

[7] See the technical Appendix for a detailed discussion of our data sample and our sample restrictions.
[8] Data on 1980 median household income, fraction black, fraction urban, and fraction poor is 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.

the index components are measured as of 2012 after most of the school finance reforms in our sample, raising concerns that some components may be endogenous to the school finance reforms. After carefully reviewing all of the index components, the only ones we believe were directly influenced by school finance reforms are the measures of school spending included in the "resources and membership" category. We therefore drop these variables from the index and recalculate it without them. In practice, this makes very little difference as these spending measures compose only 6.7% of the weight of the index.[9]

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 census region, Great-Lake area of the Midwest region, and Pacific census division, while the weakest union power 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 for education, higher teacher salaries and household income, smaller district enrollments and shares black and poor, and are more heavily urban.

We supplement our analysis with a more basic measure of state teacher union power that utilizes state laws implemented prior to our sample period, thus avoiding any concerns about endogeneity or subjectivity of the continuous measure. Specifically, we first categorize states by whether or not they mandate collective bargaining (CB), as defined in the NBER Public Sector Collective Bargaining Law Data Set, developed by Freeman (1988) and updated by Kim Rueben. For the most recent years, we also augment this data with information from Sanes and Schmitt (2014). We then augment the information on state collective bargaining laws with information on state right-to-work (RTW) status, obtained from the National Conference of State Legislatures.[10] We code states as a zero if they are CB non-mandatory *and* a RTW state, as a one if they are either CB non-mandatory and not a RTW state *or* CB mandatory and a RTW state, and as a two if they are CB mandatory and not a RTW state.

[9] Thank you to Amber Northern (formerly Winkler), Janie Skull, and Dara Shaw (formerly Zeehandelaar) for generously sharing the index and all of its underlying components. See page 27 of Winkler, Scull, and Zeehandelaar (2012) for a concise overview of the index components and their relative weightings.

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

Figure IIb shows a state map of the U.S. by this more basic measure of state teacher union power, with shading ranging from white, to light grey, to dark grey, for the weakest to strongest union states. While there are some exceptions, the geographic patterns of state union power using this alternative measure is similar to the pattern for the continuous measure shown in Figure IIa.[11] We prefer the continuous index over the more basic measure, 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 and county 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 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, which we cluster at the school district level.

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 poor. 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

[11] Appendix Table 2 provides values by state for both of our teacher union power measures. The two measures are strongly positively correlated with a correlation index of 0.78.

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 and McGuire 1997).[12]

As noted by Jackson et al. (2016), among others, the amount of intergovernmental state aid allocated to districts is likely endogenous. Following Jackson et al. (2016), we "overcome the biases inherent in relying on potentially endogenous observational changes in school resources by documenting the relationship between exogenous quasi-experimental shocks" to state aid and the allocation of school resources. Specifically, to isolate potentially exogenous variation in state aid, we use the timing of adoption of school finance reforms as instrumental variables and estimate first-stage models of the following form:

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

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

In equations (2) and (3), SFR_{st} is an indicator for whether state *s* has 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 have very different impacts on state aid for low- and high-income districts, with the goal of at least partially equalizing school funding between districts. 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.

[12] 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).

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:

$$y_{ist} = \sum_{j=-5}^{8} \gamma_j T_{j,st} + \delta_i + \lambda_t + \eta_{ist}, \quad (4)$$

where, $T_{j,st}$ represents a series of lead and lag indicator variables for when state *s* adopted 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 that $T_{0,st}$ always equals one in the year in which state *s* adopted a school finance reform. We include indicator variables for 1, 2, 3, 4 and 5 plus years prior to adoption of a SFR ($T_{-5,st}, T_{-4,st}, T_{-3,st}, T_{-2,st}, T_{-1,st}$), and years 1 - 8 or more years after adoption ($T_{1,st} - T_{8,st}$). Note that $T_{-5,st}$ equals one in all years that are 5 or more years prior to the adoption of a SFR in state *s* and similarly, $T_{8,st}$ equals one in all years that are 8 or more years after the adoption of a SFR in state *s*. The omitted category is therefore the year a state adopted a SFR, $T_{0,st}$.

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_{-5} to t_{+8}. The estimated coefficients on the lead treatment indicators ($\gamma_{-5}, \ldots, \gamma_{-1}$) 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 ($\gamma_{+1}, \ldots, \gamma_{+8}$) allow the effect of school finance reforms on state aid to evolve slowly over time.

V. Results

We begin our analysis by showing that school finance reforms (SFRs) led to exogenous increases in state aid. To do this, we estimate the event study model from equation (4), and plot the $\gamma_j's$ both for the full sample of school districts and separately by district within-state median income tercile. Figure 3a (all districts) shows that after a school finance reform, state aid increases rather quickly to between $500 and $800 per pupil above the pre-reform level, and remains at this level through at least 8 years after the reform. Importantly, there is no evidence of trending state aid prior to school finance reforms. Figure 3b shows more dramatic effects for districts in the bottom income tercile, where state aid increases by more than $1,000 per pupil. Figures 3c and 3d show the effects for the middle and top income tercile districts, where both sets of district experience increases around $500 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.[13] 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, the proceeding interactions also interacted with the union power index, and 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 poor interacted with the linear time trend and the proceeding interactions also 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 at the mean level of state teacher union power (where the standardized index equals zero) experienced an increase of $1,130 per-pupil; the increase was $607 for the middle tercile and $609 for the top tercile. The increase for districts with a one standard deviation higher level of state teacher union power was $208 larger for the bottom income tercile, $196 smaller for the middle tercile, and $180 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 132 and 135, respectively.[14] In sum, school finance reforms provide large and statistically significant plausibly exogenous increases in state aid.

A. Effects of State Aid on Revenue 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

[13] First stage results for the more basic measure of teacher union power, presented in Appendix Table 3, show a similar pattern and have similar F-statistics ranging between 91 and 140.

[14] 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 97).

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 62 cents with every dollar increase in state aid, while a one SD increase in teacher union power leads to a 36 cent larger increase in total revenue. This pattern of results is similar after adding the expanded controls – a 64 cent increase at the mean level of union power, and a 45 cent larger increase given a one SD increase in union power (column 2). These results demonstrate that while total revenue goes up by almost 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' unions.

As property tax relief is the likely source of this crowd-out, we examine the effects of increased state aid on local revenue (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 33 cents for each additional dollar of state aid, with a near zero local reduction among states with teacher union power one standard deviation higher and 0.73 cent reduction in local revenue among states with teacher union power one standard deviation lower. These results explain most of the heterogeneity in total revenue increases by union power – districts in 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 (columns 5 and 6). Using our preferred specification (column 6), we find that a SFR-induced dollar increase in state aid translates into a 60 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 and Payne 2002). However, we find that the increase is 37 cents larger (or smaller) given a one SD higher (or lower) level of teacher union power, suggesting substantial heterogeneity in the fly-paper effect by the strength of a state's teachers' unions. Taken together, these results 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.[15]

[15] 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.

B. Boosting Teacher Compensation or Shrinking Class Size

The results presented in columns 1-6 of Table 2 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 consistent with our basic conceptual framework showing that teachers' unions cause school boards to spend more of a windfall from intergovernmental grants on education. However, unions may also shape the allocation of resources to different inputs. For example, a union's indifference curve may be shaped such that an increase in state aid will lead to a larger increase in teacher compensation, and less of an increase in teacher employment. 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 0.62 pupils among districts in a state with the mean value of union power.[16] This represents a 3.8% decrease in class size, relative to the sample mean of 16.3 students. The decrease in class size is only 0.32 pupils (-0.62 + 0.30), or 2.0%, in a state with union power that is one SD higher. In contrast, the decrease is 0.92 pupils (-.62 – 0.30), or 5.6%, in a state with union power that is one SD lower. [17]

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 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.[18] We focus on district base teacher salary, which is available in every wave and 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

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

[17] To ensure that this difference is being driven by differential effects on teacher hiring and not student enrollments, we examine the effects on the number of teachers in a district, and find that a positive effect of state aid on the number of teachers in both types of states that is substantially smaller in mandatory CB states.

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

years and overlap of districts across waves, we lose about 90% of our sample size and 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 32 cent increase in teacher salaries for districts in a state with mean teacher union power, and a 41 cent larger increase for districts in states with one SD deviation higher teacher union power. These results are less statistically precise than the revenue, expenditure, and class size results, but the coefficient on the interaction of state aid and union power remains statistically significant at the 1% level. Consistent with our basic conceptual framework, strong teacher unions appear to be focusing education expenditures more on increasing teacher salaries than on hiring new teachers.

C. Allowing for Non-Linear Effects of Teacher Union Power

The results reported in Panel A, implicitly assume a linear relationship between the continuous measure of teacher union power and our outcomes of interest. In Panel B we relax this assumption by including a quadratic in the teacher union power index. We then calculate the estimated effects at various percentiles of the state teacher union power distribution. For example, in the bottom rows of Panel B in Table 3, we show the estimated effects at the 25^{th} and 75^{th} percentiles. States near the 25^{th} percentile of teacher union power increase total revenue by 39 cents for a one dollar increase in state aid, whereas states near the 75^{th} percentile increase revenue by about 92 cents on the dollar. We also plot estimated coefficients from this excise at each vigintile (20 percentiles) of the index. Figure IVa presents the results from this exercise where total revenue is the outcome. States with very low teacher union power (near the 5^{th} percentile) increase total revenue by about 25 to 30 cents for every dollar of SFR-induced increase in state aid, whereas states with very high teacher union power (near the 95^{th} percentile) increase total revenue about 1-for-1 with increases in state aid.

Continuing to look across either the bottom rows of Panel B in Table 3 or Figures 4b-e, local revenues in weak teacher union states (25^{th} percentile) decrease by 55 cents, but only by 9 cents in strong states (75^{th} percentile); and total expenditures increase by 41 cents in weak union states and by 83 cents in strong union states. Class sizes decrease by 0.80 pupils in weak union states and by 0.39 pupils in strong union states. Finally, there is a statistically insignificant 0.09 cent increase in teacher salaries among weak union states and a 0.57 cent increase among strong union states.

D. Alternative Measure 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 measure of state teacher union power that avoids any possible concerns of endogeneity or subjectivity of the continuous measure. As noted previously, our alternative measure of state teacher union power codes states as a zero if they are CB non-mandatory and a RTW state, as a one if they are either CB non-mandatory and not a RTW state or CB mandatory and a RTW state, and as a two if they are CB mandatory and not a RTW state. Thus, in panel C, the main state aid term reflects the effect of a dollar increase in state aid for states with a value of zero for the union power measure. 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. Finally, for states with a value of 2 for the measure, the result is calculated by adding the main coefficient to twice the coefficient on the interaction term.

We find that for every dollar increase in state aid due to school finance reforms, total revenue increases by 21 cents in states with the weakest unions, and by 47 cents (=0.21+0.26), and 73 cents (=0.21+2*0.26), for every dollar of additional state aid in states with medium and high teacher union power, respectively (Table 3, Panel C, column 2). These states experience a reduction in local revenue of 54 cents, 40 cents, and 26 cents, respectively, and an increase in education expenditure of 33 cents, 50 cents, and 68 cents, respectively. This pattern of results mirrors the pattern obtained using the continuous union power measure: to the extent that mandatory CB and RTW laws affect state teacher union power, the states with weaker union power see less of each dollar of state aid going toward education expenditures, and more going toward local property tax relief.

As shown in columns 7-9 of Panel C, we also find results similar to those reported in Panels A and B for our pupil teacher ratio and base salary specifications when using the alternative measure of union power. Specifically, a one thousand dollar increase in state aid reduces the PTR by 1.44 pupils among school districts in states with the weakest unions (column 8). This represents a 9% decrease in class size, relative to the sample mean of 16.0 in these states. In contrast, among districts in states with the strongest unions, a one thousand dollar increase in state aid reduces the PTR by only 0.50 pupils or 3.1% relative to the sample mean of 16.3. We find a small negative and statistically insignificant effect of a dollar of SFR-induced state aid on base teacher salaries among districts in states with the weakest unions. However, the coefficient on the state aid and CB interaction term is positive and statistically significant suggesting an increase of slightly over 50 cents in teacher salaries for every dollar increase in state aid among districts located in states with the strongest unions. Again, consistent with our basic conceptual framework and with the results using the continuous teacher power measure, strong teacher

unions appear to be focusing education expenditures more on increasing teacher salaries than on hiring new teachers.

E. Possible Teacher Union Endogeneity

One potential concern with the differences we find between the allocation of resources in strong versus weak teacher union states is that they may not be due to the teachers' unions, but rather due to unobserved differences across these states in preferences for 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 toward K-12 education spending and the allocation of K-12 education spending more specifically, implying that 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 about the possible endogeneity of state teacher union power 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 our 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 directly attempt to deal with this potential endogeneity of state teacher union power. We move forward using our preferred specification, which includes the expanded set of controls and uses the continuous teacher union power measure.

Our first strategy designed to address the potential endogeneity of teacher union power, involves controlling directly for observable characteristics of states 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 several 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:

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

where $Char_s$ includes two state characteristics that are highly correlated with state teacher union power: 1988 presidential democratic vote share and 1990 median income. 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. If β_2 withstands the addition of these state teacher union power correlates, it provides reassurance that it is identifying the effects of teacher union power and not state characteristics associated with teacher union power.[19]

We first interact state aid with the state share voting democratic in the 1988 presidential election. Controlling for heterogeneity by democratic vote share does not change the main results on total or local revenue, expenditures, class size, or base salary (Table 4, Panel A). We next examine whether the results are robust to controlling for the interaction of state aid with state 1990 median income (Panel B), and finally to including both the interaction of state aid with the state share voting democratic in the 1988 presidential election and the interaction of state aid with state 1990 median income (Panel C). Again, the point estimates vary in magnitude somewhat, but the overall pattern remains the same – the only exception is that when controlling only for state 1990 median income, the main state aid effect for the base salary results is attenuated and loses statistical significance, but the interaction with union power is still positive and statistically significant (Panel B, column 5).

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. The similarity of these adjoining school districts that differ in their state teacher union power thus provides us with 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 strategies to restrict our sample to counties along borders. First, we restrict to counties where the county centroid is less than 50 miles from the nearest state border. Figure 5a 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 to include counties close to

[19] 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.

that border on either side. The inclusion of the border 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 differ in their states' teacher union power, but are otherwise similar, we conduct a series of balancing tests by estimating models of the form:

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

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, 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 (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 the 1984, 1988, and 1992 presidential elections, have higher median household income, lower fraction below poverty, and higher educational attainment.[20] 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 its previous value, and the coefficient on fraction non-white, which shrinks to approximately 1/3 of its previous value. The pattern is similar when we restrict the sample instead 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.

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

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 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. These districts spend less in reducing class size and more on increasing teacher salaries. While the magnitude of the point estimates varies to some extent, the pattern is robust across both border samples.

The robustness of the results to these two conceptually very different strategies is reassuring that the heterogeneity that we document is due to the differences in teachers' unions and not due to other unobserved characteristics across states.

F. School Finance Reform Coding Robustness Checks

In this section, we present three checks to examine the robustness of our results to decisions about the sample and way we code school finance reforms. First, 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. Second, while we tend to follow Lafortune, Rothstein, and Schanzenbach (2016) in our coding of school finance reforms, there are some cases where we disagree with their choice, and so in Panel B we present results following their coding exactly. 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 coefficients remain statistically significant.

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 C we add these states back into 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 total and local revenue specifications. Specially, the results reported in Panel C now imply that for a state with the mean value of union power, total revenue increases by 75 cents while in states with union power one standard deviation below the mean total revenue increases by 58 cents (as compared to 19 cents in Table 3). Given that three of the four states that implemented matching aid formulas tend to be weak union states, it is not surprising that we now find significantly less crowd-out among states with weaker unions. The results reported in column 2 of Panel C (local revenue) tell a

similar story. There we find that local revenues decline by only 37 cents (as compared to 73 cents in Table 2) in states with union power one standard deviation below the median. Once again, these results are expected given that we do not predict strong crowd-out effects associated with introduction of matching aid.

G. A Deeper Dive into Total Expenditures

We've shown that states with strong teacher unions used the increases in state aid from school finance reforms primarily to increase teacher salaries, while states with weaker teacher unions focused funds on reducing class size. We focus on spending on teacher salaries and class size reductions (i.e., teacher hiring), because teachers represent the single largest expense in public education. However, other inputs to education production besides teachers can be important as well. In this section, we first conduct back-of-the-envelope calculations to translate the estimated effects on class size and teacher salaries into fractions of the marginal dollar that these states spend on these inputs. We then examine how much of each dollar of school finance reform induced state aid passes through from total expenditures to various subsets of expenditures, for example, current expenditures versus capital outlay, and among current expenditures, instructional versus non-instructional spending.

Using our preferred specification, we found that in strong teacher union states a $1,000 increase in state aid led to a 0.39 pupil reduction in the PTR, and in weak states a 0.80 pupil reduction in the PTR (Table 3, Panel B, column 8).[21] These represent class size reductions of 2.4% and 5.0%, respectively. Assuming that class sizes are reduced by hiring beginning teachers, and that the average base salary during the sample period is $37,478, this means that $901 and $1,883 is being spent, respectively, per class to reduce class size.[22] Given mean class sizes of 16.3 and 16.0, respectively, and that these effects are from increases of $1,000 per pupil, this means that in strong teacher union states, only $901 of every $16,300 increase in state aid, or 5.5%, was spent on hiring new teachers, whereas in weak teacher union states, $1,883 of every $16,000 increase in state aid, or 11.8%, was spent on hiring new teachers. Given that total expenditures increased by 83 cents on the dollar in strong teacher union states, this means that only $13,529 passed through to total expenditures, and of that they spent $901, or 6.7%. Total expenditures increased by 41 cents on the dollar in weak union states, so only $6,560 of total expenditures passed through and districts spent 28.7% of this on teacher hiring.

[21] Here we use the 75th and 25th percentiles of the state teacher union power distribution to proxy for strong and weak teacher union power, respectively.

[22] These dollar amounts are calculated by multiplying the percent class size reduction by the starting teacher salary, because to achieve those reductions districts would presumably have to hire that percent more teachers.

Now we examine what fraction of the marginal dollar districts spent on boosting teacher salaries. Base salaries increased by $572, or 1.5%, in strong union states, and $91, or 0.2%, in weak teacher union states. Assuming that the same percent raise was issued to all current teachers, this means that in a class with a teacher making the average salary during the sample period of $67,299 in strong union states, that teacher's salary increased by $1,009 (=1.5%). So, of the $13,529 that passed through to total expenditures, 7.5% (=$1,009/$13,529) of it was spent on raising teacher salaries. In weak union states, average teacher salary was $53,925, and the increase was $108 (=0.2%), so only 1.7% of the $6,512 that passed through to total expenditures was spent on raising salaries.

Given that only a small percentage (14.2% in the case of strong teacher union states and 30.4% in the case of weak teacher union states) of the increase in total expenditures due to school finance reform was spent on teachers in the form of hiring new teachers or increasing salaries, where else did the funds go? Column 1 of Table 8 repeats the preferred specification from column 6 in Panel A of Table 3 for total expenditures, showing a 60 cent increase for a state with the mean value of union power, while a one SD increase in teacher union power leads to a 37 cent larger increase in total expenditures. States with mean teacher union power increase current expenditures by 49 cents and capital outlays by 11 cents. Among current expenditures, they increase instructional spending (i.e., spending on teachers) by 38 cents and non-instructional spending (i.e., school nurses, librarians, counselors, administration, etc.) by 24 cents. States with teacher union power that is one standard deviation higher increase current expenditures by an additional 28 cents, capital outlays by an additional 14 cents, instructional spending by an additional 13 cents, and non-instructional spending by an additional 17 cents. Clearly, while instructional spending is the single largest expense, districts are spending a large share of their additional dollar on other expenditures, and the same pattern emerges where more of each dollar in additional state aid passes through to these education spending categories in strong than in weak teacher union states.

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 the increases in state due to school finance reform 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 due to school finance reforms 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. In particular, we find that 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 allocated primarily toward teacher salaries, while districts in weaker teacher union states spent the money primarily on hiring new teachers. Our results are robust to two strategies that address the potential endogeneity of teacher union strength, suggesting that we are identifying the effects of the teachers' unions, and not unobserved differences correlated with state teacher union power.

Our results have several implications. First, our results support Inman (2008) that an important source of heterogeneity in crowd-out is local politics, and specifically, the strength of local unions or other special interest groups in ensuring that grants stick where they land. This is important for policy: federal and state policymakers seeking to transfer funds to lower levels of government should be aware of any relevant unions, and proactively consider the influences these unions may have on the extent of crowd-out of the transfers. Second, in the context of school finance, our results provide an important new perspective on the importance of unions and on the effectiveness of the school finance reform movement that began in the 1970's. We are the first to empirically document an important source of heterogeneity in the effects of these reforms, and our results can help state education policymakers redesigning their state aid formulas to consider the ways in which their state's teachers' unions will influence the effects of their proposed reform.

References

Anzia, S. (2011). Election timing and the electoral influence of interest groups. *Journal of Politics*, 73 (2): 412-427.

Ballou, D., & Podgursky, M. (2002). Returns to seniority among public school teachers. *Journal of Human Resources*, 892-912.

Brueckner, J. K., & Neumark, D. (2014). Beaches, sunshine, and public sector pay: theory and evidence on amenities and rent extraction by government workers. *American Economic Journal: Economic Policy*, 6(2), 198-230.

Brunner, E. J., & Squires, T. (2013). The bargaining power of teachers' unions and the allocation of school resources. *Journal of Urban Economics*, *76*, 15-27.

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

Cascio, E. U., Gordon, N., & Reber, S. (2013). Local responses to federal grants: evidence from the introduction of title I in the South. *American Economic Journal: Economic Policy*, 5(3), 126-159.

Chakrabarti, R., & Roy, J. (2015). Housing markets and residential segregation: Impacts of the Michigan school finance reform on inter-and intra-district sorting. *Journal of Public Economics*, 122, 110-132.

Chaudhary, L. (2009). Education inputs, student performance and school finance reform in Michigan. *Economics of Education Review*, 28(1), 90-98.

Cowen, J. M., & Strunk, K. O. (2015). The impact of teachers' unions on educational outcomes: What we know and what we need to learn. *Economics of Education Review*, 48, 208-223.

Cutler, D. M., & Glaeser, E. L. (1997). Are ghettos good or bad? *Quarterly Journal of Economics*, 112(3), 827-872.

Dahlberg, M., Mörk, E., Rattsø, J., & Ågren, H. (2008). Using a discontinuous grant rule to identify the effect of grants on local taxes and spending. *Journal of Public Economics*, 92(12), 2320-2335.

Dhar, P., & Ross, S. L. (2012). School district quality and property values: Examining differences along school district boundaries. *Journal of Urban Economics*, 71(1), 18-25.

DiNardo, J., & Lee, D. S. (2004). Economic impacts of new unionization on private sector employers: 1984–2001. *Quarterly Journal of Economics*, 119(4), 1383-1441.

Dougan, W. R., & Kenyon, D. A. (1988). Pressure groups and public expenditures: The flypaper effect reconsidered. *Economic Inquiry*, 26(1), 159-170.

Downes, T. A. (1992). Evaluating the impact of school finance reform on the provision of public education: The California case. *National Tax Journal*, 405-419.

Downes, T., & Figlio, D. (1998). *School Finance Reforms, Tax Limits, and Student Performance: Do Reforms Level-Up or Dumb Down?* (No. 9805). Department of Economics, Tufts University.

Dube, A., Lester, T. W., & Reich, M. (2010). Minimum wage effects across state borders: Estimates using contiguous counties. *The Review of Economics and Statistics*, 92(4), 945-964.

Dye, R. F. and McGuire, T. J. (1997). The effect of property tax limitation measures on local government fiscal behavior. *Journal of Public Economics*, 66, 487-496.

Eberts, R. W., & Stone, J. A. (1986). Teacher unions and the cost of public education. *Economic Inquiry*, 24(4), 631-643.

Eberts, R. W., & Stone, J. A. (1987). Teacher unions and the productivity of public schools. *Industrial & Labor Relations Review*, 40(3), 354-363.

Epple, D., & Ferreyra, M. M. (2008). School finance reform: Assessing general equilibrium effects. *Journal of Public Economics*, 92(5), 1326-1351.

Evans, W. N., Schwab, R. S., & Wagner, K. (2016). The Great Recession and public education. Working paper.

Feiveson, L. (2015). General revenue sharing and public sector unions. *Journal of Public Economics*, 125, 28-45.

Filimon, R., Romer, T., & Rosenthal, H. (1982). Asymmetric information and agenda control: The bases of monopoly power in public spending. *Journal of Public Economics*, 17(1), 51-70.

Frandsen, B. R. (2016). The Effects of Collective Bargaining Rights on Public Employee Compensation Evidence from Teachers, Firefighters, and Police. *ILR Review*, 69(1), 84-112.

Gordon, N. (2004). Do federal grants boost school spending? Evidence from Title I. *Journal of Public Economics*, 88(9), 1771-1792.

Hines, J. R., & Thaler, R. H. (1995). Anomalies: The flypaper effect. *The Journal of Economic Perspectives*, 9(4), 217-226.

Hirano, K., Imbens, G. W., & Ridder, G. (2003). Efficient estimation of average treatment effects using the estimated propensity score. *Econometrica*, 71(4), 1161-1189.

Holmes, T. J. (1998). The effect of state policies on the location of manufacturing: Evidence from state borders. *Journal of Political Economy*, 106(4), 667-705.

Hoxby, C. M. (1996). How teachers' unions affect education production. *The Quarterly Journal of Economics*, 671-718.

Hoxby, C. M. (2001). "All school finance equalizations are not created equal. *The Quarterly Journal of Economics*, 116(4), 1189–1231.

Hoyt, W. H. (1999). Leviathan, local government expenditures, and capitalization. *Regional Science and Urban Economics*, 29(2), 155-171.

Hyman, J. (2017). Does money matter in the long run? Effects of school spending on educational attainment. *American Economic Journal: Economic Policy*, forthcoming.

Imbens, G. W., & Wooldridge, J. M. (2009). Recent developments in the econometrics of program evaluation. *Journal of Economic Literature*, 47(1), 5-86.

Inman, R. P. (2008). The flypaper effect (No. w14579). National Bureau of Economic Research.

Jackson, C. K., Johnson, R. C., & Persico, C. (2016). The Effects of School Spending on Educational and Economic Outcomes: Evidence from School Finance Reforms. *The Quarterly Journal of Economics*, 131(1), 157-218.

Knight, B. (2001). Endogenous federal grants and crowd-out of state government spending: theory and evidence from the Federal Highway Aid Program. *American Economic Review* 92 (1), 71– 92.

Lafortune, J., Rothstein, J., & Schanzenbach, D. W. (2016). School finance reform and the distribution of student achievement (No. w22011). National Bureau of Economic Research.

Lovenheim, M. F. (2009). The effect of teachers' unions on education production: Evidence from union election certifications in three midwestern states. *Journal of Labor Economics*, 27(4), 525-587.

Lutz, B. (2010). Taxation with representation: Intergovernmental grants in a plebiscite democracy. *The Review of Economics and Statistics*, 92(2), 316-332.

Murray, S. E., Evans, W. N., & Schwab, R. M. (1998). Education finance reform and the distribution of education resources. *American Economic Review*, 88(4), 789–812.

Moe, T. M. (2005). Teacher unions and school board elections. In W. G. Howell (Ed.), Besieged, school boards and the future of education politics (pp. 254–287). Washington DC: The Brookings Institution.

Moe, T. M. (2006). Political Control and the Power of the Agent. *Journal of Law, Economics, and Organization*, 22(1), 1-29.

Papke, L. E. (2005). The effects of spending on test pass rates: evidence from Michigan. *Journal of Public Economics*, 89(5), 821-839.

Rose, H., & Sonstelie, J. (2010). School board politics, school district size, and the bargaining power of teachers' unions. *Journal of Urban Economics*, 67(3), 438-450.

Roy, J. (2011). Impact of school finance reform on resource equalization and academic performance: Evidence from Michigan. *Education Finance and Policy*, 6(2), 137-167.

Sonstelie, J., Brunner, E., & Ardon, K. (2000). For better or for worse?: School Finance Reform in California. San Francisco: Public Policy Institute of California.

Sieg, H., & Wang, Y. (2013). The impact of unions on municipal elections and urban fiscal policies. *Journal of Monetary Economics*, 60(5), 554-567.

Singhal, M. (2008). Special interest groups and the allocation of public funds. *Journal of Public Economics*, 92(3), 548-564.

Strumpf, K. S. (1998). A predictive index for the flypaper effect. *Journal of Public Econom*ics, 69(3), 389-412.

Winkler, A. M, Scull, J, & Zeehandelaar, D. (2012). How strong are teacher unions? A state-by-state Comparison. *Thomas B. Fordham Institute Research Report*, Washington D.C.

Winters, J. V. (2008). Property Tax Limitations. Fiscal Research Center, FRC Report 179, Georgia State University.

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	10,909	3,852	11,725	4,124	9,213	2,465
Local Revenue	5,268	3,828	5,975	4,175	3,795	2,382
Total Expenditures	11,007	4,165	11,856	4,473	9,241	2,679
Other Outcomes						
Pupil-Teacher Ratio	16.3	3.1	16.6	3.3	15.8	2.6
Base Instructional Salary	37,433	5,402	39,383	5,801	35,196	3,832
Control Variables						
Baseline District Enrollment	3,731	15,057	3,372	16,724	4,477	10,753
District Median Income in 1980	17,212	5,344	18,494	5,528	14,545	3,722
District Fraction Urban in 1980	0.549	0.299	0.607	0.290	0.430	0.282
District Fraction Black in 1980	0.066	0.110	0.048	0.074	0.102	0.154
District Fraction Poor in 1980	0.311	0.142	0.275	0.128	0.384	0.141
Number of States	42		21		21	
Number of Districts	9,207		6,139		3,068	
Number of Observations	183,155		123,701		59,454	

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

	State Aid		State Aid * Union	
	(1)	(2)	(3)	(4)
SFR * Q1	1130***	1129***	173***	139***
	(65)	(64)	(49)	(45)
SFR * Q2	607***	629***	-106***	-119***
	(53)	(53)	(37)	(36)
SFR * Q3	609***	607***	-105**	-98**
	(72)	(71)	(45)	(43)
SFR * Union * Q1	208***	79	1363***	964***
	(58)	(55)	(57)	(53)
SFR * Union * Q2	-196***	-237***	624***	563***
	(42)	(41)	(38)	(35)
SFR * Union * Q3	-180***	-179***	282***	478***
	(54)	(53)	(47)	(42)
F-Statistic	132	135	129	97
Observations	183,155	183,155	183,155	183,155
Expanded Controls	No	Yes	No	Yes

Notes: The sample is at the school district-year level. 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 the state union power measure 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 poor all 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 the district-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 (Linear)									
State Aid	0.615***	0.636***	-0.351***	-0.330***	0.598***	0.603***	-0.626***	-0.623***	0.325***
	(0.035)	(0.037)	(0.034)	(0.036)	(0.041)	(0.042)	(0.039)	(0.039)	(0.114)
State Aid * Union	0.364***	0.449***	0.316***	0.397***	0.293***	0.367***	0.224***	0.299***	0.408***
	(0.032)	(0.043)	(0.030)	(0.040)	(0.036)	(0.049)	(0.036)	(0.047)	(0.076)
Panel B. Union Power Index (Quadratic)									
State Aid	0.534***	0.559***	-0.431***	-0.410***	0.523***	0.538***	-0.674***	-0.674***	0.292***
	(0.033)	(0.035)	(0.032)	(0.035)	(0.038)	(0.040)	(0.037)	(0.038)	(0.111)
State Aid * Union	0.343***	0.389***	0.294***	0.339***	0.273***	0.311***	0.242***	0.304***	0.383***
	(0.034)	(0.041)	(0.032)	(0.039)	(0.039)	(0.048)	(0.036)	(0.044)	(0.075)
State Aid * Union Squared	0.153***	0.126***	0.148***	0.126***	0.143***	0.115***	0.131***	0.102***	-0.007
	(0.024)	(0.024)	(0.023)	(0.023)	(0.028)	(0.026)	(0.023)	(0.024)	(0.059)
Estimated Effect at:									
25th Pctle. of Union Index	0.397***	0.391***	-0.544***	-0.552***	0.420***	0.407***	-0.765***	-0.804***	0.091
	(0.034)	(0.035)	(0.033)	(0.034)	(0.040)	(0.040)	(0.044)	(0.045)	(0.132)
75th Pctle. of Union Index	0.872***	0.917***	-0.132***	-0.089***	0.804***	0.831***	-0.422***	-0.392***	0.572***
	(0.050)	(0.057)	(0.049)	(0.055)	(0.059)	(0.066)	(0.046)	(0.051)	(0.120)
Panel C. Mandatory CB Plus Right-to-Work Status (0, 1, 2)									
State Aid	0.147*	0.213**	-0.688***	-0.544***	0.227***	0.330***	-1.154***	-1.441***	-0.113
	(0.075)	(0.101)	(0.065)	(0.092)	(0.080)	(0.109)	(0.107)	(0.151)	(0.205)
State Aid * Union	0.297***	0.259***	0.219***	0.140***	0.237***	0.175***	0.324***	0.471***	0.314***
	(0.039)	(0.057)	(0.034)	(0.053)	(0.042)	(0.063)	(0.054)	(0.079)	(0.094)
Observations	183,155		183,155		183,155		182,913		16,902
Expanded Controls	No	Yes	No	Yes	No	Yes	No	Yes	No

Notes: 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"), and the instruments are an indicator for school finance reform 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, and are estimated for the sample, listed in the notes for Table 2. Robust standard errors, clustered at the district-level, in parentheses.

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

Table 4: Effect 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	0.598***	-0.367***	0.582***	-0.712***	0.429***
	(0.041)	(0.040)	(0.045)	(0.042)	(0.131)
State Aid * Union	0.358***	0.308***	0.265***	0.262***	0.360***
	(0.040)	(0.038)	(0.046)	(0.045)	(0.076)
Panel B. 1990 Median Income					
State Aid	0.658***	-0.307***	0.586***	-0.672***	0.172
	(0.039)	(0.038)	(0.041)	(0.042)	(0.124)
State Aid * Union	0.505***	0.455***	0.379***	0.213***	0.309***
	(0.044)	(0.042)	(0.049)	(0.046)	(0.071)
Panel C. Include Both					
State Aid	0.618***	-0.346***	0.571***	-0.743***	0.270*
	(0.042)	(0.041)	(0.044)	(0.045)	(0.139)
State Aid * Union	0.415***	0.367***	0.281***	0.203***	0.267***
	(0.041)	(0.039)	(0.047)	(0.045)	(0.071)
Observations	183,155	183,155	183,155	183,155	16,902
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 C from Table 3. All specifications include the controls and fixed effects, and are estimated for the sample, listed in the notes for Table 2. Panel A controls for state aid interacted with the 1988 state share voting for the Democratic presidential candidate, instrumented for by the school finance reform (SFR) and income tercile dummies interacted with the vote share. Panel B replaces the 1988 vote share with 1990 state median income. Panel C includes both variables interacted with state aid, and instruments for each separately. Robust standard errors, clustered at the district-level, in parentheses.

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

Table 5: 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	3.23**	0.011	-1.26	0.709	1.24	0.635
Dem Vote Share 1988	3.319***	0.001	-0.69	0.855	2.57	0.356
Dem Vote Share 1992	4.02***	0.000	0.59	0.811	1.89	0.322
1990 District-level Characteristics						
Total Population	-2,008	0.613	-3,853	0.489	-1,041	0.822
Population Density	91.95**	0.024	47.42*	0.065	64.35*	0.053
Number of Households	-870	0.557	-1,543	0.470	-512	0.773
Median HH Income	4,593***	0.000	1222	0.386	856	0.472
Fraction Non-White	-0.040*	0.074	0.015*	0.095	0.006	0.407
Fraction Below Poverty	-0.029***	0.007	0.001	0.852	0.004	0.435
Fraction Unemployed	0.009	0.512	0.006	0.289	0.010	0.245
Fraction Population 65 Plus	-0.001	0.671	0.002	0.529	0.006*	0.088
Fraction Less Than HS	-0.041***	0.000	-0.004	0.599	-0.005	0.503
Fraction HS Degree	0.004	0.614	-0.010	0.360	-0.006	0.519
Fraction Some College	0.013*	0.058	0.004	0.586	0.002	0.801
Fraction BA or Higher	0.024***	0.000	0.010	0.343	0.010	0.334
Fraction Homeowner	-0.004	0.643	0.001	0.916	-0.003	0.713
Number of Districts	9,207		4,399		3,198	

Notes: Each point estimate is from a separate district-level regression of the listed county or district characteristic on our continuous state teacher union power measure. Columns 1 and 2 are the full sample of districts used in tables 1-3. 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 border fixed effects. 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: Border Analysis Robustness

	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	0.689***	-0.304***	0.655***	-0.664***	0.388**
	(0.057)	(0.056)	(0.067)	(0.054)	(0.179)
State Aid * Union	0.468***	0.484***	0.474***	0.185***	0.287
	(0.059)	(0.058)	(0.068)	(0.059)	(0.179)
Panel B. Counties Adjacent to State Border					
State Aid	0.600***	-0.404***	0.541***	-0.631***	0.347*
	(0.062)	(0.060)	(0.072)	(0.057)	(0.184)
State Aid * Union	0.431***	0.443***	0.374***	0.140*	0.229
	(0.067)	(0.066)	(0.078)	(0.075)	(0.176)
Observations - Panel A	103,539	103,539	103,539	103,348	9,860
Observations - Panel B	62,969	62,969	62,969	62,786	6,145
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 C 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 adjancent to a state border. All specifications include the controls and fixed effects (FEs) listed in the notes for Table 2, except that the region-by-year FEs are replaced with border-by-year FEs, where border fixed efects include counties on either side of a state border. Robust standard errors, clustered at the district-level, in parentheses.

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

Table 7: School Finance Reform Coding 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	0.613***	-0.364***	0.581***	-0.568***	0.309***
	(0.032)	(0.031)	(0.039)	(0.034)	(0.106)
State Aid * Union	0.396***	0.359***	0.340***	0.356***	0.231***
	(0.039)	(0.037)	(0.046)	(0.044)	(0.062)
Panel B. LRS (2017) Coding					
State Aid	0.820***	-0.128***	0.892***	-1.114***	0.431**
	(0.049)	(0.046)	(0.056)	(0.063)	(0.183)
State Aid * Union	0.422***	0.329***	0.299***	0.747***	0.455***
	(0.054)	(0.050)	(0.063)	(0.076)	(0.086)
Panel C. Include KS, KY, MO, TX					
State Aid	0.756***	-0.217***	0.672***	-0.646***	0.544***
	(0.033)	(0.033)	(0.038)	(0.034)	(0.101)
State Aid * Union	0.179***	0.152***	0.246***	0.249***	0.146**
	(0.035)	(0.034)	(0.042)	(0.037)	(0.063)
Observations - Panel A	302,738	302,738	302,738	302,125	25,183
Observations - Panel B	183,155	183,155	183,155	182,913	16,902
Observations - Panel C	216,949	216,949	216,949	216,707	19,423
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 notes for Table 2. Panel A uses a stacked difference-in-differences specification, which uses all SFRs isntead of choosing one from each state (see text for details). Panel B changes the coding of school finance reforms to match Lafortune, Rothstein, and Schanzebach (2017). Panel C changes the sample to include Kansas, Kentucky, Missouri, and Texas. Robust standard errors, clustered at the district-level, in parentheses.

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

Table 8: Effects by Expenditure Type

	Total Expenditures				
		Current Expenditures			Capital Outlays
			Instruction	Non-Instruction	
	(1)	(2)	(3)	(4)	(5)
State Aid	0.603***	0.491***	0.378***	0.239***	0.112***
	(0.042)	(0.030)	(0.021)	(0.017)	(0.032)
State Aid * Union	0.367***	0.276***	0.129***	0.169***	0.135***
	(0.049)	(0.034)	(0.023)	(0.019)	(0.037)
Sample Mean	11,007	9,362	5,758	3,468	1,022
Observations	183,155	183,155	183,155	183,031	182,203
Expanded Controls	Yes	Yes	Yes	Yes	Yes

Notes: 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 C from Table 3. All specifications include the controls and fixed effects, and are estimated for the sample, listed in the notes for Table 2. Robust standard errors, clustered at the district-level, in parentheses.
* significant at 10%, ** significant at 5%, *** significant at 1%.

Figure Ia
School District Responses to Intergovernmental Aid

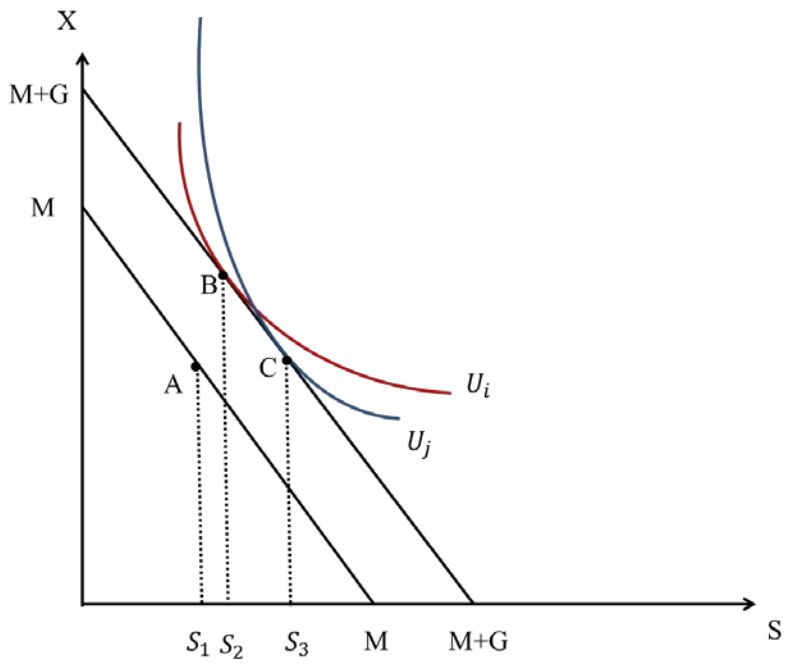


Figure Ib
School District Allocation Decisions

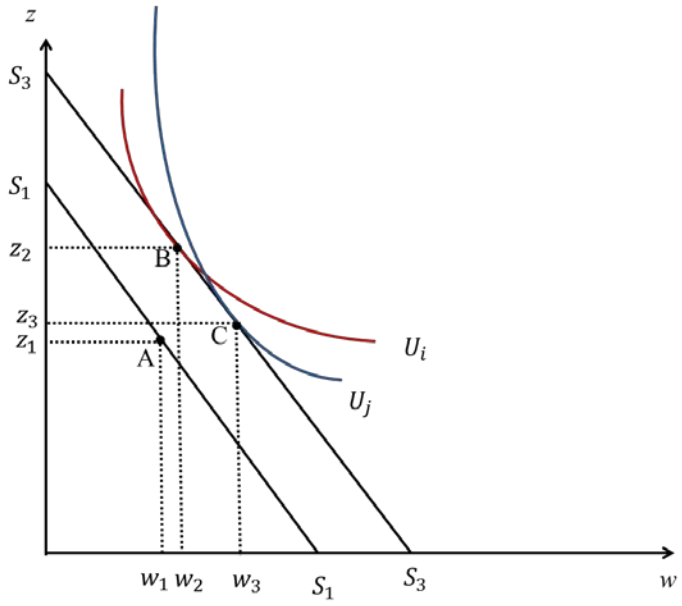
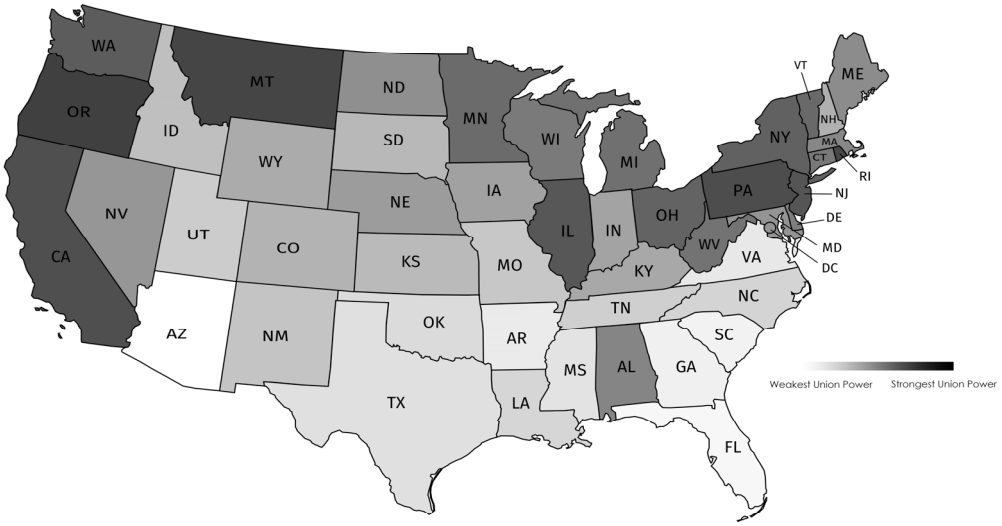
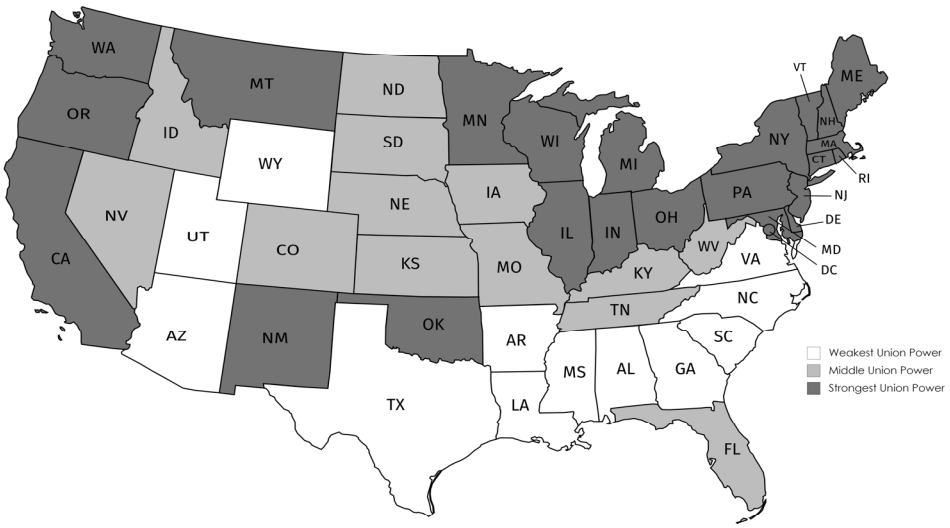


Figure II: United States Map, by State Teacher Union Power

(a) Continuous Teacher Union Power Index



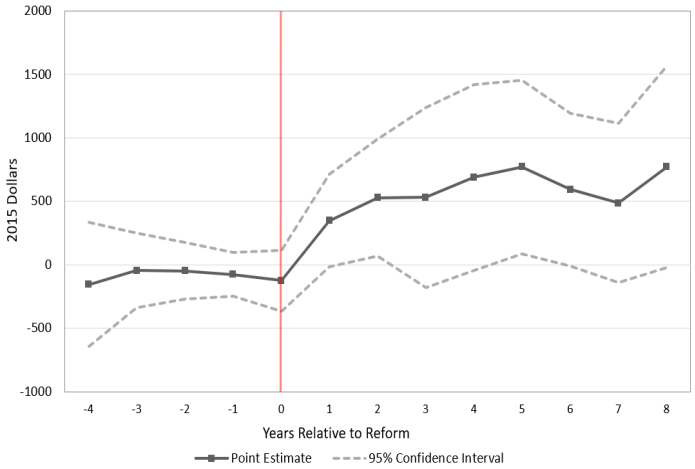
(b) Mandatory Collective Bargaining and Right-to-Work Status



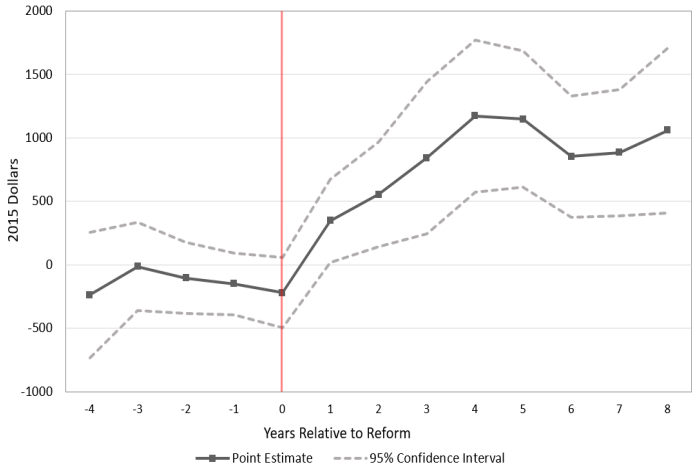
Notes: Map shows states by their values for the two teacher union power measures used in this paper. Figure (a) shows states by the continuous teacher union power index provided by the Fordham Institute (2012), and figure (b) by their public sector collective bargaining law status and right-to-work status.

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

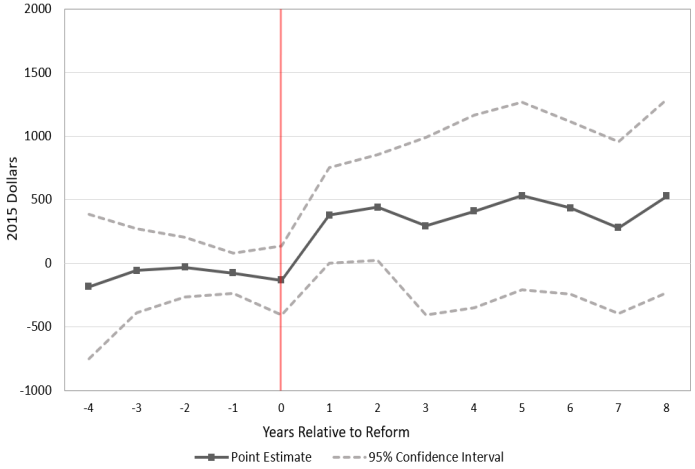
(a) All Districts



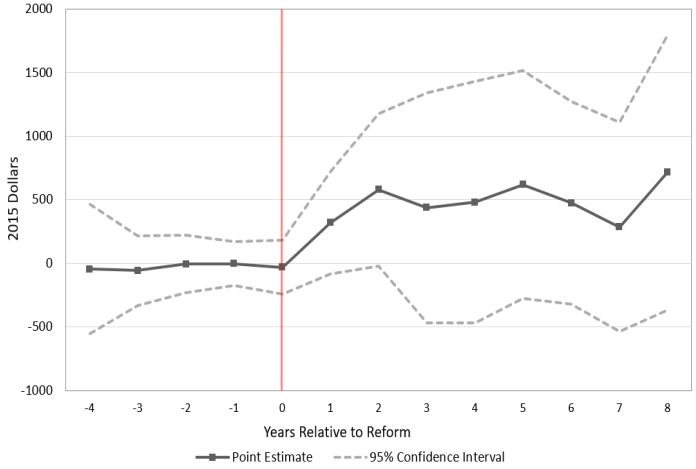
(b) Bottom Income Tercile



(c) Middle Income Tercile



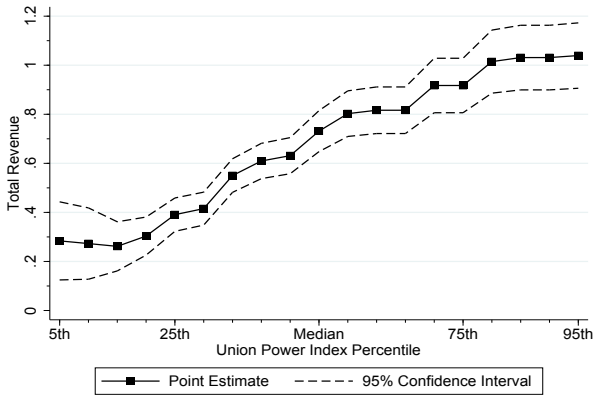
(d) Top Income Tercile



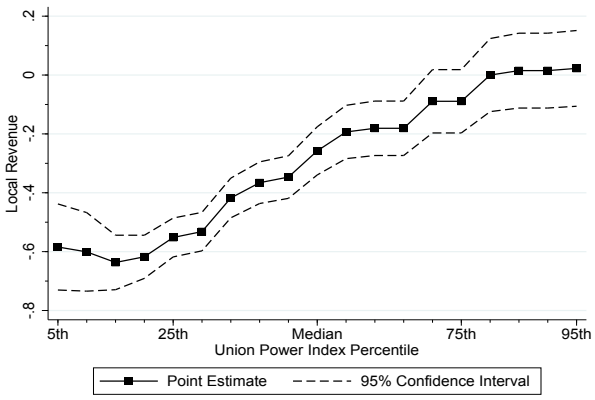
Notes: Figures show event study style estimates of the effects of school finance reforms on state revenue, by 1980 district income tercile. Solid lines are point estimates, and dashed lines are 95% confidence intervals.

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

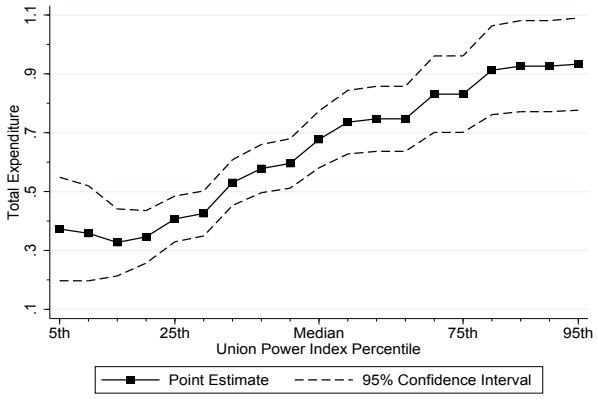
(a) Total Revenue



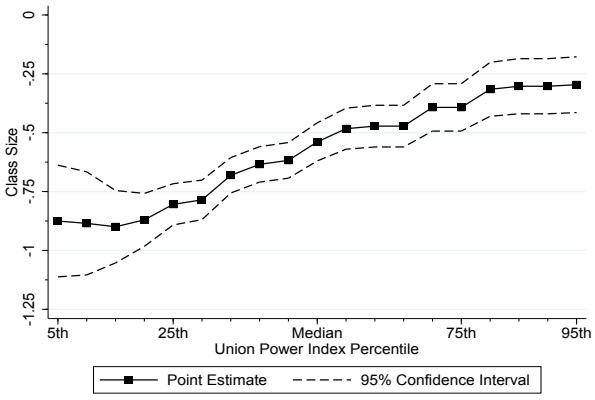
(b) Local Revenue



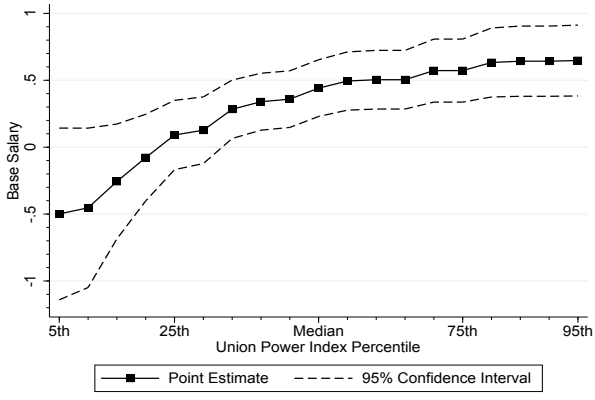
(c) Total Expenditures



(d) Class Size



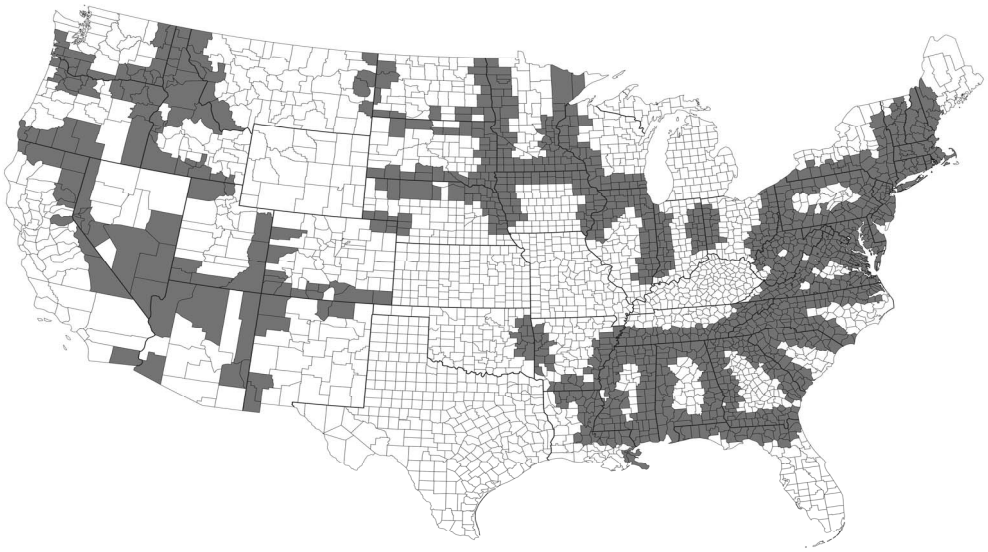
(e) Wages



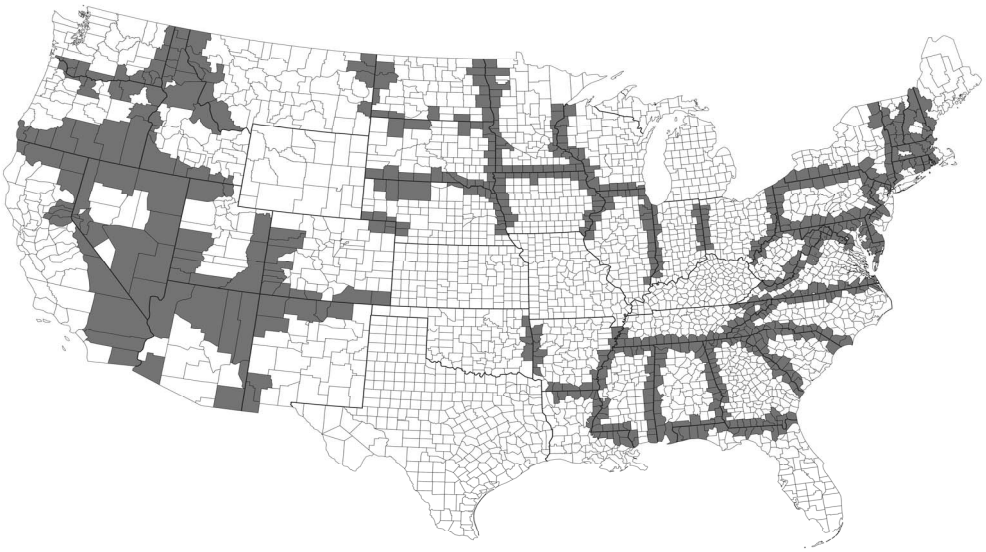
Notes: Each figure shows point estimates (solid line) and 95% confidence intervals (dashed lines) from 2SLS regressions of the dependent variable on state aid per pupil and aid interacted with a quadratic in a standardized index of state teacher union power. The figures show the calculated point estimate 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 25 cents. For states with the strongest teacher unions, it increased approximately 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).

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
Wyoming	**1995**	**Court**	**Campbell County School District v. State**
Wyoming	1997	Legislative	The Wyoming Comprehensive Assessment System; The Education Resource Block Grant Model

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 analysis.

Appendix Table 2: List of State Teacher Union Power Measures

	Mandatory Collective Bargaining	Right-to-Work	Combined CB and RtW Index	Fordham Index	
				Index	Rank
	(1)	(2)	(3)	(4)	(5)
Alabama	No	No	0	2.25	20
Arizona	No	No	0	0.72	51
Arkansas	No	No	0	1.02	47
California	Yes	Yes	2	2.84	6
Colorado	No	Yes	1	1.78	33
Connecticut	Yes	Yes	2	2.37	17
Delaware	Yes	Yes	2	2.30	19
Florida	Yes	No	1	0.99	50
Georgia	No	No	0	1.01	48
Idaho	Yes	No	1	1.66	36
Illinois	Yes	Yes	2	2.72	8
Indiana	Yes	Yes	2	1.93	29
Iowa	Yes	No	1	1.99	28
Kansas	Yes	No	1	1.69	35
Kentucky	No	Yes	1	1.91	30
Louisiana	No	No	0	1.29	42
Maine	Yes	Yes	2	2.20	22
Maryland	Yes	Yes	2	2.13	24
Massachusetts	Yes	Yes	2	2.24	21
Michigan	Yes	Yes	2	2.45	15
Minnesota	Yes	Yes	2	2.50	13
Mississippi	No	No	0	1.08	45
Missouri	No	Yes	1	1.52	38
Montana	Yes	Yes	2	3.06	3
Nebraska	Yes	No	1	2.01	27
Nevada	Yes	No	1	2.05	26
New Hampshire	Yes	Yes	2	1.86	32
New Jersey	Yes	Yes	2	2.82	7
New Mexico	Yes	Yes	2	1.54	37
New York	Yes	Yes	2	2.61	10
North Carolina	No	No	0	1.38	41
North Dakota	Yes	No	1	2.17	23
Ohio	Yes	Yes	2	2.59	11
Oklahoma	Yes	Yes	2	1.26	43
Oregon	Yes	Yes	2	3.18	2
Pennsylvania	Yes	Yes	2	2.85	5
Rhode Island	Yes	Yes	2	2.86	4
South Carolina	No	No	0	1.00	49
South Dakota	Yes	No	1	1.75	34
Tennessee	Yes	No	1	1.44	40
Texas	No	No	0	1.11	44
Utah	No	No	0	1.48	39
Vermont	Yes	Yes	2	2.55	12
Virginia	No	No	0	1.06	46
Washington	Yes	Yes	2	2.72	9
West Virginia	No	Yes	1	2.44	16
Wisconsin	Yes	Yes	2	2.33	18
Wyoming	No	No	0	1.91	31

Notes: This table lists values by state for each of the teacher union power measures we use in the paper. The list includes all states in the continental U.S., excluding D.C. The teacher union power index in columns 4 and 5 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 for Alternative Union Power Measure

	State Aid	Aid *Union
	(1)	(2)
SFR * Q1	486***	14
	(57)	(62)
SFR * Q2	526***	171***
	(41)	(52)
SFR * Q3	469***	80
	(51)	(71)
SFR * Union * Q1	432***	1351***
	(56)	(100)
SFR * Union * Q2	77*	608***
	(44)	(82)
SFR * Union * Q3	99	637***
	(66)	(127)
Expanded Controls	Yes	Yes
Observations	183,155	183,155
F-Statistic	140	91

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 and 3 and state aid per pupil interacted with the mandatory collective bargaining and right-to-work status union power index in columns 2 and 4. All specifications include the complete set of controls and fixed effects described in Table 2. Robust standard errors, clustered at the district-level, in parentheses. * significant at 10%, ** significant at 5%, *** significant at 1%.

Appendix Table 4: OLS Estimates

	Total Revenue	Local Revenue	Total Expenditures	Pupil-Teacher Ratio	Base Salary
	(1)	(2)	(3)	(4)	(5)
Panel A. Union Power Index (Linear)					
State Aid	0.819***	-0.212***	0.716***	-0.213***	0.117***
	(0.023)	(0.021)	(0.036)	(0.023)	(0.025)
State Aid * Union	-0.039***	-0.030**	-0.038*	0.053***	0.073***
	(0.014)	(0.013)	(0.020)	(0.012)	(0.023)
Panel B. Mandatory CB Plus Right-to-Work Status (0, 1, 2)					
State Aid	0.745***	-0.270***	0.646***	-0.137***	0.294***
	(0.013)	(0.013)	(0.015)	(0.006)	(0.061)
State Aid * Union	0.012	0.015	0.011	0.077***	-0.094***
	(0.011)	(0.010)	(0.014)	(0.008)	(0.033)
Observations	183,155	183,155	183,155	182,913	16,902
Expanded Controls	Yes	Yes	Yes	Yes	No

Notes: All results are from OLS models where the independent variables of interest are state aid and its interaction with state teacher union power ("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, and are estimated for the sample, listed in the notes for Table 2. Robust standard errors, clustered at the district-level, in parentheses.

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

Appendix Table 5: Main Estimates Including 2009-2012

	Total Revenue	Local Revenue	Total Expenditures	Pupil-Teacher Ratio	Base Salary
	(1)	(2)	(3)	(4)	(5)
Panel A. Union Power Index (Linear)					
State Aid	0.725***	-0.239***	0.773***	-0.625***	0.390***
	(0.032)	(0.031)	(0.038)	(0.032)	(0.106)
State Aid * Union	0.396***	0.377***	0.399***	0.226***	0.351***
	(0.036)	(0.034)	(0.042)	(0.038)	(0.073)
Panel B. Mandatory CB Plus Right-to-Work Status (0, 1, 2)					
State Aid	0.237***	-0.612***	0.261***	-1.410***	0.021
	(0.085)	(0.075)	(0.091)	(0.116)	(0.187)
State Aid * Union	0.304***	0.237***	0.317***	0.467***	0.260***
	(0.048)	(0.044)	(0.053)	(0.061)	(0.088)
Observations	216,586	216,586	216,586	216,344	20,117
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 C from Table 3. All specifications include the controls and fixed effects, and are estimated for the sample, listed in the notes for Table 2, except that the sample adds years 2009-2012. Robust standard errors, clustered at the district-level, in parentheses.

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