SCHOOL FINANCE REFORMS, TEACHERS' UNIONS, AND THE ALLOCATION OF SCHOOL RESOURCES

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

I.   Introduction

THE school finance reforms that occurred across the United States beginning in the early 1970s caused some of the largest transfers from states to local governments in U.S. history. Recent work has linked these reforms to sustained improvements in student achievement and long-run increases in educational attainment, earnings, and intergenerational mobility (Jackson, Johnson, & Persico, 2016; Hyman, 2017; Lafortune, Rothstein, & Schanzenbach, 2018; Candelaria & Shores, 2019; Biasi, 2017). However, some of the earliest and most fundamental questions regarding school finance reforms were not about their effects on student outcomes. Rather, early studies focused on the effect of school finance reforms on the distribution of school spending across districts and whether local school districts responded to increases in state aid by reducing local taxing effort (Murray, Evans, & Schwab, 1998; Hoxby, 2001; Card & Payne, 2002). These studies found a substantial incidence of "flypaper," with most of the increases in state aid translating into increased education spending.

The finding that state aid from school finance reforms tended to "stick where it hit" contributes to a larger literature on the flypaper effect, in which some studies find very little or no evidence of local effort crowd-out of intergovernmental aid (Dahlberg et al., 2008; Feiveson, 2015), while others find substantial or near total crowd-out (Knight, 2002; Gordon, 2004; Lutz, 2010; Cascio, Gordon, & Reber, 2013).

One leading explanation for the flypaper effect is about local politics, and specifically that special interest groups influence the allocation of resources by lobbying for intergovernmental grants to be spent on the preferred good (Inman, 2008; Singhal, 2008). In education, teachers' unions are the most prominent special interest group, and an extensive literature examines their impact on the size of school district budgets, district resource allocations, and student outcomes (Hoxby, 1996; Lovenheim, 2009; Frandsen, 2016; Lovenheim & Willen, 2018). However, despite the long-standing interest in how teachers' unions and school finance reforms have affected school spending and student achievement, the question of whether and how teachers' unions influenced local responses to school finance reforms remains unexplored.

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

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

We find that unions played a critical role in determining both the amount of state aid that translated into education expenditures and the allocation of these funds. Consistent with a basic model of teachers' union preferences, school districts in states with the strongest teachers' unions increased education expenditures nearly one-for-one with increases in state aid in response to school finance reforms, whereas states with

[1]We also use more traditional measures of state teachers' union power that rely solely on state public sector collective bargaining laws and right-to-work status.

the weakest unions reduced local tax effort by approximately eighty cents on the dollar. Districts in strong teachers' union states allocated more of the additional spending toward increasing teacher salaries, while districts in weak union states spent the money primarily on teacher hiring. Spending in noninstructional areas such as capital outlays, administration, and classroom support also increased more in strong teachers' union states than in states with weak teachers' unions. Finally, we find that the larger expenditure increases in strong teachers' union states translated into larger impacts on student achievement: ten years after a reform, students in low-income districts in weak teachers' union states scored 0.08 standard deviations (SDs) higher than those in strong teachers' union states scored 0.16 SD higher.

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

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

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

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

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

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

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

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[2] As detailed in the online appendix, we classify all the school finance reforms in our sample into six types. We show that the correlations between teachers' union power and reform type are quite low, and whether they are positive or negative does not consistently support the hypothesis that unions would favor reform types that discourage local crowd-out.

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

III. Data

Our primary data source is the Local Education Agency (School District) Finance Survey (F-33) maintained by the NCES. The F-33 surveys contain detailed annual revenue and expenditure data for all school districts in the United States for the period 1990–91 to 2011–12. We augment these data with earlier versions of the F-33 survey provided by the U.S. Census for the years 1986–87 to 1989–90. For this period, 1986–2011,[3] we also utilize the annual NCES Common Core of Data (CCD) school district universe surveys that provide student enrollments and staff counts for every school district.

We restrict our sample in several ways. First, note that we aim to examine whether teachers' unions affect the degree to which intergovernmental aid "sticks where it hits"—the flypaper effect. As discussed in Inman (2008), one of the explanations for why prior studies have found strong evidence of a flypaper effect is that researchers may have misclassified matching grants as lump-sum grants. Furthermore, we acknowledge that SFRs vary in their design and intended impacts (Hoxby, 2001). Thus, to avoid misclassifying matching grants as lump-sum aid and to focus as much as possible on similarly designed SFRs, we omit Kansas, Kentucky, Missouri, and Texas since these states implemented "reward for local effort" (matching grant) formulas as part of their SFRs. We also omit Michigan and Wyoming because these states adopted SFRs that eliminated local discretion over funding. Second, because the NCES F-33 financial data tend to be noisy, particularly for small districts, we follow Gordon (2004) and Lafortune et al. (2018) and exclude small districts (with enrollment below 250 students) from the analysis.[4] Finally, in our preferred specifications, we omit the final

three years (2009–2011) of our sample due to the severe and potentially confounding influence of the Great Recession on school finances during that time (Evans, Schwab, & Wagner, 2019). We show in appendix table 7 that our results are robust to this sample restriction.

We combine the school district financial data with data on median household income, fraction black, fraction urban, and fraction of adults 25 and older with a bachelor's degree from the Special School District Tabulations of the 1980 Census. We obtained a comprehensive list of SFRs from Jackson et al. (2016) and Lafortune et al. (2018). Our primary coding of these SFRs is based on the coding structure developed by Lafortune et al. (2018), though we differ from their coding in a few cases. We show in appendix table 7 that our results are robust to using a stacked difference-in-differences strategy that uses all SFRs for states with multiple reforms (including the reforms where we differ from Lafortune et al., 2018), and to using only court-ordered reforms, as in Jackson et al. (2016).[5]

Finally, our primary teachers' union power measure is based on an index created by researchers at the Fordham Institute (Winkler, Scull, & Zeehandelaar, 2012). The index combines administrative and original survey data across five areas related to teachers' union power: (a) resources and membership, (b) involvement in politics, (c) scope of bargaining, (d) state policies, and (e) perceived influence. Many of the index components are measured as of 2012, after the SFRs in our sample, raising concerns that some components may be endogenous to the reforms. After carefully reviewing all of the index components, the only ones we believe would have been directly influenced by SFRs are the measures related to school spending included in the "resources and membership" category. We therefore drop these variables from the index and recalculate it without them.[6]

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

To address possible concerns about endogeneity or subjectivity of the continuous teachers' union power measure, we supplement our analysis with measures of state

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[3]Here and subsequently, we refer to a school year by its fall year (e.g., 2011 refers to 2011–12).

[4]See the online appendix for a more detailed discussion of our data and sample restrictions.

[5]See appendix table 1 for a listing of the school finance reforms used in our main analysis.

[6]See appendix figure 2, from Winkler et al. (2012), for a concise overview of the index components and their relative weightings.

(a) Continuous Teacher Union Power Index
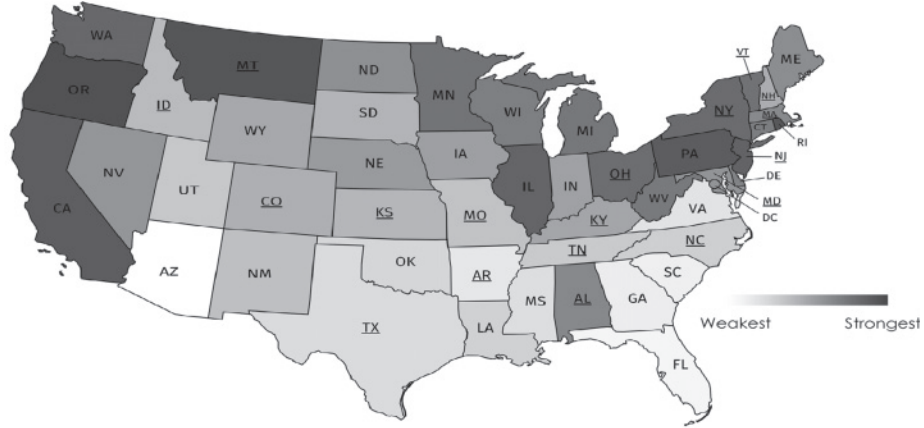
(b) Mandatory Collective Bargaining (CB) Status
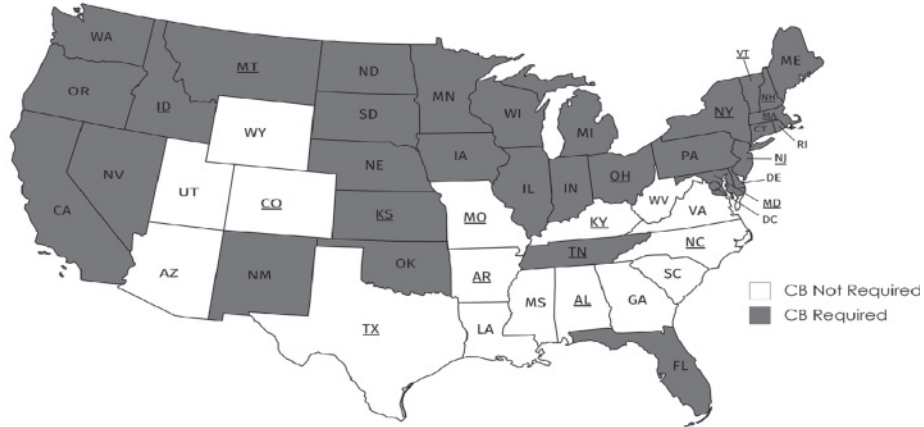
(c) CB and Right-to-Work Index
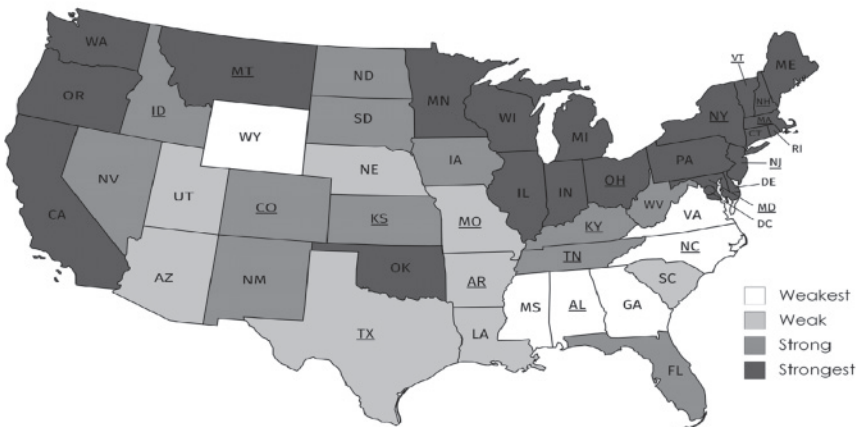
Map shows states by their values for the three teacher union power measures used in this paper. (a) States by the continuous teacher union power index provided by Winkler, Scull, and Zeehandelaar (2012). (b) Public sector collective bargaining (CB) law status. (c) The four-value index incorporating CB law and right-to-work status. States that experienced a school finance reform have an underlined state abbreviation.

TABLE 1.—SUMMARY STATISTICS

	Full Sample		Strong Union States		Weak Union States	
	Mean (1)	SD (2)	Mean (3)	SD (4)	Mean (5)	SD (6)
Per pupil outcomes						
Total revenue	10,890	3,814	11,704	4,083	9,200	2,431
Local revenue	5,217	3,760	5,919	4,108	3,762	2,305
Current expenditures	9,347	3,091	10,051	3,323	7,887	1,817
Other outcomes						
Pupil-teacher ratio	16.3	3.1	16.6	3.4	15.9	2.6
Base instructional salary	37,305	5,329	39,289	5,809	35,038	3,555
Control variables						
Baseline enrollment	3,751	15,112	3,393	16,795	4,495	10,781
Median income in 1980	17,204	5,327	18,495	5,506	14,527	3,708
Fraction urban in 1980	0.550	0.299	0.608	0.289	0.430	0.282
Fraction black in 1980	0.066	0.110	0.048	0.074	0.102	0.154
Fraction BA or higher in 1980	0.137	0.090	0.149	0.097	0.113	0.064
Number of states	42		21		21	
Number of districts	9,177		6,111		3,066	
Number of observations	181,756		122,635		59,121	

The sample is all school districts in the continental United States, 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.

teachers' union power that use state laws implemented prior to our sample period. Specifically, our first alternative measure is an indicator for whether a state mandates collective bargaining (CB), as defined in the NBER Public Sector CB Law Data Set, developed by Valletta and Freeman (1988). As our second alternative measure, we augment the information on state CB laws with information on state right-to-work (RTW) status, obtained from the National Conference of State Legislatures. In this more flexible alternative union power index, states first receive a value of 0 if CB is prohibited, a value of 1 if CB is allowed but not mandatory, and a value of 2 if CB is mandatory. Then a state's value on the index is increased by 1 if they are not RTW. This index thus has four values. The weakest union power states are CB prohibited and RTW and have a value of 0 (= 0 + 0). The strongest union power states are CB mandatory and not RTW and have a value of 3 (= 2 + 1).[7]

Figure 1b shows a state map of the United States by our first alternative teachers' union power measure of whether a state mandates collective bargaining, with CB mandatory states shaded dark gray and CB nonmandatory states (where CB is either prohibited or allowed, but not mandatory) shaded white. Figure 1c shades states from white to dark gray for the weakest to strongest union states according to our second alternative measure. While there are some exceptions, the geographic patterns of state union power using these alternative measures are similar to the pattern for the continuous measure shown in figure 1a.[8] We prefer the continuous in-

dex over the alternative measures, because it provides a much finer measure of teachers' union power with a unique value for each state, and thus more variation across states to exploit. However, we show that the pattern of results that we find is similar regardless of which teachers' union power measure we employ.

IV. Empirical Framework

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

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

where y_{ist} denotes an outcome of interest for district i in state s in year t; Rev_{ist} denotes state aid per pupil; $Union_s$ is a measure of the teachers' union power in state s; X_{is} is a vector of school district characteristics at baseline interacted with a linear time trend, θ_t; δ_i is a vector of school district fixed effects; λ_{rt} is a vector of Census region-by-year fixed effects; Q_{is} is a set of indicators for whether a district was in the first, second, or third tercile of the within-state distribution of school district median household income in 1980 (we discuss these indicators in more detail below); and μ_{ist} is a random disturbance term. In all specifications, we cluster the standard errors at both the school district and state-year level.[9]

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

[8]Appendix table 2 provides values by state for all three teachers' union power measures. The three measures are strongly positively correlated with

a correlation of 0.69 for the continuous and dichotomous measure, 0.75 for the continuous and four-value measure, and 0.89 for the dichotomous and four-value measure.

[9]We report the results clustering at the state level in panel C of appendix table 4.

In our most parsimonious specification, X_{is} includes 1986 district enrollment and 1980 district median income. We then add 1980 district fraction black, fraction urban, and fraction of adults 25 and older who have a bachelor's degree. We exclude time-varying characteristics because they could be affected by the SFRs (i.e., endogenous controls). Therefore, we include each characteristic interacted with a linear time trend to allow for differential trending by districts with different baseline values of these characteristics. We additionally include $X_{is}\theta_t Union_s$ to allow these trends to differ by state union power. Finally, in all specifications, we include an indicator for whether the district is subject to a binding tax or expenditure limit, given that such limits have been shown to affect local government fiscal behavior (see Dye & McGuire, 1997).

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

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

and the second model is identical to equation (2), but where the dependent variable is $Rev_{ist} \times Union_s$. In equation (2), SFR_{st} is an indicator for whether state s implemented an SFR in year t and all subsequent years, and $Q1_{is}$, $Q2_{is}$, and $Q3_{is}$ denote indicators for whether a district was in the first, second, or third tercile of the within-state distribution of school district median household income in 1980. We separate the effects of SFRs by within-state 1980 income terciles because reforms were designed to differentially have an impact on state aid for low- and high-income districts, with the goal of equalizing school funding.[10] Given that other factors could be changing over time across these district terciles, we include $Q_{is}\theta_t$, the tercile dummies interacted with a linear time trend in equations (1) and (2), to allow for differential trending across these terciles.

A. Dynamic Event Study Specifications

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

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

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

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

V. Results

We begin our analysis by showing that SFRs led to exogenous increases in state aid. Specifically, we estimate the event study model from equation (3) for the full sample of school districts and also separately for school districts in each within-state median income tercile. We then plot the estimated γ_j's and associated 95% confidence intervals. Figure 2a (all districts) shows that after an SFR, state aid increases to between $500 and $1,000 per pupil above the prereform level and remains at this level through at least ten years after the reform. Importantly, there is no evidence of trending state aid prior to SFRs. Figure 2b shows more dramatic effects for districts in the bottom income tercile, where state aid increases by between $1,000 and $1,500 per pupil. Figures 2c and 2d show the effects for the middle- and top-income tercile districts, where both groups experience increases of between $500 and $850 per pupil, though the effects are not statistically different from 0 for the top-tercile districts. Importantly, there is no evidence of trending state aid prior to the reforms in any of the figures.[11] Having established that the timing of SFRs appears to have been exogenous, we move

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[10]Our results are robust to using a just-identified model that includes only the bottom-tercile SFR effect and its interaction with union power as instruments (see panel B of appendix table 4).

[11]Appendix figures 3 and 4 present similar event study pictures, plotting state aid, total revenue, local revenue, and current expenditures at the 25th and 75th percentile of state union power. In no case do we find evidence of differential pretrends by union power.

FIGURE 2.—EFFECTS OF SCHOOL FINANCE REFORMS ON STATE AID, BY DISTRICT INCOME TERCILE
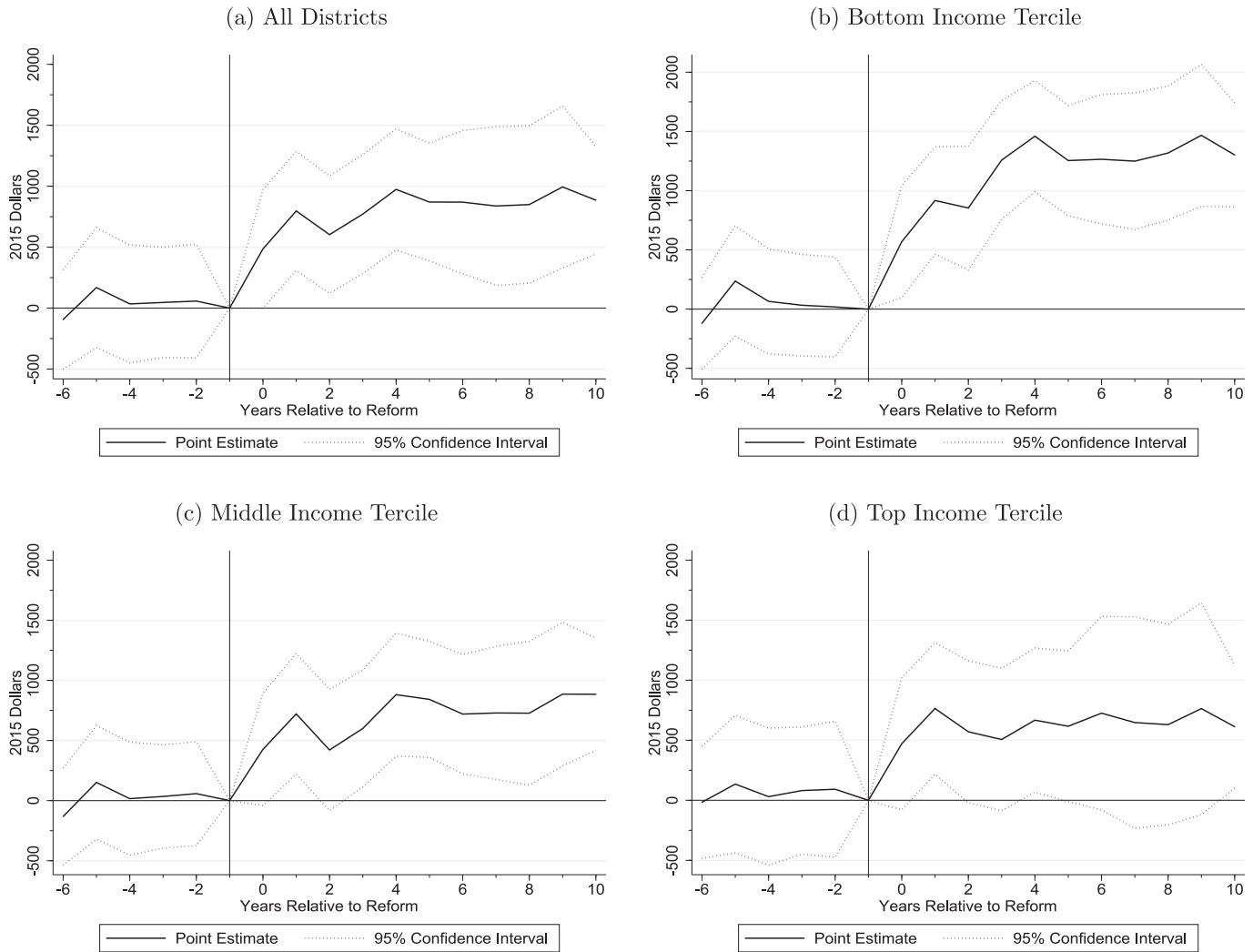
Panels show event study estimates of the effects of school finance reforms on per pupil state aid to school districts, by 1980 district income tercile. Solid lines are point estimates, and dotted lines are 95% confidence intervals.

to our two-stage least-squares (2SLS) framework to estimate the effects of SFR-induced increases in state aid.

A. Effects of State Aid on Revenues and Expenditures

We present estimates from the second stage of our instrumental variables (IV) analysis in table 2.[12] Columns 1 and 2 in panel A show the effects of a SFR-induced $1 increase in state aid on school district total revenue. Before adding the expanded controls, the results reported in column 1 reveal that for a state with the mean value of union power (index = 0), total revenue increases by 64 cents with every $1 increase in state aid, while a 1 SD increase in teachers' union power leads to a 32 cent larger increase in total revenue. This pattern

of results is similar after adding the expanded controls: a 68 cent increase at the mean level of union power and a 30 cent larger increase given a 1 SD increase in union power (column 2). These results demonstrate that while total revenue goes up by two-thirds of a dollar for every dollar increase in state aid at the mean level of union power, there is substantial heterogeneity in the degree of crowd-out depending on the strength of a state's teachers' union.

As property tax relief is the likely source of crowd-out, we next examine the effects of increased state aid on local revenue (table 2, columns 3 and 4). Using our preferred specification with the additional controls, districts in a state with mean teachers' union power reduce local revenue by 29 cents for each additional $1 of state aid, with a 27 cent smaller reduction (i.e., only a 2 cent reduction) in states with teachers' union power 1 standard deviation higher and a 0.56 cent reduction (29 + 27 cents) in local revenue among states with teachers' union power 1 standard deviation lower. These results explain most of the heterogeneity in total revenue

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[12]First-stage results, presented in appendix table 3, match closely with those seen in figure 2. The first-stage F-statistic for the regression of state aid on the instruments is 23, and for the regression of state aid interacted with union power on the instruments, it is 36.

TABLE 2.—EFFECTS OF STATE AID BY TEACHER UNION POWER

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

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

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

Finally, we examine the extent to which these revenue effects translate into effects on education expenditures. We find that an SFR-induced $1 increase in state aid translates into a 50 cent increase in current education expenditures at the mean level of state teachers' union power (table 2, column 6). This is similar to the mean flypaper effect estimated in the earlier SFR literature (Card & Payne, 2002). However, we find that the increase is 19 cents larger (or smaller) given a 1 SD higher (or lower) level of teachers' union power, suggesting substantial heterogeneity in the flypaper effect by the strength of a state's teachers' unions.

In figures 3a to 3c, we plot the estimated coefficients reported in table 2 at each vigintile (i.e., 20 percentiles) of the union power index.[13] Figure 3a presents the results from this exercise where total revenue is the outcome. For states with very low teachers' union power (near the 10th percentile), total revenue increases by only 10 cents for every $1 of SFR-induced state aid. In contrast, for states with very high union power (90th percentile), total revenue increases nearly dollar-for-dollar with increases in state aid. The heterogeneity in

total revenue across union power percentiles is explained by heterogeneity in local revenue. In states near the 10th percentile of union power, school districts reduced local tax effort by about 80 cents for every $1 of SFR-induced state aid, while in states near the 90th percentile of union power, there is very little change in local taxing effort due to SFR-induced increases in state aid (figure 3b). Finally, the heterogeneity in total revenue across the union power distribution also translated into similar heterogeneity in educational expenditures (figure 3c). Taken together, the results reported in table 2 and figures 3a to 3c reveal that differences in state teachers' union power were highly influential in shaping the extent to which the state aid increases from SFRs translated into changes in total revenues and expenditures for education.[14]

B. Boosting Teacher Compensation or Shrinking Class Size

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

[13]The teachers' union power distribution is skewed such that the top of the distribution is 1 SD above the mean, and the bottom of the distribution is 2SDs below the mean. We report in the bottom two rows of table 2 the coefficients and standard errors at the 25th and 75th percentiles of union power.

[14]Appendix table 4 presents OLS effects of state aid. Similar to Jackson et al. (2016), we find that the OLS results are strikingly different from the instrumental variable estimates.

FIGURE 3.—EFFECTS OF SCHOOL FINANCE REFORMS BY STATE TEACHER UNION POWER PERCENTILE

(a) Total Revenue

(b) Local Revenue

(c) Current Expenditures

(d) Class Size

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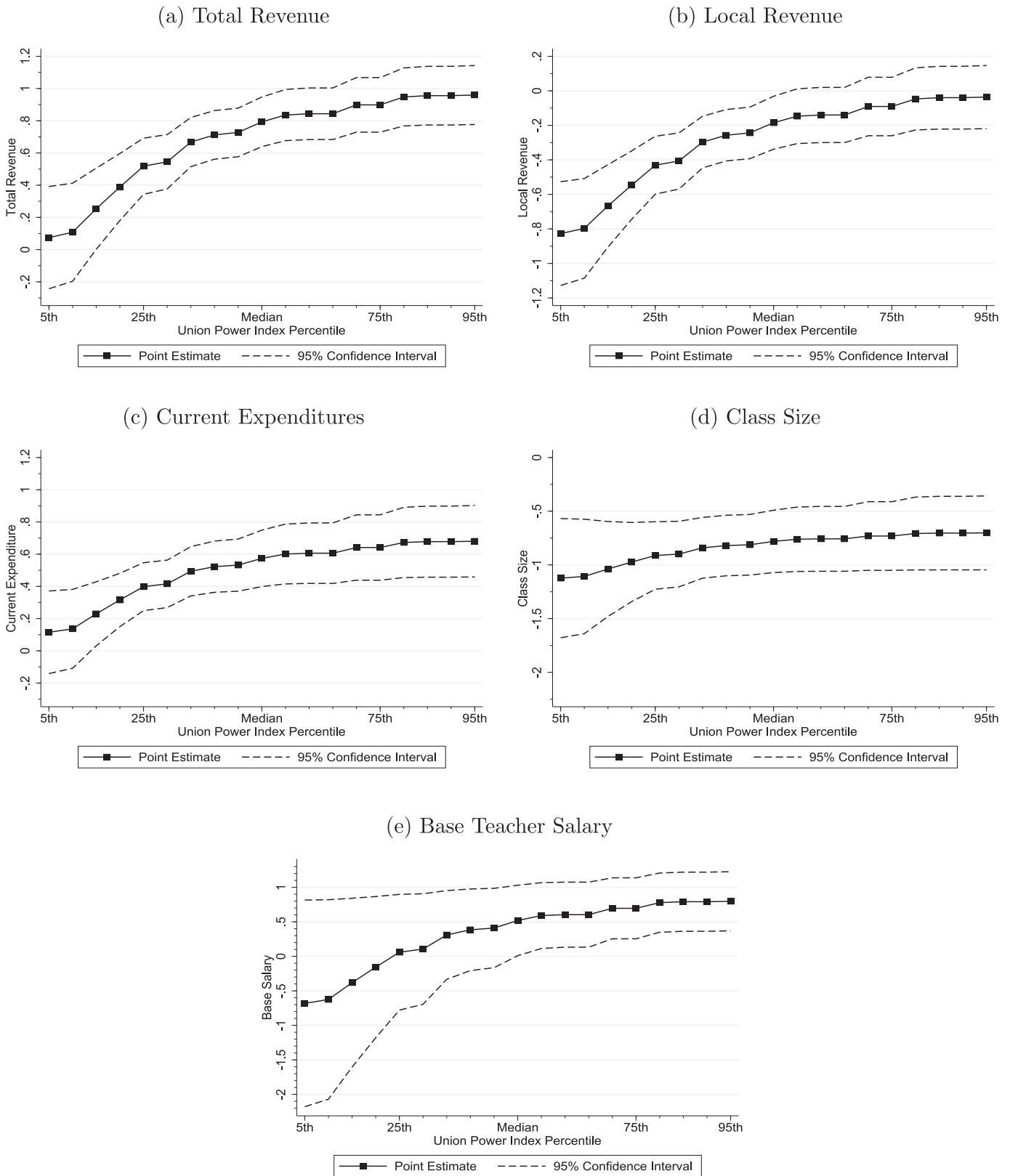
Each figure shows point estimates (solid line) and 95% confidence intervals (dashed lines) from 2SLS regressions of the dependent variable on state aid per pupil and aid interacted with our continuous state teacher union power index. The panels show the calculated point estimate at percentiles of the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

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), our measure of class size. A $1,000 increase in state aid reduces the PTR by 0.84 pupils among districts in a state with the mean value of union power (table 2, column 8, panel A). This represents a 5.2% decrease in class size relative to the sample mean of 16.3 students.

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

We next examine the effects of SFR-induced state aid increases on teacher compensation. Teacher salaries are typically a lock-step schedule based on level of experience and education. While district average teacher salaries are provided in the CCD, these conflate changes to the teacher salary schedule with changes in the hiring of new teachers, who are usually paid less than the average teacher in the district. Information on teacher salary schedules is not available in our primary CCD data, so we use salary schedule information from the Schools and Staffing Survey (SASS), which surveys a random cross-section of school districts every few years about staffing, salaries, and other school, district, teacher, and administrator information. We focus on base teacher salary, which is available in every wave and is particularly informative about average teacher salaries given the high rate of teacher attrition and relatively large degree of compression in teacher wages. Unfortunately, given the limited number of years and overlap of districts across waves, we lose about 91% of our sample size.[15] Consequently, we exclude the controls interacted with the linear time trend, given the limited number of years in the sample with which to estimate the trend.

We find that a $1 increase in state aid leads to a statistically insignificant 32 cent increase in teacher salaries for districts in a state with mean teachers' union power and a statistically significant 51 cent larger increase for districts in states with 1 SD higher teachers' union power. Consistent with our basic conceptual framework, stronger teachers' unions appear to focus the increases in education expenditures more on in-

creasing teacher salaries than on hiring new teachers. Taken together, these findings suggest that teachers' unions affect not only the fraction of SFR-induced increases in state aid that pass through to spending but also the allocation of the spending increases across inputs.

C. Alternative Measures of State Teachers' Union Power

While we prefer the continuous measure of state teachers' union power, we examine whether the results are robust to using our alternative measures of state teachers' union power that avoid any possible concerns about the endogeneity or subjectivity of the continuous measure. Our first measure is simply an indicator variable for whether a state mandates collective bargaining (CB). Thus, in panel B of table 2, the main state aid term reflects the effect of a dollar increase in state aid for CB nonmandatory states. For CB mandatory states, the effect is calculated by adding the coefficients on the main and interaction terms. Our second alternative measure incorporates CB and RTW status, taking on four values from 0 (weakest union) to 3 (strongest). Thus, in panel C, the main state aid term reflects the effect of a $1 increase in state aid for the weakest union power states with a value of 0 on this index. For states with a value of 1 for the measure, the result is calculated by adding the coefficients on the main and interaction terms. The effects for the strongest states are calculated by adding the main coefficient to three times the coefficient on the interaction term.

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

D. Possible Teachers' Union Endogeneity

One concern with the results presented thus far is that our measures of teachers' union power may be correlated with state-specific unobservables that also influence education spending and the allocation of education resources. For example, state teachers' union power may be correlated with

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[15] Appendix table 5 shows the number of district observations by state and year used in this analysis. In panel D of appendix table 4, we show that the results for revenues, expenditures, and class size are robust to restricting to the SASS sample of district-years.

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

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

We use two different state border samples. First, we restrict the sample to counties where the county centroid is less than fifty miles from the nearest state border. This strategy includes some counties not adjacent to a state border in geographically small states and excludes some counties adjacent to a border in large states with geographically large counties. We alternatively restrict to only counties adjacent to state borders.[16]

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

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

$$C_{is,1990} = \rho_0 + \rho_1 Union_s + \gamma_b + \upsilon_{is}, \qquad (4)$$

where $C_{is,1990}$ denotes a 1990 characteristic of school district i in state s and γ_b is a border fixed effect. Since we analyze SFRs that occurred during the 1990s, we base our balancing test on predetermined district characteristics as of 1990. The

coefficient of primary interest in equation (4) is ρ_1, which represents the average difference in $C_{is,1990}$ by state union power among districts located close to the border. If focusing on the border discontinuity sample leads to a more homogeneous set of districts, then ρ_1 should be statistically insignificant or at least substantially smaller in magnitude when compared to estimates obtained from equation (4) that are based on the main sample of school districts and exclude the border fixed effects.

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

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

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

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

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[16]See appendix figure 5 for a county map of the United States with the border samples shaded gray.

TABLE 3.—STATE BORDER SAMPLE BALANCING TESTS

	Full Sample		Counties Less Than 50 Miles from State Border		Counties Adjacent to State Border	
	Union Coefficient (1)	p-Value (2)	Union Coefficient (3)	p-Value (4)	Union Coefficient (5)	p-Value (6)
County-level democratic vote shares						
1984	3.254**	0.011	−1.11	0.733	1.31	0.621
1988	3.340***	0.001	−0.59	0.869	2.64	0.348
1992	4.036***	0.000	0.73	0.752	1.98	0.299
1990 district-level characteristics						
Total population	−2,017	0.612	−1,578	0.757	1,327	0.786
Population density	92.16**	0.024	46.37*	0.063	60.56*	0.053
Number of households	−874	0.556	−658	0.735	443	0.811
Median household income	4602***	0.000	1,207	0.388	815	0.475
Fraction nonwhite	−0.039*	0.076	0.015*	0.086	0.004	0.679
Fraction below poverty	−0.029***	0.007	0.001	0.836	0.002	0.595
Fraction unemployed	0.009	0.508	0.006	0.248	0.007	0.234
Fraction population 65 plus	−0.002	0.658	0.002	0.467	0.007**	0.049
Fraction less than HS	−0.041***	0.000	−0.005	0.508	−0.007	0.362
Fraction HS	0.004	0.615	−0.011	0.357	−0.005	0.608
Fraction some college	0.013*	0.057	0.005	0.557	0.002	0.691
Fraction BA or higher	0.024***	0.000	0.011	0.300	0.010	0.309
Fraction homeowner	−0.004	0.625	−0.002	0.779	−0.003	0.640
Number of districts	9,177		5,148		3,154	

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

TABLE 4.—STATE BORDER SAMPLE ANALYSIS

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

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

strategy, by adding terms $Rev_{ist} \times Char_s$ and estimating specifications of the form:

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

where $Char_s$ includes one of three baseline state characteristics that are shown in columns 1 and 2 of table 3 to be highly correlated with state teachers' union power: 1988 presidential Democratic vote share, 1990 median income, and 1990 fraction of adults 25 years of age and older with a bachelor's degree or higher. Note that because $Char_s$ is interacted with state aid, we instrument for the interaction term $Rev_{ist} \times Char_s$ using a first-stage specification that is identical to equation (2) except the dependent variable is now the $Rev_{ist} \times Char_s$ interaction term.[17] If β_2 withstands the addition of these union power correlates interacted with state aid, this provides reassurance that β_2 identifies the effects of union power and not unobserved characteristics associated with union power.

Panel A of table 5 presents results based on specifications where we interact state aid with the state share voting

[17]There are three additional instruments: $Q1_{is} \times SFR_{st} \times Char_s$, $Q2_{is} \times SFR_{st} \times Char_s$, and $Q3_{is} \times SFR_{st} \times Char_s$.

TABLE 5.—EFFECTS CONTROLLING FOR HETEROGENEITY BY STATE-LEVEL UNION POWER CORRELATES

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

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

Democratic in the 1988 presidential election. While the point estimates change somewhat in magnitude, controlling for heterogeneity by Democratic vote share does not change the pattern of results. In panel B, we interact state aid with state 1990 median income, and in panel C, we interact state aid with 1990 fraction BA or higher. Again the results are largely robust to both of these additions.[18]

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

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

E. School Finance Reform Coding and Sample Restriction Robustness

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

Finally, recall that we drop Michigan and Wyoming because they adopted reforms that effectively eliminated local discretion over funding. However, a number of states also adopted reforms that imposed a limit on how much a district may spend on education. As Jackson, Johnson, and Persico (2014) noted, such reforms are likely to reduce spending per pupil particularly for the highest-income districts in a state for which spending limits are most likely to be binding. To examine that possibility, we drop districts in the top tercile

TABLE 6.—EFFECTS BY EXPENDITURE TYPE

| | Total Expenditures | Current Expenditures | | | Capital Outlays |
| | | All Current | Instruction | Noninstruction | |
	(1)	(2)	(3)	(4)	(5)
State aid	0.656***	0.498***	0.371***	0.272***	0.162***
	(0.088)	(0.078)	(0.056)	(0.062)	(0.060)
State aid × union	0.200**	0.193***	0.098**	0.091**	0.043
	(0.078)	(0.066)	(0.043)	(0.044)	(0.045)
Estimated effect at:					
25th percentile of union index	0.552***	0.398***	0.321***	0.225***	0.140***
	(0.092)	(0.076)	(0.055)	(0.062)	(0.053)
75th percentile of union index	0.804***	0.641***	0.444***	0.339***	0.194**
	(0.110)	(0.104)	(0.071)	(0.076)	(0.081)
Sample mean	10,987	9,347	5,749	3,463	1,019
Observations	181,756	181,756	181,756	181,636	180,822
Expanded controls	Yes	Yes	Yes	Yes	Yes

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

of 1980 household income from our sample. Our results are generally robust to all of these checks.

F. Effects by Expenditure Type

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

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

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

ing teacher compensation than did districts in weak union states.

We also find heterogeneity by teachers' union strength in the effects of SFR-induced increases in state aid on noninstructional expenditures (column 4) and on capital outlays (column 5), though the interaction of state aid and union power is statistically insignificant for the latter. Districts in strong union states see a 34 cent increase in noninstructional spending and a 19 cent increase in capital outlays for every $1 increase in state aid compared to only a 23 cent and 14 cent increase, respectively, in weak union states. Thus, while there are important differences in how teachers' union power affects instructional spending, there are also important differences across these other spending categories. This suggests that teachers' unions prefer not only higher teacher salaries, but also increases in items that may improve working conditions, such as curricular and administrative support and infrastructure improvements.

G. Effects on Student Achievement

To examine whether the differences in spending by teachers' union power translated into differences in student performance, we use restricted-access microdata from the NAEP, which provides representative samples of math and reading test scores in grades 4 and 8 from over 100,000 students nationwide every other year since 1990. Following Lafortune et al. (2018), we standardize the individual scores by subject and grade to the distribution in the first tested year and then aggregate the microdata to the district-subject-grade-year level, weighting the individual scores by the individual NAEP weight.[19] Unlike effects on expenditures, effects of the reforms on achievement are not expected to appear

[19]For more details about the NAEP microdata, see the online appendix, Lafortune et al. (2018), and Jacob and Rothstein (2016). Our results are robust to aggregating the data to the state-by-district income quintile-by-subject-by-grade-by-year level as in Lafortune et al. (2018) (see appendix table 8).

TABLE 7.—REDUCED FORM EFFECTS OF SCHOOL FINANCE REFORMS ON STUDENT ACHIEVEMENT

	All Districts		Bottom Tercile		Top Tercile	
	(1)	(2)	(3)	(4)	(5)	(6)
Years postreform	0.007***	0.009***	0.009***	0.011***	0.004*	0.006***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Years postreform × union		0.004*		0.006**		0.002
		(0.002)		(0.003)		(0.002)
Estimated effect at:						
25th percentile of union index		0.007***		0.008***		0.005**
		(0.002)		(0.002)		(0.002)
75th percentile of union index		0.012***		0.016***		0.007***
		(0.003)		(0.004)		(0.003)
Observations	64,901		17,159		27,328	
Expanded controls	Yes	Yes	Yes	Yes	Yes	Yes

The sample is at the district-subject-grade-year level. Each column presents results from a separate regression of weighted mean NAEP scores on a linear postreform trend (columns 1, 3, and 5), and the postreform trend interacted with our measure of union power (columns 2, 4, and 6). All specifications include the controls and fixed effects listed in the table 2 notes. Robust standard errors, clustered at both the district and state-year level, in parentheses. Significant at *10%, **5%, ***1%.

immediately. Consequently, we modify our main specification in two ways. First, we focus on the reduced-form impact of the reforms instead of instrumenting for spending. Second, we allow the impact to evolve linearly during the postreform period instead of including a single post indicator as we do in our first-stage analyses. Specifically, we estimate the following specification,

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

where $NAEP_{ijgst}$ is the average score in district i, in tested subject j and grade g, in state s, and year t; $YearsPost_{st}$ equals 0 for nonreform states and for reform states prior to the reform, and equals the number of years since the reform in reform states; π_{jg} is a vector of subject-by-grade fixed effects; ζ_{ijgst} is a random disturbance term; and all other terms are as defined in equation (1). As before, we cluster the standard errors at both the district and state-year level.

Table 7 presents the reduced-form effects of SFRs on achievement. Without including the union interaction, we find an overall impact of SFRs of 0.007 SD per year, or 0.07 SD ten years after a reform. This impact is driven by increases of 0.009 SD per year in districts in the bottom tercile of within-state median income. These effects, however, mask important heterogeneity. When we include the union interaction for all districts, there is a 0.009 SD per year impact at the mean level of state teachers' union power and a statistically significantly larger 0.004 effect for 1 SD higher union power. For low-income districts, the effect is 0.011 SD per year at the mean union power level and 0.006 SD greater for a 1 SD higher level of union power. This translates to an effect of 0.008 SD per year, or 0.08 SD ten years postreform, for weak teachers' union states (25th percentile). For strong union states (75th percentile), the effect is twice as large, or 0.016 SD per year (0.16 SD higher ten years postreform).

The effect among the top income tercile districts is smaller and not significantly different by teachers' union power.[20]

In appendix figure 6, we show event study pictures that, as in Lafortune et al. (2018), show no pretrend in achievement followed by a steady postreform increase in test scores driven by the lowest-income districts. As in table 7, a gap in test scores between weak and strong union states emerges after the SFRs, with the effects concentrated among the lowest-income districts. These findings suggest that the larger expenditure increases in strong teachers' union states in response to SFRs translated into larger student achievement gains.

While a thorough exploration of the mechanisms behind these achievement impacts is difficult in this context, we conduct back-of-the-envelope calculations to understand the extent to which effects are due to changes in class size versus spending on other inputs. We use the results from the Tennessee STAR class size experiment, which reduced class sizes by 33% and increased achievement by 0.22 SDs, as a benchmark to estimate the fraction of our achievement results that are due to class size reductions (Krueger, 1999). We find that almost half of the achievement increase among weak union states (25th percentile) is due to class size reduction, while only a quarter of the increase is due to class size in strong union states (75th percentile).[21]

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

[20] Appendix table 9 shows that the results are not sensitive to the inclusion of controls.

[21] See the online appendix for calculations.

VI. Conclusion

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

Our results suggest 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 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 substantially reduced local tax effort, with education expenditures increasing less than 25 cents on the dollar. Furthermore, the school spending in strong teachers' union states was allocated more toward increasing teacher salaries, while districts in weaker teachers' union states spent the money primarily on hiring new teachers. We find that achievement gains due to the reforms were significantly larger in strong teachers' union states than they were in weak teachers' union states.

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

That said, our results are subject to several caveats. First, it is possible that our results are driven in part by the influence of strong teachers' unions on the specific design elements of reforms. Strong teachers' unions may have used their influence to advocate for specific structures in the school finance reforms that would discourage local crowd-out or level-up school spending. While we find little evidence that the type of reform implemented by states is correlated with state teachers' union power, it is nevertheless possible that strong unions influenced the design features of reforms in a way that affected both the amount of state aid that passed through to local expenditures and the allocation of those expenditures. Second, some states bundled other policy changes into their school finance reform efforts. The differential achievement effects by union power that we identify may have been partly driven by unions advocating for (or against) other reforms, such as school accountability and school choice policies, that states implemented in conjunction with their school finance reforms.[22]

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

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

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